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Manufactured Science, the Attorneys' Handmaiden: The Influence of Lawyers in Toxc Substance Disease Research

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MANUFACTURED SCIENCE – THE ATTORNEYS’ HANDMAIDEN:
THE INFLUENCE OF LAWYERS IN TOXIC SUBSTANCE
DISEASE RESEARCH

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This dissertation marks the end of an extended journey. During the late decades of the twentieth century my firm and I represented major asbestos manufacturers in asbestos litigation. During that period, we retained well-regarded asbestos researchers as expert witnesses and collected thousands of documents related to the history of asbestos disease knowledge. Our Fortune 500 former asbestos manufacturing client joined us in assisting certain plaintiff counsel across the country to pursue other former asbestos product manufacturers not normally involved in asbestos litigation. Following my departure from my law firm in the mid-1990s, my former client allowed me to continue assisting plaintiff counsel in pursuing other former manufacturers. During this period I collected additional asbestos and silica related documents, and also took courses in biology and toxicology. I also held an interest in plaintiff asbestos cases in Massachusetts, Louisiana, and Texas. Upon retiring from active practice in the first decade of the twenty-first century, I retained an interest in the asbestos cases, while obtaining my masters in the History of Science, then becoming a doctoral student in history at Florida State University. Today, I still retain an interest in the Texas cases but have not been involved in their prosecution in any manner for several years.

As a master’s candidate at Oregon State University, I decided to use my collection of documents to write a history of attorney involvement in silica and asbestos medical research during the twentieth century. This history evolved into my master’s thesis and formed the foundation for this dissertation. I have now substantially rewritten and expanded my silica and asbestos chapters, as well as added additional chapters on three other well known toxic substances, tobacco, chromium, and benzene.

Needless to say, this effort has consumed both substantial time and exhaustive research. The individuals who assisted are too numerous to thank individually, but all of their help is greatly appreciated. A few deserve special recognition. Maggie Baumgardner, the archivist for the Johns
Manville Trust, provided generously of her time and knowledge during several visits to the Trust document repository—a warehouse that contains thousands, if not tens of thousands, of boxes relating to Johns Manville and historical asbestos events. Texas plaintiff counsel Mark Lanier generously provided access to his archive of Industrial Hygiene Foundation documents, as well as court documents related to Georgia Pacific attorneys’ recent foray into medical research. Prior to his firm’s settlement with Georgia Pacific, New Jersey attorney Jerry Kristal also provided court documents related to Georgia Pacific. Finally, and most importantly, I must thank the members of Florida State University’s interlibrary loan department. Over several years they sought and found tens, if not hundreds, of often-obscure medical and other historical articles used in my dissertation.

I also must thank all of my professors at Oregon State University and Florida State University. I can truly say I enjoyed all of their classes. They all increased my understanding of history immeasurably. In particular, I must thank Oregon State University Professor (now Emeritus) Mary Jo Nye, for agreeing to accept this ancient student into her program, and providing encouragement and good counsel. In the same vein, the members of my FSU committee—Michael Creswell, Kris Harper, Jim Jones, and Woody Wise—have been extremely supportive and helpful in improving my scholarship. They have undertaken their own daunting task in agreeing to read and comment on my long historical study. Ron Doel, my lead professor, has providing excellent guidance and advice throughout this process. He has been not only a mentor, but also a friend. His frequent and substantive edits have dramatically improved the dissertation’s clarity and flow. Any errors or mistakes that remain are mine and mine alone.

Finally I must thank my wife, MariAnna, for standing by me through this long and arduous process. At times, it must have seemed I was never around, as I struggled to obtain information about secretive events and assemble the far-reaching research into a narrative.
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ABSTRACT

Since the early twentieth century, manufacturers and distributors of toxic products have sought to discredit research linking their products with disease. At the same time they conducted research designed to demonstrate minimal risks associated with their products. Much of this activity came about by or through corporate retained attorneys, whose endeavors are the subject of this dissertation. Such attorney involvement has allowed for shielding undesired results through the court-sanctioned attorney right to secrecy. In many cases, this legal participation and even management of medical research has changed the topography of the medical literature, distorting it toward the null hypothesis for disease potential of the subject materials. This is because attorneys, whether they are defense or plaintiff, only sought credible evidence for their position at trial or in regulatory practice, not the advancement of science. Furthermore, the distortion is primarily one-sided, toward the defense of toxic substances. This results from the virtually unlimited financial backing defense lawyers have from large corporations, while plaintiff counsel are almost uniformly reluctant to spend their own money.

To date, only limited historical accounts about this attorney effort have been published, largely because of the veil of secrecy created by attorney privileges. This dissertation seeks to look behind the veil to examine the full range of legal activities in case studies of five substances—silica, tobacco, asbestos, chromium, and benzene. These activities include lawyers identifying, hiring, and controlling experts, preparing contracts for research that limited public disclosure, managing research, editing final research papers, harassing opposing experts, and manipulating regulations and workers’ compensation laws. This lifting of the veil is possible primarily through disclosures found in bankruptcies and legal proceedings, assets not normally considered by historians of science.

The activities of lawyers in manufacturing science had varying degrees of success as they evolved over the course of a century. During the early decades of the twentieth century, attorneys
were largely successful in limiting victims’ recovery for silicosis and keeping it out of the public eye. Similarly, at first, cigarette and asbestos product manufacturers were successful in limiting litigation’s effect on the bottom line. However, a growing number of public health advocates and plaintiff attorneys brought these controversies increasingly into the public legal arena, resulting in massive settlements by the tobacco companies and bankruptcies of many asbestos product manufacturers. The settlements and bankruptcies also provided a treasure trove of documents, many of which detailed extensive involvement of lawyers in the manipulation of medical research.

To date, chromium and benzene manufacturers, as well as certain asbestos product manufacturers, have been more successful in limiting damage through lawsuits and regulations. In part, this is because of the newest evolution in research tactics. During the last two decades of the twentieth century, “Litigation Support Firms” began undertaking an increasing amount of the attorney-managed research. These companies worked hand in hand with attorneys, as they transformed the peer reviewed medical literature on toxic substances by publishing carefully structured industry friendly research (and reviews of past research) in peer-reviewed, but often industry controlled, journals. Even when researchers have been free to publish their findings, the approval was often subject to final approval of a report exclusively provided to the client. Thus, the public articles rarely disclosed any hazard. On occasion, the researchers published the same data in slightly altered forms in two to four publications, thus slanting the entire balance of the peer review literature.

Attorney involvement in medical research is a fundamental problem in the production of medical knowledge. The ability to hide and manipulate science has delayed recognition of hazards such as silica, tobacco, asbestos, chromium, and benzene by decades. Today, it continues to skew the understanding of toxic substance diseases.
CHAPTER 1

INTRODUCTION

This dissertation is about scientific controversy – more specifically about the changing nature of scientific controversy in the field of medical research during the twentieth century. Scientific controversy is not new to the twentieth century. It likely was present with the astronomers of Mesopotamia and the doctors of Egypt. As the concept of scientific theory developed in ancient Greece, opposing theories arose as to the nature of matter. Even the greatest controversy of the Scientific Revolution, the opposing views of an earth-centric and heliocentric solar system, actually first arose in Hellenistic times with the competing views of the Ptolemaic solar system and the heliocentric solar system of Aristarchus of Samos.¹

Throughout the twentieth century, historians and philosophers of science have increasingly investigated these scientific controversies, examining their nature, causes and resolution. More than sixty years ago, Columbia University professor Robert K. Merton, perhaps the best known American sociologist of science, set forth a theory of how science and its controversies operate. During the 1930s, he gradually came to believe that science is a social institution with a normative framework. His 1938 classic paper, “Science and the Social Order,” laid the theoretical groundwork for the modern study of the history of science.²


In his paper, Merton contended that the scientific “ethos,” or traditional values and norms of scientific research, included four imperatives, which he described as universalism, communalism, disinterestedness, and organized skepticism. Universalism is that knowledge which transcends cultural boundaries. Communalism refers to the common ownership of the results of scientific investigation. Disinterestedness requires that scientists conduct research without consideration of personal gain or bias. Finally, organized skepticism results from applying socially established rules of enquiry that require independent critical examination. Using these four values, all scientific research could be understood and placed in historical context.

To Merton, good science required public disclosure. The secrecy found in totalitarian regimes such as Nazi Germany was inimical to his norms of science. Science could not flourish under a totalitarian regime since researchers could not receive credit for discoveries and their work could not be scrutinized and either validated or falsified. Another prominent mid-century philosopher of science, Sir Karl Popper, noted a sharp contrast between facts as gathered by scientists and in the “facts” as presented in judicial proceedings. To Popper, the important trait of science was its falsifiability. He argued that scientists should not be seeking to gather supporting evidence for their hypothesis, as occurs in a court of law, but rather should rigorously test their theories to expose any flaws. Scientists should actually go to war with their own theories. Hypotheses and theories should not be held “dogmatically.” “On the contrary, we try to overthrow them… we try to prove that our anticipations are false.” Like Merton, he believed that in pursuit of this goal, scientists should advance their work in the public forum, allowing it to be fully scrutinized.

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by fellow scientists. “The advance of science is not due to the fact that more and more perpetual experiences accumulate in the course of time ... bold ideas, unjustified anticipations, and speculative thought are our only means of interpreting nature: our only organon, our only instrument, for grasping her. And we must hazard them to win our prize. Those among us who are unwilling to expose their ideas to the hazard of refutation do not take part in the scientific game.”

Thus, like most sociologists and philosophers of science of the mid twentieth century, both of these individuals naturally assumed numerous controversies accompanied the progression of science, with research requiring openness—thus distinguishing it from legal pursuit of evidence.

By the latter half of the twentieth century, attention focused more closely on the manner in which factors outside of strict science can also influence these controversies. Historians of science now recognize that many, perhaps most controversies of science, have a strong component of social, political, and other outside influences. Perhaps the archetypal book in this area is Steven Shapin and Simon Schaffer’s *Leviathan and the Air Pump*. In the medical research area, Bruno Latour similarly demonstrated that numerous external factors influenced the acceptance of Louis Pasteur’s theories.

In recent decades, historians recognized not only the outside influences in these earlier events, but also rapid changes in the nature of scientific controversy being brought about by the emerging phenomena of big science and big business, with the profit motive and litigation increasingly entering into the arena of scientific controversies. Legislation, regulations and Supreme Court decisions during the 1980s provided new impetus for this trend. Scientific research became more and more commercialized. Today, secrecy of research has often become more the norm than

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communitarian values. As a result, historians examining scientific controversies have at times probed how conflicts of interest affect scientific controversies, peer review, and publication of scientific articles. At least two sociologists have proposed a detailed and complex protocol for this analysis.

One important consequence is that investigations of scientific controversies have become more complex and detailed. Ernan McMullin, John Cardinal O’Hara Professor of Philosophy (Emeritus) at the University of Notre Dame, has focused considerable amounts of his research on these very issues of contemporary philosophy of science. Rather than the standard description of internal versus external considerations in scientific debates, McMullin prefers explaining such issues by the epistemic, nonstandard epistemic, and non-epistemic factors embodied within the disagreements.

McMullin defines epistemic factors as those necessary for the specific quest for knowledge. They include facts, theories, scientific principles, methodological principles, and even philosophical positions, such as in the Einstein-Bohr debate of the 1930s. In certain cases, these positions can even include theological considerations. McMullin deals with this very broad palette by dividing epistemic factors into standard and nonstandard categories. His distinctions between standard and nonstandard epistemic factors can, at times be vague and flexible, often depending “on the notion of science employed by the person using the distinction . . . It is an evaluative distinction, either imposed on the grounds of present usage or proposed as a reconstruction of what it is that made the history of science develop in precisely the way it did.” One such nonstandard factor is the question of the role of the precautionary principle in regulations, particularly public health and occupational

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disease regulations. Finally, non-epistemic factors include everything else that affects scientific controversies, for example, personality traits, monetary considerations, institutional pressures, political influences, and personal hostilities, among others.\textsuperscript{11}

Scientific controversies containing these imbedded factors can occur not only over questions of fact or theory, but also principle. When the principle at issue involves nonstandard or non-epistemic factors such as moral, political, or ideological principles, McMullin characterizes the controversy as mixed. Given the broad and complex nature of these controversies, they can be the most difficult to resolve. This is particularly the case in the controversy I have examined—when corporate profits collide with the chronic, long-term health of its workers and the public.\textsuperscript{12}

External influences have had dramatic impacts on medical research and regulation of occupational diseases in the twentieth century. As the next chapter and the remainder of this dissertation will demonstrate, since the early 1990s numerous articles and books have catalogued a wide range of such influences, yet not all have been fully explored. The distinctiveness of this dissertation comes from the specific nature of the attorney influences I am describing. The fundamental reason for these influences is a non-epistemic factor—profits—with attorneys being its agents.

Since shortly before the dawn of the new millennium, a growing body of literature began lamenting the erosion of integrity in public health science. This literature pointed to two causes of the erosion: political pressure and a growing willingness of industry to challenge the scientific basis of health regulations. As one 2006 article stated, “Industries themselves are also increasingly active

\textsuperscript{12} McMullin, “Scientific Controversy,” 75.
in influencing scientific research, through challenges to the scientific basis of health regulations, targeted funding of research designed to answer particular questions and not to answer others, and participation on research and decision making panels.”

Yet, this trend is not new. Already by the early twentieth century, increasingly larger businesses and corporations played prominent roles in the economy and politics. In virtually every case, they sought to maximize their profits. To accomplish this, they not only had to sell more products but also limit costs, including labor and costly regulatory requirements. Where these corporations intersected with the public and occupational health spheres, two opposing philosophies competed for supremacy: the precautionary principle of “first do no harm,” and the principle that there should be no regulation without “sound science.”

The precautionary principle is best known for its use in the field of environmental science. The Rio Declaration of 1992 and the Wingspread Statement of 1998 describe it as follows: “when an activity raises threat of harm to human health or the environment, precautionary measures should be taken even if some cause and effect relationships are not fully established scientifically.” Industry, on the other hand, has consistently argued that “sound science” is the only legitimate basis for public health science. Yet, sound science can mean many things, especially for public health.

Drug regulations provide an excellent example of the conflict. Pharmaceutical companies argue that their studies provide a sufficient basis to demonstrate both the safety and effectiveness of their products. On the other hand, they denounce independent studies demonstrating harm caused by their product as not sufficient to require regulation or increased precautions without further study. While these tactics might seem at odds with one another, they have one thing in common: in

both cases, industry is following the course of action that means greater profits. This is not difficult, since medical research and diagnosis are fraught with uncertainties. Thus, with the appropriate incentive, it is relatively easy to influence the outcome of a medical study in one direction or the other. One commentary has succinctly described the problem.

Regulated parties who sponsor research that informs regulation of their products or activities have incentives to influence the research in ways that ensure favorable outcomes. Yet since research design and reporting is inherently layered with discretionary judgments that are difficult to discern without replicating the research directly, systemic biases in these judgments are difficult to detect from the outside. As long as sponsors control the research at some or all points in the research process, adverse results can be suppressed and the design and reporting of experiments can be biased in ways that produce results that support the sponsor’s interests, rather than offer a disinterested examination of potential harms.\(^\text{15}\)

Occupational disease research subject to such disputes can take many forms. At the heart, of the controversy, however, is the most critical and devastating occupational disease possible, cancer. Thus, cancer provides both the most important aspect of research being conducted in occupational diseases, as well as an excellent microcosm of occupational disease research. Simply put, the driving force of private cancer research involving potentially hazardous corporate products is profits, with little regard to the precautionary principle.

Devra Davis is the Director of the Center for Environmental Oncology at the University of Pittsburgh Cancer Center and a Professor in the Department of Epidemiology at its Graduate School of Public Health. From her family she has first-hand experiences with the heartache of cancer. In her *The Secret History of the War on Cancer*, Davis argues that for decades the United States has taken the wrong approach to this war. Virtually all research money is being spent on finding cures, when the real effort should be in finding the causes. Davis believes that environmental causes, from tobacco and asbestos to chemicals and drugs, have fueled the dramatic increase of cancer

during the twentieth century. In this book, she presents dozens of examples of these causes, as well as industry’s continuing efforts to block or slow research into the consequences of their products and processes.\textsuperscript{16}

One factor is evident in both of the cancer research alternatives, the profit motivation. In developing cures, or even partial cures or life-lengthening therapies, pharmaceutical companies stand to make vast profits. On the other hand, identifying causes of cancer does not enhance profits in the short run—rather, it decreases them. This occurs in two ways. First, most of the causes are toxic substances produced by industry. Eliminating the substance will eliminate the company’s profit for that substance. Second, and just as important, eliminating the substance will also frequently eliminates the cheapest method for other companies to produce their products, thus increasing the overall cost of production.

Since most medical research is conducted with private rather than public funding despite the existence of the National Institutes of Health (NIH), it is unsurprising that the profit motive prevails. Given the overall lack of public funding for medical research, in many ways the United States is fortunate that the profit motive is so strong. As \textit{JAMA} editor Catherine DeAngelis emphasized in her 2000 editorial, private research is vitally important to medical research.

\begin{quote}
Balance must be maintained between the need for research projects to be reasonably funded and performed by the best possible investigators and the relative paucity of public funds for clinical research. In 1999, the National Institutes of Health (NIH) provided $17.8 billion for research, and the major proportion was expended for basic research; the top 10 pharmaceutical companies spent $22.7 billion, primarily on clinical research…\textsuperscript{17}
\end{quote}

In a subsequent 2006 editorial, DeAngelis also emphasized the essential nature of commercial research. “The influence of commercial interests on medical science is far-reaching but, to a great degree, essential,” she wrote. “The discovery of new medications, devices, and techniques

\textsuperscript{17} Catherine DeAngelis, “Conflict of Interest and the Public Trust,” \textit{JAMA} 284 (November 1, 2000): 2237.
is funded primarily by for-profit companies; testing new modalities of treatment is funded primarily by for-profit companies...Ideally the products discovered, tested, and produced will be beneficial to many individuals for whom the products will be prescribed and who will purchase them, returning a healthy profit for the company.” But she then introduced a note of caution. “Now comes the potential problem. In some instances, the marketing goal of a company dominates the scientific aspect of the company-funded research.”

Since private pharmaceutical companies obviously have an interest in finding cures rather than causes of cancer, the allocation of the public and nonprofit funding is perhaps even more important than the preponderance of private funding in medical research. Yet, in 1988 the American Cancer Society allocated substantially less than one percent of its budget to the study of environmental causes of cancer.

While there are, in some circumstances, regulations requiring a certain amount of research by private corporations to determine the effects of their products, private industry has limited—and certainly no short-term—financial interest in developing studies to determine either if their product has long-term toxic effects or if a non-toxic substance is more effective. For example, in a study of articles published in *Weed Science* from 1983 until 1993, Sheldon Krimsky, professor of Urban & Environmental Policy & Planning at Tufts University, found that articles on herbicides vastly outnumbered articles on nonchemical weed control.

Public reviews of chemicals are, if anything, even lower on the priority list. Public funding for chemical reviews is extremely limited. Rather, OSHA and the EPA commonly rely on industry data for their regulations. The effectiveness of this approach is highly questionable. In 2001 the General Accounting Office (GAO) concluded that the EPA’s Science Advisory Board (SAB) had

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20 Ibid.
neither the information nor the procedures to ensure its oversight committees were properly independent and balanced.\textsuperscript{21}

This is not to say that the civil courts and regulatory bodies have not been active in this field. As the twentieth century progressed, medical controversies received increasing attention in the courts—particularly courts dealing with personal injuries—and regulatory bodies. Commentators have remarked about this, attributing it, at least in part, to the increased interest in the control and prevention of illness as well as human research and experimentation. However, few personal injury courts have examined law and public policy intrusions. Generally, they only attempt to deal with causal relationships between illnesses and their causes, while establishing responsibility and accountability in cases of injury or death.

This issue of law and policy highlights the core subject of this dissertation: the effects of attorneys on medical research. Medical research and attorney evidentiary requirements have quite distinct and differing needs. As one commentator has noted, “it often appears that law and medicine are making appeals to quite different notions of causality or causation.”\textsuperscript{22} Yet, as attorneys obtain evidence to defend their lawsuits or fight regulatory actions in courts, it can dramatically affect the medical, scientific, or public notion of causality. As will be seen in this Dissertation, there can be no doubt that attorneys are deeply imbedded in the formulation of medical research and medical efforts to determine the causation of diseases.

What is missing from most historical examinations of corporate medical practice, occupational disease, and public health research, is a close analysis of a key component of industry’s

efforts to forestall regulation and the public’s outcry against health and safety issues in the workplace—the attorneys who lobbied governments, defended lawsuits, and, yes, even coordinated and managed research designed to demonstrate the corporate position of the lack of harm from the company’s products and processes. The introduction of biases caused by financial interests is made easier when a cloak of legal, court-approved secrecy surrounds the formulation of scientific studies and any nonconforming results can be hidden. This is the case when attorneys and their court sanctioned privileges of secrecy become involved in decision-making and management of medical research.\(^{23}\)

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Over the past twenty-five years, several public health historians and advocates have investigated numerous instances of public perception and regulation of toxic products and practices being influenced by corporate attorneys. This system—in which attorneys enable corporate practices by working behind the scenes in matters of medical research and policy—is not new. The tobacco litigation documents released over the past twenty years provide a chilling example of how attorneys were able to influence the public’s perception, litigation, and governmental regulation of tobacco. Documents contained in the tobacco discovery disclosures imply— and, in many cases amply demonstrate—that attorneys were deeply involved in limiting knowledge of the tobacco industry’s funding of research, as well as deciding which special medical research projects should be funded. As one British scholar has written about the differences in tobacco regulation in Great Britain and the United States: “In the United States, [corporate] lawyers appear to have been dominant almost from the start…”\(^{24}\)

\(^{23}\) This is not to say that CEOs and other management personnel do not also have a large role in this story. However, as chapter two and the case studies discuss, corporate management activities have been analyzed in numerous monographs and articles, whereas the attorneys’ role as enablers has often gone unnoticed or given only fleeting attention.

This attorney-managed tobacco research relied and expanded on prior ground work by corporate attorneys in the field of occupational disease and workmen’s compensation. The influence of attorneys goes back at least to the early twentieth century and the then growing recognition of occupational disease. It is, however, a difficult topic to research and recount, primarily due to the nature of attorney activities and court allowed secrecy privileges.

To provide effective attorney representation, with full cooperation from their clients, the judicial system provides lawyers with certain privileges that give them the ability to act in secret. Two of these privileges are the attorney-client privilege and the attorney work product privilege. These two privileges protect the confidentiality of attorney-client communications and materials prepared in anticipation of litigation or for trial. Thus, discussions concerning corporate practices, studies, and concerns can often be kept private so long as litigation is foreseen and an attorney is included in the meeting as an advisor. In addition, research conducted by experts is also protected, so long as it is prepared for attorneys “in anticipation or for trial.” It is my thesis that through these two privileges, attorneys can and frequently have worked behind the scenes to influence medical opinion and regulation of occupational disease issues that dramatically affect the bottom line of corporate profits. This veil of secrecy also permits attorneys to coordinate and manage numerous research studies, only disclosing those that support their client’s position.

This is not to say that attorneys employed or paid by corporate defendants are the only attorneys who attempt to construct science. Commentators have written numerous articles and books complaining about purported plaintiff counsel malfeasance and attempts to influence

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science. However, plaintiff counsel involved in occupational disease litigation have sponsored only limited new research or publication of reviews/reanalysis of old research. Although a few have become very wealthy on such cases, as a group and—except for a very few cases—individually, they have not had the resources or the time to hire experts to conduct extensive new research. This was especially true prior to the development of asbestos mass litigation in the 1970s and 1980s. Yet, even today they rarely appear inclined to spend their own money on publishable research efforts.

Thus, to understand how attorneys can both construct science and use it as their handmaiden, we must look primarily to the attorneys who have at their disposal the resources of large companies, corporations, and trade groups, the corporate and litigation counsel who are involved in defending occupational disease lawsuits and seeking to limit regulation of their clients. As mentioned above, given the secrecy of attorney activities, examining lawyer activities is difficult. Few public records provide more than a hint of attorney involvement in occupational disease research and

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26 Most, but not all, of these writings are by conservative commentators, industry consultants, attorneys, or individuals writing on commission. One of the best examples is Peter W. Huber, *Galileo's Revenge: Junk Science in the Courtroom* (New York: Basic Books, 1991). Following the Supreme Court decision in Daubert, at least two articles have been written by defense attorneys—one with an ethicist—warning of the dangers from plaintiff counsel funded research. (After several searches, I have not found any systematic or concerted efforts by plaintiff counsel to fund medical publishable research.) The first defense attorney article is by lawyers from the prominent tobacco law firm, Shook, Hardy & Bacon L.L.P. Victor E. Schwartz and Cary Silverman, “The Draining of Daubert and the Recidivism of Junk Science in Federal and State Courts,” *Hofstra Law Review* 35 (2006-7): 217-273. The second comes from the prominent Washington law firm, Crowell and Moring L.L.P. William L. Anderson, Barry M. Parsons, and Dr. Drummond Rennie, “Daubert’s Backwash: Litigation-generated Science,” *University of Michigan Journal of Law Reform* 34, no. 4, (2001): 619-682. The Anderson article, in particular, provides an excellent overview of the problems with litigation-oriented science. This overview is as relevant for “defense” published science, as it is for the limited plaintiff published science about which they write. One example of plaintiff attorney involvement in manufacturing science is contained in a letter to the editor of *Environmental Health Perspectives*. The correspondents criticized a recent article in which the authors had not disclosed their funding by plaintiff attorneys. The correspondents also alleged that the plaintiff counsel were “actively involved in the design and administration of this study.” The correspondents’ complaints were somewhat muted by their own failure to mention that at least one of them testified for defense attorneys in industrial cases. Philip Edelman, Patricia Sparks, and Thomas Starr, “Ligitation-related Research,” *Environmental Health Perspectives* 102, no. 6-7 (June/July 1994): 512-514. See McDaniel v. CSX Transportation Inc., 955 S.W.2d 257 (Tenn. 1997). One of the best examples of a non-ideological account concerning plaintiff counsel attempting to use and construct science in litigation is by Marcia Angell, M.D., and former editor of *The New England Journal of Medicine*. Even here, little of the relevant research is published. Marcia Angell, *Science on Trial: the Clash of Medical Evidence and the Law in the Breast Implant Case* (New York: W. W. Norton & Company, Inc., 1996).
corporate policy formulation. It is only during the litigation discovery process and corporate bankruptcy proceedings that attorney activities are on occasion disclosed.

* * *

Industry attorneys’ involvement in public and occupational health research when product safety is at issue provides the focus of this analysis. Although corporate management may hold the final power of approval, attorneys often are the enablers in corporate practices relating to occupational health. Attorneys accomplish this by keeping overall product costs low by limiting any litigation fallout from injuries to human health, a process requiring close attention to the relevant medical science. As will be seen, legal and research fees might be monumental; yet can pale in comparison to potential effects claims can have on profits. As an attorney for thirty years, who has practiced toxic tort and environmental litigation as both a defense and a plaintiff counsel, I have seen and experienced several actions similar to those described in this Dissertation. As a defense counsel, I was a named partner in a Boston law firm and the national counsel for a Fortune 500 company’s asbestos property damage litigation. Our firm also managed regional asbestos personal injury litigation and was national asbestos special projects counsel. My plaintiff counsel activities involved asbestos litigation in Texas, Louisiana, Arkansas, Oklahoma, and Massachusetts. I still have an interest in cases being handled in Texas—virtually all concluded but for bankruptcy claims—but have no communications with the clients. Nor do I participate in any decisions concerning the cases. Due to the ethical requirements of attorney client privilege and attorney work products—requirements that will be more fully explained in this Dissertation—I have only rarely included any of my personal experiences in this paper. My ethical limitations come much more from my defense work, than plaintiff. As a plaintiff counsel, I neither participated in, nor was aware of any plaintiff counsel sponsored scientific research and publication for the cases in which I had an interest.
Since, an understanding of the process this Dissertation will be examining requires a firm grounding in the methods used to influence medical studies, an initial chapter is dedicated to examining how medical science can be distorted, manipulated, and misused. With this understanding, I then trace industry methods of influencing research and medical opinion through the twentieth century, using five case studies and one update. Through these studies will come an understanding of the nature of attorney’s usage of medical science and their efforts to manufacture scientific knowledge in support of their legal stances. Questions relevant to this analysis include:

How do attorneys use scientific knowledge?

What methods are used to create new scientific knowledge?

What, if any, methods are used to hide scientific knowledge?

What effects do these practices have on scientific knowledge?

Like Shapin and Schaffer in their exploration of the importance of experimentation to scientific knowledge in *Leviathan and the Air Pump*, my intent is for the answers to be historical in character. Just as these two historians of science used the events surrounding the institutionalization of experimentation to answer their questions, I believe the events surrounding medical research relating to occupational disease in the twentieth century can show how attorneys have used and manufactured science for legal purposes, for it is in the lawsuits involving occupational diseases that the veil of secrecy behind attorney activities has on occasion been pierced.\(^{27}\)

Accordingly, after discussing the techniques used to influence medical research and opinions, I have chosen five diseases that have been subject to extended mass litigation as case studies to illustrate the influence of attorneys in the manufacture of medical science. They involve natural substances that can cause severe disease and, frequently, even death. They also involve substances that have exposed the largest numbers of workers and public, in most cases, well over a million. The

\(^{27}\) Shapin et al., *Leviathan and the Air-Pump*, 3.
first is crystalline silica—in its most common form, simple sand—which caused the first occupational disease litigation crisis of the twentieth century. The second is tobacco, which, although it is not strictly the cause of an occupational disease, merits inclusion for two fundamental reasons. First, it acts synergistically with certain hazardous substances, dramatically increasing the risk of lung cancer. Second, and most importantly, it provides the archetypal example of attorney influence on medical research. A number of law firms that today defend toxic substance diseases, learned their trade in tobacco litigation or from tobacco attorneys. The third example, asbestos, has been the most litigated occupation disease-causing substance in the twentieth and twenty-first centuries. Two more recent materials subject to litigation follow—the hexavalent chromium and benzene, a derivative of petroleum—with an update on pneumoconiosis forming the basis of the final chapter.

The chapters that follow describe how attorneys have been deeply entrenched in occupational, and public health research throughout the twentieth century. By operating within a veil of secrecy that is actively promoted by legal ethics, they have influenced both the direction and the results of medical science with little oversight or knowledge by the public. Chapter two examines the historiography of occupational and tobacco health history. Initially histories of this field of medicine wrote in the manner of “the great man,” describing how occupational health doctors strove to improve working conditions in the early twentieth century. By the 1960s public health advocates began describing the disasters and heartache of working conditions and public health in America. Finally, as litigation became more of a force in occupational and public health, both plaintiff and defense experts began writing histories of events as viewed from their respective sides. Most of this writing paid little attention to the pervasive presence of attorneys and their, at times, virtual management of the science. Chapter three provides background on the manner in which scientific
studies, reports, and reviews can be manipulated, from the question being asked, to the facts gathered and analyzed, to the interpretation given.

The next five chapters contain the heart of this thesis: the first details the original occupational health litigation crisis in the United States—silicosis in the nineteen twenties and thirties; the second examines the substance in which attorneys fine-tuned their art of influence—tobacco. Next comes the substance subject to the most extensive mass tort litigation in the United States—asbestos diseases. Chromium demonstrates how attorneys have become increasingly sophisticated in their actions. Finally, Benzene provides a portrait in which little is known of specific attorney activities, yet the results are remarkably similar to the chapters preceding it. The chapters initially examine the chronology of public knowledge concerning the substances’ health risks. They then scrutinize attorney involvement in medical research and their manufacture of medical knowledge.  

As noted above: silica, in its most visible form, is simply sand. It also is a major component of most sedimentary rocks. In its basic form, sand crystals are too big to enter the lungs. But when abraded or heated, they can become small enough to enter into the upper lungs and on occasion even deeper. Silica causes silicosis: a fibrosis of the lungs, with each crystal causing scarring in a characteristic circular pattern. In its moderate forms, this scarring causes severe emphysema, similar to that caused by heavy smoking. Silicosis also makes an individual more susceptible to tuberculosis and other diseases of the lung. Silica also appears to be a mild carcinogen with some studies showing a higher rate of lung cancer among silica workers. Exposure to silica dust comes primarily from

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sandblasting, mining and grinding of sand. The silicosis crisis occurred in the late nineteen twenties and early nineteen thirties. Although silicosis is still prevalent in certain industries, since the National Conference on Silicosis in the mid nineteen thirties, it has rarely been in the news.

Tobacco is basically a weed that provides a transport system for nicotine, a drug that can be both calming and focusing. Although industry suspected its health effects at least by the early nineteen fifties, the actions of their attorneys created doubt for fifty years, resulting in a world wide epidemic of lung cancer.

Similar to silica, asbestos is a natural material. It is a fibrous mineral found in many serpentine or amphibole rocks. There are three commercially viable forms of asbestos: amosite and crocidolite come from amphibole rock, chrysotile from serpentine rock. Chrysotile also normally contains a small amount of tremolite, an amphibole form of asbestos. Amphibole fibers are straighter than chrysotile and also last in the lung somewhat longer. Asbestos causes both nonmalignant and malignant diseases, with most research showing that the amphiboles are usually more carcinogenic. The non-malignant disease, asbestosis, is a fibrosis of the interior of the lungs, caused by the mechanical scarring of the deep lung tissues by the fibers. Pleural thickening and calcification of the lining of the lungs can also occur. The malignant diseases include lung cancer and mesothelioma. Mesothelioma is a cancer of the lining of the lungs, which usually causes death by suffocation. Asbestos works synergistically with smoking to greatly multiply the risk of lung cancer above the risk of either individual cause. During the twentieth century (especially in the middle decades), thousands of products contained asbestos, including insulation, construction materials, gaskets, brakes, cloth, paper, drilling mud, plastics, and even kindergarten modeling clay.

Chromium is a major component of many processes and products. One of its forms, chromium (VI)—also known as hexavalent chromium—can have devastating health effects at relatively low doses. This account will follow the halting gain of knowledge about chromium in two
countries, China and the United States. The United States activities occurred primarily near the two coasts, in California and near Washington, D.C. The connected events that occurred in these two countries provide dramatic portrayals of the lengths to which attorneys will travel in pursuit of “evidence.”

Benzene, a derivative of petroleum, is the final examined material. This chapter examines a substance without many specific records of attorney activities. Yet, the research conducted follows the same form and style of the other case studies. In this case study, expert witnesses, both defense and plaintiff, will be used as surrogates to examine how, even without much direct evidence, the influence of lawyers can be discerned.

A concluding chapter, provides an update to the two pneumoconiosis causing substances, silica and asbestos, as the underlying science of these materials meet the ever evolving practices of attorneys in pursuit of evidence. As the chapter demonstrates, litigation driven science never stands still; it simply evolves and expands.

The picture that emerges from these case studies is not unique. As the twentieth century closed, attorneys were increasingly involved in the science of medical research. The substances considered here provide but an example of how attorney engagement affects the nature of medical research and the public’s perception of such knowledge. This is a significant, yet until now little-examined, issue in the history of recent science.
CHAPTER 2

OCCUPATIONAL LUNG DISEASE RESEARCH: PLACING THIS ISSUE IN HISTORICAL AND HISTORIOGRAPHICAL CONTEXT

The nineteenth and twentieth centuries witnessed dramatic increases in both industrialization and accompanying occupational health consequences. During the twentieth century, in particular, new techniques and materials imperiled both workers’ and the public's health. Yet, during the much of the twentieth century, the historiography of occupational medicine did not follow the pattern of the more established fields in the history of science or medicine. These typically began with “heroic” histories, developed into internal histories of advancement, then histories of ideas, the understanding of the sometimes disjointed nature of science advancement, ending with the postmodernist views of the construction of science, including the influence of external factors and styles of viewing science through many lenses.¹

For occupational health, this sequence was condensed. Through much of the twentieth century, historians ignored this history, both in America and Britain. Short synopses in medical articles and grand odes to the field’s accomplishments by company doctors and industrial hygienists provided the field’s only histories. From the 1970s onward, with the enhanced availability of information through lawsuits and the new emerging environmental consciousness, professional historians became more engaged with this subject. By the 1980s occupational health—and occupational lung disease in particular—supported an extremely diverse and lively literature.²

² This is not to suggest that historians have been negligent. Rather, through much of the twentieth century historians concentrated their attention on what they considered more important and certainly more fashionable topics. See Peter J. Bowler et al., *Making Modern Science*, 1-19.
Recognition that certain materials cause disease has been known since well before the twentieth century. As Frederick Hoffman, chief statistician for Prudential Insurance Company, wrote in 1918: “[t]he importance of dust as a factor in occupational mortality has attracted the attention of every authority on occupational diseases from Rammazini to Sir Thomas Oliver.” In fact, even ancient writers such as Hippocrates and Pliny the elder noted the hazards. The Romans in particular were aware that slaves mining gold, lead, and mercury had very short life expectancies. As a 1930s author further noted, "Tuberculosis has long been recognized as one of the major diseases among industrial workers. Despite the rapid decline during recent years in deaths from pulmonary tuberculosis in the United States, it is still one of the chief causes of death among those of working age."  

The first book-length treatment of occupational disease, including those caused by dust, may have been British occupational health expert Thomas Oliver’s *Dangerous Trades*, published in 1902. By the third decade of the twentieth century, even insurance company data documented the epidemic of respiratory disease in workers from substances such as crystalline silica. For example, Metropolitan Life Insurance Company data demonstrated significantly higher mortality from respiratory diseases for individuals exposed to silica, whether or not they had silicosis. Between the ages 45 and 54 mortality was more than three times higher.

By the 1930s, numerous published medical case reports also documented cases of silicosis and other occupational diseases after only short exposures. For example, in 1932 Boston physician

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Earle M. Chapman published an article entitled "Acute Silicosis" in the *Journal of the American Medical Association (JAMA)*. Even today, authors cite this article to substantiate the ability of high doses of silica to cause severe disease after even short exposures. Similarly, a 1937 pathology report from Saranac Laboratory, a noted tuberculosis research institution, detailed the case history of a silicotic black foundry worker, who worked for fourteen months as sandblaster. At that point, shortness of breath forced him to quit his job. He then deteriorated rapidly, dying of silico-tuberculosis fourteen months later.\(^7\)

Throughout this period, neither doctors nor historians focused on the history of occupational disease research or recognition. Historical accounts came primarily from medical case reports and other articles on occupational lung disease, which generally provided a short history of the relevant medical knowledge, as their authors understood it. Researchers in related fields, such as industrial hygiene, similarly provided a short professional history about the topic being examined, but rarely went into detail concerning the current significant disease issues of the day. On occasion, occupational health professionals wrote short articles dedicated to the history of industrial health or hygiene. These articles typically dwelled on the enormous gains the field had made in the twentieth century. Even federal Public Health Service employees such as Jack J. Bloomfield—the very individuals designated to protect the health of the American populace—ignored the problem of silicosis in their histories of industrial hygiene.\(^8\)


An event during the early 1930s provided the opportunity for a closer examination of occupational lung disease. The occasion arose because a Union Carbide subsidiary in West Virginia needed additional power for its production of ferrosilicon. They decided to obtain this power by diverting water from the nearby New River through a new three-mile long tunnel.

Construction on the tunnel—known as the Gauley Bridge or Hawk’s Nest tunnel—began on March 31, 1930 and lasted for approximately twenty months. During this period almost 1500 men worked exclusively in the tunnel, most of them African Americans. Due to ill health from the dust created from the tunneling process, most workers could not remain on the job for the entire period. Even industry occupational doctor consultants characterized the conditions in the tunnel as horrific. Subsequent autopsies confirmed the startling speed at which silicosis can form under these conditions, with hundreds of workers dying from the disease.9

Although this disaster caused a sensation in the media across the country, for almost a year there was little follow up to the initial coverage. By the mid-thirties, the National Silicosis Conference, convened by the federal government but controlled by industrial interests, declared the crisis over. As occurred with other occupational diseases from toxic substances such as lead, historians of this period ignored the entire history of silicosis in America leading up to the disaster. It did not fit into the mold of then-current practices in the field of the history of science and medicine, where researchers were primarily interested in examining the theoretical and experimental advances of science in the centuries preceding the 20th century.

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Labor and public health histories only appeared in the 1940s. Even these virtually ignored occupational diseases, including the Gauley Bridge tunnel disaster. The only book to deeply explore the worst occupational disease disaster of the 1930s was published in the 1940s and cast as a novel.10 Physician and historian of medicine George Rosen’s book about mining in the nineteenth century—perhaps because it examined a prior century—did at least allude to the negligence or even callousness of many mine owners. But his was a lonely call. As Rosen states in the preface, through the 1940s medical historians ignored the historical study of twentieth century occupational disease.11

During this period, industrial health doctors continued preparing the few histories of occupational health then available, whether in book form, articles or lectures. These histories remained adulations to the glories of science or at least catalogs of the accomplishments in industrial medicine. Books written or edited by doctors about silicosis continued to have matter-of-fact reports of the history of knowledge concerning the disease and its diagnosis. Most provided minimal, if any, attention to occupational health disasters that occurred throughout the early twentieth century. Alice Hamilton, a public health industrial medicine physician, wrote the one exception to these histories. Although her autobiography about her career in public occupational medicine normally gives individuals the benefit of the doubt concerning their knowledge and motives, it does provide several accounts of industry indifference to occupational health or preoccupation with profit. During the ensuing decades, a few other intrepid female doctors, such as public health physician Harriet Hardy, joined Hamilton in her efforts. In 1952 Hardy wrote a review of recent research in occupational medicine for the New England Journal of Medicine. Along with the

11 George Rosen, The History of Miner’s Disease (New York: Schuman’s, 1943).
slow progress in discovering causes of diseases, she noted the continuing outbreaks of silicosis among workers.  

Hamilton’s book and Hardy’s articles highlight a related critical topic that has not received the attention it deserves. During most of the twentieth century, female occupation health specialists appeared to be more sympathetic to the worker than their male counterparts. For example, in a 1975 interview, Hardy called an occupational health insurance company “really naughty in all sorts of ways.” In 1958 Public Health Service doctor Victoria M. Trasko reviewed the history of silicosis in the twentieth century. Similar to Hardy, and unlike most male doctors of her time Trasko noted that this disease had not been eliminated in the 1930s, but presented a continuing problem. These actions by female doctors raise an important question, which is beyond the scope of this paper. Is this seeming greater concern for the welfare of workers due to better female socialization—or is it because most male doctors worked for a while in public health, then either obtained employment or consulted extensively with industry, options not open to females throughout much of the twentieth century?  

Serious analysis of occupational health by public health and labor historians only began in the 1970s. Much of this delay resulted directly from industry’s silence and the limited availability of documentation. Through the middle of the century corporate representatives withheld industry-sponsored research, wrested control of the 1930s National Silicosis Conference to declare the silica problem solved, and harassed any doctors willing to question the health hazards in industry, 

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13 Mary Elizabeth Fouse, “Interview of Harriet Hardy of September 17, 1975,” Annals of the American Conference of Industrial Hygienists 7 (1984), 73-82, 75.  
frequently causing them to modify or abandon their research. Thus, much of the primary evidence was not available for study by historians. During the 1940s and 1950s, only a small number of doctors, such as environmental cancer physician and researcher Wilhelm Hueper, strove to publicize the history of occupational and environmental disease. In his 1955 monograph on environmental causes of lung cancer, Hueper called attention to numerous industrial hazards, including asbestos, chromium, beryllium, and certain fractions of petroleum; specifically noting the sustained case reporting of lung cancer in asbestotic patients during the 1930s through the 1940s.15

However, this type of reporting was unusual. Through the early 1960s, industry doctors continued to trumpet their great historic successes in the field of occupational hygiene. In these histories, the main story was the great work accomplished by occupational hygienists and doctors in both discovering occupational disease and enacting measures to eliminate it. In one typical 1962 volume, sponsored by the Industrial Medical Association, two authors provided a history of the fight against occupational disease in the twentieth century. Except for their limited descriptions of company lawyers circumventing workmen’s compensation law and companies being reluctant to allow inspections, they did not describe examples of industry delaying or thwarting research. Indeed, they barely acknowledged the numerous cases of silicosis resulting from the 1930s Gauley Bridge tunnel project in West Virginia. On the other hand, they spent numerous pages providing a scathing

indictment of the “silicosis racket” brought about by the Gauley Bridge incident, even though the government documented many more workers with silicosis than ever became part of lawsuits.\textsuperscript{16}

Not desiring to rely upon increasingly skeptical doctors, the tobacco industry produced its own history of tobacco in America, concentrating on its important role in American life since the colonial period. The author, Robert K. Heimann, was assistant to the president of the American Tobacco Company. In a lavishly illustrated book, obviously meant as a public relations tool, Heimann noted the pleasures of tobacco down through the centuries, while virtually ignoring the, by then almost overwhelming, evidence of its insidious nature.\textsuperscript{17}

An emerging public health advocacy viewpoint in the 1960s and 1970s provided an extreme contrast to these earlier works. Tobacco was one of the first substances about which critical histories were written, with Professor of Sociology Edgar F. Borgatta and a colleague in 1968 editing a book on tobacco and health. Although not strictly a history, the book included not only a discussion of tobacco’s health consequences, but chapters on the history of tobacco and the research into its hazards. The book concentrated on the recent research involving tobacco, but also provides space for a tobacco attorney, David R. Hardy, to write a rebuttal.\textsuperscript{18}

Asbestos became the first major source of occupational disease investigative historic writing. From the late 1960s and through the 1970s, as asbestos disease became pervasive throughout the asbestos related industry, and lawsuits increased, doctors, journalists, and individuals involved in asbestos litigation commenced setting down on paper the story of asbestos, both as a manufacturing material and as a pathogen. By 1970, medical doctors associated with the United Mine Workers began publicizing the continuing danger of pneumoconiosis among miners. In 1979, physician


Irving Selikoff, celebrated for his 1964 study demonstrating the breadth of asbestos disease—both in the number of people it affected and its capacity to act synergistically with cigarettes to cause lung cancer—co-wrote a monograph discussing asbestos and disease. In it, unusual for a book authored by a doctor, he devoted over thirty pages to a history of the usage of asbestos and the knowledge of its hazardous nature. This book set forth a time-line of the knowledge concerning asbestos, knowledge that was even then being augmented by documents and testimony provided through the pretrial discovery process in asbestos lawsuits against asbestos manufacturing companies.¹⁹

As litigation intensified, this chronology expanded, as one investigative journalist examined the history of the first successful lawsuits. In his two books, Paul Brodeur provided a chilling exposé of regulatory agencies’ failure and the manipulation of legal forums by corporate defendants in asbestos lawsuits.²⁰

As litigation discovery practices brought forth more information in the early 1990s, numerous books and articles examined histories of corporate malfeasance among asbestos manufacturers, many aimed specifically at assisting the litigation. In one particularly pointed account, public health advocate and plaintiff litigation expert Barry Castleman developed his environmental science Ph.D. dissertation into a book, providing a history of asbestos manufacturers’ failure to provide for the health of their employees and other individuals who worked with their products.²¹

Histories of other occupational diseases soon joined asbestos. In 1977, Journalists Willard S. Randall and Stephen D. Solomon chronicled the tragedy at a chemical plant from exposure to

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six articles, four of them covered occupational lung disease (two on coal mining and two on asbestos).  

By the early 1990s, ever more public health historians began seriously considering the role of industry in the long delayed acknowledgement of occupational causes of cancer. Between 1993 and 1995 environmental cancer expert Samuel Epstein, epidemiologist David Michael (currently Assistant Secretary of Labor for Occupational Health and Safety), and historian of science Robert Proctor all argued that politics and economics have shaped much of our approach to cancer in the twentieth century.

The 1980s and 1990s also saw an almost explosive widening of the approaches used to study occupational lung disease, as well as a widening in the journals providing space for such articles. In a 1981 article examining class struggles, British economic historian Jane Humphries wrote of an early intersection of workers and capitalism in the Mine Regulation Act of 1842. Sociologist Bennett Judkins considered the occupational health of all workers in his 1986 book, while Edward H. Beardsley examined the health care of blacks and millworkers in the south. One year earlier in England, Paul Weindling edited a book of thirteen essays examining the social history of occupational health.

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By the mid to late nineties specialized studies provided the basis for works involving both research and synthesis. Environmental and legal historian Arthur J. McEvoy used called for an ecological analysis of the workplace environment as it relates to worker safety and health.\(^{27}\) Other historians began studying how technology had changed the occupational health landscape.\(^{28}\) Ethicist and public historian Thomas Murray examined the history of asbestos research from the viewpoint of ethics and scientific values.\(^{29}\) In 1995 Jacqueline Karnell Corn, Professor of Environmental Health Sciences at The Johns Hopkins University, examined occupational health as public history, setting forth the activities of both Federal and some state agencies in the areas of occupational health, including asbestos and silica, without casting blame anywhere. Similarly, historian Christopher C. Seller’s chronicle of industrial hygiene, attempted to provide a balanced account of industry and occupational medicine.\(^{30}\)

Still others looked at the asbestos controversy as socio-legal history. In one such article, British legal historian Nick Wikeley reviewed the British asbestos regulations of 1931 in light of the knowledge of the time. He then turned his attention to asbestos product manufacturer Turner and Newall’s response to one of the first asbestos lawsuits in 1950.\(^{31}\)

However, even at this late date, a few articles still harkened back to the “heroic histories” of the early twentieth century. In 1994, public health expert Herbert Abrams wrote a history of

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occupational health. In it, he noted the trials and tribulations of occupational health advancement in
the United States in areas such as asbestos and silica. He paid tribute to those occupational health
pioneers, including Alice Hamilton, Harriet Hardy, Wilhelm Hueper, and Irving Selikoff, who
persevered in the face of adversity. However, he spent little time on the nature of the adversities.  

Through the 1980s historians had little primary evidence other than tobacco company public
relations documents, detailing the inner workings of tobacco companies and their knowledge of
tobacco hazards. This changed in the early 1990s. A series of whistle blowers and court ordered
productions of documents, provided historians with a treasure trove of tobacco company
documents, including many that detailed attorney activities. Medical professors at the University of
California, San Francisco obtained some of the first documents and established an online archive to
which millions of pages have now been added. The doctors’ opening salvo appeared in a 1995 issue
of the JAMA. These articles provided the first look inside the historical halls of tobacco concerns.
One of the articles provided perhaps the first detailed account of how attorneys can control medical
science research. They followed this research with a book detailing the British American Tobacco
Company’s (BAT) internal strategic planning from the 1950s to the 1980s. For the first time, they
revealed the evidence of a tobacco company’s awareness of the addictive nature of nicotine and the
measures taken to keep the knowledge secret. Through the archival documents, they also established
how BAT generally acted in concert with other tobacco companies.  

Since this groundbreaking book, several other historians and public health advocates have
added to this account. A former reporter and publisher, Richard Kluger beat professional historians
to the punch with a monumental—808 pages—account of the struggle between profits and health.

33-71. Hamilton, Hardy, Hueper, and Selikoff and their work will all be further discussed in later chapters of this
Dissertation.
33 Journal of the American Medical Association 274, no. 3 (July 19, 1995); Peter Hanauer, et al., “Lawyer Control of Internal”;
Kluger’s book provides a riveting, closely focused account of the struggles to reveal the tobacco industry’s actions to protect its profits.

One year later former FDA Commissioner David Kessler provided a similar focused account of his years at the FDA—beginning in 1990—fighting to regulate tobacco. The book reveals the actions leading up to the first release of whistle blower documents and describes the lengths to which big tobacco and its lawyers went to defeat regulation of the industry. Finally, in 2007, Allan M. Brandt, Harvard University Professor of the History of Medicine wrote a authoritative historical account of the cigarette industry. However, like the other accounts, while recognizing the importance of the attorneys, Brandt provides little detail about the continuing control of tobacco attorneys over much of the industry’s relevant medical research.34

These books have been supplemented with a cornucopia of articles—based on research of the University of California tobacco archive—further developing the story. The articles range widely. One details the struggle for document production in the landmark Minnesota state case against the industry. Several examine how the tobacco industry attempted to discredit knowledge about second-hand smoke. Others chronicle tobacco’s efforts overseas to limit the damage then occurring in the United States.35

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For this paper, the most important articles analyze the control of attorneys. These papers provide one of the very few glimpses of how attorneys operate behind the scenes. Yet, even here, the papers examine specific events, without providing a comprehensive historical narrative of the continuous and mounting actions and effects of attorneys from the early years until the 1990s.³⁶

As the twenty-first century began, industry increased its proactive research in assistance of lawsuits. This research even extended into the field of history, since one of the elements of negligent lawsuits requires evidence of “who knew what and when,” known in legal parlance as the “state of the art” of medical knowledge. In one typical book—partially paid for by a contracting company defending asbestos lawsuits—historian Ronald Bartrip expressed pleasure at helping to fill the gap in the inadequately covered “history of asbestos, health, and disease in the USA…” He expressed regret that the unions did not reply to his request to review their files, but believed he had still been able to tell the union story “in very considerable detail.”³⁷

Books and articles authored by occupational health historians used as experts by plaintiff counsel have kept pace with these industry contract histories. Since his first book, Castleman has continued writing a steady number of articles examining the history of corporate malfeasance and historical knowledge.³⁸ David Egilman, Professor of Public Health and plaintiff historical medicine expert, has been even more prolific. As is further discussed in Chapters six, seven and eight, his recent work concentrates on scientific research aspects of corporate knowledge and influence, from studies gone wrong, to hidden studies, to industry influence in the creation of governmental

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standards. One Australian historian, who has provided very modest assistance to plaintiff counsel, conducts considerable research into industry’s use of experts. His article describing asbestos insulation manufacturer Owens Illinois’ use of world-renowned doctor and asbestos cancer researcher Christopher Wagner as an expert witness and secretly retained consultant, provides a chilling insight into the lengths that industry and attorneys have gone to manage their lawsuits.

With the coming of the new century, the historiography of occupational and tobacco disease has joined the mainstream of writings about the history of science and medicine, although it has its own special emphasis directed toward litigation. Much like the recent sociological studies of Bruno Latour—but without clear philosophical or sociological underpinnings—writers engaged in the litigation sphere of occupational health as plaintiffs’ experts examine the manner in which scientific research is not always objective, but rather influenced by profit. They have not been alone in lamenting the influence of corporate “science.” In her book The Secret History of the War on Cancer, epidemiologist and environmental cancer expert Devra Davis argued that for decades the United States took the wrong approach to the war on cancer. Virtually all of the research money was spent on finding cures, when the real effort should be directed toward finding the causes. Similar to Hueper’s arguments in the 1940s, Davis today contends that environmental toxins, from tobacco and asbestos to chemicals and drugs, have fueled the dramatic increase of cancer during the


twentieth century. Her book, *The Secret History of the War on Cancer*, presents dozens of examples of these causes and documented industry’s continuing effort to block or slow research into the consequences of their products and processes.\(^4^2\)

Since 2000, historian Geoffrey Tweedale, alone and with coauthors, has written several articles on the controversies surrounding the history of asbestos disease knowledge. These articles examined the controversy surrounding whether or not chrysotile asbestos causes cancer, the emergence of that knowledge, and the roles played by both government and industry in recognizing and delaying that knowledge. In an event highlighting the asbestos controversy’s increasing impact in the nation, the official journal of the History of Science Society, *Isis*, made a rare foray into occupational health science by publishing one of his articles.\(^4^3\)

Among public health historians, David Michaels, now head of the Occupation, Safety and Health Administration (hereinafter cited as OSHA), remains both one of the the most prolific author and the most pointed in his criticisms of industry. His articles have spanned the range of corporate malfeasance in occupational health from asbestos to historic chemical problems in dyes and hexavalent chromium. He has also editorialized about the problems of results-directed research when used to influence regulatory policy. This article detailed the process by which the chromium

\(^{42}\) Devra Davis, *The Secret History*. Since the new millennium there have been a growing number of books that investigate non-epistemic values and factors in public science and health. Two books that examine how ideology and industry sponsored scientists (but with little mention of attorneys) influence science are: Roger A. Pielke, Jr., *The Honest Broker: Making Sense of Science in Policy and Politics* (Cambridge: Cambridge University Press, 2007); and Naomi Oreskes and Erik M. Conway, *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (New York: Bloomsbury Press, 2010).

industry, its attorneys, and consultants sought to forestall and limit any occupational health standard for hexavalent chromium.\textsuperscript{44}

Gerald Markowitz and David Rosner have played similar roles for silica. Their books and articles all stress the importance of political negotiation and power that shaped the scientific debates about silica and other toxic substances in the twentieth century.\textsuperscript{45}

Clearly, occupational and environmental health histories are now part of the mainstream of medical history research and writing. Much of the information that made this possible during the past thirty years either came from litigation\textsuperscript{46} or regulatory documents released through Freedom of Information Act requests.\textsuperscript{47} As a result, during the past fifteen years occupational and biomedical research histories have used increasingly vitriolic language. It is difficult to find a book or article that authors from the opposing viewpoint accept as unbiased, fair or even reasonably well argued.

Thus, the future of narratives covering the history of occupational health remains extremely clouded and uncertain. To a great extent, political and ideological viewpoints are progressively shaping the histories being written about occupational health. With increasingly large sums of money at stake, manufactured contract histories may become even more prevalent. Still, as with science itself, manufactured histories, in the end, must come up against the hard facts. So long as lawsuits continue to provide fodder and disclosure of events normally hidden from public view, public health historians will have a continual stream of new material.


\textsuperscript{47} All of Michaels’ works make tremendous use of regulatory materials, as well as documents disclosed during litigation.
CHAPTER 3

MANUFACTURING SCIENCE: METHODS USED TO INFLUENCE
MEDICAL, LEGAL, AND PUBLIC OPINION

Introduction

The account of how attorneys increasingly influenced medical research throughout the twenty-first centuries, first requires a basic understanding of how medical science can be influenced and even manufactured in support of litigation and regulatory positions. The discussion about the methods that can be used is not new. For the past quarter of a century, science ethicists have expressed increasing concerns about corporate medical science research being conducted to further litigation, regulatory or corporate profit goals. Public health advocates have frequently expressed criticism about the increasing hold that industry has on public health research. In one recent example of their “call for public awareness” public health historians David Rosner and Gerald Markowitz wrote an article condemning industry’s role in undermining evidence and regulations for lead. Their article lists three methods used by industry to limit lead regulations: first, the industry sought to control research by sponsoring and funding university research; second, through public relations it sought to portray lead as necessary for everyday public life; and third, it sought to quiet and even intimidate researchers who reported or identified lead as a hazard. As we shall see shortly, these methods are but the tip of the iceberg.¹

In a commentary published in the *Journal of Clinical Investigation* shortly after it instituted a new conflict-of-interest policy, noted ethicist Arthur L. Caplan considered the question of scientific bias due to conflict-of-interest. He noted that “[m]oney is an important source of conflict: study after

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study has shown the power that money can have on the publication of scientific findings.” While noting that there are other areas of concern in addition to money, he praised the editors for showing “themselves willing not to shy away in the face of the most miserable aspect of the problem: money.”

Other doctors and scientists agree that the quest for profits can dominate over medical science accuracy. As Marcia Angel, former editor of *The New England Journal of Medicine*, commented at the 2000 National Institute of Health Conference plenary presentation, “papers submitted by authors with financial conflicts of interest [impressed her as] far more likely to be biased in both design and interpretation.”

The problem is that financial interests are often surreptitious. In addition, the passion to make a fortune does not in itself drive the advancement of science but it can drive a desire to influence and corrupt scientific research. Frank Davidoff, former editor of *The Annals of Internal Medicine*, has discussed the irony of this issue as leading medical journals’ work to ensure their articles are the best possible:

> Here is the irony indeed: Over the years we editors had become hawks about transparency in the reporting of study methodology – the technical side of things. Yet we have been willing to leave readers in the dark about the key nontechnical factors (including the role of outside sponsors) that lie behind the way the project is carried out. These same factors can affect a study’s conduct and reporting as much if not more than it’s purely technical aspects can.

Corporate and conservative writers have strongly disputed these claims of harm from industry’s funding of medical and other scientific research. One of the most extensively researched articles in defense of industry funding came from Ronald Bailey, a contractual writer for (in his

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words) “the non-profit, non-partisan libertarian magazine Reason.” His monograph defended the pharmaceutical industry and critiqued “conflict of interest activists” who “view the conflicts of interest campaign as another tool to attack an enterprise which they already despise on other grounds.” In the introduction Bailey stated that the “report concludes that the conflicts of interest campaign against industry/academic collaboration research has shown: no evidence of patient harm; no evidence of loss of faith in scientific research; no evidence that integrity of science is being threatened by commercial influences; no evidence that collaboration boosts the overall costs of medical care or of consumer products; little evidence that industry unduly influences decisions of government agencies; and no evidence that environmental regulations routinely err on the side of industry.”

Notwithstanding Mr. Bailey’s objections, in the new millennium even the editors of perhaps the United States’ most prestigious medical journal, *JAMA*, decried industry’s influence of public health and medical research. During the eight-year period of 2000-2008, senior editors of the journal wrote at least four editorials about the problem of financial interests in medical science research. The first essay discussed the potential effects, but also emphasized the importance of for-profit company research. Only one year later *JAMA* editors, along with eleven other leading medical journals, wrote an editorial, announcing that the group had agreed not to “review or publish articles based on studies that are conducted under conditions that allow the sponsor to have sole control of

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6 Ronald Bailey, *Scrutinizing Industry-Funded Science*, 8. The Reason Foundation, publisher of *Reason*, is partially funded by the Koch brothers’ foundation, with one brother having served as a trustee. An attorney from the prominent corporate defense firm, Latham and Watkins, also serves as a trustee. One of *Reason’s* senior editors consistently supported tobacco companies in his writings during the early 1990s. During that period, Philip Morris made at least one donation of $10,000.00 to the foundation. The Center for Media and Democracy, [http://www.sourcewatch.org/index.php/Reason_Foundation](http://www.sourcewatch.org/index.php/Reason_Foundation), accessed August 19, 2015.

the data or to withhold publication.” The journals that published the essay received numerous responses, overwhelmingly supportive. One Harvard University doctor’s response to the highly respected *New England Journal of Medicine’s* publication of the editorial, offered an instance of attempted legal interference in medical research.

The following is an excerpt from recent correspondence between a pharmaceutical company and the legal department at my institution regarding a clinical trial in which we hoped to participate that I believe illustrates the problem: “Yours is one of the only institutions that has objected to our standard confidentiality clause. While I understand your position, I was frankly not overly pleased with our compromise and have asked [employee’s name deleted] to convey to our clinical staff that I would prefer that [company’s name deleted] not use your institutions for clinical trials in the future unless necessary.”

As the JAMA editorials became increasingly strident in their tone, they received sharp criticism from industry representatives. For example, following the 2005 editorial, a spokesperson for the Pharmaceutical Research and Manufacturers of America responded to by disagreeing “with the implication that industry-sponsored studies are at higher risk of bias and fraud than other types of studies and thus require special scrutiny.” JAMA executive deputy editor Phil B. Fontanarosa disagreed: “Dr. Loew and PhRMA contend that industry-sponsored studies are not at higher risk of bias and thus do not require ‘special scrutiny’ and that ‘vigorous government oversight … ensures the integrity of data and results.’ Despite these assurances, scientific and ethical lapses involving industry and industry-sponsored studies strongly indicate otherwise …”

The following year’s editorial provided an even harsher assessment of the state of industry influence. As De Angelis noted in her scathing indictment, “[i]n some instances, the marketing goal of a company dominates the scientific aspect of the company-funded research.” She continued,

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8 Frank Davidoff, “Between the Lines,” 241 (review or publish); and Elizabeth Loder, MD, “To the Editor,” *New England Journal of Medicine* 346, no. 4 (January 24, 2002): 291 (The following).

“There have been a number of high-profile examples of such research irregularities involving for-profit companies, such as the refusal to provide all study data to the study team, reporting only 6 months of data in a trial designed to have 12 months of data as the primary outcome; incomplete reporting of serious adverse events; and concealing clinical trial data showing harm.” She also noted that one company withdrew its publication after JAMA indicated that it would not publish the paper without an independent analysis.10

The fourth JAMA editorial, published in the April 16, 2008 issue, further castigated the pharmaceutical and medical device industry for its improper and “profound influence” in every aspect of the profession of medicine. The editor specifically called attention to two articles in that issue which “provide a glimpse of one company’s apparent misrepresentation of research data and its manipulation of clinical research articles and clinical reviews.” Documentation of the manipulation only became public because of a lawsuit involving one of the company’s products.11 With editorials and disputes such as these, there can be little doubt that medical research today is facing a severe crisis.

As noted in the 2008 JAMA editorial, litigation is involved in much of the controversy. The remainder of this chapter explores the various methods of influencing occupational disease research and opinion that can be used by attorneys to assure they obtain the evidence they need for trial. The subsequent five chapters will provide the broader historical perspective, revealing how attorneys have used these methods throughout the twentieth century.12

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10 Catherine DeAngelis, “The Influence of Money,” 996.
11 Catherine D. DeAngelis and Phil B. Fontanarosa, “Impugning the Integrity,” 1833.
12 This dissertation is not intended to take sides in this controversy, but rather to examine the nature of industry attorneys’ historical involvement in medical research leading up to the current crisis. While attorney involvement in research from either side can have similar consequences, this paper admittedly looks at one side of the issue, attorneys for defendant corporations. During my research I have found very few plaintiff funded published articles. As noted in the introduction, plaintiff attorneys do not appear to fund a significant amount of published research, except for short-term research involving limited amounts of money. Thus, published plaintiff funded research is exceedingly rare when compared to published research managed by corporate defense attorneys.
Methods of Influence

Numerous means of improperly influencing occupational disease research and regulation are available to any interested party. Below, I examine three broad areas of influence: controlling/influencing the agenda, controlling/influencing the experts, and controlling/influencing the research.

The Agenda

The importance of influencing the determination and prioritization of government funded medical and scientific research can hardly be overestimated in controlling the success of marketing of potentially toxic products, as well as the regulation and the potential of lawsuits and workers’ compensation claims. Thus, guiding the public health policy agenda onto select paths can be vital to influencing scientific research relevant to potentially toxic products or substances. There are numerous approaches to controlling the agenda, including influencing the regulatory process and reducing the spotlight of lawsuits and workers’ compensation claims.

One basic method of influencing regulations is by shaping their formulation. This can be accomplished in two manners. First, As a National Cancer Institute (NCI) funded report examining the beginnings of the war on cancer during the 1970s explained: regulators know there are jobs waiting for them in industry. The report discovered a “revolving door of industrial and government experts had operated since the earliest efforts to deal with cancer nationwide.” The revolving door is not new, having existed at least as long ago as the silicosis crisis of the early 1930s. 13

A second approach is by inserting corporate representatives into the regulatory process. The NCI again provides an excellent illustrative example. Throughout the 1980s Armand Hammer, CEO

of Occidental Petroleum, chaired the NCI’s advisory board. During the same period, this firm produced over 100 billion tons of toxic chemicals, including the ones found in Love Canal.\textsuperscript{14}

In the later decades of the twentieth century, critics noted similar activities at several federal agencies. For example, in 1985 the EPA’s internal peer review committee concluded that daminozide, the primary ingredient in the pesticide Alar, should be classified as a probable human carcinogen. The agency’s Scientific Advisory Panel then received the report for review. The “independent” advisors criticized the report, concluding that the current science did not justify removing the pesticide from the market. Following congressional hearings, a House member learned that seven of the eight members of the Scientific Advisory Panel had current or prior consulting relationships with the manufacturer of Alar.\textsuperscript{15}

During the first decade of the twenty-first century, the Federal government even encouraged closer industry-regulatory agency relationships. The George W. Bush administration increased industry representation on scientific advisory boards and diluted government regulation through several new plans and proposals. In 2002, President Bush’s Health and Human Services Secretary appointed fifteen new members to the eighteen member advisory committee to the Center for Disease Control’s National Center for Environmental Health. This committee is charged with providing the Center with advice on “program goals and objectives, strategies, and priorities.” The new members included many with close industry ties, such as Roger McClellan, former director of the Chemical Industry’s Institute of Toxicology and Dennis Paustenbach, Ph.D., a toxicologist, about whom we will hear much more throughout this paper.\textsuperscript{16}

\textsuperscript{14} Devra Davis, \textit{The Secret History}, 10.
Critics of the new policies, citing to numerous examples, have argued that these types of actions damaged public health. For example, in a review of the recent history of vinyl chloride (VC) regulation, three public health advocates pointed to industry suppression and withholding of evidence in arguing against the EPA’s trend to accept industry data in its risk assessments. They further noted that “at least 7 of the 19 external peer reviewers of the VC assessment were chemical industry employees and consultants and 4 were [Republican] administrative representatives - none represented unions or public interest groups.” Yet, even with these warnings, during 2000-2008 both OSHA and the EPA began implementing an increasing number of voluntary and self-regulatory schemes for industry.17

Perhaps the greatest assistance to controlling the agenda came from a late twentieth century limitation to the manner in which courts consider the admission of scientific evidence. In June 1993, the United States Supreme Court handed down their decision in Daubert v Merrell Dow Pharmaceuticals, Inc., which required federal judges to be gatekeepers for the admission of scientific evidence in the courtroom. In particular, the court required judges to be more proactive in their responses to motions seeking exclusion of expert witnesses. To assist trial judges, the Supreme Court provided a legally based scientific standard to be used by judges in determining whether scientific experts should be allowed to testify at court. In accordance with the Court’s decision, trial judges now only allow testimony that meets the guidelines comprising the judicially based “scientific” standard. Although the Supreme Court emphasized that these criteria should not be regarded as “a definitive checklist or test”—thus allowing judges wide discretion in determining what counts as science in the

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court room—they did form a characterization of what the high court considers “the scientific method.” The guidelines established by the Court are as follows:

1) Is the evidence based upon a testable theory?
2) Has the theory and/or methods used been peer reviewed?
3) Does the methodology have a known error rate?
4) Is the underlying science generally accepted?18

Other than capturing the Supreme Court’s limited lay understanding of the scientific process, the Daubert decision provided little philosophical help in assisting judges to determine “good science.” A majority of the members of the Court apparently believed that there is a distinct, widely accepted, and well-defined “scientific method” of comparing facts by clearly identified criteria that can be objectively applied. Evidently, they further assumed that, in essence, all science is experimental. As Sheila Jasanoff, Professor of Science and Technology Studies at the John F. Kennedy School of Government at Harvard University, put it, “in an ironic turn, the ‘science’ that the Court officially embraced remained profoundly a creation of the court’s own biases, needs, and misconceptions concerning scientific inquiry; while urging judges to defer to scientific authority, the Court gave judges new resources for writing their preconceptions regarding science into law.” With the Supreme Court’s decision in Daubert v. Merrell Dow Pharmaceuticals, Inc., new positive research studies are much less likely to be admitted into evidence. The federal courts have come to worship scientific uncertainty as a method of excluding science from the courtroom. One study has even found that some courts are requiring doctors who testify as experts to meet evidentiary standards that exceed those used in diagnosing patients.19

19 Paradoxically, courts hearing criminal trials—with their higher standard of proof making it the very type of trial where the right to challenge expert opinions would seem most useful—have rarely allowed these challenges. On the other hand, civil trials—which only require a proof of more probable than not—have frequently excluded plaintiff experts. Sheila Jasanoff, “Law’s Knowledge: Science for Justice in Legal Settings,” American Journal of Public Health 95, no. S1
Public Health advocates have universally opposed this ruling. Michaels has described the decision as “an amalgam of two incompatible philosophies of science, Popper’s and Hempel’s—neither of which is capable of supplying the criterion of reliability the Court seeks.” Michaels argues that the Court ignored the inherent uncertainty in science and the skepticism that accompanies all scientific advancements, instead requiring it to fit within legal norms.

Notwithstanding the Supreme Court’s pronouncement, scientific uncertainty is inevitable in matters concerning the human body’s reaction to substances, especially when there is a long latency period, as there is with most cancers and fibrotic diseases of the lung. Through its straitjacketed approach, Daubert has allowed industry attorneys to exclude plaintiff expert opinions on many occasions because of the inherent uncertainty of medical science. It has also encouraged industry and its attorneys to initiate and manage research tending to increase uncertainty. This greater uncertainty then acts as a drag on federal and state agency agendas, increasing the time before any action can be taken.

(2005): S50 contains an excellent critique of the Daubert opinion. Also see Jean Macchiaroli Eggen, “Toxic Torts, Causation, and Scientific Evidence After Daubert,” University of Pittsburgh Law Review 55 (1994): 889, 890; and Michael H. Gottesman, “From Barefoot to Daubert to Joiner: Triple Play or Double Error,” Arizona Law Review 40 (1998): 753, 780, 761 (discussing plaintiff difficulties in meeting the standard). For a discussion of the complexities of epidemiologic methodology that has not been considered by the Supreme Court see Richard W. Clapp and David Ozonoff, “Environment and Health: Vital Intersection or Contested Territory?,” American Journal of Law & Medicine 30, no. 2 & 3 (2004): 189-215, especially 199-212. (The authors discuss both the methods that can be used to pre-determine the outcome of a study and how easy it is to criticize past studies. Both techniques have been consistently used by industry litigation consultants; in particular see the discussion of industry brake articles in chapter 9 of this Dissertation.) For additional background, see Steve Wing, “Objectivity and Ethics in Environmental Health Science,” Environmental Health Perspectives 111, no. 14 (November 2003): 1813-1817, especially 1814 (the Daubert criteria leads to rejection of new studies that challenge normative science); Jerome P. Kassirer and Joe S. Cecil, “Inconsistency in Evidentiary Standards for Medical Testimony: Disorder in the Courts,” 288 JAMA (2002): 1382-1387 (1382 discusses legal standards exceeding diagnostic standards); and David Michaels and Celeste Monforton, “Scientific Evidence in the Regulatory,” 34, 39.

The Experts

As described in the above section, controlling the relevant medical experts provides one of the most effective methods of influencing medical science. According to New York University nutritionist Marion Nestle, in her field of nutrition science, “co-opting experts—especially academic experts—is an explicit corporate strategy”.

21 Nutrition science is not unique. Experts have been coopted at least since the 1930s silicosis crisis. In a more recent example, Tufts University professor Sheldon Krimsky experienced this problem first hand. Appointed in the late 1970s to the National Institute of Health’s Recombinant DNA Molecule Advisory committee, following two years of at times frustrating service he learned that several of his colleagues had undisclosed financial interests in biotechnology companies.

Krimsky’s experience was not unusual for advisory committees. In 2000, USA Today studied eighteen expert advisory committees of the FDA’s Center for Drug Evaluation and Research. Of meetings held between 1998 and June 2000, its journalists found that in 146 of the 159 meetings, at least one committee member had a financial stake in the topic being discussed. At least half of the members had a financial stake in 88 of the meetings. At meetings discussing broad policy issues, 92% of the members had financial stakes. Even at meetings considering specific drug application issues, 33% of the experts still had a conflict of interest. Overall, the report revealed that 54% of the time advisors to the FDA have a direct financial interest in the outcome of the drug they are asked to evaluate.

22 In addition to assistance in controlling the agenda, experts under contract also provide a ready source of expert witnesses for the defense of lawsuits and workers’ compensation claims, as well as in testimony before Congress and regulatory bodies. When combined with the Daubert

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22 Sheldon Krimsky, Science in the Private Interest, 91, 96.
decision, they can present a daunting defense for virtually any alleged toxic substance. In addition, as noted before, under *Daubert* an expert’s opinion must be based upon accepted science. If the majority of the scientific journal articles concerning a product were prepared under contract to the product’s manufacturer, the “accepted” science can be massively skewed, especially if the articles were prepared in furtherance of attorneys’ desires to obtain litigation evidence. With control of publication, and attorneys’ privilege of secrecy, studies that do not provide the desired result can be hidden. The tobacco experience provides a cornucopia of examples illustrating this result, allowing tobacco attorneys to successfully defend tobacco personal injury lawsuits for decades.

The tobacco industry has been especially adept at hiring experts. In 1993, Eugene F. Knopf resigned his position as chief Pennsylvania lobbyist for the American Cancer Society (ACS) after being retained by the “American Tobacco Institute.” Shortly before leaving his position he persuaded the ACS to support a law that prevented localities from limiting smoking in public places. Throughout the mid to late twentieth century, tobacco attorneys scoured the world in search of similar expert witnesses and consultants, often attempting to use people under the guise of “independent” associations.23

The tobacco companies are not unique in this respect. Many companies—and their attorney—that manufacture hazardous materials hire experts to dispute and reanalyze data that show adverse health consequences. As stated in 2005 by the current head of OSHA, David Michaels: “It is now unusual for the science behind any proposed public health or environmental regulation *not* to be challenged, no matter how powerful the evidence.”24

Even Government officials are often co-opted. In 1996 Warner-Lambert’s drug Rezulin was selected to take part in one of the largest diabetes studies in the United States. In its press release,

23 Devra Davis, *The Secret History*, 166.
the drug company quoted the director of the diabetes division of the National Institute of Health as stating that Rezulin “corrects the underlying cause of diabetes.” Two years later, a *Los Angeles Times* investigative report revealed that the subject director, Dr. Richard Eastman, first became a paid consultant to Warner-Lambert in 1995. The doctor subsequently admitted that he had participated in a number of deliberations concerning Rezulin while he was consulting for the company. The *LA Times* report further revealed that at least twelve of the twenty-two scientists who played critical roles in the “independent” diabetes study had received research funding or compensation from Warner-Lambert. Although we may never know the extent of Eastman’s knowledge of the drug’s deficiencies prior to the study—and we can only speculate if Eastman’s consultancy with Warner-Lambert caused it to be included in the study—his involvement, along with that of the other compensated scientists, may very well have put the participants in the study at serious risk. In 1998, the NIH called a halt to the Rezulin study due to reports of liver failure, finally cancelling its registration in 2000.25

Control of contracted medical scientists’ right of publication provides another means by which health science can be manipulated. In a 1999 study of university-industry research centers, half of the centers reported that industry funders could require a delay in publication of scientific research, with more than a third indicating that industry could delete information from papers.26 A 2004 survey similarly ascertained that a relatively large proportion of clinical trial investigators are willing to cede considerable control over the dissemination of research results to industry sponsors. These controls included allowing the sponsor to write the manuscript, allowing the sponsor to insert its own statistical analysis, and prohibiting independent publication. The survey additionally revealed

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that participants with high levels of industry support were more willing to release control than those with less support.27

In another example of sponsorship control of manuscripts, The Annals of Internal Medicine changed its policy concerning conflict of interest after an incident involving a 1995 proposed industry sponsored article. After reading the initial draft of the proposed article, journal editors and statistician believed in this case, “the authors had gone well beyond the data in stating the drug’s efficacy and safety.” They sent the article back twice with suggested changes. Each time it came back with the wording unchanged. When they called lead author, he revealed that the sponsoring company had the right to review the manuscript and its opinions “did very likely influence the report’s language.”28

The problems with these types of controls were succinctly summed up in an editorial in the 2006 issue of the American Medical Association’s primary journal, JAMA. In it, the editor described how control of publication can dramatically influence medical science:

> For profit companies also can exert inappropriate influence in research via control of study data and statistical analysis, ghostwriting, managing all or most aspects of manuscript preparation, and dictating to investigators the journals to which they should submit their manuscripts. For example, I have been told that in response to JAMA’s policy requiring an independent statistical analysis by an academician for industry-sponsored studies in which the only statistician who analyzed the data is employed by the study sponsor, some companies are insisting that the researchers not submit those studies to JAMA.29

The reports and surveys described above paint a bleak picture of academic research; yet, the problem of captured research looms even larger. Academic institutions represent a decreasing share of corporate research particularly health related research for regulatory submission or litigation. For profit, litigation oriented research firms and professional consultants have increasingly taken center

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28 Frank Davidoff, “Between the Lines,” 236, 238.
stage from academics. When university professors become involved, as has been the case in benzene, they often establish their own consulting and research business outside of the academic community. As we will see in asbestos, chromium, and benzene, when these professional consultants are called upon, it is often with a clear understating of the results desired. They rarely fail to deliver.

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As noted above, medical scientists can be influenced, even without hiring them. For scientists who either refuse to work for a particular client, or who publish a study that is contrary to the employer’s desired position, harassment can be an effective tool. Scientific harassment can take many forms, including complaints to a scientist’s superiors, threats of lawsuits, and scientific misconduct complaints addressed to funding agencies.\(^30\)

Harassment is easiest when a scientist is already under contract. The case of phenylpropanolamine (PPA), the over-the-counter drug used as decongestant and appetite suppressant, provides a clear example of this. Reports of hemorrhagic strokes in young women using PPA first appeared in the 1970s. Eventually the drug’s manufacturers agreed to select an investigator and fund a study. They chose Yale University. In 1999 that study confirmed that PPA causes hemorrhagic stroke. The manufacturers then hired the Weinberg Group, a product-defense consulting firm, to attack the study. Manufacturer attorneys also deposed the Yale researchers. While the FDA did advise manufacturers to stop making PPA in November 2000, David A. Kessler, the former head of the FDA who later became dean of the University of California at San Francisco School of Medicine, stated, “With the amount of hassle and harassment that [the Yale scientists] had

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to endure, I’m sure the next time they’re asked to undertake something like this, they’ll wonder if it’s worth the cost.”

The case of Synthroid, a synthetic thyroid hormone manufactured by Flint Laboratories, provides an even starker example of how studies can be influenced, hidden, or downplayed. In 1988, Flint funded a study testing Synthroid’s bioequivalence to one other name brand and two generic drugs. In 1990, the researcher determined that all of the drugs were bioequivalent. Not satisfied with this result, Boots Pharmaceuticals, to whom Flint had been sold, notified university officials about flaws in the study. Two years later, the school determined that any flaws were minor and easily correctible. Yet, when the researcher attempted to publish her work, the pharmaceutical company threatened to sue, citing a restrictive covenant in the research contract. In the meantime, the pharmaceutical company published its own interpretation of the data, finding Synthroid superior to all other products tested.

Even scientists who work for federal agencies can be subject to severe harassment. At the time she first began writing The Secret History of the War on Cancer, Devra Davis, Professor of Epidemiology at the University of Pittsburgh’s Graduate School of Public Health, received “friendly advice” from the man who was temporarily running the National Institutes of Health. He suggested that she should carefully consider whether she wanted to risk her career by writing the book. In describing why he had never pursued a similar study, he related the experience of Wilhelm Hueper (1894-1977), a major figure in environmental cancer research. Davis recollects the advice as follows: “Hueper started out like you. Lots of good ideas about the environment. He thought the exclusive focus on smoking would lead us away from other causes of cancer that were far more deadly. He was railroaded out of here. He wasn’t the easiest fellow to work with and rubbed lots of people the

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wrong way, but not necessarily for the wrong reasons. I decided after seeing what happened to him that I was better off sticking to basic research.”

Hueper’s story provides a classic example of scientific persecution. After obtaining his medical degree in Germany, he immigrated to the United States in 1923 in the wake of Germany’s economic collapse. While working for the University of Pennsylvania in 1930, Hueper became interested in industrial hygiene. After a visit to a DuPont plant, he advised its management that the dyes posed a potential danger of bladder cancer. As a result, Du Pont hired him in 1934 as its chief pathologist for the new Haskell Laboratory of Industrial Toxicology. In this capacity, he performed a number of research and management functions, while also learning how businesses operate.

While employed by Du Pont, Hueper found that the supervisory staff and upper management paid, at best, only lip service to good occupational hygiene procedures. In his autobiography, Hueper described one visit to a factory building in which he remarked about its excellent cleanliness. The foreman described how they had been up all night cleaning the facility. In response, Hueper visited an adjoining building used for similar benzidine operations, without advance notice to management. During his inspection, he found “white powdery benzidine on the road, the loading platform, the window sills, on the floor, etc.” Although he wrote a memo to Mr. DuPont, personally, about the incident, Hueper never received an answer—and was never permitted back into plant buildings.

53 Devra Davis, The Secret History, xi.
34 Wilhelm C. Hueper, “Unpublished Autobiography Draft,” National Library of Medicine, Hueper Papers Collection, MSC 341 Archives of the National Library of Medicine; 122-124; Wilhelm C. Hueper Deposition on June 16, 1977 in Eleanor Miller vs. Raybestos-Manhattan, Inc., Civil Action No. 76-899, U.S.D.C., W. D. Pa., 3, Hueper Papers collection, MSC 341, Archives of the National Library of Medicine; Robert N. Proctor, Cancer Wars, 37-8. Hueper’s own writings revealed he could be difficult to get along with. He had strong opinions and did not trust most companies. During his tenure at the NCI Hueper had a running feud with the federal government’s Division of Industrial Hygiene—including many of his own superiors in the NCI—who he believed were too close to industry. However, as we will see in Chapter 6, most of Hueper’s primary opinions about asbestos, as well as his concerns and complaints about companies and government officials, have been either supported by similar allegations of others or by other evidence.
Unsurprisingly, by 1937 Hueper found himself laid off for “economic” reasons and was informed that none of his “observations made during [his] time of employment could be published without their [DuPont’s] consent.” In November of that year, Hueper was threatened with legal action if he attempted to talk about or publish any of his findings regarding health dangers to workers. DuPont further enforced Hueper’s employment contract less than a year later when an official wrote to Hueper, informing him the company was “looking with disfavor on [his] acceptance of an invitation . . . to present a paper on the experimental production of bladder cancer in dogs” from benzidine. For twenty years after Hueper left Du Pont, Chambers Works—the site responsible for dye production—did not report any new cases of bladder cancer. Finally, in 1980 they disclosed that 364 cases of bladder cancer—normally very rare—had occurred at this one factory since it’s opening.

The harassment did not stop with Hueper’s departure. While Hueper was at the NCI, certain government officials routinely provided Du Pont management with pre-publication copies of articles he had submitted to his superiors for review. The doctor also experienced almost constant personal attacks. For example, during the early years of his tenure, DuPont officials first accused him of being a Nazi, and then a communist.

The Research

The importance of Daubert to medical research relates not just to its scientific failings, but also to the further openings it provides for litigation attorneys to control the agenda by manufacturing scientific evidence through medical research. Although even before Daubert any

35 Wilhelm C. Hueper, “Unpublished Autobiography Draft,” 123-125. As will be described in Chapters 4 and 6, this lack of concern was not unusual among many industrial companies. The autobiography of Alice Hamilton, a renowned public health industrial medicine doctor, also provides several similar accounts of industry indifference or preoccupation with profit, although she normally gave individuals the benefit of the doubt. See Alice Hamilton, Exploring the Dangerous Trades. See also Jim Wolfe, “OSHA: A Short Story,” Labor Studies Journal 3, no. 2 (Fall, 1978): 150.
conclusion desired in a medical study could be relatively easily manufactured, since then, scientific research initiated due to litigation and regulatory initiatives has become an industry of its own. As the following chapters will explore, industry defense attorneys have used Daubert to not only limit plaintiff expert opinions, but have also used the vast monetary resources of industry to reshape the “peer reviewed” topography of occupational health science. They accomplished this by hiring specialty consultant firms to conduct studies, reanalyze data, and deconstruct prior studies in industry friendly “peer reviewed” journals, often publishing two to four very similar articles from a single study or review. In large measure—due to their privilege of secrecy—they have accomplished this feat far below the radar of historians.38

Until the later part of the twentieth century, mainstream journals and the federal government rarely questioned industry sponsored public health research. The federal government did not begin questioning industry reports on the safety of chemicals until 1978. That year, officials discovered that the company responsible for one third of the testing for industry, Industrial Bio-Test, could not find or account for the animals it had allegedly tested.39 Since then numerous authors have examined techniques available and being used to influence medical research. While most of these authors focus on slightly different aspects of this issue, they all arrive at the same general parameters of the problem. Litigation or regulatory action provides a common thread to the various activities. Swedish scientist Lennart Hardell has studied numerous cases of industry and expert ties in cancer research and litigation. In legal cases and regulatory hearings involving Dioxin, Roundup, herbicides, cellular phones, and tobacco, large corporations have used noted experts, frequently without

38 Designing and managing a study that meets the client’s desire to have negative results is relatively easy. Positive studies are more daunting, requiring exceedingly clear and significant proof. As described by Devra Davis, “in epidemiology today there’s no balanced seesaw for determining what’s true. In studies of human hazards, the end of the seesaw showing proof of harm is generally thought to be twenty times heavier than the lighter end in order for a hazard to be considered established. Proof that a given condition actually results from a given exposure is considered to be established only when it has a a one in twenty or less chance of being false.” Devra Davis, The Secret History, 313.
acknowledgement of their prior connections to the corporation, as their face before the jury or agency. Often, the information about the prior connection only became known through litigation discovery.\textsuperscript{40}

Industry is also adept at steering medical research attention away from occupational diseases. In his autobiography, Hueper detailed four methods industry can use in this regard. They can feign blindness or not report cases. They can create negative evidence by counting disease in short-term workers but not those now dead or retired. They can pack the study population with those not exposed. Finally, they can suppress or delay the results.\textsuperscript{41} A study funded by the Cigarette and Tobacco Surtax Fund of the State of California examined the means by which sponsorship can influence the direction and results of health research. It listed several methods by which the sponsor can influence the investigator: 1) the sponsor can recommend a study design that is more likely to favor its product; 2) the sponsor can encourage the investigator to emphasize certain conclusions; or 3) investigators can feel consciously or subconsciously compelled to present findings that are not damaging to the sponsor. This is especially true in cases where the two have an ongoing relationship.\textsuperscript{42}

Other authors have described similar methods of creating bias in health studies. In his 2003 book, Krimsky described five such methods: 1) choose a sample population that is not random or representative; 2) ask questions which predispose toward certain answers; 3) choose a design that is less likely to show effects; 4) choose interpretive statistics that are more likely to support the null


\textsuperscript{41} Devra Davis, \textit{The Secret History}, 97.

hypothesis; or 5) in weighing evidence of multiple studies choose and weigh studies in a manner that supports the researcher’s position. In his article describing how to distort the scientific record without actually lying, University of Chicago Emeritus Professor and statistician John C. Bailar listed a analogous short catalog of general locations in research where distortions can be introduced: 1) choice of topic; 2) framing the question(s); 3) protocol decisions; 4) study performance; 5) data reduction; 6) analysis; 7) findings and conclusions; and 8) presentation. In effect, Professor Bailar, similar to the other three authors, is noting that research distortion can be introduced at almost any step of the process.

These areas of potential distortion can be divided into four broad areas, roughly corresponding to their position in the sequence of research: 1) the topic of the research and the specific question asked; 2) the study design which includes numerous subcategories, such as the population studied, the period studied, and the controls used; 3) data representation and statistics; and 4) interpretation and presentation. Examples of these areas of potential distortion are found throughout the medical and scientific literature. The following illustrations provide just a few examples of how the distortions are manufactured.

The research topic, along with the formulation of the specific questions being asked, provides the first and perhaps easiest way to influence the outcome of the study. In directed research, the “uncertainty principle” is one of the most frequently ignored design tenets of randomized studies. If researchers design their study to prove a specific position, there is no real advancement to the science. Given the inherent uncertainty of medical science, almost any position can have a study designed to establish a desired result. Even so, studies of this nature can be used to provide public relations support to a company’s product or to contend that the science concerning a product’s hazardous nature is contested, even when they are not answering the same relevant

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questions as studies that found a hazard. Public health advocates point to Pharmaceutical companies as a frequent abuser of this technique. One independent review of multiple myeloma studies determined “that this important principle [the uncertainty principle] can be violated, particularly when randomized trials are sponsored by or conducted on behalf of the pharmaceutical industry.”

The chapters of this dissertation are replete with examples of this practice, from asbestos manufacturers desiring to prove their products do not emit dust, to chromium manufacturers desiring to show no increase in lung cancer from their product, and refinery owners wanting to establish that its employees are exposed to only very low levels of benzene.

Simply asking a question more likely to be answered favorably can often easily modify the appearance of a study’s results. These “tricks of the trade” include testing a product against another that does not work well or testing a toxic product in the most favorable, and perhaps least likely conditions. In addition, they include publishing the results of a single epidemiological study in two parts in different publications or publishing only favorable studies and burying the rest. The problem also appears in review articles and meta-analyses (combining several studies to improve the power of the analysis); the author simply selects certain papers favorable to his or her position and synthesizes a pattern, while ignoring or disqualifying difficult papers.

Because corporate and legal records concerning the reasoning behind research design decisions are rarely part of the research record, information about the design process is not often available to researchers and historians. However, since company sponsored research often occurs while companies are in litigation concerning the product or process, relevant corporate documents are sometimes disclosed and preserved through the legal discovery process. Documents uncovered

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during the silicone-breast implant litigation provide one example of this, as well as several other methods of distorting the science. In this case, the company established four conditions before any research could be funded. The following quotation describes these conditions. I have embedded emboldened comments demonstrating how the conditions relate to the above-described methods of influencing research.

First, studies should look at traditional connective tissue diseases and not the atypical symptoms reported by clinicians in the literature. [Ask a question with a known, but not relevant answer] Second, studies should include saline as well as silicone implants … [Design the study to dilute the population] Third, the studies should use a test of significance (two-tailed) that considered both the positive and negative impacts of having silicone-breast implants, even though there were no hypotheses that silicone implants improved women’s health. [Select the most favorable method of statistical analysis] Fourth, all women who exhibited symptoms after 1991 should be excluded from the study. [Design/select a study period shorter than the disease latency period]46

In this instance the company not only ensured that the appropriate questions would not be asked during the research, but also guaranteed that the study would be designed in such a way that it studied a diluted population and was far too short in duration. In addition, the statistical test utilized made any significant positive result less likely.

Pharmaceutical studies of multiple myeloma, a cancer of the blood plasma cells, demonstrate another method of ensuring that the design is skewed toward obtaining the desired result—many studies picked an inappropriate control group. One independent analysis of randomized trials for multiple myeloma determined that

a greater proportion of industry-sponsored studies compared innovative treatment to either placebo or no therapy than did studies sponsored by public sources (60% vs 21%...). Equipose was seen in the studies in which innovative treatments were compared with active standard therapies irrespective of the source of funding (innovative vs active standard 59% vs 41% … in public sponsored studies and 50% vs 50% … in industry-sponsored trials). However, innovative treatments were favoured when the standard comparative treatment

46 Sheldon Krimsky, Science in the Private Interest, 156-57.
was placebo or no therapy (90% vs 10% … in commercially supported trials, and 70% vs 30% … in research sponsored by public funds).  

Thus, the industry studies were often designed to test the product in a manner to provide the best chance for favorable results, even though the results may not have been meaningful or relevant.

Even when a study is properly designed, its author can still relatively easily write an article that appears to buttress the sponsor’s initial position, even if the facts do not support that position, simply by manipulating the data. This type of manipulation occurred at a presentation before the November 2000 meeting of the Dioxin Review Panel of the EPA Scientific Advisory Board. During the meeting, industry representatives offered an analysis of dioxin studies that suggested that dioxin is a threshold carcinogen. If true, the EPA did not need to regulate low dose exposures. This testimony stalled action on dioxin for months. Subsequently, an independent group from Princeton University reexamined the analysis. They found that the industry group, including Dr. Dennis Paustenbach (further mentioned in most of the following case studies), had incorrectly weighted the data by cohort size. Without the improper weighting, the threshold effect disappeared.

One of the easiest ways to influence the outcome of an occupational disease study is by tweaking the exposure data. Small changes in the population or control group’s exposure data, or even eliminating certain exposure categories, can have large impacts on the results. An article in the *American Journal of Industrial Medicine* noted the importance of this issue to epidemiology:

> We believe of the two of the major methodologic issues raised in epidemiologic studies of occupational exposures, that is, confounding and exposure misclassification, the latter is of far greater concern. It is rare to find substantial confounding in occupational studies (or in other epidemiologic studies for that matter), even by risk factors that are strongly related to the outcome of interest. On the other hand, exposure misclassification probably occurs in nearly every epidemiologic study. For nondifferential

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misclassification, the type of misclassification most likely in cohort studies, the
direction of the bias is largely predictable, that is, a bias of relative risks toward
the null. In addition, the magnitude from relatively small amounts of
misclassification can be sufficient to lead to an interpretation of no effect.\(^{49}\)

Finally, data presentation can always put a proper spin on ambivalent or even doubtful data.
Suspect data can be made more credible by the inclusion of a well-known expert as author or an
author who is not commonly thought of as being biased. Certain companies have thus included an
expert as one of an article’s authors, even when the expert had little, or nothing, to do with the
article. This type of influence is not limited to the United States. Following *The New Zealand Medical
Journal’s* decision to require listing of sponsorship, one doctor complained about the medical
communication media companies that ghostwrite medical articles and ensure articles have the
“proper spin.”\(^{50}\) As will be closely examined in the chromium case study, ghostwriting can even rise
to the level of fraud.

These methods described above are not limited to any particular industry or product. Each
has been used in a wide variety of scenarios. As we shall see, the history of tobacco, asbestos,
chromium, and benzene health related attorney and industry directed research repeatedly displays
these techniques.

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The manufacture of medical research is of especial importance to attorneys defending their
client from claims that its products have caused disease. Casting doubt upon scientific evidence
supporting such a claim becomes of paramount importance. The current Administrator of OSHA,
epidemiologist David Michaels, has been especially critical of industry’s role in this type of scientific
research. In several papers and books, Michaels contends that numerous industry trade groups and

\(^{49}\) Aaron Blair, Patricia Stewart, Jay H. Lubin, and Francesco Forastiere, “Methodological Issues Regarding Confounding
and Exposure Misclassification in Epidemiological Studies of Occupational Exposures,” *American Journal of Industrial

companies are conducting scientific research with the specific objective of casting doubt on studies that demonstrate the deleterious health effects of their products. In his words, “polluters and manufacturers of dangerous products have waged sophisticated campaigns to manufacture uncertainty about the scientific evidence used to support public health protection and victim compensation.” 51

As Michaels explains in his provocatively titled article “Doubt is their Product,” uncertainty is an inherent problem of science—but manufactured uncertainty is another matter entirely. Over the past three decades, industry trade groups have frequently become involved in the scientific investigative process when their interests are threatened. If, for example, studies show that a company is exposing its workers to dangerous levels of a certain chemical, the business typically responds by hiring its own researchers to cast doubt on the studies…The vilification of threatening research as “junk science” and the corresponding sanctification of industry-commissioned research as “sound science” has become nothing less than standard operating procedure in some elements of corporate America. 52

Attorney Thomas O. McGarity has described two primary and related techniques by which “risk-producing” industries cast doubt on research that does not comport with their position: 1) “attack science” involves various techniques of casting doubt on specific studies so they can be portrayed as “fatally flawed;” and 2) the “corpuscular approach” focuses on persuading courts and agencies from relying upon “fatally flawed” studies.

McGarity argues that these attacks take many forms. Upon learning of a planned publication, a company might provide negative peer reviews or attempt to convince the journal not to publish the “fatally flawed” study. Once the article is published, the companies often hire experts to write

52 David Michaels, “Doubt is Their Product,” 96.
letters to the journal critiquing the study. This allows the companies to later make the case that the
study was “highly controversial.” They will sometimes appoint an “independent” panel to re-
evaluate the study. This panel will then conclude aspects of the study need to be improved before
the scientific community can accept it.53 One prime example McGarity uses is that of the drug
ephedra. When an article published in the New England Journal of Medicine concluded that Fen-Phen, a
diet drug, caused a serious heart valve disease, the manufacturer created a panel of cardiologists
from Harvard and Georgetown universities to evaluate the cases. Unsurprisingly, the reanalysis
conducted by these industry-hired experts determined that there was no reasonable medical risk.
They determined that any adverse event reports were instead due to the misuse of ephedra.54
Regulation of ephedra was thus delayed by four years, with a resultant large increase in both those
adversely affected and subsequent lawsuits.

Numerous other examples of “attack” science populate the medical literature. For example,
when the CDC issued an alert for Reye’s syndrome in children who took aspirin, the aspirin industry
raised 17 specific “flaws” in the studies and insisted the agency needed more reliable studies to
establish a causal link. These criticisms delayed government public educational program for two
years and mandatory labels for an additional four years.55

Pharmaceutical companies are not alone in their use of this tactic. As the following chapters
repeatedly describe, Chemical manufacturers and other industries involved in toxic substances have
historically readily used expert testimony to cast doubt on adverse reports. In fact, during the latter
years of the twentieth century, self-professed litigation support firms such as ChemRisk, Environ,
and Exponent marketed their attack science capabilities to corporations. One of these firms,

55 David Michaels and Celeste Monforton, “Manufacturing Uncertainty: Contested Science and the Protection of the
Exponent, even hired scientific experts for undisclosed clients to present evidence at conferences and prepare papers for presentation to the EPA seeking to stop or delay regulation, while at the same time its vice president sat on the EPA Science Advisory Board.\textsuperscript{56}

As will be vividly described in the chromium case study, ChemRisk’s superb client support is best illustrated by its role in the 1990s Chromium (VI) controversy. The movie \textit{Erin Brockovich} brought the drama of Chromium (VI) to the public’s attention. This movie described the efforts of one plaintiff lawyer’s paralegal to track down the cause of a concentration of illness in a local community. The full story of industry’s manufacture of science in the case of Chromium (VI) regulations is more complex. In 1993 the public interest group Public Citizen, along with the Oil, Chemical, and Atomic Workers International Union (OCAW) (now part of the United Steelworkers) petitioned OSHA to promulgate a stronger standard for Chromium VI based upon the findings of a study by the former Chief, Division of Industrial Hygiene, Ohio Department of Health, Thomas F. Mancuso. They subsequently sued OSHA for “unreasonable delay” in promulgating a new standard.

Initially the chrome industry, through its trade group known as the Chrome Coalition, did not respond to the petition, instead waiting for the completion of an EPA-funded study by The Johns Hopkins University, which they hoped would bolster the industry’s position that current standards should suffice.\textsuperscript{57} However, upon completion of the Hopkins study, the Chrome Coalition grew “concerned about how OSHA will interpret [the] information” contained in it. The Chrome Coalition therefore “felt that it was necessary to contract with a well regarded consultant in epidemiology (sic) and risk assessment to review all of the information that OSHA might use, determine the limitations and organize and develop a proper scientific basis (model) for predicting

\textsuperscript{56} Lennart Hardell, et al., “Secret Ties to Industry,” 231.
the impact of Hexavalent chromium on lung cancer in the workplace.” Subsequently, they hired ChemRisk and Environmental Risk Analysis, two self-avowed “litigation support” and “product defense” firms, to assist in limiting changes to the regulations. ChemRisk scientists outlined a strategy to reanalyze raw data from the yet-to-be published Johns Hopkins study “to forestall the [OSHA] rulemaking.” The Chrome Association’s attorneys, Collier, Shannon, Rill & Scott, PLLC, paid for the services of both ChemRisk and Environmental Risk Assessment, with the contract being administered by the Industrial Health Foundation (the successor to the industry sponsored Industrial Hygiene Foundation, and which is further discussed in Chapters 4 and 5). This enabled the companies to “…preserve the confidentiality of information, opinion, and data to the extent provided for under the attorney-client privilege and attorney work product privilege.”

The parties agreed to “full public disclosure of scientific information contained in the CSR&S-accepted final report.” However, as intimated by that sentence, the report was not “final” until completion, receipt, and approval [author’s emphasis] by the law firm. Thus, the attorneys fully controlled the publication and disclosure of the report. In the unlikely event that the report was not satisfactory, the attorney work product privilege allowed it to be kept hidden from public view. The Chrome Coalition also used the Industrial Hygiene Foundation to hire Environ—another litigation support firm—for the preparation of a new study of four plants, which used lower-exposure processes. These lower-exposures made it much more likely that the report would not have sufficient power to have a positive result. Yet, even here, they had to finesse the final report’s publication.58

58 The quotes “concerned about how” and “felt that it was necessary” are from Chrome Coalition and Ad Hoc PEL Committee memorandum of February 13, 1996, attached as an additional file 1 in David Michaels, Celeste Monforton, and Peter Lurie, “Selected Science;” the quote “to forestall the [OSHA] rulemaking” is from Chrome Coalition Ad Hoc PEL Committee Memorandum of February 13, 1996, attached as additional File 2 in David Michaels, Celeste Monforton, and Peter Lurie, “Selected Science;” and the quotes “Full public disclosure” and “Completion, and receipt” are from Paragraph 11., Agreement: Chrome Coalition, attached as an additional File 3 to David Michaels, Celeste Monforton, and Peter Lurie, “Selected Science.”
These activities culminated early in the new millennium. First, the consultants challenged the validity of the now-published EPA study. After extensive analysis, OSHA rejected those critiques. In 2004, OSHA published its proposed new regulations and conducted hearings. At the hearings, industry representatives did not mention that they had hired Environ to undertake a new epidemiologic study concerning the use of chromium in United States facilities. Then, just weeks before the close of post-hearing comments, Environ employees published an article about the relevant United States plants in the *Journal of Occupational and Environmental Medicine*. Given the limited time involved, the analysis had little statistical power, but the authors offered a “preliminary conclusion” that the lower exposures may have resulted in no elevated lung cancer risk. In its post hearing comments, the trade group Specialty Steel Industry of North America, who had been represented at the 1996 Chrome Coalition meetings, claimed it had recently learned of the study and warned OSHA that it would be “arbitrary and capricious”—a regulatory legal term—to ignore this study. The group also published a separate article on German chromate plants, which concluded that only an earlier and higher exposure group had any increased risk of cancer.59

A close analysis of the Environ study demonstrates how it both hid and manipulated medical science to arrive at the conclusions contained in the two published articles. The original report to industry looked at all four United States and German plants. When examined in this fashion, the study confirmed an elevated cancer risk for many of the workers. Even alone, the German plant final report showed risk elevated at the current regulatory allowed exposure level. However, the authors of the German paper arbitrarily combined two exposure categories that had been included in the final report, thereby resulting in the disappearance of elevated risk for those individuals right

at the current allowed exposure level. Only with the division of the report into separate articles on
the United States and German plants—contrary to the protocol of the study which repeatedly
emphasized the strength of a combined cohort—and the arbitrary combination of the German
categories allowed the authors to assert that only the high exposure cohort had increased risk. This
strategy of data reanalysis is now standard operating procedure for these litigation-support firms.60

The industrial giant Brush Wellman’s hiring of another consulting and research firm
specializing in product defense, Exponent, provides an illustration of another method that can be
used to cast doubt on research—suggest an alternative hypothesis that has not been considered in
current research. Brush Wellman is perhaps the world’s leading miner of the metal beryllium, a metal
vitally necessary in the nuclear weapons program, and perhaps the most toxic substance known to
humans. When OSHA attempted to reduce beryllium exposure limits, Exponent and Brush
Wellman employees wrote a series of articles suggesting that current research does not adequately
take into account the size and surface area of beryllium particles. They also suggested that skin
exposure could play a larger role. The net result of these hypotheses is that, if correct, current
standards might be sufficient. Brush Wellman subsequently argued that further research in this area
is necessary before any changes are made in the regulations. During the 1990s, DOE issued a new
standard, which lowered allowable exposures by a factor of ten despite this research. However,
under the George W. Bush administration, OSHA has followed Brush Wellman’s lead and called for
more research before taking a position.61

As President and cofounder of ChemRisk, Inc., and former Vice President of Exponent,
Dennis J. Paustenbach has made a career of similar industry assistance. In 1990, he developed a
proposal to the American Petroleum Institute (API), which he described as follows:

of the Integrity,” 367.
61 David Michaels, “Doubt is Their Product,” 98.
McLaren/ChemRisk is pleased to provide this proposal to develop an alternative cancer potency estimate for benzene. It is our understanding that API would like us to develop a succinct, yet scientifically compelling, integrated position statement to be used in comments to the state of North Carolina and as a possible springboard for future analyses that could be presented to US EPA and the State of California.\footnote{Dennis J. Paustenbach, “Revised Proposal to Develop an Alternative Cancer Potency Factor for Benzene,” API, July 5, 1990 cited in Susanna Rankin Bohme, John Zorbadian, and David S. Egilman, “Maximizing Profit and Endangering Health: Corporate Strategies to Avoid Litigation and Regulation,” International Journal of Occupational and Environmental Health 11 (2005): 341.}

The proposal went on to explain what the conclusion of the paper would be and stated that comments from API member companies and other API consultants would be incorporated into the paper. The paper was eventually published in *Environmental Health Perspectives* without disclosure that it had been designed around the conclusion and had been edited by industry representatives.\footnote{Susanna Rankin Bohme, et al., “Maximizing Profit,” 341; Dennis J. Paustenbach, R. D. Bass, and P. Price, “Benzene Toxicity and Risk Assessment, 1972-1992; Implications for Future Regulation,” Environmental Health Perspectives 101, Suppl. 6 (1993): 177-200; and P. R. Williams and D. J. Paustenbach, “Reconstruction of Benzene Exposure for the Pliofilm Cohort (1936-1976) Using Monte Carlo Techniques,” Journal of Toxicology and Environmental Health A 66 (1993): 677-781.}

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Any of these activities designed to influence published scientific research results activities can cross the line to become outright fraud. Until recently, fraud in scientific research was considered rare—most scientists “insisted that science was honest and virtually fraud proof.”\footnote{Lawrence K. Altman, and Laurie A. Melcher, “Fraud in Science,” British Medical Journal (Clinical Research Edition) 286, no. 6383 (June 25, 1983): 2004; also see Catherine DeAngelis, “Conflict of Interest,” 2237-2238; Catherine DeAngelis, “The Influence of Money,” 996-998; and Catherine D. DeAngelis and Phil B. Fontanarosa, “Impugning the Integrity,” 1833-1835. Historians have also addressed accusations of fraud against scientists in other fields; see for instance Daniel J. Kevles, *The Baltimore Case: A Trial of Politics, Science, and Character* (New York: W.W. Norton, 1998) and Jan Sapp, *Where the Truth Lies: Franz Moewus and the Origins of Molecular Biology* (NY: Cambridge University Press, 1990).} However, during the past twenty years, with increasing disclosure of corporate documents through lawsuits, there have been several examples of fraud for profit in medical science research.

During its efforts to forestall chromium regulations, ChemRisk was accused of overstepping this boundary. As the chromium chapter relates, ChemRisk employees stand accused of ghostwriting an article for a Chinese scientist who had previously studied chromium, and then arranging to have...
it published in the *Journal of Occupational and Environmental Medicine*. The article dramatically altered the Chinese scientist’s original findings. When this information became public, Paul Brandt-Rauf, editor of the *Journal of Occupational and Environmental Medicine*, took the unusual step of formally retracting the article. In keeping with the request of his attorneys to limit his announcement, he justified the retraction of the article by noting, “financial and intellectual input to the paper by outside parties had not been disclosed.”

This may not be an isolated incident. Four doctors who consulted as expert witnesses for plaintiff attorneys in litigation concerning Merck & Co.’s drug “rofecoxib” reviewed Merck internal documents to determine who actually wrote many of the articles published concerning the drug. They found that academically affiliated authors were frequently contacted only after an article was in preparation. Medical publishing companies ghostwrote many of the nearly complete drafts of review articles.

Another fraudulent method that can be used is the creation and funding of an allegedly “independent” research organization, which then funds medical research relevant to the company’s product. In 1954, U. S. tobacco companies created such an organization: the Tobacco Industry Research Committee. Unsurprisingly, this Committee sponsored research that found little harm in smoking. Following the public relations failure of the Committee, three United States tobacco companies created another organization in 1988, which, by its name, appeared less tied to tobacco companies: the Center for Indoor Air Research (CIAR). Its stated purpose was to fund high-quality, objective research related to indoor air and environmental tobacco smoke.

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67 Deborah E. Barnes and Lisa A. Bero, “Industry-Funded Research,” 516-517. As will be seen in Chapters 4, 6, and 7, the Industrial Hygiene Foundation has, at times, played a similar role for industry.
In fact, attorneys controlled most relevant research through a two-tier research program. The research organizations not only funded peer reviewed research, but also sponsored what were called “special projects.” Trusted attorneys designed these “special” studies to assist in developing scientific data to defend litigation. In coordination with tobacco attorneys, the organizations also cultivated private research relationships with scientists who would then be called to testify at trial or before governmental committees and agencies. All relevant research was designed to direct attention away from tobacco and toward other causes of cancer and structured to produce results that supporting the industry’s position concerning tobacco.68

These types of fraud do not stand in solitude. The editors of the United State’s leading medical journal report that even the peer review system might be suspect.

Another source that may contribute to the manipulation of research studies involves peer reviewers who have relationships with industry. Such reviewers may provide biased reviews that favor products of companies with which they have strong financial relationships, may fail to disclose their conflicts of interest to journal editors, or may even provide for-profit companies with confidential information obtained during the peer review process. For example, it was recently reported that a peer reviewer for the New England Journal of Medicine sent a confidential manuscript that he was invited to review and that demonstrated an increased mortality risk associated with rosiglitazone to the manufacturer of this drug weeks ahead of publication.69

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Vast arrays of techniques and procedures are available to manipulate medical science. They range from a subtly nuanced influence to the blunt bludgeoning of fraud. These practices are not new. Attorneys have used them throughout the twentieth century to manufacture the science they needed to reflect their litigation position. The following chapters will follow attorneys as they use the methods throughout the twentieth century. They will also explore how the actions of attorneys to

69 Catherine D. DeAngelis and Phil B. Fontanarosa, “Impugning the Integrity,” 1834.
manufacture science evolved and grew increasingly sophisticated, complex, and aggressive over time—remaining true to attorneys’ desire to have the evidence necessary for a successful outcome in their case or regulatory activity.
CHAPTER 4

SILICOSIS: THE DISAPPEARING DISEASE

The Chronology of Silicosis Public Knowledge

The Early Years

The recognition that certain dusts cause lung disease was not a new development of the twentieth century. Even ancient writers such as Hippocrates and Pliny the elder noted the hazards of certain types of mineral dust. During the Renaissance Georg Bauer of Saxony (better known by his Latin name Georgius Agricola) combined his interests in geology and medicine in his work *De Re Metallica*, which discusses metal mining and includes a section on the diseases of miners including those that "attack the lungs." He stated that dryness in the mine created great harm:

…for the dust which is stirred and beaten up by digging penetrates into the windpipe and lungs, and produces difficulty in breathing, and the disease which the Greeks calls asthma. If the dust has corrosive qualities, it eats away the lungs and implants consumption in the body.

The 1700s brought even more complete descriptions about the dangers inherent in mineral dusts. Both Italian physician Bernardino Ramazzini, often considered the father of occupational medicine, and English physician Thomas Beddoes noted that stone workers often develop pulmonary consumption from the dust entering their lungs. As Ramazzini noted, the effects of dust on stonemasons’ lungs could be quite dramatic:

Often times suck in by inspiration the sharp, tough small splinters or particles which fly off so that they are usually troubled with a cough and some of them turn asthmatic and consumptive. In dissecting the corpses of such artificers, the lungs have been found stuffed with little stones. Several stone cutters who died from asthma were opened and in their lungs were found such heaps of sand that in running the knife through the pulmonary vesicles it seemed that one was cutting some sandy body. ¹

¹ The quotes about Agricola and Ramazzini are from Rosamond W. Goldberg, *Occupational Diseases*, 14 and 37, respectively. While ancient knowledge is often not relevant to modern science, in this case it clearly demonstrates how easily recognized such diseases have always been to companies that are knowledgeable about mining and related activities.
As Frederick Hoffman, chief statistician for Prudential Insurance Company, could confidently state in 1918: “The importance of dust as a factor in occupational mortality has attracted the attention of every authority on occupational diseases from Ramazzini to Sir Thomas Oliver.”

Late nineteenth and early twentieth century doctors fully recognized both silicosis as a specific dust disease caused by exposure to crystalline silica (sand or quartz dust, for example), as well as its link to tuberculosis, a debilitating and deadly consumptive disease of the lungs. In 1887, an autopsy of a stove foundry worker described the cause of death from a condition now known as silicosis. That same year a report described a chronic air passage condition in 34 workers in a cutlery factory that resulted in the deaths of at least 23 men. Over a period of five years during the 1890s in Nevada quartz dust created by milling killed ten percent of quartzite miners. In 1889, New York Board of Health pathologist Doctor T. M. Prudden emphasized the importance of a dusty atmosphere to the spread of tuberculosis.

Yet, as recognition of the dangers of silica grew, so did the number of workers exposed to the hazard. During the opening decades of the twentieth century, foundry operations grew in both size and number. By 1914, there were 18,000 foundries in the United States, employing 660,000 workers. The concomitant growth of mechanization and technology in foundry operations was of even greater significance for the spread of both silicosis and tuberculosis. As new power and pneumatic tools with their dramatically increased abrasive actions became standard, dust increased.

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2 For a complete historical study of miners’ diseases from prehistoric times through the nineteenth century see George Rosen, The History of Miners' Disease. Many medical practitioners specializing in occupational disease have noted the long history of knowledge concerning lung disease from dusts. One of the best examples is Emery Hayhurst, “Health Hazards of Non-Poisonous Dusts,” 60-65. The Hoffman quote is at 60. Histories produced by industry-oriented associations, such as the Industrial Medical Association, also acknowledge that experts have known for centuries that rock dusts and sand cause silicosis type diseases. Henry B. Selleck, et al., Occupational Health in America. See in particular 133, 142, and 203.


5 David Rosner, et al., Deadly Dust: Silicosis and the On-going Struggle, 51-57.
Three processes primarily accounted for the intensification of dust. In most foundry operations binders were initially mixed with sand for the molding process. As the new century progressed, power mixing, screening, and cutting machines increasingly replaced handwork in this process. After the molding process and upon partial cooling of the poured metal, the mold was taken off in what was called the shakeout process. Newly developed pneumatic tools were then used to remove the remaining sand from the casting. The increased abrasion caused both increased dust and finer dust particles. During the final process, the casting was freed of all sand and polished. Modern foundries cut costs and improved the final product by using tumbling machines with sand as well as sandblasting in this final procedure. Both of these methods increased dust and decreased airborne particle sizes.6

Similar mechanization also occurred in most other industries using silica, including mining, stone work, sandblasting, etching, and brick making. The twentieth century advances in technology provided numerous new mechanical tools for mining and manufacture, resulting in faster and more efficient practices. Unfortunately, when used on dry materials, these tools often created excessive amounts of dust. At times workers could not see across their workroom. While some dusts merely caused discomfort, others, particularly silica dust, resulted in long-term progressive fibrosis of the lungs. Thus, dust diseases dramatically increased in a wide variety of industries.

In addition, the new equipment reduced both the level of worker expertise necessary in many industries and the stability of the work force. By 1918, these molding machines and tools had enabled foundry owners to reduce their skilled long-term labor force. Unskilled or semiskilled labor took up an increasing percentage of the work force. Frequently they only stayed for a few months or

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6 Ibid.
years. This turnover, often caused by increasing ill health, dramatically increased the numbers of the most exposed workers.\(^7\)

Concern about the growing problem of tuberculosis in dusty work led the Public Health Service and the Bureau of Mines to conduct the first systematic investigations of dust diseases in 1913-1915. Anthony J. Lanza led the investigations. He had joined the Public Health Service in 1907, as a young physician, becoming Chief Surgeon to the Bureau of Mines in 1914. The landmark report of the investigations, issued in 1917, established Lanza as one of the foremost experts on pneumoconioses (dust diseases involving fibrosis of the lungs).\(^8\)

These investigations established silicosis and other diseases of the lung as ubiquitous in the American mining industry. Over sixty percent of miners had some form of lung fibrosis. Of 720 miners examined, only 179 were free of lung disease; sixty-nine had more fibrosis of the lungs than normal; 330 had silicosis; 105 had silicosis and tuberculosis; and thirty-nine had uncomplicated tuberculosis. Lanza and his coworkers also established that silicosis at any stage caused breathing difficulties: "The first stage [was] characterized with slight or moderate dyspnea on exertion," even if the individual "looked robust." A similar study of Butte, Montana miners found similar disease; of 1,018 miners, 194 had early silicosis, 128 had moderate silicosis, and 110 had severe silicosis, with 63 of these severe cases being complicated with tuberculosis.\(^9\)

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7 Numerous authors have catalogued the increased industrial use of power tools in the early twentieth-century, as well as their effects. See for example David Rosner and Gerald Markowitz, "Consumption, Silicosis, and the Social Construction of Industrial Disease," *The Yale Journal of Biology and Medicine* 64 (1991): 481-498; David Rosner, et al., *Deadly Dust: Silicosis and the Ongoing Struggle*, 51-57; Rosamond W. Goldberg, *Occupational Diseases*, 41; Gerald Markowitz, et al., "The Illusion of Medical Certainty," 230; and Victoria M. Trasko, "Silicosis, a Continuing Problem," 839. Histories produced by industry oriented associations, such as the Industrial Medical Association, also noted that the increased employment and use of pneumatic rock drills in the early twentieth century made new preventive measures imperative: see Henry B. Selleck, et al., *Occupational Health in America*, in particular 133, 142, and 203.


By the early 1920s, industry doctors were also noting the early onset of breathing problems in dusty industrial and stonework environments. P. H. Hourigan, the medical director of a Buffalo business, stated in 1924 that the first sign of the disease was dyspnea. He went on "It comes on so gradually and insidiously, and the patient so unconsciously avoids efforts which increase this difficulty, that you marvel at the objective evidence of increasing difficulty of breathing developed before the person afflicted with silicosis recognizes the lessened capacity of his lungs."\(^{10}\)

By this time, Federal government, academic and industry experts all recognized that miners were not the only workers at risk for silicosis and tuberculosis. The 1918 U.S. Bureau of Labor Statistics Bulletin list of workers exposed to harmful mineral dusts included asbestos workers, molders, brick makers, stone workers, plasterers, and most miners among many others. One year later Emery R. Hayhurst, Assistant Professor of Hygiene at Ohio State University, estimated that 5,000,000 people were being exposed to hazardous industrial dust. While not certain about the specific causal connection, Hayhurst noted the over twofold increase of tuberculosis in individuals exposed to industrial dusts. Tuberculosis, long recognized as a major disease of industrial workers, was one of chief causes of death among those of working age. Charles-Edward A. Winslow, a prominent industrial medicine expert and Yale Professor, who would shortly become president of the American Public Health Association, also argued that silica was responsible for this high tuberculosis rate among industrial workers. Nor was this recognition limited to the United States. In 1921 Thomas Oliver, one of the foremost specialists in occupational medicine in the United Kingdom, also sounded the alarm about the connection between dust and tuberculosis. "[T]he tendency of modern pathology is to look upon all pulmonary phthisis or consumption as

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tuberculosis, but the fact remains that phthisis can be caused by dust." He particularly noted the
dangers of four dust diseases, or pneumoconioses, including silicosis, writing that sufferers of
pneumoconiosis were more likely to get tuberculosis.\textsuperscript{11}

Other reports during the 1920s documented the rapid rise of mortality from tuberculosis in
workers exposed to silica after introduction of powered equipment. For example, in 1920 one study
of 427 granite cutters determined that practically every worker had silicosis, tuberculosis, or a
combination of them. A follow up study of the Vermont granite industry by the Public Health
Service established that practically all workers exposed to high concentrations of silica dust
developed silicosis with a substantial number also contracting tuberculosis. Consequently, in the
early 1920s the Bureau of Mines issued warnings on the dangers of dust being created by high
velocity drills.\textsuperscript{12} The excavation of tunnels for the New York subways provided a similar stark
scenario. Occupational health specialists studied these excavation workers during 1929. They found
that 57\% of the workers had silicosis and 9\% had tuberculosis. Examination of the union member
death certificates was even grimmer; of twenty-one natural deaths due to disease, ten were due to
tuberculosis and seven to lobar pneumonia. Although this was abnormally higher than the general
population, these figures were in line with other miners of high silica content rock.\textsuperscript{13}

At an October 1929 presentation before the Industrial Hygiene Section of the American
Public Health Association, members of the Committee on Silicosis documented numerous similar
extraordinary rates of disease among silica workers in the United States and Great Britain. The
report also commented about the increasing number of studies on dust in the lungs. The studies

\textsuperscript{11}David Rosner, et al., \textit{Deadly Dust: Silicosis and the Politics}, Rosamond W. Goldberg, \textit{Occupational Diseases}, 36 and 195;
Emery R. Hayhurst, “Health Hazards of Non-Poisonous Dusts,” 60-1; and Thomas Oliver, \textit{Dangerous Trades}, 272.
\textsuperscript{12}Victoria M. Trasko, “Silicosis, a Continuing Problem”; and A. E. Russell, R. H. Britten, L. R. Thompson, and J. J.
Bloomfield, “I. The Health of Workers in Dusty Trades and II. Exposures to Silicosis Dust (Granite Industry),” \textit{Public
from Martin Cherniack, \textit{The Hawk's Nest Incident}, 38.
\textsuperscript{13}Rosamond W. Goldberg, \textit{Occupational Diseases}, 41-42.
included silicosis in mining, the sandstone industry, the granite industry, grinding, sandblasting, etching, and pottery. Committee members further observed both that "ground silica is used in many industries, and, in the crushing and grinding of the silica and in the industries in which ground silica is used, silicosis, sometimes of an acute type, occurs" and "the mining in Cornwall has had a high mortality rate from tuberculosis, and it has been shown that this is associated with silicosis."

The report finally cautioned practitioners not to rely upon the moderate levels of silicosis in some studies. Levels of silicosis might be much higher "since the men were all examined at work, any case so far advanced as third stage, and also many in the second stage, would not be found at work."

A portion of the included information came from Metropolitan Life, the employer of Lanza, the committee chairman. Metropolitan Life’s and other insurance research data showed that mortality from respiratory diseases was significantly higher for persons exposed to silica, whether or not they had silicosis; between ages 45 and 54 it was more than three times higher.

Thus, by the Great Depression, although the mechanism producing the characteristic changes was not known, occupational doctors fully understood both that silicosis is caused by crystalline silica such as sand and is correlated (indeed, historic articles point to even a stronger relationship) to tuberculosis. At that time, as it remains today, diagnoses of silicosis were based primarily on data from three sources: an occupational history, a physical examination, and a lung X-ray. Other respiratory diseases, including tuberculosis and pneumonia, frequently cause complications. Until the 1930s, most industrial health professionals accepted Doctor Gardner’s definition of silicosis.

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14 The report also declared that the suggested industry standards of between 5 and 10 million particles per cubic foot of airborne silica dust in the workplace did not prevent silicosis. The authors emphasized that the standards had been developed, in part, because they “could be reached by the use of economically practicable ventilating systems.” R. R. Sayers, et al., “Effect of Dust on the Lungs,” 368 and 372 (quote).

Silicosis is a chronic disease of the lungs resulting from prolonged inhalation of fine particulate silica. It is manifested anatomically by formation of sharply defined fibrous nodules not over four to six mm. in diameter, which in most cases are uniformly distributed throughout all portions of both lungs; and clinically by a paucity of symptoms and physical signs that usually appear only in the late stages of the disease and be a tendency to be complicated by tuberculosis. [emphasis added]

The late 1920s and early thirties also witnessed numerous reports documenting cases of silicosis after only short exposures. For example, in 1932 Earle M. Chapman, a doctor at the Medical Clinic of the Massachusetts General Hospital, authored an article in the prestigious and selective *Journal of the American Medical Association* about several cases of acute silicosis. In the article, Chapman reported on three individuals who he diagnosed with severe silicosis. At the time of the diagnosis, they had been working in the area of silica and soap being dry mixed for between eight to twenty nine months. The patient’s X-rays exhibited severe fibrosis that obliterated most of the lung fields. Lung biopsies of two individuals revealed lung silica content that was only 10% of that associated with chronic silicosis, but ten times the amount found in normal lungs. Chapman also observed that in addition to his cases there were two additional new cases of probable silicosis. Chapman provided

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16 Clayton S. Smith, et al., “The Silica Content, 1250; and Warren A. Cook, “Industrial Dust,” *Health Practices Pamphlet* no. 4 (Chicago: National Safety Council, Inc., undated, circa. 1929): 3. As occurred with most dust “experts”, Doctor Gardner—prominent industrial medicine expert, director of the respected Saranac Laboratories, and frequent consultant to industry—modified his definition in the 1930s to leave out the last phrase. As defined in the 1930s by Doctor Leroy Gardner, “[s]ilicosis means a disease of the lungs due to breathing air containing uncombined silicon dioxide dust, characterized anatomically by generalized nodular fibrotic changes throughout both lungs, which are demonstrable by x-ray examination and by autopsy, and resulting from any process of occupation involving inhalation of silicon dioxide dust.” Leroy Gardner personal communication cited in L. E. Hamlin, M.D., “Review of Silicosis for the Industrial Hygienist and Medical Practitioner,” Leroy U. Gardner, M.D., Ed. *Industrial Tuberculosis Silicosis and Compensation* (New York: National Tuberculosis Association, 1945), 45. The x-ray is by far the most important aspect of the diagnosis. In a moderate case of silicosis, even knowledgeable laymen (such as myself) can, in many cases, correctly diagnose the disease. As stated by Doctor Gardner, “[W]hile there are other conditions which may simulate the roentgenographic picture of silicosis, the shadow pattern described when taken in conjunction with the clinical story and a history of adequate exposure to free silica dust constitutes strong evidence for a positive diagnosis.” Leroy U. Gardner, “Elements of Diagnosis and Prognosis in Pneumoconiosis,” Leroy U. Gardner, M.D., Ed. *Industrial Tuberculosis Silicosis and Compensation*, (New York: National Tuberculosis Association, 1945), 76. In the 1930s, as today, the disease was normally diagnosed as one of three stages. While currently this diagnosis relies primarily on the x-ray and objective breathing tests, in the 1930s the diagnosis relied more on the doctor’s subjective opinions concerning breathing problems: see Philip Drinker et al., *Industrial Dust.*
citations to several other similar reports in Britain and the United States. This landmark article provided strong evidence of silicosis from even short exposures to silica.\textsuperscript{17}

Two years later in a study of pottery workers, Paul A. Quintance, Associate Medical Director of Golden State Hospital in Los Angeles, examined acute and chronic forms of silicosis. His review of the literature listed Chapman’s study, other published reports, and additional reports Quintance had obtained through personal correspondence. He noted that the acute form is somewhat different from chronic silicosis and death usually came from pneumonia or tuberculosis. In his study of 106 pottery employees, he took x-rays of fifty-eight employees who had an average exposure period of seven and a half years. 8.6\% had the highest stage of silicosis. 38\% had moderate silicosis. The remainder all had a demonstration of at least slight fibrosis.\textsuperscript{18} In 1937 Saranac Laboratory in upstate New York, a research facility associated with the Trudeau Foundation and a tuberculosis facility noted for its close work with industry, detailed a similar case history of a black foundry worker. He was employed as a sandblaster for fourteen months, at which time he had to give up his job due to shortness of breath. He then deteriorated rapidly and died of silico-tuberculosis fourteen months later.\textsuperscript{19} One year earlier, in what became essentially the textbook of industrial hygiene, Philip Drinker, Professor of Industrial Hygiene at the Harvard School of Public Health, wrote “lesions of silicosis in a mild degree can be produced in an animal by thirty hours exposure to intense dust clouds.” After reviewing the literature, he concluded that, in intense dust situations, the first stage of silicosis in humans could occur in as little as eight months.\textsuperscript{20}

\textsuperscript{19} Thomas H. Milby, “Pneumoconioses,” 47-48; and David Rosner, et al., \textit{Deadly Dust: Silicosis and the Politics}, 78. Saranac Laboratory was founded in 1890 as a tuberculosis research center by Edward Livingston Trudeau, an early pioneer in tuberculosis treatment and research. See David Ozonoff, “Failed Warnings,” 200.
\textsuperscript{20} L. E. Hamlin, “Review of Silicosis,” 49; and Philip Drinker et al., \textit{Industrial Dust}, 28, 77.
These increased levels of both chronic and acute disease were occurring in what should have been the healthiest segment of the population. In industrial workers, there is a natural selection of the fittest; individuals who are most susceptible to dust do not apply. The general population is sicker. As one authority wrote,

. . . if an industry involves no health hazards of consequence, the sickness rate among its employees should be lower than in the general population… In a hazardous industry this process of natural selection is of greater importance than in nonhazardous plants since it acts to remove those especially unfitted for exposure to the particular hazardous material involved.\textsuperscript{21}

\textit{The Hawk’s Nest at Gauley Bridge}

Even with this growing recognition of the dangers, it took an extraordinary event to shock the nation into full awareness of the dangers of silicosis. This event, The Hawks Nest tunnel tragedy during the early 1930s, became the greatest single industrial tragedy in the history of the United States. For a short time, silicosis became a nationally recognized nightmare, far beyond the then normally accepted risks of life. Newspaper and magazine headlines across the country expressed shock at the “silicosis menace.”\textsuperscript{22}

About thirty-five miles east of Charleston, West Virginia, as U.S. 60 begins to descend into the New River gorge, a simple roadside marker memorializes the Hawk’s Nest tunnel. The roadside marker describes the tunnel and its purpose without any indication of its cost in human lives. The three-mile long tunnel was built to divert water from the New River, primarily to provide power to a Union Carbide subsidiary’s plant.\textsuperscript{23}

On March 13, 1930, officials awarded the tunnel construction contract to Rinehart and Dennis, an experienced contractor. Ground breaking occurred on March 31, 1930, with the tunnel

\textsuperscript{21} Philip Drinker et al., \textit{Industrial Dust}, 78.
\textsuperscript{22} See for example “Silicosis Menace,” \textit{Business Week}, September 1933, 19-20; and “Silicosis Menace,” \textit{Literary Digest}, December 15, 1934, 118.
\textsuperscript{23} Martin Cherniack, \textit{The Hawk’s Nest Incident}, 1-2.
being completed in December 1931. The tunnel went through more than a mile of high silica content rock. Over 5000 men worked on the tunnel, 65% of them African Americans brought in from other states. Only 738 whites worked in the tunnel for any portion of their employment. Of the 1494 men who worked exclusively in the tunnel, 1115 were black. Most of the tunnel workers did not work for the entire construction period. In fact, the largest work force in the tunnel at one time was 600.\textsuperscript{24}

Increasing ill health constituted the primary reason most workers did not remain for the entire construction period. Work conditions during tunneling were abysmal. Even occupational doctors closely associated with industry, such as Doctor Leroy Gardner of the Saranac Laboratories, recognized that the conditions were horrific. As described by Doctor Gardner, in the mine "[n]o atmospheric dust counts were available, but from all reports excessive amounts of extremely fine dust were generated… In the tunnel which was bored without effective ventilation, the men returned to the face immediately after blasting and were required to work with dry drills in an atmosphere so exhausted by gasoline engines that they must have breathed abnormally fast. Such conditions would obviously favor the inhalation of unusually large quantities of very fine dust."\textsuperscript{25}

Subsequent autopsies confirmed the speed at which silicosis can form under these conditions. Leonidas Ryan Harless, M.D., the Gauley Bridge practitioner who first raised the alarm about the condition of the miners, sent lung material of nine miners to noted pathologist Clayton Smith for review. In his report of the results, Smith noted that a previous study had found that when SiO2 (silica) in the dried portion of the lung reached 1.6%, there were "practically no exceptions to

\textsuperscript{24} Martin Cherniack, The Hawk’s Nest Incident, 16-18, 29.

\textsuperscript{25} Martin Cherniack, The Hawk’s Nest Incident, 24-51; and Leroy U. Gardner, “Pathology of So-Called,” 1241. Philanthropic money for the Saranac Laboratories tuberculosis programs significantly decreased by the late 1920s. As a result, after becoming director in 1927, Gardner turned its focus to dust diseases rather than tuberculosis. With the onset of the 1930s silicosis crisis, business dramatically increased at the laboratories. See David Ozonoff, “Failed Warnings,” 201.
the diagnosis of silicosis.” In Harless’ group, Smith found this type of strong evidence of silica in virtually all of the lung tissue. As expected, he also found a strong correlation between the severity of silicosis and the amount of silica in the dried lung of the miners. Nor was extensive mining experience necessary for Gauley Bridge workers’ silica lung content to reach 1.6%. One miner, with no previous mining experience, had approximately 2% SiO2 in his lungs, even though he had worked only 40 weeks in the Gauley Bridge mine during 1931.

Since most of the workers at the site were transient and left the area when they stopped working, the number who died as a result of work in the tunnel has remained controversial, with industry providing numbers in the low hundreds, in contrast to labor reports of deaths of over one thousand. While there were many lawsuits filed by workers and heirs, the cases were settled and all records turned over to Union Carbide. Through meticulous research of surviving data and sophisticated epidemiological techniques, Martin Cherniack was able to arrive at a conservative number of deaths in excess of 700, thus making Gauley Bridge tunnel the greatest industrial health disaster in United States history.

Media coverage of this tragedy brought the problem of silicosis to the attention of the general public. As a result, occupational medicine experts, Federal officials, and industrial management all agreed that something needed to be done about the silicosis problem, only differing in their descriptions of the nature of the problem. In January 1932 a small group of experts representing the Federal Government, academia, and industry, met to discuss and coordinate research on silicosis. At this meeting, Drinker suggested that they continue meeting and call

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26 Clayton S. Smith and Helen L. Wikoff, “The Silica Content,” 1250. Gardner subsequently demonstrated his loyalty to industry by denying that the nine pairs of lungs were silicotic, apart from microscopic nodules. He suggested rather that the workers, all black, had “acute caseous pneumonia,” likely caused by their “lack of racial immunity.” L. U. Gardner, “Pathology, Human and Experimental,” in B. E. Kuechle (ed.), Symposium on Silicosis: An Unofficial Transcript, Trudeau School of Tuberculosis at Saranac Lake, New York, June 18 – 22, 1934 (New York, 1934): 22-50, 43, and 46.

27 Cherniack, The Hawk's Nest Incident, 112-170.
themselves the “Konicide Club,” which can be loosely translated as the killer dust club. By the end of the decade, the club members had emerged as the dominant experts on silicosis. Most, including Drinker, Lanza, representing Metropolitan Life Insurance Company, Royd R. Sayers of the United States Public Health Service, Eugene Pendergrass, Professor of Radiology at the University of Pennsylvania, and William P. Yant, of the United States Bureau of Mines, also played important roles in a new industry occupational health association formed shortly thereafter.28

Three years after the Konicide Club’s first meeting, at the height of the silicosis crisis and following broad publicity of the Hawk’s Nest disaster, numerous companies sought to solve this “dust problem” by establishing a new occupational health research and coordinating agency. This agency had its conception on January 15, 1935, at the “Symposium on Dust Problems” held in Pittsburgh, Pennsylvania. The Symposium, presided over by William P. Yant, and attended by nearly 200 industrial representatives, included addresses by, among other silicosis experts, R. R. Sayers, A. J. Lanza, Eugene Pendergrass, and Philip Drinker. Symposium attendees appointed a Temporary Organization Committee to develop plans and make recommendations for further activities. The Temporary Committee then held its own meeting at which the committee members decided to recommend the formation of a permanent confidential organization devoted to occupational health. The organization would include within its areas of interest “…medical, preventive, legal, legislative and publicity features of the industrial dust problem…” The Committee determined that the organization "must have a broad outlook, a sympathetic understanding of the problem, and wide contacts with all cooperating agencies; and it must have the confidence of the industries, and of the physicians, engineers and all institutions, groups or individuals who might cooperate to advantage.”

The new organization could determine which specialists "may not be properly qualified and whose results might, therefore, not be entirely acceptable… to be in position to make reasonable grants to qualified individuals and agencies to study special problems or phases of problems." Within a year, the Air Hygiene Foundation had attracted numerous companies to its membership and had a full program of industrial hygiene concerns.\textsuperscript{29}

\textit{The National Silicosis Conference}

By late 1935, the publicity surrounding the silicosis epidemic, particularly of the Hawk’s Nest Tunnel, was causing national outrage. Papers throughout the country headlined silicosis claims. A search for the word ‘silicosis’ in one online newspaper archive shows five front-page headlines in its nationwide newspapers for the ten year period of 1921 and 1930, but one hundred seventy seven front page headlines for the six year period of 1931 to 1936. These headlines appeared in newspapers as diverse as the \textit{New York Times}, \textit{Washington Post}, \textit{Ironwood, Michigan Daily Globe}, Charleston West Virginia \textit{Daily Gazette}, \textit{Raleigh Register}, Montana \textit{Daily News}, El Paso \textit{Herald-Post}, and the Hagerstown \textit{Daily Mail}. National magazines, such as \textit{Business Week} and \textit{Time} also carried stories about the silicosis crisis.\textsuperscript{30}

With the mounting publicity, the Labor Department of the new Democratic administration became increasingly concerned with the disparate points of view about the social, scientific, economic and political implications of the silicosis crisis. Thus, a little over one year following industry’s “Symposium on Dust Problems” the United States Department of Labor's Division of Labor Standards organized its own National Silicosis Conference. The conference convened in


Washington on April 14, 1936. Half of the attendees "were … representatives of employers in the
dusty trades," twenty five percent were Federal and state officials, insurance companies employed
ten percent, with the remaining five percent representing labor interests.\textsuperscript{31}

Following an initial daylong meeting, the Secretary of Labor appointed four committees to
carry out investigations and make recommendations. These Committees studied four issues relating
to the crisis: 1) Prevention of Silicosis Through Medical Control; 2) Prevention of Silicosis Through
Engineering Control; 3) Economic, Legal, and Insurance Phases of the Silicosis Problem; and 4)
Regulatory and Administrative Phases of the Silicosis Problem. The subsequent majority reports
prepared by the committees suggested that industry had the necessary knowledge and was taking the
actions required to control the problem. They recommended that any future silicosis cases be
handled through the states’ workmen’s compensation system. The Regulatory Committee charged
with examining the workmen’s compensation system contained numerous governmental, insurance,
and corporate officials and attorneys, along with one line labor representative. Their majority report
recommended a minimum of two years in which to file claims following the last exposure and a
medical advisory board. “Certain” members believed the board’s determination of injury shall be
final. One lone member, Martin P. Durkin, representing the Illinois Department of Labor objected
to the finality of the medical board opinion. In his transmittal of the reports to the Secretary of
Labor, The Director of Labor urged consideration of labor’s minority positions, which differed
dramatically from the majority reports, but still thanked the numerous individuals and organizations
for their efforts and called the “cooperative undertaking” a success. The federal government took no
further on the minority positions.\textsuperscript{32}

The Demise of Silicosis?

Thus, in the late 1930s, with the seeming approval of the Federal government, most industry and government occupational health experts considered silicosis to be a disease of the past. At the fourth annual Saranac Laboratory Symposium on Silicosis, held in the late 1930s, one state official lauded the technical innovations that had made possible the eradication of the disease. “The most important accomplishment of these preventive measures is that silicosis is becoming a negligible factor, and that in the future it will largely be stamped out by preventive measures that have been instituted.” This indeed appeared fortunate, since in 1939 University of Toronto pathology professor Max O. Klotz reported, “silicosis may be a definite predisposing factor in the development of carcinoma of the lung.”

By the end of World War II all industry experts, almost all government representatives and academic experts, and most union officials accepted the position that silicosis had become a disease of the past. What few cases might remain could be readily handled by the workmen’s compensation system. If cases continued to be diagnosed, then the diagnoses must be wrong, possibly the result of “racketeering” plaintiff lawyers. Gardner, Director of the Trudeau Foundation, a well-known tuberculosis research facility in the Adirondacks, was so sure of this position in 1946 that at an Industrial Hygiene Foundation Annual Meeting he apologized for discussing “such a trite and hackneyed subject.” His talk on “Accurate Diagnosis of Silicosis” at the Medical and Engineering Session was only necessary because far too many doctors and technicians were improperly diagnosing silicosis. During the discussion portion of the session, Oscar. A. Sander, federal occupational medicine specialist and frequent consultant to industry, went even further, contending

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H. Oliver represented the Glen Alden Coal Company; Leon S. Senior represented the Compensation Insurance Rating Board; and Ambrose B. Kelly represented the American Mutual Alliance.

that the apparent continuing problem of silicosis diagnosis was one of semantics. The term pneumoconiosis should be redefined because it was so “widely misunderstood” to imply pathology. It should simply mean, “dust added to the lungs without any implication of fibrosis or disability.” This, of course, ignored the long-term chronic nature of the disease.  

By 1946, journals and popular magazines also began reporting the demise of silicosis. In an article entitled “Silicosis is Not a Threat to Worker’s Health,” *Science News Letter* reported that experts announced at the Industrial Hygiene Foundation’s annual meeting that “the dangerous trades of our fathers has [sic] all but disappeared.” The experts also asserted that while “dust control continues to occupy the major place” in industrial health problems, in the post war period “the more common nuisance dust exposures [are] more injurious to the mechanical equipment than to workmen.” *Business Week* reported that silica was only a problem to hypersensitive workers. It suggested that one mill with a silicosis problem should avoid hiring blond workers. “Brunettes, who generally have more hair on the body, naturally have more hair in the nostrils, which tends to keep silica dust from reaching the lungs.”

The Industrial Hygiene Foundation (IHF) fully supported these pronouncements. Andrew Fletcher, the Board Chairman of the IHF declared the end to almost all occupational diseases in 1955. That year he told the annual meeting “both industry and the Industrial Hygiene Foundation can look back proudly on their accomplishments. Occupational diseases are for the most part eliminated.”

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Yet, pronouncements did not make it so. As early as 1937 the Air Hygiene Foundation, later named the Industrial Hygiene Foundation (in 1941), had notified its members that then current efforts might not be sufficient to eliminate silicosis. That year the Air Hygiene Foundation’s Medical Committee, chaired by Lanza, published its first medical Bulletin. The committee suggested a standard of 5 million particles per cubic foot of air for granite and other high-silica containing dusts. "It is desirable to avoid concentrations of more than 5,000,000 particles of dust containing a high percentage of free silica in working places." Yet, the committee raised serious questions about the efficacy of the proposed standard for protecting workers’ lungs. They admitted that they did not have "the knowledge upon which to base such thresholds [of dustiness]." The only data available were "bench marks ... to indicate to the engineer the result to which he should strive." They then reiterated that "[c]oncentrations to which the dust must be reduced in order to be safe have not been absolutely determined."

In his seminal book, Professor Drinker agreed with the medical committee’s conclusion about the limited usefulness of the standards: "[t]he idea of adopting standards of permissible dustiness for each harmful dust has a medicolegal appeal that is not at all justified by the data available today... In none of the original studies was there a single suggestion that the threshold figures were useable as legal standards." Drinker emphasized that one study determined that "the variations in dustiness were so great as to make unpractical an attempt to study the sickness rate in relation to dosage except in a general way."

Drinker was correct about the limitations of the data. One U.S. Public Health Service study of granite-cutting industry provided the primary source for the Medical Committee’s adoption of

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38 Silicosis and Allied Disorders, History and Industrial Importance, Medical Series, Bulletin no. I (Pittsburgh, PA: Air Hygiene Foundation of America, Inc., April 15, 1937), 169.
39 Philip Drinker et al., Industrial Dust, 78.
“safe” maximum levels of dustiness. It provided, at best, only limited support for the Committee’s standard. The study determined that a maximum dust concentration of between 10 and 20 million particles per cubic foot of air was a “desirable” limit for dust. This conclusion was based upon the fact that this limit could be reached with the use of economically practicable ventilating devices. "It should be noted that the limit established was not found to prevent the occurrence of silicosis."\footnote{R. R. Sayers, et al., “Effect of Dust on the Lungs,” 372.}

The following decades brought disturbing indications that Lanza and Drinker were justified in their concerns and cautions. Following World War II, company doctors and medical directors occasionally spoke out about the recommended levels of silica exposure being too high. For example, Doctor Emmet Kelly, Medical director of Monsanto Chemical Company, stated in a presentation before the Industrial Hygiene Section of the American Public Health Association on October 7, 1947, “…workers who have been employed in atmospheres well under the maximum allowable concentration of silica appear in some cases to be developing silicosis.”\footnote{R. Emmet Kelly, “Health Problems Resulting from Newer Technological Developments,” \textit{American Journal of Public Health} 38, no. 6 (June, 1948): 837.} New studies in the 1950s provided further confirmation of the continued prevalence of silicosis. In 1955, the Occupational Health Program of the Public Health Service began a review of data concerning the prevalence of silicosis. Through piecemeal information, they initially determined that between 1950 and 1954 twenty-two states had 10,362 cases of silicosis. Additional information increased this number to 12,763 cases in twenty-six states between 1950 and 1955. Of these, at least 3,455 of the cases occurred in men who were not employed until after 1935. This continued incidence of silicosis briefly caught the government’s attention, with Congress holding a series of hearings on health and safety in metal mines in 1956.\footnote{Victoria M. Trasko, “Silicosis, a Continuing Problem,” 841.}
However, these reports and hearings did little to affect either public discourse or reconsideration of the standard. Neither the Federal government nor the voluntary standards association, the American Conference of Governmental Industrial Hygienists, undertook a serious reassessment of silica threshold limit values. In 1958, Victoria M. Trasko, Industrial Hygienist with the United States Public Health Service, lamented that “[t]he continuance of silicosis in the United States reflects the cumulative effect on society of a preventable but still prevalent occupational disease. After more than 35 years of definitive research and almost as many years of application of controls in some measure, hazardous exposures still exist and new cases of silicosis are developing.”

The voluntary standard remained fixed until the creation of the Occupational Safety and Health Administration (OSHA) and the National Institute of Occupational Safety and Health (NIOSH) by the Occupational Safety and Health Act of 1970. With these new mandates, the Federal government became more actively involved in both standard setting and research into silicosis. In one of the first studies contracted under the new law, examination of sandblast operations cast doubt on the usefulness of respirators to protect against silicosis. Researchers with Tulane University examining shipyard workers reported at least 100 cases of silicosis. Studies of steel fabricators found additional cases of silicosis.

Thus, by 1974 both government and industry recognized that silicosis had not disappeared. Many believed it continued to claim the most victims of any pneumoconiosis. Insurance companies in California documented over 1,000 deaths from silicosis. J. P. O’Neill, chief of OSHA’s Division of Health Standards Development wrote, “the prevalence of silicosis has not dropped at all. We

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have much better methods of control, but we need a standard that would be much more effective…” In 1974, following a series of studies sponsored by the National Institute of Occupational Safety and Health that detailed deaths in Texas, NIOSH called for the banning of sand in abrasive blasting and the lowering of the exposure standard by one-half in other silica industries. Although OSHA subsequently sought to ban the use of sand in abrasive blasting, with the election of Ronald Reagan in 1980 the proposed new standard was relegated to a low level priority, thus guaranteeing that it would never be finalized.45

Unlike the regulation, the disease’s effect on workers did not take a low priority. In the late 1980s, the New Jersey Department of Health determined that over four hundred cases of silicosis had occurred in the state between 1979 and 1987. During the 1990s, agencies continued reporting substantial exposures to silica. In 1991, at least 1.7 million workers were potentially exposed to substantial amounts of silica dust. In 1993 a report estimated 121,000 workers were exposed to levels of silica dust equal to or greater than recommended exposure limits. Some of these involved substantial overexposures. These exposures continued to result in substantial numbers of silicosis cases. In the early 1990s, numerous cases of silicosis occurred in West Texas oil fields among Hispanic sandblasters hired to clean pipes and storage tanks. NIOSH once again issued a silicosis alert in 1992. It called for a ban of sand in abrasive blasting and a reduction in allowable maximum dust in the work place. In 1994, the National Institute of Health identified hundreds of new silicosis cases in just four states conducting surveillance of the disease. A special screening of Pennsylvania surface coal miners found 8% with silicosis.46 The following year a study in the American Journal of

Public Health reported that gold miners who had been exposed over the course of their working life to silica dust at OSHA’s reduced Permissible Exposure Limit still had a 35 to 47 percent chance of developing silicosis. Although there has been a downward trend in silicosis death rates between 1981 and 2000—due to increased inspection after 1970 and the decrease in industrial jobs—Ki Moon Bang of the Division of Respiratory Disease Studies at NIOSH believed that this long-term downward trend might have ceased as of 2000. The author called for “renewed emphasis on education and enforcement.” Unfortunately, although industry has repeatedly pronounced the demise of silicosis, it continues to rise again like a Phoenix.

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This chilling chronology raises numerous questions about the structure and advancement of medical science in the field of occupational health. First, and most fundamental, why has it taken so long to eradicate a known industrial health hazard? Why did the allowable dose remain at the same level for so long? Why was there for decades so little research into the appropriate safe level of exposure for silica dust? Why did doctors in the 1930s change their definition of the disease and its very characteristics with no new data? Why for decades did many repeatedly claim the disease was conquered when it remains with us today? To answer these questions we must turn to the activities which underlaid most of the pronouncements, conferences, research, articles, and preventive actions relating to silicosis—the threat of legal action by workers claiming to have silicosis and the counter activities of corporate attorneys.

As the twentieth century opened, larger corporations began supporting the new fields of occupational health and safety, especially with regards to safety and acute illnesses. Corporate executives believed that medical and safety programs could protect the firm against false workmen’s compensation claims, could act as a form of “welfare” to demonstrate enlightened capitalism and forestall union entry, and could provide a means of firing troublesome or inefficient workers.  

During the second decade of the new century, a few states began encouraging corporations to provide a safe work place by means of workers’ compensation laws. At that time, they typically covered only workplace injuries. These initial workers’ compensation programs paid for employees’ on-site injuries without regard to who was at fault. The payment depended upon the severity of the injury rather than the amount of income that was lost or whether the worker could remain at work. While payment schedules differed by state, the principle of set awards for predictable events became the universal basis of the programs. During the 1920s, compensation boards commonly held that an employee “may become permanently partially disabled by the loss of some member of his body without suffering a loss in earning capacity,” thus becoming eligible for recompense.

The practice of industry attorneys finding methods to sidestepping provisions of compensation laws began with the very first 1911 compensation law in Illinois. As the 1962 History of Occupational Health prepared by the History Committee of the Industrial Medicine Association, an association primarily composed of industrial doctors, stated with regard to the 1911 legislation:

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“Company attorneys found ways of circumventing many provisions of the first law, and the battle for more decisive legislation dragged on for nearly three decades.”

However, even this admittedly flawed system was not universal, but remained limited to safety and acute diseases. Although the new academic and governmental fields of occupational medicine and industrial hygiene documented the rise of silicosis and accompanying respiratory diseases and began exploring measures to limit dust in industry, industry showed little interest. One National Founders’ Association book actually claimed that mechanization had decreased the health hazard. The annual meetings of the association of industry doctors, the American Association of Industrial Physicians & Surgeons, (AAIP&S), provide a succinct record of this lack of focus. Between 1920 and 1930, nearly 150 speakers are listed in the annual meeting programs. Forty percent of the speakers spoke about trauma and related topics. Only two presented papers about silicosis and dust diseases.

Most companies paid little notice to the growing problem of chronic and long term lung fibrosis and tuberculosis (Later, they claimed they did not know about the problem). Companies could initially ignore silicosis since it did not cause problems on a daily basis, as did accidents. Unlike accidents, it was not highly visible. Rather than an individual being taken to the infirmary in front of all of his colleagues as happened with accidents, over the months and years a silicotic worker simply became increasingly tired until finally he was fired or quit because he could not handle the work.

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51 selleck.
52 David Rosner, et al., *Deadly Dust: Silicosis and the Politics*, 63. The audience at the AAIP&S meetings consisted primarily of doctors responsible for health care in American industrial plants. In contrast to the prior decade, between 1932 and 1935 at least twenty-five speakers presented papers at the AAIP&S meetings on “medical and legal phases of the silicosis problem” (emphasis added). By then industry attorneys had set an industrial medicine agenda of assistance to the increasing lawsuits. Silicosis was simply redefined to fit the requirements of legal defenses. As Dr. Sappington, consultant editor to the industry journal, *Industrial Medicine*, wrote about the then increasing lawsuits: “In many instances silicosis is claimed on clinical evidence which in no way differs from that of normal persons who are symptomless and without disability.” By this he meant that when initially diagnosed, silicosis has often not yet progressed to the stage where it noticeably affects an individual’s breathing. See Henry B. Selleck, et al., *Occupational Health in America*, 207, 232, 233.
Furthermore, for diseases such as silicosis, most industrial concerns saw little need to provide increasing protection when the workers most affected were easily replaceable unskilled labor. Some foundries did their sand shakeout work at night, when they typically replaced their highly skilled labor force with unskilled recent immigrants.  

Although by 1915 at least a few states workmen’s compensation laws provided implicit coverage for silicosis—and plaintiff attorneys began representing workers with silicosis—in most states, the number of claims remained small. Throughout the war and Prohibition boom years, workers fired due to mild silicosis could find other work. When they were eventually struck with severe tuberculosis or extreme breathing problems, they usually did not attribute it to work undertaken years before. Businesses often kept the problem quiet by not informing workers with more advanced cases or tuberculosis that the disease was work-related.

With the stock market crash of 1929, conditions worsened. The decade of the 1930s saw the worst economic depression in this country’s history. During this decade, much of American industry faced huge economic and financial difficulties. Profits shrank; for many companies they became nonexistent. Companies took whatever measures they could to reduce costs, including cutting many corners. Those corners included increasing the use of unprotected power tools and cutting back on the work force. Although new studies began demonstrating both the high levels of exposure and the increasing levels of silicosis in America’s workforce, labor was cheap. If one worker quit, became ill or died, several applicants vied to replace him. Silica workers with lung problems often tried to hide their discomfort, knowing they were one step from the unemployment line. One typical example of such a worker is Michael Farina, sandblaster for General Electric in New York. After a company

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53 David Rosner, et al., *Deadly Dust: Silicosis and the Politics*, 63-64, 72-73, and 89. As noted previously, by at least 1924 companies were aware of the gradual onset of the disease, with a gradual accompanying loss of breathing ability and concomitant work productivity.  

54 Martin Cherniack, *The Hawk’s Nest Incident*, 38.
doctor diagnosed him with silicosis, the company laid him off. They assured him there would be another job but it never appeared. Businesses had to keep an eye on their ability to obtain insurance, since insurance companies such as MetLife pressed their policyholders to fire anybody found with silicosis.55

Studies that did not comport with industry’s views and interests, especially those potentially problemsome at trial, were suppressed whenever possible. Charles-Edward A. Winslow and Leonard Greenburg’s study of sandblasters and foundries provides perhaps the best example of how industry tried to eliminate the “problem” by suppression, propaganda and “expert” opinions. In 1931 the National Safety Council (NSC), a large industry trade group, hired Winslow, a noted industrial medicine authority, and Greenburg, an assistant professor at Yale and in the Public Health Service, to prepare a report on sandblasting and the danger of silicosis in foundries. The submitted report acknowledged the danger in foundries from silica dust. The authors indicated that dust-collecting equipment could keep the dust to the voluntary standards, but concluded that in actuality the standards were rarely met. “The air of the workroom generally contains a highly hazardous dust concentration...such an atmosphere cannot fail to predispose in a high degree to silicosis.” Upon receipt of the report, NSC officials expressed dismay, informing the authors that the foundry industry, then burdened with a large number of lawsuits, would not like the findings. Winslow initially hoped to convince both the NSC and foundry owners that the findings were accurate. He believed, in retrospect perhaps naively, that the industry and Council would recognize that his report was accurate and “stand for the criticism involved.” Winslow opined that the foundry owners were concerned “that any mention of the fact that there is a hazard in sandblasting will stir up legislation and litigation, and, like the ostrich, they want to hide their heads in the sand and pretend that

55 David Rosner, et al., Deadly Dust: Silicosis and the Politics, 74, 79.
nothing is going on.” In effect, Winslow recognized that the foundry owners wanted science to play handmaiden to the needs of their ongoing litigation.

NSC officials’ concerns and Winslow’s opinion about the factory owners proved correct. The report caused an uproar when it was leaked to member companies. Some corporate letters to the NSC officials even claimed that workers could be rotated in and out of jobs with little harmful effects. In light of this furor, the NSC shelved the report until it could be rewritten. Winslow subsequently wrote a letter to the council denouncing its suppression.

The basic issue is that they [industry spokesmen] do not want any publication from the Council to imply that silica dust is hazardous under any conditions. They know that it is, but they don't want any authoritative body to say so. Now you cannot very well discuss protection against silica dust without the implication that silica dust is undesirable.

Two versions of the report were finally published. The first was published in a German periodical in late 1932. It followed the lines of the original report. The second was published in the *American Journal of Industrial Hygiene*. It represented the results of negotiations with the NSC by Winslow’s coauthor, Greenburg. Winslow was not listed as an author. This version of the report never explicitly said silicosis was a problem. The article drew absolutely no conclusions about hazards.  

Winslow was fortunate that he was able to separately publish the original report. Research contracts, especially when they involve issues concerning litigation, then gave—and even on occasion still give—the funder complete control over publication. This type of control pervaded the entire industry. That same year, Lanza scolded the Saranac Laboratory for its unauthorized release of information; relaease that resulted in a Wisconsin Industrial Commission report that was critical of

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56 David Rosner, et al, *Deadly Dust: Silicosis and the On-Going Struggle*, 67-69; The quote is from Gerald Markowitz, “C.-E. A. Winslow: Scientist, Activist, and Theoretician of the American Public Health Movement Throughout the First Half of the Twentieth Century: Commentary,” *Journal of Public Health* 19, no. 2 (1998): 155-156. Winslow correctly noted that what really worried foundry owners was the possibility that mentioning hazards might “stir up legislation and litigation, and, like the ostrich, they want to hide they heads in the sand and pretend that nothing is going on.”
the industry. Although a few such reports have surfaced (either inadvertently or through litigation/bankruptcy discovery), we may never know the full extent of similar silicosis report suppression, or in fact, any other hazardous substance prepared for industry.\(^{57}\)

In the case of silicosis, there is documentation of at least one similar report involving Saranac in the early 1930s. In January 1933, M. D. Harbaugh, the Secretary of the Tri-State Zinc and Lead Ore Producers Association (an industry trade group in Missouri, Kansas and Oklahoma), wrote to Donald Cummings, a research scientist at the Saranac Laboratory, about a study Cummings had prepared for the Association entitled "Silicosis Occurring in the Lead and Zinc Mines of Ottawa County, Oklahoma." Harbaugh informed Cummings that the Association would not allow publication of his study since it detailed the prevalence of disabling and often life-threatening silicosis among the area's miners. The Association refused to publish the study even though both Metropolitan Life Insurance Company and the U.S. Bureau of Mines had participated in the organization of the study and decided which of the mine owner clinics would be used to collect the data. Lanza subsequently warned the staff, “if you and the Saranac Laboratory are going to stay in the consulting business with respect to the mining industry, it will not be possible for you to publish these papers.”

In denying publication rights, Harbaugh made one simple point: the results were "potentially damaging to the lead and zinc mine operators of this district." Harbaugh went on to inform Cummings that "[t]here are now pending suits for hundreds of claims for total disability for occupational diseases." If juries returned plaintiff verdicts, "the mining companies now in existence here would have to bear the burden of paying for disabilities." Surely, Cummings could "see what a brief your paper would be." "The industry would be entirely wrecked if it were saddled with the care

of everyone who might be persuaded to take action against it on the grounds of disability from silicosis.\textsuperscript{58}

Harbaugh had good cause for concern. As the 1930s progressed, workers such as Farina increasingly sought recourse for their disease by retaining lawyers to file lawsuits claiming occupational illness caused by the conditions at their work. These civil actions caused considerable financial expenses for companies and their insurance carriers, finally leading to significant industry attention being directed toward the problem of silicosis. Thus began the long and intimate involvement of attorneys in the public perception of silicosis and research regarding the disease.\textsuperscript{59}

With the depression affecting everyone, by 1932 the disease no longer remained hidden; attention focused on the increasing lawsuits. For example, the head of New York State's Division of Industrial Hygiene wrote in a New York state Bulletin that silicosis "would continue to exist unnoticed in the community were it not for the fact that workers suffering from the disease, or relatives of people who had died from the disease, have recently taken civil action against employers and have recovered considerable sums of money therefore." These lawsuits burgeoned partly because even in states allowing silicosis claims workers fired due to early signs of silicosis often received no compensation. As one Wisconsin court noted, since at the time they were still capable of working and had lost no wages up to the date they were fired, they were not eligible for relief under the occupational disease legislation.\textsuperscript{60}

The burgeoning level of lawsuits in the 1930s drew a quick response from industry. In the first issue of Industrial Medicine Andrew J. Ferrell, head of the claims department of a major Chicago insurance company, argued that the problem was not due to worker silicosis-caused disability, but

\textsuperscript{58} M. D. Harbaugh to Donald E. Cummings, Jan. 21, 1933 cited in David Rosner, et al., “Workers, Industry, and the Control,” 29; and A. J. Lanza to D. E. Cummings, April 11, 1933.

\textsuperscript{59}David Rosner, et al., Deadly Dust: Silicosis and the Politics, 75-77, 79-80.

\textsuperscript{60} North End Foundry Company v. Industrial Commission, 217 Wis. 363 (1935); and David Rosner, et al., Deadly Dust: Silicosis and the Politics, 75-80.
from worker unemployment that caused them to use the legal system as a welfare system. Farrell contended that this had led to a broad liability crisis threatening the "closing of industrial plants and a vast economic loss." Farrell specifically complained about the “racket” of Illinois plaintiff attorneys who had nearly 200 pending silicosis lawsuits. In his mind, the lawsuits were especially egregious because they were not from the “steady, consistent worker[s]” but from “the worker who has been discharged for inefficiency.” By this means, Farrell placed the focus upon the lazy, money grubbing—and the now unemployed worker—rather than the cause of much of the inefficiency, silica dust.

Industry experts provided ready support to Ferrell’s charges by denigrating the medical backing for the suits. For example, shortly after Ferrell’s charge, George Davis, a clinical professor who consulted with industry, was quoted in the journal *Industrial Medicine*, "What is the problem of the pneumoconioses in industry? … The problem of the pneumoconioses in industry today is the medicolegal jurisprudential aspect." He believed too many doctors were willing to appear as experts about lung x-rays with little or no training. Through similar repeated loud proclamations of a “racket,” industry attorneys and consultants turned the nation’s attention away from the devastating consequences of silicosis to the lawsuits being generated by the disease. Other industry representatives complained about the lack of uniformity in the medical community’s opinion. For example, E. O. Jones of the National Founders Association groused to its members about the "considerable difference of opinion among the medical fraternity and scientists as to the pathological effects of certain types of dust.” He did not indicate which specific dusts he believed this included. However, on an attachment to the letter he suggested that a proposed federal code labeling processes using “powdered” quartz as hazardous was “decidedly sweeping.”

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61 E. O. Jones to Members of National Founders Association, August 2, 1934 (in author’s possession) Johns Manville Archives, Box 04-3302.
Although industry repeatedly complained about plaintiff attorney “racketeering,” they knew that far more workers contracted silicosis than those that filed claims. As the medical advisor to American Mutual Liability Insurance Co. advised his audience in a 1936 speech at the National Safety Congress, “twenty-five per cent of all workers exposed to high concentrations of inorganic dust are expected to develop changes within their lungs due to that agency.” He also warned the corporate attendees about the progressive nature of the disease; additional structural changes in the lungs can occur months or years after removal from the dust.

To counter the potential of this flood of cases industry doctors and lawyers began work on establishing codes that redefined silicosis in a manner that protected industry from lawsuits in which the disease was only minor or moderate. Research and case reports focused on establishing that many individuals with the initial stages of silicosis were clinically asymptomatic. This fit very agreeably with many employers’ position that they did not have a problem if doctors could not show that their current workers suffering with physically debilitating disease. Little mention was made of the fact that few, if any, current workers would be debilitated if the employer fired all “inefficient” workers.62

**Gauley Bridge Revisited**

But for the nationwide publicity surrounding the disaster at Hawk’s Nest Tunnel by Gauley Bridge, West Virginia, industry might have successfully weathered the lawsuits of the early 1930s.

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62 Andrew J. Farrell, “Silicosis in Certain of Its Legal Aspects,” *Industrial Medicine* 1 (October 1932): 35; David Rosner, et al., *Deadly Dust: Silicosis and the Politics*, 78-9; Robert B. Hunt, “The Lesser Known Facts About Common Occupational Diseases,” *Transactions of the Twenty-fifth National Safety Congress*, (1936): 117-121, 118-119. (“twenty-five percent”); and E. O. Jones to Members of National Founders Association, August 2, 1934 (“considerable difference”) (in author’s possession Johns Manville) Archives, Box 04-3302. The use of the words “racket” or “quack” became almost mandatory in industry articles and headlines after Farrell, a Chicago insurance lawyer, made this charge. Soon similar headlines and statements appeared everywhere. Some examples include “Cement Plants Fight Silicosis Racket;” “Missouri is a paradise for this type of racketeering;” and “eliminate as far as possible the ambulance-chasing and quack lawyer.” Seemingly, although the Gauley Bridge incident was tragic, it could be ignored by industry since “a great many of the workers had died” before inquiries were made. Thus, evidence brought forth in court about conditions “was of questionable validity.” Besides, it was just one small incident that was meaningless to the larger issues of industrial hygiene. See Henry B. Selleck, et al., *Occupational Health in America*, 232, 233-242, 288; George Davis, "The Pneumoconiosis Problem in Industry," *Industrial Medicine* 5 (1936): 111-112, cited in Christopher C. Sellers, *Hazards of the Job*, 204.
The Hawk’s Nest incident, however, spurred even greater involvement by plaintiff attorneys. Between July and December, 1932 plaintiff attorneys filed eighty claims involving the Hawk’s Nest tunnel work with the West Virginia Compensation Commission. On the chance that the contractor might not be covered under West Virginia Workmen’s compensation laws, the attorneys also filed several lawsuits in the Fayette County Circuit Court. In February 1933, the West Virginia Supreme Court denied the defendant’s motion to dismiss one of the first cases. Plaintiff counsel filed an additional 111 suits within two weeks of the ruling. By the time the first trial ended and the two sides had negotiated settlement agreements, 336 tunnel workers or their survivors had sued the power company or its contractor.63

Both the plaintiffs and defendants utilized medical experts in their presentations at the trials. The trial of Raymond Johnson, a silicotic tunnel worker who later died, provides a representative example. The plaintiffs used three local doctors, the most prominent of whom being Doctor Leroy Harless. Harless had conducted approximately 175 of the initial exams, identifying silicosis even before plaintiff attorneys became involved. He subsequently testified for the plaintiffs during most of the series of trials in 1932-1933. When asked how many of the many that he examined suffered from acute silicosis, he replied, “I think I am safe in saying that perhaps those that I have a record of, practically ninety-five per cent.” Plaintiffs also presented the testimony of Doctor Elmer Hayhurst, a nationally renowned industrial medicine expert. Defense attorneys had contacted Doctor Hayhurst first, but he refused their offer to retain his services. Given this attempt to hire Hayhurst and the subsequent actions of defense attorneys, it appears likely that throughout the Hawk’s Nest Tunnel trials the attorneys for defendants attempted to hire or retain as many of the limited number of national experts as possible, thus precluding their use by plaintiffs.

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63 Martin Cherniack, *The Hawk’s Nest Incident*, 55-57, 72. The settlements required that the plaintiff attorneys turn over all legal papers and not take any more cases, thus ensuring the events would not be brought again to the public’s attention.
Defense attorneys did not have to rely upon their own staff to identify experts. Lanza, now with MetLife, received instructions to put Met Life’s resources at the disposal of lawyers defending the Hawk’s Nest claims. Many of the witnesses came from the Konicide Club, a group of silica experts, first proposed by Lanza in 1926. As part of his assistance, Lanza suggested the defense counsel contact Philip Drinker as a defense witness on respirators. He also helped the defense obtain lungs for analysis from deceased victims without the knowledge of their families or lawyers. 64

For the Johnson trial, the defendants hired two nationally renowned experts, James A. Britton from Chicago and Doctor Henry Pancoast, the foremost authority on x-rays. At trial, radiologist Henry K. Pancoast denied knowledge of the existence of acute silicosis and asserted the x-rays failed to show the typical characteristics of silicosis. Incredibly, this testimony conflicted with a paper he wrote only four months previously. In this paper, Pancoast and Pendergrass described acute silicosis in X-ray findings that lacked “distinct round opacities,” noting instead the “appearance of prominent... shadows and linear markings...” [emphasis added] The paper then continued: “[I]n this particular instance under discussion, the individual may develop the appearance in a comparatively short period of from 1 to 5 years...” [emphasis added]. 65

Pancoast was not alone in his efforts to support defense attorneys in their litigation practice. Experts on the stand then as now must support their opinion with recognized research or the


During this trial defense attorneys apparently left nothing to chance. One black tunnel worker who had testified for the defense subsequently changed his testimony, swearing that although the defense had threatened and bribed him, his conscience required him to tell the truth. The judge also issued a contempt of court citation against one of the jurors who had been chauffeured to and from the court house by defendant employees; see Martin Cherniack, The Hawk’s Nest Incident, 60-63. Doctor Pancoast was correct in stating that the x-rays did not fit the normal pattern of chronic silicosis. However, he apparently ignored the fact that they did fit the normal pattern of acute silicosis; a pattern that had been described in several articles by 1933. Interestingly, in an earlier article, Pancoast described the x-ray of the first stage of pneumoconiosis as showing fine linear markings, similar to those described by others in acute silicosis. In the same article he also agreed with a fellow physician’s statement that early pneumoconiosis is often mistaken for tuberculosis. See Henry K. Pancoast, et al., “A Roentgenologic Study,” 101.
support of other experts. Since Pancoast was a recognized expert, his opinion by itself may have been enough. However, it is quite possible that defense counsel ensured Pancoast’s opinion was believable by encouraging other medical experts to submit articles to journals concerning “acute” silicosis. In particular, two articles relating to the Gauley Bridge cases read like defense counsel drafted them. The first article was by Leroy Gardner, M.D., Director of the Saranac Laboratory of the Trudeau Foundation; “Pathology of So-Called Acute Silicosis.” The article supposedly described Gardner’s examination of autopsied lungs from Gauley Bridge that had been forwarded by Dr. Albert Russell, of the Public Health Service) and Harlees. When Gardner read a paper about the lungs at a British Medical Association meeting during the summer of 1933, he described them as “atypical complicating tuberculosis infection.” At another conference, he described the lungs as containing abundant silica, with histological lesions of silicosis.66

However, in the paper, in what may have been an effort to get back in the good graces of industry after earlier missteps, Gardner began with the comment that silicosis usually requires "at least" ten to twelve years to develop and under the worst of conditions, three or four years. He did not reveal that he had privately agreed at least some of the victims had silicosis. He admitted however, that his opinion was not absolute since studies and case reports had already appeared about fast or acute silicosis among sand pulverizers and other factories. However, since dust counts were not available in those studies, Gardner believed that the question remained open as to when and under what conditions this could occur.67

Gardner then considered the tissue samples he was studying. He first described the conditions to which the lungs were exposed. In describing the conditions at Gauley Bridge, Gardner

wrote of “excessive amounts of extremely fine powder,” in the tunnel “without effective ventilation,” in an atmosphere “exhausted by gasoline engines” such that the men “must have breathed abnormally fast.” Later in the article, he also characterized the exposure as “high concentrations of silica dust.” Curiously—or perhaps as should be expected if he were trying to please a defense counsel—Gardner questioned whether it was worth providing a report because "information as to the exact character and duration of the dust exposure is so limited, the justification for this presentation might be questioned." Then, seemingly unconvinced by his description of the conditions in the tunnel, in the conclusion—the section of most importance to industry attorneys—Gardner characterized the exposure simply as “allegedly heavy exposures of silica.”

Throughout the article, Gardner never mentioned the company’s failure to provide air sampling or safe conditions. Nor did he attempt to make any estimate of the exposure levels. He did, however, portray the sick workers as liars, stating that the occupational histories had to be treated with suspicion since claims for compensation were pending, even though he recognized that the age of the workers and the conditions under which they were hired "precluded any prolonged exposure to silica in previous occupations."

Based upon other published articles of the time, some of which he cited, Gardner’s clinical findings appeared to demonstrate that these individuals had acute silicosis. "It is indisputable that this group of workmen exposed …to high concentrations of silica dust had developed the histological lesions of silicosis at the time of their death." In one of the samples from an individual he did not believe had silicosis, he admitted finding microscopically "definite microscopic evidence of silicosis consisting of characteristic hyaline fibrous nodules.” While he found the nodules smaller than typically found with silicosis, the nodules were "appreciably more abundant than those

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encountered in the usual run of silicosis cases...In many of the sections they are extremely numerous." His findings also included one sample where "silicotic nodules were embedded in areas of massive fibrosis from which most of the air spaces had disappeared or were represented by distorted slits." In spite of this evidence, he still distinguished these findings from silicosis because the character of the silicotic nodules different from those usually observed in three ways: 1) They were small and massed ("matted together in a mass of fibrous tissue"); 2) The lymph nodes were not affected as much; and 3) The lungs showed an appreciable thickening of the aveolar walls. Even though these findings comported with a fast-acting acute case of silicosis from overwhelming exposure as described in Chapman’s case study published in the *Journal of the American Medical Association* the previous year, Gardner reported that the silicosis was *de minimus*—that is, minimal to the point of being unimportant. 69

Instead, Gardner argued that “silicosis can develop under these conditions of exposure but the reaction is of microscopic proportions”—thus at best constituting a pre-silicotic condition. "Although there is histological evidence of silicosis, atypical in character, it seems doubtful whether there is justification for describing the process as 'acute.'’ Furthermore, he concluded he had insufficient evidence to make the diagnosis. “At least this should be only done after serial roentgenograms together with post-mortem examinations have demonstrated the outcome of the allegedly heavy exposures to silica." Since he took the samples from an autopsy, this was clearly impossible to obtain. Therefore, he implicitly declared that a diagnosis could not be made when industry (as frequently occurred) had not conducted serial roentgenograms of its employees. 70

In line with what industry needed for its court cases, Gardner found that the individuals had not died from silicosis, but rather the deaths were due to pulmonary infection: eleven from

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70 Leroy U. Gardner, “Pathology of So-Called,” 1245 and 1249.
tuberculosis; two from pneumonia; and three probably from tuberculosis, although no bacteria were found. None of these conditions was due to the individual’s work. “Tubercle bacilli were rare in all instances, and in some of the obviously tuberculous lesions none could be found.” He believes the group of "young adults, negroes in most cases" probably had little or no immunity to tubercle bacillus, so the infection ran an acute course to fatal termination in 15 months or less. Because of the acute nature, it could not have happened while they worked. "If they had become infected while their silicosis was in its formative stages they would not have been able to continue so strenuous an employment and many would have died months earlier." He admitted that it would be tempting to say their "microscopic silicotic lesions" made them more susceptible to tubercle bacillus, but it was impossible to know if that was the case when there was not an "advanced" case of silicosis. He concluded: "For these reasons it is believed that the localization of the silicotic nodules in this group of cases was not materially influenced by the terminal tuberculosis.”

If ever there was a clear case to show how silicosis can arise from short heavy exposures and tuberculosis was associated with the very initial stages of lung damage, this was it. Instead, Gardner chose to be as conservative as possible, given the horrendous facts. Although evidence that industry representatives edited this article or that Gardner believed his funding would be cut off if he lambasted the mining industry has not surfaced, the language of the article and its strained logic clearly make this idea plausible. When considered in conjunction with similar articles described in the next chapter, it is perhaps highly probable.

Although this is a harsh allegation, industry lawyers’ opinions of Gardner lend it substantial credence. During the 1940s, Leroy Gardner became reluctant to testify in silicosis suits because he wanted to maintain his “neutrality.” Industry lawyers scoffed at such sensitivity: “We did not agree with him that his refusal to testify would help maintain Saranac as an independent research

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organization because ... all of the mining companies were associated with Saranac and certainly, in the eyes of everyone, excepting Dr. Gardner, Saranac was a ‘mining company institution.”

George W. Wright, a colleague at Saranac, further demonstrated the facility’s industry bent when he argued in the late 1940s that it is more appropriate for compensation boards to rely upon “the experienced plant physician” than on personal or local doctors. There was a serious potential that non-specialist “physicians may be inclined to ascribe all the pulmonary ailments of men who have been exposed to dusts or fumes to the inhalation of these foreign substances.” Furthermore, workers could not be trusted to discuss their ailments because “frank malingering” often motivated them. He did not mention whether he believed the desire to retain his job motivated the plant physician.72

An incident a few years later illustrates just how closely the Saranac Laboratory and Trudeau Foundation allied themselves with the defense attorney community. In the late 1940s at the request of an industry client, the Trudeau Foundation and the Saranac Laboratory reviewed the medical records of a Finnish-born immigrant who worked as a miner from 1908 until his death in 1947 at the age of 61. His x-rays going back to 1933 confirmed a diagnosis of silicosis. During the period 1933 to 1936, the disease progressed from first degree in 1933 to third degree in 1936. However, in 1947 the company sought to escape having to pay compensation, since the immediate cause of death was a cerebral hemorrhage. Upon learning of the cause of death, the manager of the Trudeau Foundation field office wrote to his research director, "It looks as if they may win this one as death was apparently caused by cerebral hemorrhage; if successful, a good sum of money will be saved."73

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Other articles and incidents also support this judgment. An article by industry expert Homer L. Sampson, Director of the X-ray Laboratory at the Trudeau Sanitarium entitled, “The Roentgenogram in So-Called ‘Acute’ Silicosis,” was even more in line with industry attorneys’ trial requirements. He began the article by admitting that, given the infrequency that companies gave x-rays, he was unlikely to ever diagnose acute silicosis. "I believe until serial roentgenograms reveal rapidly progressive or retrogressive shadows, and also the stigma of an infection be satisfactorily excluded, one is not justified in interpreting the roentgenogram as rapid uncomplicated silicosis." He then confides that even then he would be unlikely to make such a diagnosis. "If the picture changes with extreme rapidity it is hard to conceive that these alterations are due only to the inhaled silica; it is more probable that one is dealing with a complicating infection."74

Doctor Sampson found two types of infection pictures in the cases he studied—likely the same cases as contained in Gardner’s article—one very closely resembled the characteristic shadows of pulmonary tuberculosis, while another had the same shadows but also a diffuse nodulation. In other pictures, he identified diffuse nodulation of "a distinctly fluffy character-diffuse cotton ball appearance,” unlike the longstanding nodulations of the non-infective group. Although Sampson had no prior experience with this type of X-ray, without further consultation he inferred this to be an infective process, rather than the effects of silica. "There is reason to expect, in any acute process, shadows of a rather characteristic nature. In practically all cases the one outstanding feature of the roentgen shadow complex is its cottony or fluffy appearance and its ill-defined margins." Sampson added, "Undoubtedly there are persons exposed to heavy concentrations of silica for short periods

74 Homer L. Sampson, “The Roentgenogram in So-Called “Acute” Silicosis,” American Journal of Public Health, 23 (1933): 1238-1239. Defense litigation experts Gardner and Sampson did not represent the mainstream of medical thinking. As discussed earlier, by 1934 numerous individuals had identified individuals with acute silicosis following intense exposures. In the early 1930s respected researchers in New Jersey reported on similar cases, at least some of whom contracted acute silicosis in less than a year. M. Kummel, “Medicolegal Aspects of Silicosis,” Reprint from The Medical Record (May 16, 1934): 1-17, 4, obtained at Johns Manville Archives, Box 04-2509.
who exhibit various symptoms of acute disease, but the serial roentgenograms which I have seen do not confirm the diagnosis of uncomplicated silicosis.” (He did not indicate whether he had seen cases of acute silicosis complicated, as these are, with other respiratory diseases or if he had read Doctor Chapman’s article.) He did admit, however, to one small problem that “needs to be cleared up” before excluding the possibility of acute silicosis in these cases, for "in many cases apparently suffering from silico-tuberculosis or tuberculo-silicosis, tubercle bacilli are not demonstrated." Thus, while he could ignore clear nodular signs of silicosis, the absence of any necessary proof of tuberculosis presented but a small problem to his exclusion of silicosis.

The articles and testimony of doctors such as Gardner greatly helped to confuse the issue, however, in the end the company was forced to settle. The settlement once again demonstrated attorneys’ ability to teeter on the ethical line, if not overstep it. As part of the global Hawk’s Nest settlement agreement, the defendants agreed to give an additional sum to the plaintiff attorneys in return for an agreement not to take on any similar cases. This is one of the earliest reported buyouts by which plaintiff lawyers received payments in return for secret agreements not to engage in any further legal action. To the dismay of plaintiff counsel, the judge found out about the secret agreement and required them to include the money in the plaintiffs’ settlement proceeds. While this activity tainted both plaintiff and defense attorneys, other events demonstrated defendant attorney’s willingness to use any means to keep from paying plaintiffs. In one case, they appealed the verdict partially upon a claim that the West Virginia workmen’s compensation law relieved subscribing corporate members from lawsuit from any injury, regardless of whether it is compensable under the compensation law. In effect, they were arguing to the appellate court that since silicosis is not covered by workmen’s compensation, workers had no right to compensation, no matter how egregious the company’s actions. The court disagreed and allowed the civil trial award to stand.

75 Ibid.
Defense counsel activities during the trial went even further, at times ignoring ethical rules. During one Hawk’s Nest trial, the defendants were caught providing chauffeur service to at least one of the jurors.  

Notwithstanding the industry-oriented medical articles downplaying the effects of silicosis at Gauley Bridge and the sealed settlement agreements that ensured neither the documents or facts would be released, the publicity surrounding the trials experienced a modest life of its own through newspaper articles and, in 1936, congressional hearings. That year, the Subcommittee on Labor in the House of Representatives held hearings on the dangers of silicosis, focusing narrowly on the cases from the Hawk’s Nest Tunnel. These hearings also raise questions about the reach of defense counsel in stifling medical information concerning occupational health. Only fourteen persons testified before the committee. The trial defendants refused to appear. Doctor Harless, the M.D. who first diagnosed silicosis at the tunnel, agreed to appear—but three days prior to his testimony indicated he was now unwilling to participate. In a startling letter to William P. Connery, Jr., the chairman of the House Committee on Labor, Harless basically denied his prior testimony. “I examined a large number of these workmen, perhaps as many as 200, on most of whom I kept no record, who claimed to be affected by reason of their employment. I found very little impairment of their health which I could attribute to their work in the tunnel.” He further cited only fifteen known cases of silicosis, the same number who were known to have died from silicosis in 1933. 

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76 Dora Jones v. Rinehart & Dennis Co., Inc., 113 W. Va. 414 (1933); Martin Cherniack, *The Hawk’s Nest Incident*, 63; and Jack B. Weinstein, “Secrecy in Civil Trials: Some Tentative Views,” *Journal of Law & Policy* 9 (2000): 64-65. Federal District Court Judge Weinstein went on to provide his opinion of the “buyout”: “These “buyouts” need to be supervised by the court in the same way as a settlement of a class action would be. The court has an obligation to the clients and to the community to see that the clients understand the arrangement and that it is fair. The court should also be able to veto any arrangement for secrecy under which files are returned to defendants, and plaintiffs’ lawyers agree to take no future cases. The Hawk’s Nest case was a shameful episode in American jurisprudence; without the judge’s intervention it would have been even worse.”

77 Martin Cherniack, *The Hawk’s Nest Incident*, 76-77. While it is possible that the testimony and the subsequent letter can be reconciled as simply the distinctions between those with records versus those without records, Harless’ refusal on the latter date to recognize the existence of any silicosis cases from the tunnel other than those that were publicly acknowledged deaths, and his short notice refusal to attend the Congressional hearings, both raise the strong inference
Although there is no direct evidence, such an abrupt turn-around in opinion and the letter’s almost legalese writing style suggests that Harless had by now become one more expert retained by industry attorneys. The ethical standards of the trial attorneys involved in the case on both sides certainly do not warrant dismissal of such a possibility. Two other facts add weight to this possibility. First, Harless was a former company physician. Second, even though he agreed to testify for plaintiffs, before the trials he provided lung samples to Lanza and Gardner, who he knew were working with the defendants.

With the added stimulus of the national publicity surrounding the Hawk’s Nest trials, by 1934 silicosis damage suits in other areas of the country became big business for plaintiff attorneys. One writer estimated that by 1934 “damage suits amounting to over 300 million dollars had originated in that period. Other articles in the early 1930s [told] of damage suits for silicosis for as much as 58 million dollars in one state, and against single companies for nine million dollars. Single awards by civil courts of $12,000.00—which is the maximum compensation benefit in many states today [1980]—were common, and some as high as $20,000.00 and $50,000.00.” As Anthony J. Lanza, who began his career with the Public Health Service lamented, it was as if "out of a clear sky and with dramatic suddenness the insurance companies were faced with a situation that was ... terrifying." 78

Lanza came to play a large role in industry’s response to this frightening occurrence. After being hired by Metropolitan Life in 192, he began directing industrial hygiene surveys for asbestos companies as part of his duties. He appears to have been genuinely concerned about the effects of silica dust upon the laborers of Metropolitan Life’s clients. In early 1935 he wrote a memo in which

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78 David Rosner, et al., Deadly Dust: Silicosis and the Politics, 74, 78, 79; Peter, S. Barth, et al., Workers’ Compensation and Work-Related Illnesses and Disease, 3.
he expressed such concern—and the linkage between silicosis and tuberculosis—specifically noting, “those exposed to silica dust have an extremely high tuberculosis death rate… There are over half a million industrial workers exposed to silica dust and… the problem of family infection due to incident tuberculosis is a serious one. . . Our motives are in accord with our general policy of being willing to help as far as practicable, in any undertaking that has to do with the conservation of life.” However, near the beginning of the memo he also stressed the extent of litigation, “in excess of $100,000,000.00,” with whole industries in turmoil. He further claimed later in the memo that he would not help in litigation matters, but as this study makes clear, when potential lawsuits began affecting Metropolitan Life’s profits, Lanza proved more than willing to assist in issues related to litigation in both silica and asbestos suits.79

Nor was Lanza alone in expressing concern and seeking to assist in the litigation crisis. Spokesmen of the insurance industry, such as Henry D. Sayer and F. Robertson Jones, both of the Association of Casualty and Surety Executives, also expressed deep concern about the impact of silicosis lawsuits on the insurance industry’s stability.80 The stone monument industry’s experience in Wisconsin provides limited yet still striking support for such a concern. Upon introduction of full silicosis coverage in the state’s worker’s compensation program, "the insurance premium for monument workers… promptly soared higher than the payroll itself, with the result that the entire industry was closed." By the mid 1930s business and insurance representatives throughout the

79 A. J. Lanza memoranda of February 27, 1935, to Dr. Armstrong, Third Vice President Metropolitan Life, obtained at Johns Manville Repository, Denver, Colorado and in author’s personal collection.
country, faced with increasing silicosis claims and verdicts, argued that silicosis lawsuit decisions were being dictated by sympathy for the victims rather than by “objective” science and law. They called for both a standardization of diagnosis and incorporation of the disease within state workmen’s compensation systems.\textsuperscript{81}

The Industrial Hygiene Foundation to the Rescue

In the aftermath of the Hawks Nest tunnel disaster, with national press coverage expanding and the federal government under President Franklin D. Roosevelt becoming increasingly interested in intervention, segments of industry determined that a national strategy was necessary. Attorneys would play a large rule in this strategy. The “dust problem,” as industry saw it was not silicosis, but the lawsuits threatening to bankrupt an entire industry. Led in many cases by such lawyers as Henry D. Sayer, representatives of insurance companies, foundries, metal mine owners, and glass industries sought ways to eliminate the silicosis litigation crisis. They recognized that an integrated approach to the problem was necessary. Alfred Hirth, outside counsel for the Owens-Illinois Glass Company in Toledo, Ohio played a prominent role in this effort. By September 1934, Hirth had spent the better part of two years devoted to silicosis litigation. That month he approached the Mellon Institute of Industrial Research at the University of Pittsburgh for help. The Institute, which had been organized in 1913 with funding from the Mellon steel and banking family, “sponsored field and laboratory research on its premises and also functioned as an umbrella for organizations directly involved in the politics of industrial and environmental health.” At the time, the Institute had a large measure of public credibility from its work on air pollution in Pittsburgh.\textsuperscript{82}

On December 11, 1934 Edward R. Weidlein, President of the Mellon Institute, sent letters out to approximately 85 industries inviting them to attend a meeting on the dust problem. The invitation described the silicosis crisis as having many angles, yet called particular attention to the “racketeering feature.” The tone of the letter suggests that Hirth may have been involved in its drafting; the first paragraph describing the legal situation as grave, with the Institute offering its facilities for the meeting. In a sentence that portended the activities of future tobacco counsel, Weidlein further advised that an “independent” organization would have greater standing to “continue direct future investigations.” 83

To the pleasant surprise of the Mellon Institute and Hirth, almost two hundred industry executives met at the University Club in Pittsburgh for a “Symposium on Dust Problems.” The attendees included representatives and attorneys from a wide variety of industries, as well as corporate and/or outside counsel to some of the industries. For example, Johns Manville sent its lead corporate attorney, Vandiver Brown. Admission was by invitation only, with no reporters present and no stenographic notes kept. The Symposium included addresses by numerous industry retained experts and attorneys, including R. R. Sayers, Anthony J. Lanza, Eugene Pendergrass, Philip Drinker, Donald S. Cummings, Alfred C. Hirth, and F. Robertson Jones. 84

According to Brown, the meeting revealed four areas of common interest among the participants, with the foremost being the "menace of ambulance chasing lawyers in combination

83 E. O. Jones to Dr. Leroy U. Gardner, December 20, 1934, with attachment, (in author’s possession), Johns Manville Archives, Box 04-3302.
84 Although no official records were kept of the meetings and subsequent activities that established the Air Hygiene Foundation, four documents prepared at the time provide an outline of the activities: Roger A. Hitchins, "A Brief Outline of the Discussion and Action Taken by the Temporary Organization Committee at a Special Meeting Held at the Duquesne Club, Pittsburgh, Pa., on Friday, February 1, 1935," Hazard to Thompson, March 21, 1935, National Archives, Record Group 90, Records of the Public Health Service; Roger A. Hitchins, Report and Recommendation of the Temporary Organization Committee Which was Elected at the Symposium on Dust Problems’ Held at Pittsburgh, Pa. January 15, 1935, National Archives, Record Group 90, Records of the Public Health Service; Vandiver Brown, “Memorandum Re: - Mellon Institute of Industrial Research Symposium on dust Problems – Pittsburgh – Jan. 15, 1935,” author’s private collection; and A. J. Lanza, “Memorandum to Dr. Armstrong, dated February 27, 1935 and attached list of Representatives at the Silicosis Meeting, author’s private collection.”
with unscrupulous doctors." The wayward doctors found by plaintiff attorneys meant that the "uncertainties surrounding diagnosis" of the pneumonoconioses, which meant that the jury could decide between plaintiff and defense expert witnesses, with them “not likely to favor the opinions of the experts produced by the employer.” A second area of general concern grew out of the first: "the desirability of making various dust diseases compensable under properly drawn workmen's compensation laws." Brown observed that the attendees desired legislation that would "eliminate the jury and empower a Medical Board to pass upon the existence of the disease and the extent of the disability," while at the same time eliminating “the shyster lawyer and the quack doctor.” Brown needed only two lines to cover the attendees’ interest in the "problems of ventilation, dust collecting and elimination, and respiratory devices.” Finally, in an issue also potentially related to litigation, Brown noted the group’s concern about the need for "establishing of standards" for dust counting and particle size, the "taking of x-rays for diagnostic use," and interpretation of the x-ray film markings.85

The attendees appointed a Temporary Organization Committee to develop plans and make recommendations. The Temporary Committee then held its own meeting at which its members decided to recommend the formation of a permanent confidential organization. The organization would cover “…medical, preventive, legal, legislative and publicity features of the industrial dust problem…”86 The legal issues were the core component of the proposal and, as we shall see, the subsequent program. The Medical, Preventive Engineering, Legislative, and Publicity features were all tailored to provide supporting roles to the overwhelming problem of litigation defense.

85 Vandiver Brown, “Memorandum Re: - Mellon Institute of Industrial Research Symposium on Dust Problems – Pittsburgh – Jan. 15, 1935” (in author’s possession). Brown apparently attended the meeting to ensure that asbestos hazards did not become part of the group’s agenda.

86 Roger A. Hitchens, Report and Recommendation of the Temporary Organization Committee Which Was Elected at the “Symposium on Dust Problems” Held at Pittsburgh, Pa., January 15, 1935.
The committee next obtained a proposal from the Mellon Institute for air industrial hygiene research. Given the importance of the legal issues involved, the Mellon representative, Weidlein, fully understood the necessity of both confidentiality and research structures to assist in litigation defense. In his initial proposal letter, Weidlein gave the assurance that “the work could be carried out in a most confidential manner as to who was supporting the research and no one would know what industries or individuals was [sic] contributing to the fund.” He also acknowledged that the coordinating agency must have a “sympathetic understanding of the problem and wide contacts with all cooperating agencies…” He urged support for a program “important from both medical and legal standpoints in the preparation of cases.” In another statement foreshadowing tobacco research, Weidlein suggested that a permanent research organization could provide industry with a measure of control over relevant information dissemination.

The proposed research program involved several steps. He desired at the outset to collect data. Upon compilation of the data, he proposed the preparation of a “comprehensive program designed to provide the industries concerned with legitimate protection.” Weidlein then outlined six aspects of the research. He argued that understanding “[t]he pathological and psychological effects of dust of various kinds, alone and in combination” was necessary before any other action. Furthermore, “[f]or this a classification of dusts is necessary.” He next acknowledged the critical impact this had for the companies’ legal defense: “the preparation of court cases” was “important from both medical and legal standpoint.” His next two research topics covered air sampling in plants and determining appropriate methods for minimizing dusts. The research topics concluded with recognition that the new agency must disseminate authoritative information, maintain close contact with other agencies in its research program, assist in the review and preparation of legislative proposals.
Weidlein’s proposal next suggested a five-point program that could control industry actions and set a nationwide silicosis agenda, including Medical, Preventive, Legal, Legislative, and Publicity programs. Each section included a number of legal-oriented activities. The Medical section proposed collecting information on diseases to focus attention on areas needing more research and, in an action that would assist legal defenses nationwide, enlisting the American Medical Association in setting approved standards of diagnosis. The Preventive section stressed the importance of air quality standards for litigation. He called for the setting of approved standards for the control of dusts such that they will “act as a defense against personal injury suits.” In the pure legal area, the organization would seek the cooperation of the American Bar Association in stopping the lawsuit “racket” and serve as a clearinghouse of legal related information. The organization could also help in the legislative area by recommending laws to “properly protect the interest of industry and of enlisting the co-operation of the Federal Government in that direction.” The interest of industry, of course, was to eliminate personal injury lawsuits against the companies. Finally, Weidlein proposed that the organization assume the role as industry’s mouthpiece, informing governments and the public of the actions industry was taking and what “should be taken in the direction of protecting both human life and property.” It appears that the property he meant were the businesses of the owners being overwhelmed by lawsuits. This proposal constituted an integrated plan, designed to both shape the silicosis debate and reduce, if not eliminate, lawsuits by a collective defense effort; that effort being supported by intensive confidential research, legislative lobbying and a nationwide publicity program.87

The Temporary Committee took this proposal and prepared a tentative “Program of Initial Activities” which, while somewhat different then Weidlein’s proposal, had his approval. The

proposal included the same program elements as Weidlein’s: Medical—enlisting the cooperation of the AMA in setting “authoritative standards of diagnosis;” Preventive—“setting up authoritative and approved standards for the control of industrial dusts which, if complied with…will act as a defense against personal injury suits;” Legal—assembling data on all legal suits to go to the ABA about “rackets;” Legislative—obtaining information about all pending legislation and court decisions “affecting the dust hazard” in order to secure the enactment of laws that “will fairly and properly protect the interests of industry and of those engaged in industry and of enlisting the co-operation of the Federal Government in that direction;” and Publicity—recommending how governments and the public can be advised about “the measures which are being and should be taken in the direction of protecting both human life and property.”

As an attorney, Hitchins must have well understood the wariness of industry to publicity about dust, particularly publicity that could resurface in a lawsuit. He thus also promoted the idea that the “Industrial Dust Institute” would maintain strict confidentiality about membership and contributions. In addition, his proposals implicitly recognized the confidentiality of any research or surveys unless the company found them useful for publicity or their defense of personal injury lawsuits and workmen’s compensation claims.88

From this beginning was formed the Air Hygiene Foundation, subsequently renamed the Industrial Hygiene Foundation (IHF); its primary purpose being to reduce legal lawsuits and litigation costs. The founders sought to achieve this in ways remarkably similar to those of tobacco. The organization had a mission of limiting the definition of dust diseases, conducting research favorable to industry, obtaining “reasonable” state hygiene standards, limiting lawsuits, and

publicizing industry’s story. It succeeded in all of these tasks. The IHF quickly demonstrated its worth to industry. The IHF immediately directed considerable attention to controlling lawsuits from airborne dusts. Section 4 of the Foundation’s initial Code of Regulations clearly delineated these responsibilities:

The functions and duties of the Committee on Legal Statistics shall be to secure from all affected industries their present experience as to personal injury suits and claims growing out of air pollution; to make a study of the assembled data and to report, from time to time, to the Board of Trustees as to losses suffered by industry in general and by classification from industrial air pollution; to make recommendations as to what concerted action should be taken with reference to those classes of claims which appear to be fraudulent; and to seek the cooperation of the American Bar Association and local bar associations in combating any and all unethical conduct of lawyers and others engaged in fraudulent practices against industry.

In seeking new members, the IHF understood that the quest to control litigation supplied the strongest enticement to potential new corporate members. In perhaps its first advertising brochure, the Air Hygiene Foundation explained both the purpose of the IHF and four reasons why companies should want to join. The motivations were listed in the following way: “For Dollar and Cents,” “For Legal Factors,” “For Industrial and Public Relations,” and “For Human Welfare.”

This first emphasis of profit, legal, and related issues continued to be the primary draw for the IHF even into the war years. In its 1940-41 annual "Report of [the] Membership Committee," C. E. Ralston, safety director of the Pittsburgh Plate Glass Company, provided his company’s reasons for joining the IHF. He saw the optional plant surveys as a major benefit to member companies because of its legal ramifications. "In case of claims, you have a record showing what the health conditions actually were at the time of alleged injury… The report you receive from the Foundation removes the 'guess work' and speculation as to whether there was or wasn't a hazard at the time in question. Further, as Mr. Fletcher pointed out here a year ago, a survey report from an outside, independent

agency carries more weight in Court or before a compensation commission than does a report prepared by your own people. In other words, an outside party can talk with greater weight about you than you can talk about yourself.” In the marketing of the IHF, as in most aspects of early twentieth century businesses, the control of costs, the reduction of lawsuits, and public relations, each an aspect of a business’s profits, all came before “human welfare.”

Insurance companies quickly came on board. On February 1, 1935, the American Mutual Alliance’s (an association of five large insurance companies) Special Committee on Undesirable Risks, recommended that its members encourage corporate policyholders support the proposed “silicosis research and service organization.” In the report they included a copy of the proposed organizations initial activities, which clearly demonstrated the proposed organization intended to go well beyond mere medical research, with early activities including providing legal assistance to member companies and approaching the ABA to stop the “racket.”

Lanza, to his credit, participated very reluctantly, unsuccessfuely attempting to steer the new organization toward pure research and away from litigation assistance. In a late February meeting with members of the Temporary Organizing Committee he voiced his misgivings about the directions the attorneys were taking the organization. Hitchens assured him it was not his intention for the new entity to become directly involved in litigation. Rather he envisioned it serving as an information center and collector of statistics on claims. Although Lanza stated he did not believe that would be necessary, he apparently did not push the issue. Five days later, he wrote to Gardner about his continuing doubts. “If they want to center their efforts on the litigative and legal aspects of the question, then neither you nor I nor the Public Health Service will be interested. If they wish to

91 American Mutual Alliance Special Committee on Undesirable Risks, “Second Report of Sub-Committee on Claims and Medical,” February 1, 1935 (in author’s possession), Johns Manville Archives, Box 04-3302.
sponsor and coordinate research, then they cannot get along without the support of the Public Health Service and Saranae.” Even as late as April, Lanza still hoped to limit the legal side of the organization and have outside and governmental officials constitute approximately half of the entity’s Board of Trustees. Instead of a “Legal” Committee, he proposed a Legislative Committee.92

As the new organization first met, Lanza must have been sorely disappointed as the new foundation followed through on the attorneys’ commitment to support litigation efforts; even so Lanza continued his participation in the new Foundation, even chairing the Medical Committee. At the first meeting, Hirth gave a rousing speech emphasizing the importance of vigilance and assistance in litigation. He lectured the crowd on their duties to each other and the primary problem of the “ambulance chasers,” not silicosis.

You should be concerned not only with the successful defense of your own cases but of others as well. In this situation the answer to the age old query, Am I my brother’s keeper is emphatically, “Yes”. Each verdict against the defendant encourages others to bring suit and provides ambulance chasing lawyers with arguments and money to see that they do.93

In one of his first acts, the foundation’s new managing director, H. B. Meller, similarly voiced the greatest concern for litigation, not the health of workers. In his opinion, one of the primary purposes of the new organization was to counter the “misleading publicity about silicosis…. this publicity will result in a flood of claims, whether justified or unjustified, and will tend toward improperly considered proposals for legislation.”94

The first informational tracts issued by the IHF, the Legal Bulletins, demonstrated the IHF’s understanding of the focus of its mission. These tracts, designed to assist in the defense of

92 A. J. Lanza to Donald E. Cummings, February 21, 1934 (sic) (Given the topic, Gardner obviously wrote this letter in 1935, likely writing 1934 out of habit) (in author’s possession); A. J. Lanza to Dr. Leroy U. Gardner, February 26, 1935, Johns Manville Archives, Box 04-3302; and A. J. Lanza to Dr. Leroy U. Gardner, April 2, 1935, with enclosure, Johns Manville Archives, Box 04-3302.
occupational disease cases, provided details of the federal and state laws and court decisions related to occupational disease. Bulletin Number 1 of the Legal Series, published on April 1, 1936, provided a survey of existing pneumoconiosis relevant statutes. The following year the Foundation published two new Legal Bulletins and a supplement to the first Bulletin. The first 1937 Bulletin, published on January 2, provided a “Critical Study of Provisions for Occupational Disease Legislation.” The second, published on December 27, 1937 provided a critical review of compensation legislation. Dissemination of medical information was slower. In contrast to the efforts put forth in the legal arena, during the same period only one medical bulletin was issued.

In addition to the Bulletins, the Legal Committee also presented programs during the scientific portion of the IHF’s first annual meeting. Thereafter, legal programs became a regular feature of the annual conferences. For example, the 1950 annual meeting included a full day Legal Conference, running concurrently with other conferences, as well as a half-day Medico-Legal Conference attended by both lawyers and doctors. This conference, co-chaired by Lanza, covered the administration of workmen’s compensation laws. A similar Medico-Legal Conference in 1953 heard questions seeking to find methods of controlling the science and limiting lawsuits. Typical questions included the following: “What steps should be taken to secure general cooperation among compensation attorneys, industrial physicians and commissions to eliminate the racketeering lawyer and the unethical medical witness?”

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1937 found the new foundation on firm footing, its membership having grown to 168 companies and trade associations. It attracted virtually all of the top names in silica research and engineering. In the midst of the depression, with federal and charitable grants declining yearly, experts must have seen this new organization as a godsend. With this full stable of experts, the Air Hygiene Foundation became a major force in shaping the debate about silicosis. The first opportunity came in the Foundation’s very first year.

*National Silicosis Conference*

As previously noted, the federally organized National Silicosis Conference followed less than sixteenth months after the first general industry symposium. Although the National Silicosis Conference of April 14, 1936 was convened by the U. S. Department of Labor through its Division of Labor Statistics, the actual Conference was controlled by industry. This is not surprising; for the last four years and in particular since the prior year’s formation of the Air Hygiene Foundation, industry had studiously courted medical experts in support of its litigation defense. Although the Labor Department desired to help workers and recognized that most industrial health experts, including those in the Federal Government’s employ, were biased toward industry, the difficulties of the depression and the fear of job losses if entire industries collapsed likely caused it to choose what it saw as the lesser of two evils—the lessened but continuing specter of silicosis deaths resulting from industry control of the Conference. For if the Conference failed, new bankruptcies might leave hundreds of thousands of additional workers unemployed. In addition, as was stated in the summary of the final reports of the conference, the Labor Department acknowledged that “a few incompetent [or perhaps too competent] attorneys and physicians, particularly in court cases, have further
complicated the situation, so that today a condition exists that fully deserves thoughtful consideration and some definite declarations by those who are competent to speak.96

More than 300 individuals attended the first conference session on April 14, 1936. They represented workers, employers, State and Federal agencies, industry trade associations, insurance companies, and others. Most, however, leaned heavily toward industry’s position. At least sixty percent represented a dusty industry or an insurance company that serviced such industries. A fair number of the twenty-five percent of the attendees representing state and Federal agencies also supported industry’s position. Several of these individuals also had a fiduciary interest in the outcome of the conference, since they were already consulting with the fledgling IHF. The approximate five percent of the attendees who were worker and trade union representatives must have felt mighty lonely through most of the Conference and Committee sessions.97

The newly born IHF tied together an extraordinary number of the Conference’s key participants. Three major papers were read during the first day of the Conference; two were by IHF participants. In one paper, Alfred C. Hirth, a litigation defense attorney and Chairman of the IHF’s Legal Committee, presented industry’s viewpoint of the crisis. He argued that through currently available engineering and technology, industry was achieving safe levels of dust exposure. In his words, “the existence of a dust hazard is already on its way out.” The remaining small amounts of silica dust would result in minimal amounts of silica in the lungs. This should not disable or affect a worker during his work life. He left unsaid what would happen to the workers’ health after

96 “Broadly speaking, state and federal labor administrators generally believed that governmental medical and public health opinions regarding industrial disease was [sic] biased in favor of industry rather than the work force, despite its claim of scientific objectivity. New Deal administrators in the United States Department of Labor and some state departments saw themselves as allies of organized labor during the turbulent years of the 1930s and generally adopted labor’s distrust of medicine.” Gerald Markowitz, et al., “The Illusion of Medical Certainty,” 236; see also United States Department of Labor, Division of Labor Standards, National Silicosis Conference Summary of Reports, Final Draft, dated January 23. The Labor Department quote is from David Rosner, et al., “Workers, Industry, and the Control,” 38.
97 United States Department of Labor, Division of Labor Standards, National Silicosis Conference Summary of Reports, Final Draft.
retirement. Medical doctor R. R. Sayers, employed by the United States Public Health Service, but also a member of the IHF’s Medical Committee, presented the second paper. He delivered an address on the engineering and medical aspects of the silicosis hazard.

At some point during the daylong conference, federal officials recognized that smaller groups would have to work on actual solutions to the crisis. By the end of the day, Zimmer had apparently allowed the participants to organize themselves into the four working groups of the Conference—the groups who would prepare the actual recommendations of the conference.

Following the initial meeting an IHF report touted its influence in the groups: "Five trustees, three committee chairmen and six members of committees of Air Hygiene Foundation are members of one or other of Mr. Zimmer’s committees; three of them are chairmen.” The report continued, “It is plain that the Foundation is being extended an opportunity to collaborate fully, to aid especially in giving the views and attitude of the industries.”

The IHF did not exaggerate its importance in this report. Industry representatives, attorneys and consultants heavily dominated the committees. Dr. R. R. Sayers chaired the medical committee. V. P. Ahearn, representing the National Industrial Sand Association, became the chairman of perhaps it’s the most important and influential unit, the Economic, Legal, and Insurance Committee. While couched in objective scientific terms the subsequent committee discussions and reports were structured not around the disease, but rather around the social crisis of silicosis litigation and the technological and economic constraints on engineering methods to reduce silica exposures. The majority—that is all industry and insurance representatives and a fair portion of state and Federal agencies.

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98 Alfred Hirth, “Proceedings of the National Conference on Silicosis and Similar Dust Diseases,” April 14, 1936, National Archives, Record Group 100 (7-0-4(1), Pittsburgh, Pa.), 4-5, contained in Gerald Markowitz, et al., “The Limits of Thresholds,” 255.

agency representative—reached a consensus that economically feasible engineering methods could prevent silicosis from shortening a person’s work life [author’s emphasis]. Although the committees did address a few questions about the definition and prevalence of the disease, the reports almost exclusively examined issues critical to legal issues of industry liability. By specifying controls and exposure limits by which it was hoped most workers would not be physically disabled during their working life, the Committees hoped to control the crisis through a compensation system that provided assistance only to only those who experienced a disruption of wages, rather than to anyone who contracted the disease, or became debilitated upon retirement. Furthermore, by defining the hazard as only being present in a few select industries and job classifications, all amenable to engineering controls, and calling for a workmen’s compensation system of medical boards to provide for those few serious cases still arising, the reports sought to reassure the public and limit any adverse publicity.\textsuperscript{100}

Even Sayers’ medical committee broke no new ground, except in further limiting the definition of silicosis. The committee’s definition of silicosis included the requirement of a diagnosis of a reduced capacity for work. Without any new evidence, it endorsed an exposure level of five million particles per cubic foot of silica dust; the exposure level being increased in direct proportion to any reduction in the percentage of free silica in the dust. They included this recommendation despite the recognition that “there is evidence [that] for prolonged exposure a concentration of more than five million particles per cubic foot of a highly siliceous dust is dangerous” and their recognition that specific conditions in industry varied so much that “there can be no universal

\textsuperscript{100} The “work life” nature of the claim is an important distinction. By this time everyone recognized that silicosis is progressive. Most experts, including Lanza, also recognized that most workers in sandy trades had at least mild silicosis by the time they retired. The battle was over whether industry should pay for disabilities that occurred after an individual retired. See United States Department of Labor, “Abstracted Proceedings of Second National Silicosis Conference and Committee Draft Summary Reports,” dated February 3, 1937, Record Group 90, Records of the Public Health Service, National Archives, Silver Springs, Maryland.
regulatory standard of permissible dust concentration at the present time.” Thus, rather than being based solely on safety or health, they believed the “arbitrary standard should be based upon what is believed to be within the limits of good engineering practice and that which, from a medical viewpoint, [even though no evidence supported it] it is judged will largely control the silicosis hazard for most industrial exposures.”

Thus, through a slight of hand in the definition and a complete default on the establishment of a safe exposure level, the Committee provided significant support to industry’s litigation defense. Industry attorneys could now argue that any individual still working did not have “silicosis.” Furthermore, even in cases of admitted silicosis, industry reasonably followed exposure standards implicitly recognized by the Federal government. As Weidlein had originally proposed two years previously, the establishment of a 'safe' limit within easy technological and economic reach meant that companies could now be relatively safe from negligence liability suits.

The Committee on the Economic, Legal, and Insurance Phases of the Silicosis Problem provides perhaps the best example of how industry controlled the committees. As previously noted, the committee chairman represented the Industrial Sand Association. The members of the committee included three members of the insurance industry, four members of industry (including one from Union Carbide), two members of labor, five members of state commissions and one member, attorney Theodore C. Waters, who was listed as a state commission representative, but who in fact provided very significant legal advice to the IHF and actively worked to assist industry. The subcommittees continued this trend. A representative of the Pennsylvania Self-Insurers Association chaired the Economic Subcommittee. This committee also included two other industry

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101 “Summary Report of the Committee on Prevention of Silicosis through Medical Control,” 2-7, “Abstracted Proceedings of Second National Silicosis Conference and Committee Draft Summary Reports.” The complete definition is contained on page 2: “Silicosis may be defined as a chronic disease due to the breathing of air containing silica (SiO₂), characterized anatomically by generalized fibrotic changes and the development of military nodulation in both lungs, and clinically by shortness of breath, decreased chest expansion, lessened capacity for work, absence of fever, increased susceptibility to tuberculosis (some or all of which may be present), and by characteristic roentgenological findings.”
members, one state representative and one labor member. Theodore C. Waters chaired the Legal Subcommittee, which also included another industry representative, two state representatives, and one labor representative. The Insurance subcommittee was chaired by an insurance representative and contained two other insurance reps, two state representatives and one labor representative.\textsuperscript{102} The final report of this influential committee, similar to the other committees, is replete with industry prepared documents. Its recommendations follow the industry line, with union and labor representatives objecting too much of the report and writing a minority position report. The minority report was noted by the Labor Department, and then ignored by all.

When the conference met again on February 3, 1937, the Labor Department was faced with a fait accompli. The Committees had finished their work. They had arrived at general majority consensus. Most medical and technical experts at the conference—primarily consultants for industry—accepted the positions in the reports. Given the problems of the depression the Labor Department could not simply start over. To raise questions about the efficacy of the reports meant the possible collapse of several industries, massive lawsuits, and the potential undermining of the United States Public Health Service. Thus Verne Zimmer, Director of the Labor Department Division of Labor Standards and “ceremonial” Conference Chairman, accepted the reports.\textsuperscript{103}

In his largely forgotten “Foreword to the Committee Reports,” Zimmer reiterated that the Federal Government had taken no part in, nor voiced any opinions about, the conclusions and recommendations of the committees. He also wrote about the controversies in the committees:

It was recognized that certain phases of the subject were controversial, and because of the opposing interests involved it was not expected that there would be complete unanimity among the committees on all the findings and recommendations that would be made. Moreover, because of the highly technical factors involved in the problem, it was impracticable to attempt to


\textsuperscript{103} United States Department of Labor, \textit{Abstracted Proceedings of Second National Silicosis Conference and Committee Draft Summary Reports}, dated February 3, 1937.
balance the voting power of the various committees equally as between the major interests of management, labor, and the public, although each had definite representation in all discussions and deliberations. This fact should be kept in mind in appraising the committee report… [W]e wish to stress the need for giving careful consideration to labor’s views, which in many fundamental respects are at variance with those expressed in the committee reports. Labor’s position is indicated in the supplemental report and will probably be brought forward by the labor groups in the States in which legislation is under consideration.104

Experts Lend a Hand

Industry representatives left the National Silicosis Conference revitalized. With his reluctant approval of the reports, Zimmer, representing the U.S. Labor Department, implicitly admitted that the state of the depressed economy mandated that industry’s position on this important social issue remain predominant. Industry and its attorneys wasted little time in consolidating its position. The National Safety Council used the conclusions of the committee reports to prepare its own report trivializing silicosis.105 The committee reports provided the IHF with additional support for its continuing efforts to capture professional opinion and assist industry in their defense of silicosis litigation and workmen’s compensation claims hearings.106 The committee reports also supplied industry attorneys with a wealth of new opinions and quotes to present to civil litigation juries.

While all of these activities provided important benefits to industry, the most important benefit of the Conference Report may have come from the ability of industry attorneys and lobbyists to place pressure on state legislatures to enact industry friendly workmen’s compensation legislation. When seeking favorable hearings before state legislatures, industry lobbyists could now simply point to the recommendations of the National Silicosis Conference, conducted under the auspices of the “liberal” Federal Government and printed as a Department of Labor Bulletin.

105 David Rosner, et al., Deadly Dust: Silicosis and the Politics, 127-128.
106 Ibid.
The Conference Report was but the culmination of industry’s efforts to obtain a broad coalition of support in lobbying state governments for “appropriate” workmen’s compensation legislation. Throughout the 1930s industry representatives and attorneys strove for close cooperation from the various professional associations, such as the American Public Health Association, the American Medical Association, and the American Industrial Hygiene Association in advancing their program of silicosis minimization. They similarly sought to influence and co-opt governmental occupational and public health officials. At the same time, they developed strategies to limit the influence of local doctors, the primary source of silicosis diagnoses and the resultant new silicosis cases and claims. Finally, the IHF also coordinated closely with the American Bar Association to ensure proposed new workmen’s compensation laws were not overly inclusive.

The efforts directed toward medical professional associations included working on committees, having IHF representatives and consultants elected to association offices, presenting speeches at association conferences, and proposing industry friendly resolutions and standards to the association membership. Industry representatives, consultants, and possibly attorneys had become closely involved with the American Public Health Association at least as early as 1930 when Lanza was one of three members on its Committee on Silicosis. That year the committee proposed a standard for airborne silica dust. Committee members recognized that it “was not found to prevent the occurrence of silicosis.” However, it “could be reached by the use of economically practicable ventilating devices.”

In addition to recommending a silica exposure standard easily met by industry, the American Public Health Association also provided assistance in influencing workmen’s compensation legislation. As early as 1933, in spite of the lack of medical consensus regarding the way in which silica affected the lungs or its speed in causing changes, the American Public Health Association

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(APHA) announced its support for an expert medical panel for silicosis, a scheme that would take away the power of a jury to decide silicosis legal cases. They used cost savings as a justification. “Without some form of medical control, the management of compensation for a disease such as silicosis would be difficult.” They did not mention the potential dual benefit to industry of fewer claims or the elimination of most family or worker doctors from the diagnosis. Industry and the IHF were both grateful for this and subsequent assistance. In a speech before the association that was published in a 1939 issue of the association’s journal, Theodore Waters publicly thanked the group for their interests in industrial hygiene and assistance in the preparation of occupational disease laws. In the same speech, he reiterated industry’s strong support for medical panels to provide final decisions on medical diagnosis.

Nor was the APHA the only medical association that experienced increasing lobbying from industry. Even before the inception of IHF, industry doctors and researchers spoke at American Medical Association meetings. For example, at the June 1934 Convention of the American Medical Association, Doctor Leroy U. Gardner of Saranac Lake gave a featured presentation on silicosis. As previously discussed, the IHF considered the American Medical Association so important that its “Program of Initial Activities” prominently included enlisting the help of the American Medical Association in developing authoritative standards that would be of use at trial. Consultants, representatives and employees of the IHF and industry also sought elected office in all medically relevant associations, perhaps the most prominent being Theodore Hatch’s election as President of

108 David Rosner, et al., Deadly Dust: Silicosis and the Politics, 89. This willingness of the APHA to back industry’s call for removal of silicosis cases from trial by jury demonstrates industry’s firm control of the group. Given the state of knowledge concerning silicosis, such control provides the only reasonable explanation for the association’s apparent belief that experts could agree better than a jury—for as physician Emery Hayhurst, one of the very few independent occupational medicine silicosis experts, wrote, silicosis “is today the most controversial as to cause, course, complications, and prognosis.” See Emery Hayhurst, “Review of Symposium on Silicosis–Trudeau School of Tuberculosis, Saranac Lake, N.Y., June 18-22, 1934. Wausau, Wis.: Employers' Mutuals,” American Journal of Public Health 24, no. 12 (1934): 1269.

the American Industrial Hygiene Association (AIHA). That year, the Directors of the AIHA also included employees of Westinghouse and Owens Illinois, both prominent members of the IHF.¹¹⁰

Since any workmen’s compensation scheme necessarily involved legal issues, the American Bar Association played a supporting role similar to those of the medical associations. Obtaining the American Bar Association’s cooperation in these legislative efforts occupied a prominent location not only in the Program of Initial Activities for the IHF but also in its initial code of Regulations. Once again the IHF had a direct connection with the “unbiased” American Bar Association to ensure cooperation.

Writing a letter to the IHF’s Board of Trustees, Hirth proposed that the foundation prepare a model workmen’s compensation law, which could be provided to any interested state. However, recognizing that the IHF was “not the best organization to publicly sponsor the contemplated move” because it was "on last analysis, an employer organization," Hirth cautioned the Board that any direct "legislative proposal it may make is subject to the same scrutiny and criticism as is given proposals of organizations such as the National Manufacturers Association, United States Chamber of Commerce, etc." Hirth informed the Board that the Legal Committee agreed with his conclusion that an independent organization be asked to sponsor the proposed law. The committee suggested the "Committee on Legislation of the American Bar Association, who, because of their non-partisan character are ideally suited to appear publicly as the sponsor of this work." He further explained that the IHF was "rather fortunate in the fact that the present President of the American Bar Association has been associated with both Mr. [Theodore] Waters [partner in Mulliken, Stockbridge & Waters, of Baltimore, Maryland, frequent legal advisor to the Foundation, and member of the Foundation’s Legal Committee] and the writer in silicosis matters. He will therefore appreciate the importance of

¹¹⁰ "Medical Session Opens Tomorrow," New York Times, June 10, 1934, n2; and Industrial Hygiene Newsletter 7, no. 7 (July 1947): 6.
this work and we believe that we can rely upon him to give us his active personal assistance."\textsuperscript{111} Although there are no similar letters relating to the American Medical Association or the American Industrial Hygiene Association, the same considerations and rationales suggest the possibility that the IHF pursued a similar approach with them.

In addition to direct activities within the professional associations, the IHF also sought to influence practitioners and government officials by sponsoring symposia and speakers at symposia. These 1930s silicosis conferences almost always included a section on the legal aspects of silicosis, including legislative control and compensation. Speakers at these conferences included most of the important names in the field. They also provided a ready venue for defense attorneys to meet, talk with, and retain the most notable experts in the field.\textsuperscript{112}

The IHF and other industry agents and attorneys did not direct their activities exclusively toward professional associations. They also took the direct approach of hiring the experts. Occupational doctors and industrial hygiene engineers needed money to run their businesses, conduct research, or—in the case of academic and government employees—provide some additional income. Federal and state agency interested in conducting studies also required industry agreement before they proceeded. Industry attorneys and the newly established IHF supplied both the money and the cooperation to the experts and agencies. In return, they expected to see reports that were optimistic and that industry could edit. The manner and extent of this cooperation is evident in a series of interviews with important pioneer figures in industrial hygiene that was published in the 1984 \textit{Annals of the American Conference of Industrial Hygiene}. Readers of the interviews are struck by the number of individuals who during their career worked both in Federal or state

\textsuperscript{111} David Rosner et al., “Workers, Industry, and the Control,” 43.
agencies and in industry. The interviews also offer evidence that most of the interviewees who remained with a governmental agency closely cooperated with industry and, at a minimum, provided the benefit of the doubt to industry in its dealings with workplace hazards.\footnote{Charles D. Yaffe, Ed., “Some Pioneers of Industrial Hygiene,” \textit{Annals of the American Conference of Governmental Industrial Hygienists} 7 (1984): 3-135.}

The interview of Henry F. Smyth, Jr. provides a good example of the industry perspective found in most of the interviews. Smyth was an engineer. He became a charter member of the American Industrial Hygiene Association and served on its Board of directors. During the 1960s, he was an Administrative Fellow at the IHF’s parent, the Mellon Institute. In his interview, Smyth discussed his early work for his father providing industrial hygiene services to various companies. Some of this work involved investigations for litigation purposes, such as the silica investigations he and his father conducted in the 1930. “During this interium (sic), we did a lot of work in the then contemporary silicosis racket in south Jersey. Sand – oh, in the sand industry – much of it was ground for household cleansers, and we began to get more and more silicosis cases. So we went around the very unsatisfactory deal of making tests in the operation after a suit was brought. Nothing else could be done about it. We realized it was very unsatisfactory, even in those days, as far as evidence goes.”\footnote{Charles D. Yaffe, “Interview of Henry F. Smyth, Jr. on May 18, 1976,” \textit{Annals of the American Conference of Governmental Industrial Hygienists} 7 (1984): 118; and Henry F. Smyth, Jr., “Toxicology of Industrial Chemicals,” \textit{Archives of Environmental Health} 8 (March 1964): 384. This story also illustrates the methods some companies used both historically and currently in toxic litigation. Even today, some businesses using toxic substances such as silica or asbestos often do not sample to see if it is being released into the air or water unless required to do so by regulation. If a toxic hazard is not reported, short-term expenses will be lower. If later sued, the company can clean the site, then perform sampling under controlled conditions for use at trial. If the measurements still turn out high, the sampling can be hidden through the attorney work product privilege. This same tactic can be used when state officials seek to inspect facilities. As described by the Director of the Research and Education Department of the Molders and Workers Union, “How often did we complain about the conditions in the shop only to have the inspector call and make an ‘appointment’ to visit the plant. Through this process we were at least able to get the place cleaned up once in a while, for the inspector always allowed some time before his visit. His usual ‘clean bill of health’ was signed and posted for a few days before the place was back in its neglected state.” Jim Wolfe, “OSHA: A Short Story,” \textit{Labor Studies Journal} 3, no. 2 (Fall 1978): 150.}

By the time of the National Silicosis Conference, almost all experienced occupational doctors and industrial hygienists succumbed to this siren’s call, even those in Federal service. Clara
Beyer, confidential aide to Labor Secretary Frances Perkins, and back-room advisor on much New Deal social legislation, even remarked that if the Public Health Service was going to be an advocate for industry, then the Department of Labor “should be a service agency for labor.” Beyer had good reason for concern about the biases of government Public Health officials. Many developed very tight, cozy, relationships with industry. R. R. Sayers sat on the board and various committees of the Air Hygiene Foundation. Lanza went from the Public Health Service to Metropolitan Life Insurance Company. William Yant left the Bureau of Mines to join Mine Safety Appliances Company. Oscar Sander, Leroy Gardner, Henry Pancoast, Eugene Pendergrass, and even Philip Drinker worked as paid consultants to industry attorneys. In 1934, Roy Jones of the United States Public Health Service even complained that the many proposals calling for standard workmen’s compensation in cases involving silicosis could bankrupt entire industries: "We apparently think of every case of pulmonary fibrosis as requiring compensation,” he wrote, “whether disabled or able to work." It apparently did not matter to him that a person fired from a well paying job would have difficulty finding employment at the same pay level, let alone any employment during the depression.\footnote{Traditionally, standard workmen’s compensation paid for specified injuries at work without regard to a loss of wages. The Clara Beyer quote is from David Rosner, et al., \textit{Deadly Dust: Silicosis and the Politics}, 126. “Clara Beyer, 98, dies, New Deal Official,” \textit{New York Times}, September 28, 1990, Section A, 18; see also Gerald Markowitz, et al., “The Illusion of Medical Certainty,” 231. Some of the consultation to industry was undoubtedly due to the often inadequate salaries, budgets, facilities and budgets in many of the state industrial hygiene divisions; see Victoria M. Trasko and J. J. Bloomfield, “An Analysis of Industrial Hygiene Activities in State and Local Health Departments, 1940-41,” \textit{Public Health Reports (1896-1970)} 57 (June 5, 1942): 867; and Victoria M. Trasko, “Industrial Hygiene Milestones in Governmental Agencies,” \textit{American Journal of Public Health} 45, no. 1 (January 1955), 44.}

In return for this monetary support, most occupational doctors and researchers in turn supported industry’s position to the fullest extent possible. Medical articles throughout the period are replete with examples of this support. Even when the facts provided strong evidence against an industry position the author often found a way to minimize or ignore them. As noted above in discussing the history of the Gauley Bridge disaster, these industry-oriented articles commenced by the early 1930s. If anything, the efforts to downplay silicosis in medical articles increased following
the National Silicosis Conference. Perhaps the best examples come from articles considering the foundry industry, businesses employing numerous extremely dusty trades. A medical article excusing the foundry industry’s past failures to take corrective action, written in 1938 by Sander, provides an illustration of the typical industry slant taken by occupational health researchers. In this article Sander managed to claim both that foundries were justified in failing to take precautions as the result of understandable ignorance and that there was little advanced silicosis in foundries. Sander further argued that industry was naturally late in realizing its dust problems because dust concentrations dramatically increased between 1915 and 1925. Finally, Sander maintained that before the 1920s there were few cases of silicosis.

…[E]arlier cases having been so relatively infrequent that they were not suspected as occupational in many instances…So little was the foundry industry concerned with these earlier isolated cases that a survey to determine the extent of the hazard was not attempted until 1931.\[116\]

In his article, possibly written to assist in his expert testimony at trial, Sanders ignored several facts. First, he did not mention or cite the numerous previous studies demonstrating disease in sandblasters, a trade extensively used by the foundry industry. Second, power tools used in the enclosed spaces of foundries created large amounts of dust. Observers of sandblasting and other activities in foundries could readily see that the dust created was similar to that created in mining, an occupation that had seen considerable research. In addition, while there were numerous small foundry businesses, Sander did not limit his judgment to them, but included the entire industry. Large corporations, such as General Electric and Union Carbide, owned many foundries—and most of the largest. They also either owned mines or were well aware of the potential of disease from excess dust such as that created by power tools. Finally, Sander either ignored or was unaware of

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industry’s suppression of articles that documented the problem, such as the censorship undertaken by the National Safety Council on Winslow’s article.

In writing the conclusion to his article, Sanders incredibly implied that foundry owners still did not have much of a problem. His relevant conclusions are as follows:

2. With half of the total group having had less than 10 years of foundry exposure, about 7 per cent with definite silicosis were found.
3. Of the 279 with silicosis, 60 (or 22 per cent) had tuberculosis which either was definitely active or in which activity was indeterminate. In 8 of these (or 3 per cent of the silicotics), there was definite activity of the tuberculosis.
4. Serial observations have suggested that silico-tuberculosis as seen in foundry workers is primarily a reactivation of a previously acquired but walled-off tuberculosis.
5. Simple silicosis as seen in foundry workers is only very slowly progressive, so much so that no visible changes have appeared in 4 years of observation. Moreover, the simple silicosis among these workers is only rarely sufficiently advanced to cause symptoms and incapacity for work.
6. These studies suggest that the tuberculosis rate, among foundry workers at least, is not raised by dust exposure per se, but rises only after silicosis becomes definitely established.117

Sanders did not indicate what he meant by “definite” silicosis. Given the large percentage of tuberculosis in the silicosis he found, it likely meant what had previously been characterized as moderate or severe silicosis. He also did not indicate if any of the possible silicotics had tuberculosis, thus throwing doubt on his conclusion that tuberculosis “only rises after silicosis definitely becomes established.” This distinction would become important for workmen’s compensation, since if silicosis was not severe enough to cause disability by itself, industry medical experts could claim that an accompanying tuberculosis was unrelated to the silicosis and, thus, not compensable.

Other industry doctors, while admitting the dusty conditions, also claimed that little disease had actually developed from foundry operations. As late as 1945, Lloyd E. Hamlin, Medical Director of the American Brake Shoe Company (a foundry owner), stated:

In the foundry the atmosphere is often dense and the various operations produce quantities of dust and fume containing fair amounts of silica. Molders and other workers have breathed this air over many years and one would expect to see evidence of silicosis in their chest roentgenograms. But while a few employees' X-rays do show evidence of nodulation, the amount of actual definite fibrosis is relatively low. Routine examination of a large number of chest radiographs of these individuals had led me to call this type of film "The Foundryman's Chest," to indicate a rather typical partial obliteration of linear markings and general lack of sharp detail without clear evidence of nodulation. X-rays of these men show changes which are more marked than normal, but they are not sufficiently definite to warrant a classification of silicosis.

Furthermore, in Hamlin's view, even under the worst of conditions silicosis required at least eight years to develop from a pre-silicotic stage to a definite degree of nodulation.\textsuperscript{118}

Significantly, Hamlin limited his comments to individuals during the period they worked for the company, not after they left or were fired. Nor did he discuss what actions the company took when it found abnormal x-rays that did not yet “warrant a classification of silicosis.” While certainly not provable at this time, actions at the time by other corporations and insurance companies suggest that the workers were then fired for “inefficiency.” Drinker, perhaps the most recognized industrial hygienist of this period, described why employment constrained comments such as Doctor Hamlin’s were meaningless as a measure of foundry induced silicosis.

In the foundry industry...unskilled workers on the shakeout and the like are exposed to the highest dust concentration while the skilled molders (comparable to expert granite cutters) produce relatively little dust during molding. The unskilled foundry worker, having no deep feeling for his work, quits if he experiences any respiratory trouble. Thus there is a high labor turnover which accounts, in part, for the relative absence of silicosis and tuberculosis in this industry.\textsuperscript{119}

\textsuperscript{118} L. E. Hamlin, “Review of Silicosis,” 47 and 49.

Lanza also assisted in this venture. Lanza’s Magnus opus on pneumoconiosis, which he edited and had published in 1938, provided a supposed full discussion of the medical and public health aspects of the silicosis. It had a complete roster of international experts who seemingly covered every issue involving silicosis. Yet, the book clearly aimed to assist “confused and terrified industrialists and insurance officials.” In it Lanza described the rise of common law lawsuits as “preposterous and unbelievable,” making no mention of Gauley Bridge or its victims. The 439-page book mentioned “acute silicosis” on little more than a page, describing it as a medical oddity with minimal literature; suggesting many purported cases were in fact tuberculosis, “a disease that can play queer tricks with silicosis.” Clearly, the writings and statements of Hamlin, Sander, and Lanza did not reflect the facts of silica disease and its onset, but rather the evidence needed to defend companies at lawsuits and workmen’s compensation hearings.

Although efforts at providing a “head in the sand” negligence defense undoubtedly helped defense counsel in civil actions, this was not an element in workmen’s compensation cases. Workmen’s compensation was designed to provide plaintiffs with easier procedures for prevailing than civil suits. Contrary to the requirement of a showing of the defendant’s negligence in a silicosis civil lawsuit, legislators established workmen’s compensation as a no fault procedure. In return for a claimant giving up his or her right to sue the employer, the claimant did not have to demonstrate employer negligence. However, the proceeding still required two elements in common with a civil law action: first, the plaintiff had to prove he or she suffered from silicosis as defined by the law; and second, the plaintiff had to prove the disability resulted, at least in part, from exposure while working for the defendant.

120 Anthony J. Lanza, ed., *Silicosis and Asbestosis*, 413 (confused and terrified) and (preposterous), 223-5 (a disease that can play); and Jock McCulloch, et al., “Anthony J. Lanza,” 103.
The ongoing changes to the medical science and diagnosis being instituted by industrial medicine and hygiene professionals lessened the chance of plaintiffs prevailing on both of these issues. Changes made by the “experts” to the definition and requirements for diagnosis of silicosis provided the most help to attorneys in both civil actions and the increasingly ascendant workmen’s compensation cases. It was this change that enabled Sander to claim so little silicosis in the foundry workers included in his study.

The chronic nature of silicosis, generally a slow but progressive disease, also presented enormous problems for the workmen’s compensation system, a difficulty taken advantage of by industry attorneys. How should payment be determined? An injury generally provided immediate evidence of its extent. Payment could be based upon the specific injury, which generally affected all individuals similarly. Yet, silicosis normally took a number of years to become evident. Even then individuals and doctors could not determine how severe it would become during a further period of years. Often, it did not become disabling until the later stages of a career or even after the worker’s retirement. This presented great difficulties in determining which company was responsible for the injury and how much should be paid. Furthermore, the scale of potential liability was staggering. The number of workers with at least mild silicosis in certain industries, such as foundries, approached fifty percent of the work force, an extraordinary number of potential payments; one that might bankrupt the system. In addition, silicosis predisposed individuals toward and even led to a number of other diseases not commonly thought of as being occupation-based, such as pneumonia and tuberculosis. Thus, we read the almost panic stricken articles and lectures by industry representatives during the mid 1930s.

Industry attorneys responded by attempting to stabilize and minimize the number of payments to sufferers of silicosis. The greatest support by experts resulted from the changes in the way that the occupational health community viewed silicosis. Mild silicosis could be tolerated within
industry; only when it became so severe that a person could no longer work should it be considered worthy of consideration. This viewpoint dominated the National Silicosis Conference and helped industry control the state workmen’s compensation legislation that followed. Following the National Silicosis Conference it seemed that every industry “expert” was reporting on silicosis being a disease of the past, even while numerous studies and inspections showed otherwise.\footnote{Gerald Markowitz, et al., “The Illusion of Medical Certainty,” 232.} In one sense, silicosis, as it had previously been known, was a disease of the past. Setting standards that gave the appearance of providing protection against the disease, limiting the medical practitioner who could diagnose the disease, arguing that only full disability should be included in any compensation scheme, and redefining silicosis—all these provided the primary means of accomplishing this. Thus, when the insurance and corporate lobbyists called on the legislature to pass legislation with a narrow definition of disability based on lost wages, they were able to point to a similar narrowing of the definition of silicosis within the public health community.

At the National Silicosis Conference, industry experts emphasized that with the new voluntary silica exposure standard industry now had the problem under control. Yet, even as they promulgated the standard, medical experts admitted that it had limited usefulness. In his seminal industrial hygiene volume, Drinker agreed with the concern expressed by the National Silicosis Conference medical committee about the limited usefulness of a universal standard. In considering whether the standard could be made accurate, he cautioned that one study found "the variations in dustiness were so great as to make unpractical an attempt to study the sickness rate in relation to dosage except in a general way." Furthermore, he understood the standard’s real purpose. "The idea of adopting standards of permissible dustiness for each harmful dust has a medicolegal appeal that is not at all justified by the data available today… [Yet,] in none of the original studies was there a single suggestion that the threshold figures were useable as legal standards." Drinker expressed
particular concern about the U.S. Public Health Service study of the granite-cutting industry, the primary source cited by most industry experts for their designation of “safe” maximum levels of dustiness. That study found that a maximum dust concentration of between 10 and 20 million particles per cubic foot of air was a “desirable” limit for dust. It concluded that this limit could be reached with the use of economically practicable ventilating devices. However, as the article also stated, "[i]t should be noted that the limit established was not found to prevent the occurrence of silicosis."

Unfortunately, even this minimal and admittedly inadequate standard, while useful for publicity and critical to the defense of lawsuits, was often not followed. While many state commissions did not conduct comprehensive studies of foundry operation, in two states that did, Massachusetts and North Carolina, the commissions found that most foundries had not implemented the changes.

During the late 1930s and 1940s, industry medical experts not only wrote medical articles to assist defense attorneys, but they also attempted to co-opt the ability to diagnose it. They did this by embarking on a campaign to educate family doctors about the complexity of silicosis diagnosis. They warned local doctors not to attempt any diagnosis on their own. The experts maintained this caution was necessary because local doctors had neither the training nor the technology to distinguish between silicosis and benign pneumoconiosis. In addition, the IHF experts maintained that a diagnosis should not be made, even with a work history in a foundry, unless measurements had been taken of the silica dust in the air at the work place and—as mentioned by Sampson and Gardner—a series of X-rays over a number of years. The unspoken underlying reason for this effort was not medical. Rather it was legal. Local doctors diagnosing silicosis meant additional lawsuits. In addition,

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124 David Rosner, et al., *Deadly Dust: Silicosis and the Politics*, 186; and Homer L. Sampson, “The Roentgenogram.”

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requiring that dust measurements and X-rays be obtained before rendering a diagnosis was extremely helpful to defense counsel, since, as was the case with the sites Smyth sampled in the 1930s (or the cases Sampson reviewed in 1933), most work locations had not been sampled prior to suits being filed and most individuals did not have a series of x-rays.

Some industrial medicine professionals focused specifically on assisting defense attorneys and lobbyists in their efforts to have industry friendly workmen’s compensation legislation enacted. George Wright, a doctor whose research was frequently funded by industry and who continued to testify for toxic substance defendants into the 1990s, was one such professional. In a 1945 article, he argued against partial disability in workmen’s compensation. He began by recognizing that the diagnosis of silicosis "virtually precludes the employment of such a man by a new employer in any job having a silicosis hazard. This means the silicotic is no longer a free agent and is denied the opportunity for advancement under a new employer." However, even while recognizing that silicotics were denied opportunities, Wright argued that partial compensation was rarely warranted. Interestingly, he attempted to cast his opposition as concern about the worker. He admitted that breathing discomfort is "not rare even now… [but] would probably be greatly exaggerated by partial disability legislation… [A] [p]sychological element in breathlessness exists to a varying degree in nearly all silicotics."125 Thus, for their own psychological good, silicotics should be denied partial compensation even though they could no longer work at their better paying job and were being denied the opportunity of advancement.

However, the redefinition of silicosis remained the most important aspect of medical assistance to workmen’s compensation defense. By the late 1930s industry’s experts and consultants, as well as friends in the Federal and state public and industrial health services had redefined the

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medical criteria for silicosis diagnosis. This redefinition, undertaken at the instigation of insurance companies, attorneys and businesses, provided a more restrictive definition of silicosis as a compensable disease under workmen’s compensation laws. Rather than using purely medical criteria, the new definition used the loss of earning capacity as the objective "criterion" to determine which silicosis was worthy of consideration for compensation.\textsuperscript{126}

Industrial medical experts redefined silicosis in three ways. First, as previously stated, they defined silicosis in terms of the inability to work, rather than by its physical manifestations. Second, they separated it from other diseases, such as tuberculosis or pneumonia, to which individuals with silicosis became predisposed. Gardner’s and Sampson’s articles concerning the Gauley Bridge workers provide prime examples of this. Finally, they minimized the respiratory effects of mild silicosis.

Respiratory obstruction was the means by which simple silicosis caused disability. If a worker was having trouble breathing, then his workload had to be limited. This in turn potentially affected his pay level. Thus, under the workmen’s compensation schemes that provided for partial disability, a worker with silicosis involving breathing obstructions could potentially make a workmen’s compensation claim. As early as 1915, Lanza had remarked upon the ability of even relatively short exposures to produce such a disability. In a speech before the American Public Health Association he cautioned that “[e]xposure to this dust [flint dust composed of 95% silica] for a few years brings on disablement -from dyspnea and cough.” Two year later, in his previously discussed landmark study he stated that even “the first stage [of silicosis] is characterized with slight or moderate dyspnea [labored respiration] on exertion.”\textsuperscript{127}

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\textsuperscript{126} Gerald Markowitz, et al., “The Illusion of Medical Certainty,” 231, 239.

\end{flushright}
Yet, by the mid 1930s industry medical consultants and researchers were almost universal in their minimization of respiratory obstruction in the initial stages of silicosis. Lanza no longer mentioned dyspnea during first stage silicosis. In fact most industry experts maintained that first stage silicosis caused no measurable harm and produced no disability. They ignored the earlier studies, which had found the disease could be progressive even at this early stage. Rather, they maintained that it could advance to silicosis‘ second stage, the point at which breathing was affected, only if exposure continued. By 1935, even the Public Health Service had ceased mentioning shortness of breath as a characteristic of first stage silicosis. Rather, it disregarded the progressive nature of the disease, focusing instead on the arbitrary and subjective stage when silicosis decreased earning capacity. “The term of disability… may be defined as a decreased capacity to do the work required of the individual in the course of his usual occupation and/or increased susceptibility to respiratory infection causing a loss of time from work which may reasonably be considered as primarily the result of pulmonary fibrosis.” In 1936, Lanza went even further stating, “disability in silicosis is seldom due to the silicosis itself.” The following year, Attorney Waters, in his guise as a member of the Maryland Compensation board, declared, “simple silicosis… causes relatively little severe disability.”

In the end, industry’s efforts at modifying the medical paradigm and practice for silicosis, whether directed toward experts or family doctors, were simply building blocks for the main structure: the defense of silicosis lawsuits and enactment of industry friendly workmen’s compensation programs. Upon these edifices corporate profits rose or fell.

In seeking appropriate workmen’s compensation legislation, industry lobbyists and attorneys were especially concerned about the issue of having to pay for opportunistic diseases that often

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accompanied silicosis and often were the direct cause of the disability at issue. In one occupational
disease information pamphlet, insurance industry executive and attorney Henry D. Sayer specifically
remarked (from an insurance viewpoint) about the necessity of limiting workmen’s compensation
coverage. He began with an understanding that “[w]e all start out with the proposition that all
diseases fairly chargeable to an industry should be compensated by the industry." This
understanding, however, had its limits. "We differ, however, in the method of coverage, and the
difference in method means vast difference in the rule of liability and the burden imposed upon
industry." Sayer feared "blanket" coverage would lead to virtually any disease allowing compensation
claims. The "great danger lies in the fact that such general and vague language will lend itself to the
inclusion of any and every sort of illness and disease to which human flesh is heir." He ironically
concluded by derisively belittling these ridiculous ideas as potentially leading to the inclusion of
"natural" diseases such as tuberculosis, pneumonia, and cancer as occupational diseases: "We surely
do not think of colds, pneumonia, tuberculosis… and cancer as occupational diseases." Sayer argued
that blanket coverage could not resolve the liability crisis; rather, without strict new definitions,
occupational disease coverage would provoke a new spate of lawsuits and compensation claims
along with enormous discontent. He concluded that implementation of an all-inclusive method "will
give rise and lead certainly to a great volume of litigation, all looking to court interpretations as to
what is and what is not an occupational disease." 129

As late as 1945 B. E. Keuchle, Vice President at Employers Mutual Liability Insurance
Company continued to caution against blanket coverage. That year he wrote "The basis for the

discussing industrial occupational health hazards listed the tuberculosis rate of industrial workers as much higher than
unskilled workers—the individuals who worked most closely with silica and were therefore the most susceptible to
silicosis—the tuberculosis rate in the 1930s was seven times as high as for professional workers. Unskilled laborers were
also the easiest to fire or replace. Abel Wolman, “A Century in Arrears,” American Journal of Public Health 28 (December
1938): 1373.
present opposition (in certain states) to extending the laws to include compensation for diseases is
the fear that through loose administration, these laws will be interpreted to offer complete social
insurance against all sickness and old-age infirmities at industry’s expense. Able administrators
recognize this trend.”

Notwithstanding these concerns about broad workmen’s compensation coverage,
throughout the 1930s industry pushed for “appropriate” worker’s compensation laws in selected
states (usually those with numerous lawsuits). When coupled with industry-friendly loopholes,
workmen’s compensation legislation provided the fastest and most effective means of reducing the
burden of silicosis lawsuits. Thus, from the IHF’s inception, attorney Alfred Hirth and member
companies fully supported inclusion of silicosis in workmen’s compensation legislation. Their
proposals called for tight limitations on the compensation with numerous restrictions to limit
“racketeering” plaintiff attorneys. These restrictions also served as loopholes—allowing
companies to avoid paying legitimate claims. As even the association of industry doctors had noted,
finding loopholes in compensation programs had been a standard practice for company lawyers
since the first law was enacted in 1911.

Industry representatives believed the best way to limit compensation for opportunistic
diseases was by limiting the use of the jury. They were so sure of their control over the most
nationally respected experts that in their legislative proposals they frequently sought to have the
issue of diagnosis left to a panel of experts. They argued initially that the diagnosis of silicosis is so
complicated only silicosis experts—experts largely controlled by industry—can establish disability.
For example, at the Oregon legislature industry representatives proposed that any claim to be

130 B. E. Keuchle, “Occupational Disease Liabilities – Financial and Humanitarian,” in Industrial Tuberculosis Silicosis and
131 Henry B. Selleck, et al., Occupational Health in America, 50.
referred to three experts who would have the final say, with any appeal being based solely upon the record.\textsuperscript{132}

Industry attorneys also argued before legislatures that the time to file a case should be limited to a short period (normally two years) after the last employment by the specific manufacturer. They justified this with the argument that employers needed some certainty and stability to their liability. The industrial medicine community apparently stood mute while this occurred. In researching this issue, I have not found a single article written by an industrial medicine doctor in the 1930s or 1940s, which sought to increase time available for workmen’s compensation claims due to silicosis’ long latency. These lobbying efforts succeeded in many states, with new workmen’s compensation laws requiring applications for silicosis benefits be filed within one or two years after leaving employment. In Kentucky the law went even further, only allowing claims if the worker had reported the applicable lung illness to the state health board within sixty days of learning about the disease. The claimant also had to have worked in Kentucky for at least two years immediately prior to the diagnosis.\textsuperscript{133}

These activities continued for decades. As late as 1963, Waters, in his capacity as an IHF consultant, continued to recommend that companies lobby for very restrictive claims periods in state workers’ compensation schemes for silicosis. In his 1963 Legal Series Bulletin Number 4, he recommended virtually the same legislative program as that proposed in 1937 by him and other industry lawyers. These proposals were not due to a lack of knowledge concerning the progressive nature of silicosis, as he wrote in the same Bulletin; “It is known that frequently disability does not occur until many years after the termination of employment or exposure.” Incredibly, this admission occurred less than a page before he endorsed “a provision with respect to the time limitations of


filing claims similar to that contained in the laws of the State of New Jersey, to wit: Two years after last exposure or last payment of compensation, or one year after employee knew or should have known of the existence of the disease, with an overall limitation of five years after last exposure.” As suggested by Waters, this proposal provided a degree of certainty of liability termination to employers—yet it left many, if not most, sufferers of this chronic and progressive disease without recourse. Under his scheme of compensation, companies need pay claims only at the time of disability. Yet as recognized by both attorneys such as Waters and occupational doctors, under these schemes payment could often be precluded because disability might be “many years” following the worker’s last employment in a job that could cause the disease. Waters attempted to justify this position with the incorrect statement that “the existence of pneumoconiosis is medically determinable upon the termination exposure to dust…”\textsuperscript{134} In actuality silicosis at lower exposure rates can have a latency period of twenty years or more before it shows on x-ray, but then it can be a very progressive disease. In addition, the new definition of silicosis required not only x-ray findings, but also decreased lung function, which for moderate exposures, typically occurred only several years following initial x-ray findings. This fact was well known to the medical experts retained by the IHF and industry. They apparently never corrected or even objected to the well-publicized statements of Waters.\textsuperscript{135}

During the 1960s, workmen’s compensation laws in many states still required occupational exposure to silica or other toxic substances within a set number of years prior to the compensation claim. For example, a Texas court denied an asbestosis claim for a long-time asbestos worker


\textsuperscript{135} As early as 1915, Lanza documented that symptoms from toxic exposures can arise years and sometimes decades later. Drinker also emphasized this point in his book. In particular, Drinker stressed that silicosis can become disabling years after last exposure. In making this assertion, he used the example of Welsh miners. Many of these miners passed their physical for the army, fought through World War I, and then returned home to later die of silicosis, without returning to the mines. David Rosner, et al., \textit{Deadly Dust: Silicosis and the On-Going Struggle}, 34; Philip Drinker, et al., \textit{Industrial Dust}, 28; and Peter S. Barth, et al., \textit{Workers’ Compensation}, 67.
because he was diagnosed four years after his last job working with asbestos. South Dakota law required a person be exposed in five of the last ten years preceding disablement. Colorado had not only the five-year rule but the five years also had to be in Colorado. Other states required minimum periods of exposure, often five to ten years, before compensation would be provided. In one case a claimant (whom all parties agreed had debilitating silicosis), was denied compensation because his exposure length was four months less than the statutory five years.\textsuperscript{136} Other states followed the lead of Kentucky, requiring that a certain period of recent exposure be in the state. Arizona required that 1200 shifts during the last twelve years be in the state. South Dakota required that at least half of the most recent ten years of work be in the state.\textsuperscript{137}

Cases of this kind occurred throughout the U.S. In Alabama, an employee contracted a debilitating disease after working for same employer for 32 years, 23 of them in mines—yet was denied compensation because he could not prove exposure to dust in his last five years of work. Georgia and Minnesota required claims to be filed within three years of the last exposure to silica dust. Texas and West Virginia (one year) also had short claims periods. In South Carolina, widows had to file within six years after the last exposure of the worker, notwithstanding when the disease was determined or death occurred. In at least one state, Utah, a claim was denied because the individual died before the commission acted, even though the original claim was timely submitted.\textsuperscript{138}

With the broadly based assistance of both professional associations and medical researchers, the IHF and the silica industry largely obtained the legislation it desired. As Lorin Kerr discusses in his study of class conflict in occupational disease, even before the National Silicosis Conference and

the IHF’s establishment industry sought and in many cases obtained control of workmen’s compensation for silicosis:

Management control of compensation is obvious in the case of the West Virginia silicosis statute. In 1933 the legislature attempted to make silicosis compensable. The bill was strongly endorsed by the West Virginia Federation of Labor and legislators from mining counties, but was defeated by strong pressure from the mining industry. In 1935, management secured the enactment of legislation notable for its severe restrictions on workers’ compensation. The Bulletin of the International Juridical Association stated in 1936: “Whether or not the West Virginia workmen’s silicosis law is declared unconstitutional, the subservience of the West Virginia legislature to the interest of employers is almost unparalleled in its hypocrisy and the statute must be wiped out.” 139

In Wisconsin, a court held that the compensation board’s refusal to provide money to a diseased worker was appropriate despite his medical disability. The court ruled, “medical disability, does not, in the absence of an actual wage loss, entitle one to compensation.” The court was simply following the state legislature’s method of determining compensation for occupational diseases, a method that turned traditional workmen’s compensation on its head. Traditionally injuries on the job were immediately visible and failure to pay compensation would likewise immediately hurt morale. However, since it was a chronic progressive disease, failure to pay for silicosis had much less effect on morale unless the worker had not retired or been fired for “inefficiency” before the silicosis became so disabling that he could no longer work. With the apparent blessing of the federal government’s National Silicosis Conference and the establishment of the IHF, the silica industry and its insurance companies, attorneys, and public health supporters were successful in enacting similar schemes in states throughout the country. 140

Once the workmen’s compensation schemes were in place, occupational medicine and industrial hygiene experts still played a large role. As had occurred in civil litigation cases, industry

attorneys retained many of them as expert witnesses at the compensation proceedings. For the period of the 1930s and early 1940s, the official federal and state case reporters were replete with workmen’s compensation cases involving silicosis. Almost all cases involved the testimony of numerous expert witnesses, with the most nationally respected ones normally appearing for the defense. The decisions often turned not on the progressive nature of the disease but rather on the experts’ specific testimony about whether the worker currently had a specific applicable disease, whether the individual was capable of work, and to what extent he could work.\footnote{See for example Pennsylvania Pulverizing Co. v. Butler, Circuit Court of Appeals, Third Circuit, 61 F. 311, 313-314 (At trial Professor Drinker had testified for the Pennsylvania Pulverizing Co.); Fergson & Lange Foundries, Inc., et al., Defendants in Error, vs. The Industrial Commission et al., Supreme Court of Illinois, 380 Ill. 185, 187 (June 11, 1942); North End Foundry Company, v. Industrial Commission, 367-8, (March 5, 1935); George McGhee, v. Geo. S. Mepham and Company, Court of Appeals of Illinois, Fourth District, 279 Ill. App. 115, 124-126, (January 4, 1935); and Coburn v. North American Refractories Co., Court of Appeals of Kentucky, 295 Ky. 566, 571, 577-581, (June 25, 1943).}

The definitions, restrictions, and limitations on silicosis that became generally accepted by occupational medical practitioners during the 1930s greatly assisted both defense attorneys and lobbyists, making plaintiff decisions much less likely. These changes provided the underlying basis for industry’s embracement of workmen’s compensation during the 1930s. The changes, including minimizing the history of the disease, defining silicosis in terms of its specific and unitary effect on the ability to work, and requiring greater quantification of exposure before diagnosis, were in large part successful. None of these changes came about as the result of new research findings, but all were necessary to ensure workmen’s compensation claims would not become overwhelming to industry or insurance companies. With these changes, silicosis no longer attracted the attention of the media and faded from the public view. The possibility of silicosis linked lung cancer, although reported in technical medical journals, never became a media story.\footnote{Sayers and Gardner noted in their 1943 yearly report on pneumoconiosis that although silicosis compensation claims costs were continuing to rise, “the silicosis problem is not now attracting the attention it received a few years ago.” R. R. Sayers, et al., “Pneumoconiosis: Industrial Hygiene Section,” American Journal of Public Health 33 (July 1943): 849. In fact, in the state of Montana compensation benefits for silicosis almost doubled between 1941 and 1947; see “Silicosis Costs Rise in Montana,” Industrial Hygiene Newsletter 7 (November, 1947), 3. That same year the medical director of Monsanto Chemical Company noted that “workers who have been employed in atmospheres well under the maximum allowable}
A 1950 U.S. Bureau of Mines report identified what it claimed caused the lack of information and interest. According to the account, the public and worker perception of safety came from an “apparently concerted effort to hide the facts regarding dust diseases in industry.” The authors could not find any justification for a specific safe exposure threshold limit in the literature. They declared, “the attempt to embody in laws and regulations having the force of law rigid standards as to air dustiness would seem to be a travesty of justice.” They concluded that silicosis was “a widespread industrial hazard which is probably increasing.” The report recommended as a “remedy” the “education of workers in the necessity of taking available precautions, including physical exams.

Despite this scathing indictment on the industry, nothing happened. Even as late as 1959, academicians could write books about the foundry business and sand casting that ignored virtually all safety precautions. For example, in 1959 professors at the Massachusetts Institute of Technology published a book describing foundry engineering. Throughout the book, they explained the importance of sand to foundry operations. The chapter on sand casting has pictures of eight individuals working with various casting processes. None of the individuals is shown wearing a mask.\(^{143}\)

Even in the new millennium, businesses safety precautions for silica remained lax. In 2001 OSHA reported that more than 30% of silica samples OSHA collected between 1982 and 1992 exceeded the exposure standard.\(^{144}\) Yet, some progress has been made. As individuals exposed to

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silica declined steadily since 1970, deaths from severe silicosis have similarly declined, from approximately 1,157 in 1968 to 187 in 1999.\textsuperscript{145}

CHAPTER 5
THE TOBACCO STORY: MANUFACTURED SCIENCE AT ITS FINEST

Introduction

Today, almost everyone recognizes the substantial health risks associated with smoking, yet for almost fifty years the tobacco industry successfully created doubt about smoking's harmful consequences. Tobacco’s downfall came around the turn of the 20th century, over the course of little more than a decade. Describing the smoking reduction as a downfall may be a misnomer; since no major tobacco company has gone bankrupt, and even today most major U.S. tobacco companies are widely structured conglomerates, with healthy worldwide profits. Although smoking has declined, the fall has been a gentle one, with smoking percentages declining an average of approximately 0.5% per year since 1965. As late as 2014, roughly 18.1% of all American adults (aged 18 or older) still smoked.¹

Rather than downfall, perhaps the change is better characterized as a revelation, leading to an alteration in culture and lessened acceptance of the practice. Today, we understand that tobacco is addictive and causes emphysema, heart disease and lung cancer. Antismoking education programs abound, many grudgingly funded by the tobacco industry, while advertising in the United States has ceased. This turnaround is due, in large part, to a few courageous individuals, their testimony, and the documents they provided or which where obtained, at least in part, due to their efforts.

The reversal came at the start of a third wave of tobacco smoking product liability litigation against tobacco companies. The first wave began in the mid 1950s, following British epidemiologist

Sir Richard Doll’s seminal work on smoking and lung cancer, and lasted almost two decades. The second wave did not begin until almost the early 1980s, when public concern over health concerns from environmental pollution reached a peak. It included the landmark case *Cipollone v. Liggett Group Inc.*, the first jury verdict against a tobacco company. In the end, neither of the waves resulted in plaintiffs’ receiving any compensation from tobacco companies.

The final wave, which began in 1994, was different. Both immediately before and during this litigation, disclosures of internal tobacco company documents threw light on tobacco companies’ strategy and accompanying tactics designed to circumvent liability. The strategy centered on creating doubt about tobacco smoking health risks, while the tactics included minimizing damaging statements and documents being made by company officials or retained experts, managing research with preordained results, avoiding discovery of adverse company research and management documents, and casting doubt on what they called “anti-smoking propaganda.”

Back in 1992, Federal District Court Judge H. Lee Sarokin had set the stage for the third wave of litigation with his scathing tobacco smoking case opinion. In his ruling, Judge Sarokin excoriated the tobacco industry and its attorneys for claiming certain Center for Tobacco Research (CTR) documents need not be disclosed due to attorney-client privileges. He suggested attorneys had withheld the documents because they did not support industry’s position: “As the following facts disclose, despite some rising pretenders, the tobacco industry may be the king of concealment and disinformation.” Industry then appealed to a federal appeals court, which removed the judge from the case because of an appearance of bias. Long-time tobacco attorney David K. Hardy—an individual about whom readers will hear much more—told *Business Week* that the judge was

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mistaken, since the CTR encouraged researchers to publish. Yet, jurists and plaintiff attorneys took notice of Judge Sarokin’s outrage. They recognized that tobacco industry internal documents might open the vaults of the tobacco companies to someone other than its own attorneys, if only the documents could be accessed. They received further encouragement when, that same year, the Supreme Court provided a new impetus to smoking cases in a decision arising out of the Cipollone case. The justices ruled that although the 1960s warning labels would prevent “failure to warn” liability, plaintiffs could still sue for intentional misrepresentation and the concealment of health hazards.\(^3\)

The story now became one of the chicken-versus-the-egg, as whistle blowers and litigation combined to reveal increasing numbers of important documents. Merrill Williams, the initial and most crucial whistle-blower, first attempted to release tobacco documents in 1990. A former drama professor, Williams began working for Wyatt, Tarrant & Combs as a paralegal in 1988, remaining there until laid off in 1992. The law firm had retained Williams to review, classify, and file internal tobacco documents related to liability actions. Shocked with what he read, Williams began secreting documents in his pants as he left the office. He then photocopied and returned the stolen pages. Over the course of three years he accumulated more than 4,000 pages. The neophyte whistle-blower initially provided the papers to *Washington Post* reporter Morton Mintz. However, after reviewing the material and discussing the potential criminal liability with an attorney, the reporter decided not to write an article about them. After Mintz returned the articles, Williams kept them at home until after he underwent quintuple bypass surgery in 1993. Following this near-death (and costly) experience,

Williams approached his former employer, offering to return the documents in return for compensation for his smoking health claims. Instead, Wyatt, Tarrant, and Combs attorneys filed an action accusing him of breach of confidentiality, and convinced a Kentucky judge to prohibit disclosure of the documents. Williams next turned to Mississippi liability lawyer Don Barrett, who put him in touch with his politically connected legal colleague, Richard (Dickie) Scruggs. After a series of meetings, Williams agreed to turn the documents over to Scruggs, who at the time was negotiating with the state attorney general to file a state government action against the tobacco industry for tobacco-related state health expenditures. Scruggs immediately had the documents flown from Florida to Mississippi, and then provided copies to Congressman Henry Waxman. Once in Waxman’s hands, congressional privileges protected the documents from subpoena and state court orders.

Although the exact trajectory is unclear, the documents soon found their way to New York Times and Business Week reporters as well as anti-smoking activist Stanton Glantz, a professor at the University of California at San Francisco’s medical school. They arrived at Glantz’s office in a big FedEx box with a return address of “Mr. Butts,” the Doonesbury cartoon character famous for talking kids into smoking. One year later, Glantz and colleagues published five peer-reviewed articles in JAMA, using the 4,000 pages to provide the first detailed look inside the halls of tobacco. They also posted the material to the internet; a move that the California Supreme Court subsequently declared made the documents subject to protections under the U.S. Constitution’s First Amendment—the Right to Free Speech.4

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Since this first disclosure, numerous state lawsuits have forced disclosure of documents totaling millions of pages, with the largest cache coming in *State [of Minnesota] ex rel. Humphrey v Philip Morris, Inc.* In this case the judge appointed a special master to review tobacco company claims of attorney-client privilege. The master found that “one method by which attorneys may have controlled research is through maneuvers intended to ‘create’ privileges.” As a result, the judge ruled the attorney-client privilege not applicable, forcing the tobacco companies to produce 39,000 documents for which companies claimed the privilege.\(^5\)

As these state lawsuits began, the Food and Drug Administration (FDA) intensified its investigation into nicotine’s effects on the nervous system. A whistle blower located in this investigation, Jeffrey Wigand, provided crucial aid in the investigation. From 1989 until 1993, when he was fired, Wigand held the position of vice president for research at Brown & Williamson. Originally hired to work on the development of a safer cigarette, his position became redundant when the project was dropped in 1993. Within a year of his dismissal, Wigand began hesitatingly releasing information to the media and then FDA investigators. When he saw tobacco executives testify before Waxman’s committee that nicotine was not addictive—including Tommy Sandefur, his ex-boss, now CEO of Brown & Williamson—Wigand agreed to meet with FDA head David Kessler. Through his help, by the fall of 1994 FDA had obtained thousands of pages documenting cigarettes as drug delivery devices.

Wigand’s testimony also jump-started the Justice Department’s investigation. He soon became one of the government’s most important sources, ultimately assisting the federal government in its suit against the tobacco industry and forcing the release of additional documents. His importance was underscored by a ruling by federal judge in Washington, D.C. one month after

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his deposition. Reacting to attempts by Brown & Williamson to question Wigand as part of their lawsuit against him, the judge put a protective shield around him for the next six months as the DOJ proceeded to debrief him.6

The documents released to date represent a historian’s archaeological goldmine—in excess of 14 million documents and 80 million pages—readily accessible in the UCSF digital repository, http://legacy.library.ucsf.edu; available to anyone at the touch of a key. An excess of 300,000 books would be required to contain the pages, more than the number held by many libraries. Researchers are still exploring them: for instance, Stanton Glantz’s group has conducted a thorough search of one portion of the documents, the Brown and Williamson papers. Along with other researchers, they have also expanded the search into the entire range of produced documents. In addition, various plaintiff attorney firms have added documents from their discovery and research to the library.

Since tobacco research is not the primary focus of my investigation and substantial work has already uncovered significant evidence documenting attorney activities, I have not attempted to systematically mine the archive. However, even my limited searches over the course of two months revealed numerous additional documents demonstrating a deep and abiding control by attorneys over much of the tobacco companies’ medical research.7

The story told by the documents is both shocking to the uninitiated—the non-lawyer—and compelling. As Federal District Court Judge Gladys Kessler announced in her August 17, 2006 decision in the Department of Justice (DOJ) tobacco case, the major US tobacco companies engaged in a “fifty-year history of deceiving smokers, potential smokers, and the American public

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about the hazards of smoking and second hand smoke, and the addictiveness of nicotine.” Judge Kessler specifically noted tobacco attorney misconduct, writing: “at every stage, lawyers played an absolutely central role in . . . the implementation of [the tobacco industry’s] fraudulent schemes.”

Among other misdeeds, Judge Kessler determined that lawyers “directed scientists as to what research they should and should not undertake;” “vetted scientific research papers and reports as well as public relations materials to ensure that the interests of the Enterprise would be protected;” funded “friendly” scientific researchers; and “devised and carried out document destruction policies and took shelter behind baseless assertions of the attorney client privilege.” Nor had the conduct been limited to historic events. Kessler further noted that one attorney before her had “grossly misrepresented” himself in the interest of representing a company “aligned with the Defendants.” In a moving summation, Judge Kessler declared: “[w]hat a sad and disquieting chapter in the history of an honorable and often courageous profession.”

Perhaps the most important legacy archive document is one that was written by tobacco outside counsel at Jones Day, Reavis & Pogue (Jones Day). Originally a small Cleveland law firm, by the 1980s it had a national presence, with a large and growing Washington Office. In 1998 the law firm had prepared a report analyzing plaintiff theories relating to “corporate misconduct” and the evidence available to support those theories. A complete copy of the 449-page draft report is located in this archive. The authors intended it to be a worst-case scenario. “The Corporate Activity Team, therefore, made no attempt to be diplomatic or to generously characterize various issues.” The memorandum also articulated some responses, or lack thereof, to plaintiff themes. Much of the project narrative provides Jones Day’s assessment of what the evidence could show at trial if a plaintiff counsel used all of the internal documents known to Jones Day—and it did not have access

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to all of the documents. The importance of the analysis arises from the stark nature of the report, including realistic impressions of the evidence and the availability of responses to plaintiff evidence, or the lack thereof. Although the evaluation uses only a small portion of the documents now available in the archive, it presents a very troublesome portrait of both the tobacco industry and its attorneys.9

According to Business Week, what emerged from the released internal documents “is a picture of a group so powerful that even high-level executives felt unable to challenge it.” This group included not only the companies’ general counsels and attorney staff, but also outside retained law firms. And not just any law firms. The law firms hired by tobacco companies include many of the most respected names in the legal world: Washington heavyweights Covington & Burling and Arnold, Fortas & Porter represented Philip Morris, while R. J. Reynolds retained Williams and Connolly. Other distinguished firms working for tobacco companies included Chadbourne & Parke and King & Spalding, both of whom represented Brown & Williamson Tobacco Co. High-powered New York firm Debevoise & Plimpton advised the Council for Tobacco Research (CTR), a tobacco-controlled research organization. Other prominent firms included Jones Day, Reavis &

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9 Paul G. Crist, William E. Marple, Stephen J. Kaczynski, and Thomas L. Abrams, “Jones, Day, Reavis, & Pogue, Draft Report,” undated, online at http://legacy.library.ucsf.edu/tid/nek96b00/pdf (accessed July 8, 2014 and in author’s possession), 1-3 (Jones Day Report). Jones Day prepared the draft “Corporate Activity Project” at the request of the tobacco industry for the purpose of analyzing “plaintiffs theories . . . pertaining to ‘corporate misconduct’ issues and the evidence which plaintiffs’ counsel can be expected to present in support of those theories.” As the Jones Day Report described, in private tobacco company employees repeatedly admitted that they “failed to investigate the allegation that cigarette smoking adversely affects health.” Even the research conducted did not serve to investigate the allegations of cancer. Rather, “this research tended either to support the contention that cigarette smoking causes adverse health consequences, or at least not to erode that contention.” Jones Day Report, 16-17. Attorney controls thus both limited the potentially dangerous research and kept any adverse results from becoming public. Throughout this chapter I will repeatedly quote and cite to the Jones Day Report, both to catalogue the extent of attorney involvement and to emphasize what the industry—and particularly its attorneys—thought when they believed it safe to speak with complete candor. Like the Jones Day Report, many of the documents cited in this chapter come from the digital legacy tobacco archives established online by the University of California at San Francisco. The archive is located at http://legacy.library.ucsf.edu. In some cases I have downloaded the documents and in others simply reviewed them online. For ease of reading and consistency I will reference the archive only by the individual document’s locator on the website—in this case (nek96b00)—if I also possess the document. Otherwise, I will provide the document locator and indicate the date on which I accessed it.
Pogue, Shook, Hardy & Bacon—now a large Midwest law firm that was constructed on tobacco money—and Kirkland & Ellis. The firms hired top-gun litigators and former senior governmental lawyers, including FDA attorneys such as Arthur Levine, Donald Beers, Gordon Smith, Gene Pfeiffer, and former FDA chief counsel Richard Cooper.  

Yet, even with this now-clear knowledge of attorney involvement, including substantial media coverage, the full story of lawyer activities and control has yet to be told. For example, in his expose, “A Question of Intent,” former FDA Commissioner David Kessler explained early on how he came to understand that after 1964 the industry “put itself in the hands of its lawyers.” Yet strikingly, attorneys do not have a large role in his narrative. Nor are they substantial figures in other well-researched books on the issue. In addition, while several articles had examined specific events and activities of tobacco attorneys throughout the mid to late twentieth century, none have followed their story from beginning to end. Thus, this chapter presents an old, yet not fully told story. The narrative comes from a wealth of primary documents rarely available to historians—documents usually kept secret because of attorney involvement in their production. In addition, it provides a substantive framework for examining other instances of attorney manipulation and manufacture of science.

When companies turned to in-house and retained attorneys for protection against lawsuits in the early 1950s, the legal community was ready. Over the next half-century they maintained an unparalleled success rate in litigation, for much of that time, 100%. In their zeal to defend tobacco clients, legal counsel used virtually every available means to win lawsuits and curtail or postpone

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11 David Kessler, A Question of Intent, 203-5. See also Richard Kluger, Ashes to Ashes; and Stanton A. Glantz, et al., The Cigarette Papers. The production of the attorney documents is itself a fascinating account that will be described in greater detail later in this chapter.
regulation. They and their clients had one overriding strategy, the creation of controversy, doubt about the accuracy of the accumulating medical science concerning smoking and health.\(^\text{12}\)

Although simple in concept, this strategy became complex—and costly—in execution. Numerous law firms and lawyers became wealthy in defense of tobacco. Implementation required close attention to all aspects of medical science relating to tobacco, extending even to marketing, public relations, and economic analysis. For this, a select group of counsel created extensive networks of pliant experts. Some of the experts were true believers, while others were in it for the money. Along with the networks, law firms funded and often managed “independent” research institutes and associations. The army of consultants—using attorney managed research—testified extensively before federal and state agencies, as well as legislative bodies. However, their first priority remained creating doubt through research and criticism.

Throughout the last half of the twentieth century, attorneys assumed increasing control over relevant tobacco health research. They pursued any conceivable research conclusion that might raise doubts about the health consequences of tobacco. At the same time they steered researchers away from dangerous issues, such as the addictive properties of nicotine. They rewarded consultants for providing the intended results and punished those who attempted to publish adverse results. Most importantly, they participated directly and intimately in the scientific research process. At times, they participated in the planning process of protocol development, ensuring the methodology would likely provide the desired result. They then closely monitored the experimental process, ready to close down research at the first sign of improper data or analysis (conflicting with their litigation positions), even when millions of dollars had been invested. They often participated in analysis of the resulting data, frequently closely editing the publication drafts. In short, tobacco counsel manufactured whatever science lawsuits and regulatory defense required. In accomplishing this, they

\(^{12}\) Naomi Oreskes, et al., *Merchants of Doubt.*
expanded upon the early strategies of silica and asbestos, creating a foundation for the activities of attorneys and litigation scientific experts in the late twentieth and early twenty-first centuries. In effect, they created a prototype that a number of attorneys and experts in other fields are currently fine-tuning.

Tobacco legal representatives also frequently took extreme defensive actions against adverse research. These actions included letters to the editor—correspondents received up to $10,000 for each letter to the editor edited by an attorney before submission—as well as research designed to raise questions about adverse findings. Legal consultants also lambasted opposing medical articles—and often the researchers—at conferences and to the media. Finally, attorneys sponsored articles attempting to limit the generic credibility of findings linking tobacco smoke to disease. These included criticisms about the politicization of science and recommendations to put greater limits on scientific methodology, the proposals being calculated to reduce the potential for significant risks being found in epidemiological or other medical studies.

Despite the best efforts of these lawyers, on occasion company scientists or consultants obtained results or expressed opinions that reflected adversely on the policy of doubt. Tobacco company counsel and their retained legal representatives sought to limit this damage by keeping these documents from the public eye. Their tactics in this area included three general policies. The first and simplest involved lawyer-designed document retention policies limited to the bare minimum required by law. The second used the court sanctioned attorney-client privilege and the attorney-work product rule, both of which allowed for withholding documents from the opposing

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Company lawyers and their outside counsel expanded use of these privileges to the fullest extent possible—and beyond. This policy substantially increased attorney participation in science. In some cases all research documents from sister companies went through attorneys before being provided to the relevant scientists. Finally, lawyers also participated in numerous scientific research conferences and decision-making to ensure attorney privileges protected the discussions and work. At times they overruled senior researcher decisions. They even trained new and old scientists in the appropriate scientific opinions concerning tobacco smoke. In short, they kept any unhelpful science hidden through court allowed secrecy practices. This chapter analyses these practices in detail.

**Tobacco Attorneys Gain control of Research**

*Tobacco Lawyers in the 1950s-60s: A fast learning curve*

Tobacco’s health troubles requiring attorney control began in the early 1950s. As the new decade commenced, along with the Korean War, the future of the tobacco industry never seemed brighter. A generation of American men and many women had learned to smoke during the period spanning the buildup to World War II and the beginning of the Korean War. During both wars servicemen and women had access to free or inexpensive cigarettes. Cigarettes were cool. They seemed to calm the nerves and give a kick of energy during times of stress and long stretches of boredom. Virtually every star smoked on the silver screen. The percentage of American smoking had never been higher and seemed to still be climbing. By 1951 consumers in many countries spent 3 to 5 per cent of their total income on cigarettes. However, the tobacco industry euphoria would not last. By the end of 1953, the forecast clouded considerably.

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14 The attorney-Clinet privilege allows a defendant to withhold information that involves advice and activities of its attorneys directed at litigation. The attorney-work product rule similarly allows/requires lawyers to hold in confidence any work for a client in anticipation of or during litigation.
The first jolt occurred even before the start of the Korean War. The May 1950 issue of JAMA contained the first major study linking smoking and lung cancer. Another article in the same issue found that 96.5% of lung cancer victims interviewed by the authors had smoked at least moderately. Four months later, British doctors Richard Doll and Bradford Hill—both later knighted for their medical research accomplishments—reported in the British Medical Journal that smokers are fifty times more likely to contract lung cancer as nonsmokers. Fortunately for the tobacco industry, with the start of the Korean War, the popular media had little time for medical research or statistics.

Three years later, circumstances had changed. As the Korean War slowly crawled toward an armistice, Dr. Ernst Wynder, a researcher at the Sloan Kettering Institute, published the first biological report linking smoking and cancer, a tobacco-related production of cancer in a lab animal by painting cigarette tar on the backs of mice. Later that year noted medical practitioner Dr. Alton Ochsner, founder of the nationally recognized and revolutionary Ochsner clinic in Louisiana—and recognized by many as the first anti-smoking crusader—gave a speech in New York declaring “the male population of the United States would be decimated if cigarette smoking increases as it has in the past unless some steps are taken to remove the cancer-producing factor from cigarettes.” The next day tobacco company stocks dropped dramatically. Six days later, in response to an urgent telegram by American Tobacco President Paul Hahn, tobacco executives met in New York. The following day John Hill, founding partner of the New York public relations firm Hill & Knowlton, joined the meeting. Together, they mapped out tobacco’s response to the medical reports.15

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Even at this early point in tobacco’s defense, attorneys played an unexpected role. At the meeting, Hill & Knowlton was told that Tommy Ross, counsel for American Tobacco Company, was near completion of a "white paper" on the scientific facts involved in the health issue. It is not clear why an attorney had to prepare a report for distribution to the media when the industry already had a scientific assessment of the medical literature. The prior February, Dr. Claude Teague of RJ Reynolds had delivered a survey of the tobacco medical literature to company officials. Perhaps Teague’s assessment did not sufficiently distance itself from Wynder, Doll, or Ochser. In later testimony Teague stated his belief that RJ Reynolds’ general counsel tried to have the report destroyed. The attempted destruction may have occurred because the report concluded:

Studies of clinical data tend to confirm the relationship between heavy and prolonged tobacco smoking and the incidence of cancer of the lung. Extensive though inconclusive testing of tobacco substances on animals indicates the probable presence of carcinogenic agents in those substances. One worker has identified known carcinogens in a tobacco pyrolymate. Compounds are present in tobacco which on pyrolysis could theoretically give rise to compounds similar to known carcinogens.16

In the end, the tobacco industry and Hill & Knowlton decided that their best strategy consisted of a public relations program claiming the issue is still “an Open Question,” which tobacco companies would strenuously attempt to answer. They announced the "Open Question" position in “A Frank Statement to Cigarette Smokers” advertisement in January 1954. Its four main elements, which evolved throughout the 1950s, were:

a. It has not been scientifically established that smoking is a cause of disease, particularly lung cancer.
b. The solution lies in more research to which the industry is committed.
c. Scientists have been unable to establish any ingredient as found in cigarette smoke which has produced lung cancer in animals or human beings.

d. The industry believes that cigarettes are not injurious to health.

This mantra remained at the heart of the industry defense even into the late 1990s.  

As part of the stated “research” to which the companies committed themselves, five of the six largest companies formed the Tobacco Industry Research Committee (TIRC), with a stated agenda of “funding independent research into smoking and health issues.” Concerned that the public might question the organization’s credibility, industry also established an “independent” Scientific Advisory Board (SAB) of eminent scientists to screen research applications and recommend funding for third party research. In actuality, as the Jones Day Report points out, “TIRC’s mission was more aptly described in 1954 by the first SAB Chairman, Dr. Clarence Cook Little: ‘[T]o build a foundation of research sufficiently strong to arrest continuing or future attacks’ on tobacco.” In fact, one of its principle activities in the 50s and beyond was as a tobacco industry public relations vehicle in interviews and testimony in litigation and before agencies and the Congress.  

One further event in 1954 provided a keystone of attorneys’ growing power. In 1954 Philip Morris hired David Hardy, of Shook, Hardy & Bacon law firm, (Shook Hardy) to defend a Kansas City smoker’s suit that was part of the first wave of litigation. At the time, Shook Hardy was a relatively small Midwest law firm. From this humble beginning, Hardy and his law firm developed the closest and most extensive outside counsel ties to the tobacco industry. Extremely successful in every lawsuit they would handle, at various times its attorneys represented four of the six major

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17 Jones, Day, Reavis, & Pogue, Draft: Corporate Activity Project Part 1 extract of 74 pages, July 15, 1998, 8 (oju87h00). A complete copy of the 449 page draft report is located at (nek96b00). Jones Day prepared the draft “Corporate Activity Project” at the request of the tobacco industry for the purpose of analyzing “plaintiffs theories . . . pertaining to ‘corporate misconduct’ issues and the evidence which plaintiffs’ counsel can be expected to present in support of those theories.” The authors intended it to be a worse case scenario, thus, they inform the readers that “[t]he Corporate Activity Team, therefore, made no attempt to be diplomatic or to generously characterize various issues.” The memorandum also articulated some responses, or lack thereof, to plaintiff themes. Much of the project narrative provides Jones Day’s assessment of what the evidence could show at trial if a plaintiff counsel used all of the internal documents.

18 Jones, Day, Draft: Corporate Activity, 111 (nek96b00).
tobacco companies. The help went both ways. Tobacco money funded the firm’s dramatic growth and breadth of service. In 1994 Shook Hardy employed not only 203 attorneys, but also more than 100 analysts, primarily PhDs and other experts.\(^\text{19}\)

As the decade progressed, unwelcome medical research results—both external and company—and the first wave of litigation placed growing stress on the industry with resulting increased reliance on public relations activities and concomitant greater legal involvement. 1958 was another pivotal year, with disappointing industry sponsored research findings resulting in further development of the “fortress” tobacco industry culture. That year a CTR sponsored study failed to repudiate Wynder’s mouse study, finding “Cigarette smoke condensate is a weak mouse skin carcinogen.” Industry easily buried this result, as well as other internal reports of the carcinogenic properties of tobacco constituents. We may never know the exact number of these reports, since the 1950s also bore witness to the first of many instances of attorney-sponsored or recommended tobacco-related document destruction, in this case by R. J. Reynolds’ general counsel, Henry H. Ramm. In the late 1950s he escorted an employee to the research and development library archives and ordered him to destroy all records concerning a carcinogenic product of tobacco burning—benzopyrene.\(^\text{20}\)

More importantly for the immediate future of tobacco profits, in January 1958 twelve companies—including most of the largest—formed the Tobacco Institute (TI). The press release announcing its formations gave its principal purpose as “providing a better understanding of the tobacco industry and its place in the national economy and to compile and disseminate information relating to the industry and to the use of tobacco products.”


Initially the Tobacco Institute’s primary advisor was Hill & Knowlton. Eventually industry attorneys exercised almost full control. The underpinnings of this attorney takeover occurred at the very first Tobacco Institute board meeting, held on January 30, 1958. In addition to the directors, seven company general counsels attended. Either there or shortly thereafter, general counsel of the six major tobacco companies decided to meet regularly. Until the formal adoption of the name Committee of Counsel, the group had several informal names, including Committee of Six, the Secret Six, and the Committee of General Counsel. For much of its existence, the group met every few weeks.\(^{21}\)

That fall, another stalwart of the outside tobacco counsel group, Edwin Jacob, a named partner in New York law firm, Jacob, Medinger, Finnegan & Hart—an outside counsel to RJ Reynolds since at least 1953—deepened his involvement in tobacco’s public relations defense. In November he provided Addison Yeaman, General Counsel of Brown & Williamson, with the draft of “Another Frank Statement to Smokers.” For the most part, this statement reiterated and emphasized Tobacco’s prior 1954 advertisement. In a larger sense, however, it demonstrated attorneys deepening involvement in larger areas than simple legal advice, in this case public relations.\(^{22}\)

Medical news for tobacco did not improve during the 1960s. In 1961 a TIRC-funded study of autopsies reported that a history of smoking was positively correlated to carcinoma. In 1963 TIRC-underwritten researchers linked chronic smoking to early onset of coronary artery disease and higher rates of coronary occlusion. Another industry-supported 1965 study reported higher risks of premature birth and lower birth weight among children born of smokers. Although summarized in

\(^{21}\)The Tobacco Institute, Press Release, “Ex-Congressman Richards to Head Newly-Formed Tobacco Institute, Inc., January 30, 1958 (noq36b00) (accessed August 20, 2014); Horace G. Hitchcock, The Tobacco Institute, Inc., Minutes of the First Meeting of the Board of Directors, January 30, 1958 (strn03f00) (accessed August 20, 2014); and Mike France, “Inside Big Tobacco’s.”

\(^{22}\)Edwin J. Jacobs to Addison Yeaman, Esq., November 10, 1958, with attachment (qsx95a00).
the annual report, TI never emphasized or published these and similar studies. The bad news was not limited to external research groups. At an industry-sponsored meeting in 1969, R. J. Reynolds researcher Dr. Price mentioned in conversation that he had induced emphysema in rats during chronic cigarette smoke exposure studies.\(^{23}\)

Although the tobacco industry was able to keep most of this information from the public eye, they could not control government and independently supported research and analysis. In 1962 and 1964 respectively the British and American governments reported on the accumulating research. The American report came via the 1964 Surgeon General’s report on Tobacco. Work on the report began in November 1962 when the appointed Advisory Committee held its first meeting. At the time, Assistant Surgeon General James M. Hundley listed the resources available to the committee during its analysis. In addition to the Public Health Service, the National Library of Medicine, and public health organizations, the tobacco firms and their agents had offered to provide papers, research, and data. In March 1963 Hundley officially asked TI for research (‘actual data, summaries of data or literature citations’) that had been conducted by the tobacco companies or industrial research groups. Most of the companies submitted research, primarily citations of published work along with some unpublished papers; the TIRC provided similar types of information, but at a much greater volume (‘mountains of material’) to both the PHS and individual Committee members. Although there is little information available about the extent of attorney document review, the companies, TI, and TIRC kept their attorneys informed of the activities.\(^{24}\)

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24 U. S. Department of Health, Education, and Welfare, “Press Release,” October 28, 1962 (xlt84f00) (accessed September 25, 2014); James M. Hundley to George V. Allen (The Tobacco Institute), March 12, 1963 (request for information) (fmj68e00); Peter V. V. Hamill, “TIRC Summary,” June 28, 1963 (mountains of material) (jud66b00); Robert C. Hockett to William G. Cochran, et al., January 21, 1963 (enclosing a paper) (tg6aa00); Clarence C. Little to Stanhope Bayne-Jones, June 12, 1963 (enclosing a paper) (plq96c00); Stanley L. Temko memorandum to Janet C. Brown, et al., April 9, 1963 (Attorneys kept informed); and Robert C. Hockett to Pater V. V. Hamill, March 7, 1963 (CC. with enclosure) to Allan, Brown, Hotman, and Jacob) (ehn36b00).
There is also some evidence that attorneys participated in selecting information to send the advisory committee. At a minimum, the withheld information included the British American Tobacco’s (BAT) nicotine research, the Ecusta Corporation’s American tobacco-financed condensate and whole smoke experiments, smoke carcinogens, and RJ Reynolds’ Claude Teague’s “Survey of Cancer Research”—all documents that tobacco attorneys had no desire to see put into evidence at trial. The one known involvement of counsel came from Brown & Williamson’s General Counsel. He recommended withholding B&W commissioned research conducted by the Battelle Institute. The research concerned nicotine’s sedative and addictive effects as compared to tranquilizing agents.

Hoyt [TIRC research Executive Director] ... agreed to withhold disclosure [of the] Battelle Report to TIRC members or SAB until further notice from me. Finch [executive at Brown & Williamson] agrees submission Battelle or Griffith developments to Surgeon General undesirable and we agree continuance of Battelle work useful but disturbed at its implications re cardiovascular disorders.25

The Advisory Committee published its report the following year. Notwithstanding the “mountains of material” suggesting doubt provided by the tobacco industry, the committee members found a clear causal connection between cigarette smoking and lung cancer: “Cigarette smoking is causally related to lung cancer in men . . . The risk of developing lung cancer increases with duration of smoking and the number of cigarettes smoked per day.) They also suggested that the potential link between smoking and coronary disease is sufficiently strong as to warrant a warning: “It is also more prudent to assume that the established association between cigarette

smoking and coronary disease has causative meaning than to suspend judgment until no uncertainty
remains.”

As briefly mentioned above, the tobacco industry did not require the Surgeon General to inform
them about the hazards of smoking. Numerous tobacco company studies from the period
demonstrate the disease-making capability of tobacco, while internal tobacco documents
demonstrate that executives and attorneys at the companies were, at the time, fully aware of the
studies’ implications. As the Jones Day Project report noted:

Plaintiffs will argue that even if the tobacco companies believed in the
“open question” position in 1954, they have known it to be untrue for
almost 25 years. For example, on October 16, 1962, the President of Brown
& Williamson wrote to the President of Reynolds:
“Let me make my position perfectly clear. If we were able to make strong,
affirmative, well documented statements which might tend to convince the
public that the charges against our industry are invalid and unsupportable, I
would subscribe wholeheartedly to a series of paid advertisements in which
we could tell our story. But since we cannot take such a position I think it
far better to continue to let the public make its own judgments which since
1953 it has done with results not wholly unfavorable to the tobacco
industry.

The Chairman of the Board of Lorillard agreed with the above
quotation and added:
“I believe that paid advertisements which would satisfy all of us,
including our respective legal counsel and litigation counsel (and which
would still remain firm and positive rather than negative or defensive)
would be almost impossible to arrive at.”

About the same time, a document drafted for TI indicates even
'among friends' many believe the industry had no "adequate defense." Also
in 1962, some members of the SAB of the TIRC had 'mixed feelings about
the industry's' (the quote ends here with no explanation. It was likely cut off
in the original through oversight.)

RJ Reynolds’ early knowledge of tobacco smoke carcinogenicity did not stand in solitude. It was just
one of many pieces of undisclosed information cataloged in the Jones Day Project.

27 Jones Day Report, 51- 52 (nek96b00).
Drs. Alan Rodgman and Lawrence C. Cook carried out extensive in-house studies of the composition of tobacco smoke. They confirmed the presence of BAP [a potent carcinogen] in cigarette smoke as early as 1956. In addition, according to an internal 1959 Reynolds memorandum: “Some thirty-odd polycyclic hydrocarbons have since been similarly characterized in these laboratories. Of these, eight are carcinogenic to mouse epidermis. Cholanthrene, a potent carcinogen, is one of these not yet reported by other investigators. In April of 1959, the first positive isolation and identification of 3,4-benzpyrene, citing data similar to ours, was reported by other investigators.”

Thus, Reynolds identified BAP in cigarette smoke three years before the first published confirmation of its presence, and, as of 1959, had detected “a potent carcinogen.”

One year before publication of the Surgeon General’s 1963 momentous report on smoking, the General Counsel of Brown & Williamson, Addison Yeaman, wrote a remarkably forthright letter about the tobacco industry’s conduct, recommending a change in course for the tobacco industry: “We are, then, in the business of selling nicotine, an addictive drug effective in the release of stress mechanisms.” He next forecast confirmation of other smoking related diseases: “Cardiovascular disorders will in all probability, also be found related to smoking.”

Yeaman then suggested a radical strategy. Rather than gathering around the wagons, industry should go on the offensive by conducting a legitimate massive scientific research program designed to find and remove the carcinogens in tobacco smoke. He argued that the program must be conducted in cooperation with the Surgeon General, the American Cancer Society, and all other responsible organizations, rather than through TIRC. This is because [TIRC] “was conceived as a public relations gesture and, however undefiled the Scientific Advisory Board and its grants may be, it has functioned as a public relations operation.” Yeaman believed this far-reaching strategy would

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28 Jones Day Report, 51-52 and 97-98 (nek96b00).
not only allow the industry to create a safer cigarette, but in the meantime allow them to continue criticizing any scientific research that suggested a clear causal link with smoking.\textsuperscript{30}

Yeaman’s hopeful recommendation was not to be. In 1964 only two changes occurred at the TIRC, neither of which involved it becoming something more than a public relations ploy. The first adjustment was minor in nature, merely a name modification: it became the Council for Tobacco Research (CTR). The second was more fundamental: attorneys assumed growing control of the research. As one tobacco executive admitted, the Surgeon General’s Report provided the impetus for the increased attorney involvement. In contrast to Yeaman’s hopes, this deepening participation meant not only continued denial, but also ever-increasing deceit. B&\text{W} attorneys, including Yeaman, eventually fell in line with the rest of industry’s attorneys, strongly urging their company not to engage in research that could have damaging results.\textsuperscript{31}

For industry-wide issues, the corporate general counsel Committee of Counsel became all-powerful, at times even rivaling the power of the CEOs. Its assertion of power did not take long. In 1964 two British tobacco executives spent a month studying the U.S. tobacco industry. Their report of the experience concluded that the “Committee [of Counsel] is very powerful, it determines the high policy of the industry on all smoking and health matters . . . and it reports directly to the President.”\textsuperscript{32} Robert Kersey, former head of tobacco research at Liggett, agreed: “Almost everything that transpired had to be done under the advice of counsel so that nothing . . . would incur a potential liability.”\textsuperscript{33}

\textsuperscript{30} Addison Yeaman, “Implications of Battelle Hippo” (xrc72d00). This type of candor continued in a few private letters and memos into the late 1960s. For example, in 1967 J. S. Dowdell of RJ Reynolds acknowledged that "the industry has little, if any, positive evidence" to refute health charges. In 1968 William Kloepfer, at the time senior vice president of the Tobacco Institute, conceded: “Our basic position in the cigarette controversy is subject to the charge, and may be subject to a finding, that we are making false and misleading statements to promote the sale of cigarettes.”\textsuperscript{30} Jones, Day, “Draft: Corporate Activity,” (little, if any at 9) (our basic position at 9-10) (nek96b00)

\textsuperscript{31} Alix M. Freedman, et. al., “Smoke and Mirrors,” A1; and Stanton A. Glantz, et al., \textit{The Cigarette Papers}, 55-56.

\textsuperscript{32} Mike France, “Inside Big Tobacco’s.”

\textsuperscript{33} Alix M. Freedman, et. al., “Smoke and Mirrors,” A1.
Along with the tobacco industry “research” arm’s new name came a more focused strategy—spearheaded by attorneys—of conducting research either of little value in tobacco health research, or designed to assist in the defense of litigation. The CTR—now closely monitored by counsel—funded the former, while the newly created “special projects” category conducted most of the latter. The general fund of the CTR chiefly solicited proposals from outside researchers, while special projects directed toward litigation and regulatory issues often resulted from attorneys deciding they needed particular research and seeking out scientists who might provide it. The tobacco companies funded the attorney-controlled “special projects” on a market share basis administered through the Committee of Counsel. As litigation requirements increased, attorneys also assumed greater control over CTR general research grants and publications. Even the Scientific Advisory Board, supposedly a board of independent scientists, established to peer review proposals, frequently had an attorney overseer. For example, in 1969 attorney Henry H. Ramm, a vice president at R. J. Reynolds, held the position of Chairman, Pro Tem on the Scientific Advisory Board of the CTR.34

The New York law firm Jacob, Medinger, Finnegan & Hart served as counsel to the CTR, with senior partner Edwin Jacob taking a lead role in the Special Projects unit. As manager of “special projects,” long-time tobacco attorney Edwin Jacobs kept a close eye on all funded activities. “Special Projects” functioned both inside and external to CTR. At times he released the results through CTR’s voice to gain credibility, but often the projects were covert. Virtually all such projects were predetermined in accordance with the needs of a particular attorney. One notation read “Clinical studies to be conducted . . . to show that duration and amount of smoking have no relation

to the age of peak incidence of lung cancer.” As a further precaution, by the late sixties experts who took part in “special projects” were assigned a “keeper” lawyer who stage-managed their public appearances.

Roy Morse, former research chief at R. J. Reynolds described the primary benefit of this arrangement: As soon as Mr. Jacob funded a research project, “it was a privileged relationship and it couldn’t come into court.” When necessary, the lawyers also eliminated funding at any point without repercussions.35

Although Jacobs ran the special projects program from New York, David Hardy and his firm also played a central role in research efforts designed to keep open the question about tobacco health concerns. In 1963 Hardy successfully concluded the tobacco case, Ross V. Philip Morris, he had been given back in the early 50s. Philip Morris thought so highly of his tenacious defense that they put him in charge of their nationwide litigation. Almost immediately, Hardy expressed his disappointment with the Council’s research efforts and began looking for his own experts willing to accept industry’s positions and testify counter to the 1964 Surgeon General’s report. The “Special Projects” initiative, which commenced shortly after this, may have been developed as a result of Hardy’s efforts and recommendations.36

By the beginning of 1967, Shook Hardy controlled at least seventy Ad Hoc or “Special” Projects, although many of them at this early date did not involve medical research. Hardy stressed that the research his firm managed be practical and of use in either litigation or interactions with the government. In January of that year he requested that attorneys working on special projects provide their opinions concerning attaining “some practical use out of the various CTR and Ad Hoc projects

35 “Research Notation,” March 7, 1966 (Clinical studies) (kig86b00); David Kessler, A Question of Intent, 203-4; Richard Kluger, Ashes to Ashes, 323; and Alix M. Freedman, et. al., “Smoke and Mirrors,” A1.

in time for the expected [Congressional] hearings.” Several significant early projects focused on countering the mounting number of studies finding severe health problems from smoking.

For example, Hardy supported one set of studies because “[i]t is also expected that the results here may tend to refute some of Auerbach’s work” on Beagles and smoking. At the time, the industry contended that animal experiments involving inhalation of cigarette smoke had consistently failed to cause lung cancer. Auerbach’s work threatened this position: in 1970 he reported having induced squamous cell cancer in beagles exposed to cigarette smoke.\(^{37}\)

As the 1960s progressed, “special projects” demonstrated an increasing sophistication as well as ever-closer attention to results and involvement in most phases of research by the attorneys. The following three examples provide a flavor of lawyer influence on medical research as they manufactured medical evidence.

First, when selecting which scientists to fund, the initial consideration was not the quality of the research, but the opinions of the researcher. The lawyers amply rewarded scientists willing to work with them and provide “friendly” reports. After having an initial contract for $54,000.00 to produce an industry-slanted report, statistician Theodor Sterling received over $2 million from the Committee during the next twenty years. The Committee provided so much funding to scientists like Sterling because, as Veritas—the code name for one of the FDA’s insider sources—stated, they “protected the paradigm.” Among Sterling’s many projects, one stands out for its audacity: his 1967 critique and reanalysis of the groundbreaking “mortality study” of the National Center for Health Statistics (NCHS) on diseases other than lung cancer. After one British-American Tobacco scientist expressed doubt about relying on Sterling’s work, an American colleague replied that he shared the

\(^{37}\) David R. Hardy to Philip Grant, April 21, 1967 (expected that the results) (ueo66b00) (accessed August 14, 2014); and David R. Hardy to Miss Janet Brown, et al., January 12, 1967 (some practical use) (fbw66b00) (accessed August 114, 2014). Jones Day Report, 404-5. The report also informed management that in future lawsuits plaintiff lawyers would contend the industry had mounted a “smear campaign unequalled in American history.” Jones Day Report, 405.
concern about Sterling’s report. However, “we must use every possible method to cast some doubt on the statements and data presented by our opponents.”

Sterling subsequently testified at congressional hearings that the “mortality study” contained “a number of major flaws” in the data and analysis that were “so serious that clear-cut conclusions should not be drawn.” Tobacco-friendly legislators ignored the serious shortcomings in Sterling’s analysis. Following the testimony, a North Carolina Senator announced it had “blown the mortality study right out of the water.”

Second, attorneys did not stop at hand-selecting researchers. They often managed the project every step of the way through required reports and updates. On occasion they even participated in the study design process. Following approval by the research directors of a Hazleton Laboratories project in early September 1969, Covington & Burling attorneys H. Thomas Austern and Allan J. Topol met with representatives of Hazleton and a subcommittee of industry scientists. At the meeting, Austern and Topol sought to add defensive language to the protocol. In seeking the language, Topol lied, telling the Hazlett representatives that tobacco research directors had requested it: “AJT [Topol] stated that the research directors had decided that the objective [statement of the protocol] should include a sentence stating that the relationship of skin painting to smoking and health had not been established.” Austern also made a number of substantive scientific suggestions about the protocol, many related to public relations and litigation concerns. Pursuant to Austern’s request, the grant recipients also agreed to provide the finished report to the nine sponsoring

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tobacco companies for review prior to publication. In addition, they also discussed having a lawyer present at any future meetings between the lab and industry representatives.\textsuperscript{39}

Finally, legal counsel frequently commented on and edited research papers. For example, in 1967 the well-regarded research firm Arthur D. Little conducted research for the tobacco industry on the “epidemiological aspects and significance of the smoking and health problem.” The company then provided the “ruff ruff draft” of the project to Shook Hardy and other law firms while soliciting their “comments on both organization and content.”\textsuperscript{40}

Attorneys did not limit their influence to scientific research. They also became progressively more involved in the marketing of “doubt.” During the 1960s the tobacco industry and Hill & Knowlton provided behind-the-scenes backing for several seemingly independent popular articles downplaying the risks of tobacco. By the later years of the sixties, attorneys were deeply immersed in the process. In 1968 the tobacco industry clandestinely sponsored an article in \textit{True Magazine} arguing the evidence concerning cigarette hazards was contradictory and inconclusive. The byline was from a widely read sports writer, Stanley Frank, who did not reveal that by the time the article was published he had begun to work for Hill and Knowlton. Prior to agreeing to the article the company’s legal department received firm assurances that Frank “has the desired point of view.” Even so, before writing the article Frank provided an outline to Brown & Williamson. Project managers assured the legal department they had sufficient influence with Frank to change the “tone” if necessary. Throughout the project public relations personnel kept B&W’s general counsel, Addison Yeaman, fully advised of its status.\textsuperscript{41}

\textsuperscript{39} Stanton A. Glantz, et al., \textit{The Cigarette Papers}, 213-16; and Allan J. Topol memorandum to Mr. Austern, September 16, 1969 (jzy95a00) (accessed August 14, 2014).
\textsuperscript{40} Charles J. Kensler to David R. Hardy, July 20, 1967 (vgu88d00) (accessed August 14, 2014).
\textsuperscript{41} Stanton A. Glantz, et al., \textit{The Cigarette Papers}, 179-80; and J. V. Blalock confidential memorandum to A. Y. Yeaman, March 28, 1967 (sir03f00) (accessed August 14, 2014).
This marketing of doubt also included other methods of manipulating public opinion. One technique is evident in a survey proposal that suggested wording the questions in such a way to achieve positive responses. In 1964 Arnold, Fortas & Porter attorney Abe Krash informed tobacco company executive attorneys—perhaps at a Committee of Counsel meeting—that experts believed “strong support for the industry position that labeling and warnings in advertisements are not necessary might be developed through a public opinion survey.” The executives further learned that the legal committee had not only already considered the issue in its May 7, 1963 meeting, but had also solicited such a questionnaire from Professor Gary Steiner of the University of Chicago’s Graduate School of Business. Following receipt of a draft, industry attorneys had revised it “with a view toward seeking information . . . without raising extraneous issues.”

In discussing this course of action, Krash recognized the uncomfortable position the industry might be in if the survey turned out unfavorable—a concern raised by Austern. He stressed that this possibility could be dealt with. His solutions involved either the use of attorney privileges to hide the data or the outright destruction of the entire set of records.

We have been assured by both [the proposed survey company] Elrich [sic] & Lavidge and by [industry consultant] Professor Steiner that they would transmit to us every interview and every copy of the analysis. Thus, when it is completed, there will be nothing in the records of Elrich & Lavidge or Professor Steiner to subpoena. The danger of a successful subpoena would be reduced (though not entirely eliminated) if the survey were in an attorney’s files. In any event, if the returns were unfavorable they could be destroyed and there would be no record in any office of the nature of the returns…we have tried to avoid that problem by avoiding unnecessary questions which might elicit answers harmful to us.42

Industry explanations for this proposal have not been forthcoming. In 1998 Arnold & Porter declined numerous requests from Business Week for an interview to explain the memo.

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42 Abe Krash, memorandum to Ramm, et al., undated (We have been assured) (zyn77h00); and Eric M. LeGresley, Monique E. Muggli, and Richard D. Hurt, “Playing Hide-and-Seek with the Tobacco Industry,” Nicotine & Tobacco Research 7, no. 1 (February 2005): 27-40, 33.
Although another industry source averred that the survey was never completed, that is not the issue. The proposal itself documents an attorney’s unreserved offer to both misuse attorney privileges and destroy evidence. Krash was not the only individual in the 1960s to make this suggestion. Other tobacco company records describe an almost eager readiness to systematically remove or destroy information to protect against disclosure in lawsuits. Indeed, an R. J. Reynolds memo straightforwardly advocated this position:

We do not foresee any difficulty in the event a decision is reached to remove certain reports from Research files. Once it becomes clear that such action is necessary for the successful defense of our present and future suits, we will promptly remove all such reports from our files.\footnote{Mike France, “Inside Big Tobacco’s”; Unsigned Memorandum to M. Crohn (Legal Department), December 18, 1969 (we do not foresee) (qzw66b00) (accessed September 23, 2014); and Eric M. LeGresley, et al., “Playing Hide-and-Seek,” 35.}

Such was the extent of attorney involvement, interference, and potential fraud that eventually even executives at the Hill & Knowlton public relations firm—an industry not known for having a sensitive nature—believed matters had gone too far. According to one former chief executive of Hill & Knowlton, the firm quit the tobacco account in the late 1960s due to concern over liability. “The lawyers had this thing under control. For legal reasons [the tobacco industry] felt it couldn’t admit to anything on tobacco and health because then it would be sued out of existence.”\footnote{Alix M. Freedman, et al., “Smoke and Mirrors,” A1.}

**The 1970s, 80s and 90s: Mature Control and Production**

Tobacco industry strategies did not waiver through the closing years of the twentieth century. Attorney involvement in medical research, public relations, regulations, and other activities affecting litigation only deepened and intensified. Even industry CEOs recognized that their legal counsel now ran the show. As Curtis H. Judge, president of Lorillard Tobacco, grumbled in a note he penned at a 1978 Scientific Research Liaison Committee meeting, “we have again ‘abdicated’ the
scientific research directional management of the industry to the ‘lawyers.’” Insider attorneys privately admitted that corporate and consultant research scientists had close relationships with legal departments and outside law firms. In fact, lawyers directed most relevant smoking research. Lawyers screened out risky project proposals, designed "special projects" for litigation purposes, and terminated research that began to identify smoking health problems. Their reach extended far beyond the courtroom, encompassing public relations, preparation of hearing and conference statements, and even managing economic cost analysis.45

The take-over came from a cabal of corporate tobacco general counsel and several closely aligned outside firms and counsel. David Hardy’s memo written in 1970—towards the end of the first wave of litigation—demonstrates both the tenor of attorney influence and the growing power of a few select outside counsel. In a lengthy confidential missive he wrote to the general counsel of B&W, DeBaun Bryant, Hardy provided his general observations and opinion about the state of smoking litigation. He further cautioned that, even without knowing the full extent of B&W and BAT scientists’ damaging statements, he still had “sufficient documentation from you to conclude that the dangers I describe in this letter have a very real foundation.”

The statements to which Hardy alluded had occurred while industry scientists attended the 1969 Kronberg Research Conference. They included factual observations of tobacco experiments and conferences, as well as proposed models of tobacco smoke’s biological activity. Most damaging of all, the conference attendees concluded that “at the present time the Industry had to recognize the possibility of distinct adverse health reactions to smoke aerosol” including lung cancer and emphysema.

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After describing the specific statements causing concern, Hardy then summarized why they were troublesome. The explanation is long, but provides an exceptional view into the inner mind of an exceptional trial attorney. His letter explained how attorneys defended tobacco and why they became involved in wide-ranging issues. Hardy began by assuring B&W’s general counsel that the statements could be “explained” but cautioned that the subtle distinctions he would be forced to urge might not be followed by the jury. He then recounted the tobacco industry’s litigation strategy, explaining why corporate scientist comments suggesting there are tobacco smoke health issues threatened to destroy the foundation of tobacco’s defense. It is worth quoting at length.

As you know, with the testimony of independent and well-informed doctors and scientists, it has been repeatedly demonstrated in court to the satisfaction of impartial jurors that cigarette smoking has not been scientifically proved to cause disease . . . Jurors are, however, aware that a substantial segment of the medical and scientific community has accepted smoking as a cause of disease notwithstanding the deficiencies in the proof. This group includes many well intentioned but inadequately informed doctors and scientists who operate on a philosophy that if smoking may be hazardous to health no further inquiry is necessary. In other words, they are willing to settle for suspicion in lieu of proof in condemning cigarettes . . . Obviously our problem becomes entirely different and far more serious when agents and employees of the defendant cigarette company or its parent become the spokesmen against us.

. . . A plaintiff would be greatly benefited by evidence which tended to establish actual knowledge on the part of the defendant that smoking is generally dangerous to health, that certain ingredients are dangerous and should be removed, or that smoking causes a particular disease. This would not only be evidence that would substantially prove a case against the defendant company for compensatory damages, but could be considered as evidence of willfulness or recklessness sufficient to support a claim for punitive damages. The psychological effect on judge and jury would undoubtedly be devastating to the defendant. To be more specific:

(1) It would certainly be difficult for a defendant to effectively . . . question the work of . . . "anti-cigarette" scientists if such work had been labeled as "valid" by defendant's own people. How . . . would our position that "mouseskin painting" does not provide data which can be extrapolated to humans stand up if [jurors see] the reference to mouse-skin painting "as the

ultimate court of appeal on carcinogenic effects" from page 5 of the Kronberg minutes.2

(2) . . . after one experience of being disputed by statements of our own employees, it is doubtful that . . . experts would agree to testify again.

(3) If . . . causation of a disease by cigarettes seems to be supported by statements and opinions of our own scientific employees, this important issue . . . would undoubtedly be decided against us . . . 47

Hardy concluded his letter by reiterating the standard corporate position of doubt. As an experienced and extremely successful litigation attorney, Hardy chose his words carefully, even in a confidential letter to other attorneys. He maintained the fiction that any statement subject to the interpretation that smoking causes disease must be a mistake:

We of course know that the position of BAT, as well as B&W, is that disease causation by smoking is still very much an open question. Cigarettes have not been proved to cause any human disease. Thus, any statement by responsible and informed employees subject to a contrary interpretation could only result from carelessness.48

Hardy’s letter accurately explained the damage in-house scientist statements could cause to tobacco smoke litigation. The problem was how to keep them quiet, for, by the late 1960s, tobacco company (and consultant) scientists understood the true nature of tobacco smoke.

British American Tobacco documents, in particular, provide excellent examples of this knowledge. One of the first references to the cancer potentiality of tobacco smoke occurred in a March 1957 BAT memo entitled “Smoke Group Program for Coming 12-16 Week Period.” The memo had suggested two hypotheses for the causal relationship between tobacco smoking and cancer. The company also had funded substantial work on delivering nicotine without the toxic effects found in normal cigarette smoke. By 1970 the company was in contact with the Battelle Memorial Institute laboratory in Geneva, Switzerland for research on nicotine, with the ultimate

48 D. Hardy to D. Bryant, August 20, 1970, 7 (jwv56b00); and Stanton A. Glantz, et al., The Cigarette Papers, 271.
purpose of developing a nicotine delivery system without the toxicity of typical cigarettes. In projects named Hippo I and II, the lab examined the various effects of nicotine on animals. In addition, Project Ariel pursued means of delivering nicotine with only negligible amounts of toxins.49

As evidence of tobacco’s health risks continued to mount—at the same time litigation mandated an unswerving commitment to doubt—even senior researchers and executives admitted the serious dilemma facing the tobacco companies. In the fall of 1976 Sidney J. Green, a member of British American Tobacco’s board of directors and its chief of research and development, wrote a reflective essay entitled "Cigarette Smoking and Causal Relationships." In it he conceded:

The public position of the tobacco companies with respect to causal explanations of the association of cigarette smoking and diseases is dominated by legal considerations. In the ultimate companies wish to be able to dispute that a particular product was the cause of injury to a particular person. By repudiation of a causal role for cigarette smoking in general they hope to avoid liability in particular cases. The industry has retreated behind impossible demands for "scientific proof whereas such proof has never been required as a basis for action in the legal and political fields. Indeed if the doctrine were widely adopted the results would be disastrous.

Green—demonstrating that he understood the necessity of the company’s litigation posture and dilemma—then attempted to unlink causality in specific cases from the epidemiologic evidence. However, as a scientist he could not sustain the charade, in the end admitting that “for certain groups of people smoking causes the incidence of certain diseases to be higher than it would otherwise be.”50

If not everyone in the public and medical communities yet understood the nature of the TI and CTR, tobacco insiders knew they acted merely as fronts for tobacco litigation and regulatory positions. As Dorothea Cohen, who had authored CTR’s annual research summaries for twenty-four

50 Sydney J. Green, “Cigarette Smoking and Causal Relationships,” October 27, 1976 (lil36b00).
years, told *Wall Street Journal* reporters several years after her 1989 retirement, “When CTR researchers found out that cigarettes were bad and it was better not to smoke, we didn’t publicize that . . . The CTR is just a lobbying thing. We were lobbying for cigarettes.” The Jones Day 1998 report listing the legal evidence demonstrating tobacco company malfeasance admitted as much:

In 1972, Pied Pander of TI glibly reviewed what had been the industry strategy during the first 20 years after the health scare. The document is set forth in some detail because it contains damaging admissions, provides plaintiffs with a roadmap of the Open Question strategy and reveals that the purpose of Open Question strategy was to manipulate judges, juries, politicians, and public opinion.

“For nearly twenty years, this industry has employed a single strategy to defend itself on three major fronts -- litigation, politics, and public opinion . . . [I]t has always been a holding strategy, consisting of - - creating doubt without actually denying it . . .” 51

Indeed, the Jones Day report did not provide a single anomalous example. Tobacco documents released during the 1990s abound with similar corporate admissions. TI became the unified headquarters for a strategy of doubt. Under its President, Horace Kornegay—and supported by a multitude of lawyers, public relations specialists, and scientific consultants—TI reassured the public that every mountain of damning evidence was simply a molehill. Throughout the closing decades of the twentieth century, TI touted future tobacco research as the answer to what it called still pending questions about the toxicity of tobacco. In doing so, they pointed to the CTR, claiming, “independent advisors evaluate and fund research proposals by individuals and institutions. Awards are made with no strings attached . . .” Yet the tobacco industry knew better: as one magistrate wrote, CTR’s research “was of little value in addressing issues relating to the causal link between smoking and health.” In 1975 Addison Yeaman, by then retired general counsel and executive vice president of Brown & Williamson Tobacco Corp, called the CTR “the best and cheapest insurance

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51 Alix M. Freedman, et. al., “Smoke and Mirrors,” A1 (When CTR researchers); Jones, Day, “Draft: Corporate Activity,” 54 (nek96b00).
the tobacco industry can buy. . .” It had acted as a “shield” for the tobacco industry since its predecessor’s formation in 1954. By the 1970s, attorneys even maintained internal control over its activities. Attorney Henry H. Ramm had held the position of chairman and president of CTR from 1971 to 1975. In 1975 Yeaman had assumed the position. In 1984 CTR promoted its executive vice president—and attorney—Robert F. Gertenbach to President.

Internal documents also challenged the relevance and quality of CTR research. Most of the CTR approved research had been “directed at issues remotely related to cigarette smoking.” The lawyers curtailed central nervous system research proposals, denied funding for “dangerous project” proposals, and approved “special projects” designed specifically for litigation.” After approval, attorneys had kept close track on each project’s status, ready to “pull the plug” the moment a study appeared dangerous to the industry. Gradually, attorneys increased their use of CTR for litigation purposes. They started by sending “special project” proposals to CTR’s scientific director. If he endorsed the application, it became a CTR special project. If he did not, lawyers funded it through their “special projects” allowance.

Science was far from the only consideration in determining whether to fund a scientist. In Doctor Hickey’s case, his connections and strong support of the tobacco industry’s positions counted even more than the value of his work. As stated by Arthur J. Stevens:

. . . Hickey has established a good relationship with two other scientists who remain important to us, viz., Burch and Feinstein. . . He remains one


54 Jones, Day, “Draft: Corporate Activity,” 111 (directed at issues) (nek96b00); Arthur J. Stevens to David R. Hardy, February 8, 1972 (ayn66b00) (accessed August 14, 2014); Richard Kluger, Ashes to Ashes, 479-480; and Lisa Bero, et al., “Lawyer Control,” 243.
of the limited number of strong supporters who is willing to speak out on a variety of epidemiological questions, and has written a strong statement for the Waxman hearings.\textsuperscript{55}

As this letter noted, special projects money also had been used for other purposes, including researcher presentations at legislative hearings and conferences. In 1972 Shinn recommended that a panel about the “effects of Pollutants on Human Health,” organized by one of their consultants for an international symposium, be funded as a non-CTR special project in order to hide tobacco’s input. In a subsequent letter, Shinn recommended that pro-industry scientists attend scientific meetings to counter unfavorable data. He believed the panel would be helpful because the consultant’s work had been previously used to defuse harmful publicity. His letter then described how Dr. Gary Huber, then director of the CTR-supported research program at Harvard University, would lead another conference session: “Dr. Huber has agreed to conduct a two-hour presentation at an American Thoracic Society/American Lung Association meeting in May. There will be several participants, who will discuss the status of tobacco research in the fields of cancer, cardiovascular disease and chronic obstructive pulmonary disease. Dr. Huber also contemplates a section on the nonsmoker and tobacco smoke.”\textsuperscript{56}

Attorneys also used “special project” funds in at least one attempt to influence public policy through the creation of an “independent” advisory group, without disclosing the role of tobacco. A tobacco-funded consultant convened the “Advisory Group” to make recommendations concerning the US Public Health Service Report entitled “Cigarette Smoking and Health Characteristics,” as well as to criticize the process used to generate the report. Like most other research, the activities of the

\textsuperscript{55} Arthur J. Stevens to David R. Hardy et al., March 11, 1982, (leo66b00) (accessed August 14, 2014).
“Advisory Group” were reviewed and monitored by attorneys, including David Hardy of Shook, Hardy & Bacon.\(^5^7\)

Occasionally, CTR had to be reminded that it was primarily a tool for the attorneys. The Committee of Counsel quashed one attempt at independence during its February 23, 1978 meeting. During the discussions, numerous counsel expressed concern about CTR desiring more independence, with the group’s general agreement “heartily endorsed by Yeaman, that the industry should be reconsidering CTR as a whole.” Some counsel even questioned its legal necessity and public relations utility. In the end, they agreed that associate scientific director John H. Kreisher would be fired after the symposium. They further discussed changing the direction of the CTR, as well as ensuring the new Scientific Director understood that certain work needed to change direction and wind down. More than a decade later, Kreisher spoke to a *Wall Street Journal* reporter about his time at CTR. In the interview he complained that everything about the special projects was “cloak and dagger . . . we weren’t allowed on their floor,” thus revealing the attitude that probably led to his firing.\(^5^8\)

Despite these occasional lapses, industry lawyers recognized that they needed the CTR. In a 1978 memo to the CEO of B&W, General Counsel Ernest Pepples described the evolution of the CTR and its importance to the industry and its attorneys. The note confirms CTR was formed as a public relations ploy and operated throughout its existence as a political and legal shield, conducting many of its research efforts for the purposes of litigation rather than science.

Originally, CTR was organized as a public relations effort . . . There was even a suggestion by our political spokesmen that if a harmful element turned up[,] the industry would try to root it out. The research of CTR also discharged a legal responsibility. The manufacturer has a duty to know its product . . . Theoretically SAB is showing us the way in a highly complex field. There is another political need for research. Recently it has been

\(^5^8\) Meeting of Counsel minutes of February 23, 1978 (heartily endorsed) (lln96b00); and Alix M. Freedman, et. al., “Smoke and Mirrors,” A1.
suggested that CTR or industry research should enable us to give quick
responses to new developments in the propaganda of the avid anti-smoking
groups . . . Finally the industry research effort has included special projects
designed to find scientists and medical doctors who might serve as industry
witnesses in lawsuits or in a legislative forum.59

Approximately four months later, Pepples wrote B&W’s chairman and chief operating
officer, C. I. McCarty, informing him of Shook, Hardy attorney Bill Shinn’s similar views about
CTR’s importance: “[Shinn] mentions two aspects of particular value to CTR: (1) the direct legal
protection derived by Brown & Williamson and (2) the political and public relations advantage
accruing to the industry.” The memo next stresses CTR’s key importance:

[It] avoids the research dilemma presented to a responsible manufacturer of
cigarettes, which on the one hand needs to know the state of the art and on
the other hand cannot afford the risk of having in-house work turn sour . . .
A corollary is that without CTR, the government and the American Cancer
Society would be the only game in town. In that situation, the strong
minded scientists willing to deviate from the party line of the Mary Laskers
of this world would have no place to go. As long as there is a CTR they can
– and do – have some place to bring their applications.60

Shinn reiterated these beliefs at the November Committee of Counsel Meeting. As related by
another attendee, Shinn believed ”special projects are the best way-that monies are spent. On these
projects, CTR has acted as a ‘front’; however, there are times when CTR’ has been reluctant to serve
in that capacity and in rare instances, they have refused to serve in that capacity.” Shinn emphasized
his opinion by describing the history of the CTR, noting that by the 1970s CTR’s importance for
research was drastically diminished. Attorneys could not count on the scientists to provide the
correct evidence for litigation.

The tobacco industry looked around for more beneficial ways to-spend
their research dollars on smoking and health. It was at this time that special
projects were instituted at Washington University, Harvard University, and
UCLA. Bill Shim noted that the industry received a major public relation
"plus" when monies were given to Harvard Medical School.” Still, “Bill

59 Ernest Pepples memorandum to J. E. Edens, April 4, 1978 (ovv36b00) (accessed August 14, 2014); and Stanton A.
Glantz, et al., The Cigarette Papers, 44.
60 E. Pepples memorandum to C. I. McCarty, September 29, 1978 (mhc72d00) (accessed August 14, 2014).
Shim mentioned that the “public relations” value of CTR must be considered and continued . . . There is a ‘CTR basket’ [for research] which must be maintained for “PR” purposes.  

These memos struck at the heart of tobacco’s true dilemma: smoking causes disease; if CTR funded relevant independent research, the results would quickly confirm outside reports. Thus, CTR research required close monitoring by the legal community. But, when attorneys tightly manage the research, directing it specifically toward research that obtains the specific evidence necessary for trial or public relations, the search is not for accurate science, but rather for litigation victory. The result of these efforts is “junk science” in its truest form. As shown in that 1978 meeting and contemporary memos, even the lawyers understood that the medical research being conducted by the tobacco industry lacked value other than for litigation and to forestall regulation.

By 1981 some lawyers again began voicing internal frustrations with the direction of tobacco’s strategy. When this happened, they had to be reminded of the true importance of special projects. Such an event occurred during the morning of September 10, 1981 when thirteen Committee of Counsel attorneys met in a conference room at Chadbourne & Parke, a law firm in midtown Manhattan. The group, which may have been the Committee of Counsel, included attorneys from Philip Morris, R. J. Reynolds, Brown & Williamson Tobacco, Liggett Group, and Lorillard Tobacco, as well as outside counsel from Shook Hardy & Bacon and Jacob, Medinger & Finnegan. The tobacco legacy archive contains both an official report for the meeting as well as notes kept by Francis K. Decker, an outside counsel for Liggett. According to the report, at the meeting Lorillard general counsel Arthur Stevens repeatedly complained about the special projects, emphasizing that he was concerned about the science. "[The] science becomes diluted to the extent that attorneys' interests are met" and that grantees' research must withstand hostile examination. He

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added that there have been no problems in the past, but that in the future he wants to ‘get better science for our buck’” Decker’s notes suggest Stevens’ expressed even greater doubts about special projects: “He [Stevens] is concerned with degree to which we make advocacy primary and science becomes secondary.” Stevens boss was “concerned if the science is not worth a damn.”

Although it appears that one other attorney, who was not present, had expressed similar concerns, at the meeting Stevens did not receive much support. Another attorney frankly suggested that Stevens was looking at the value of the science by the wrong measure: results mattered, not the actual work. “[O]ne couldn’t say that special projects research isn't worthwhile. Such research demonstrates the validity of scientific questions (20 years ago it might have lead to "answers") and makes it possible for witnesses to testify on the basis of their own research. "Wrong conclusions" — often seen in the work of opponents -- make scientific work questionable, not the work itself.” In effect, Stevens was told to shut up and keep his eye on the important issue, litigation. As long as special projects assisted in keeping tobacco’s success rate at 100%, it did not matter whether they were “quality science.” Only “wrong conclusions” made scientific research questionable.62

The Methods Attorneys used in the Manufacture of Science

The “legacy” documents confirm that special projects’ principal objective— essentially prolonging doubt about the adverse effects of tobacco usage—involved generating useful results for industry attorneys. Attorneys also used them to keep informed about the latest scientific data concerning tobacco and health. Special projects further supported the industry’s public policy stance that governments should not regulate cigarettes. On occasion they created positive publicity for the tobacco industry. The use of “special projects” to perform these functions necessitated close control

62 Francis K. Decker, “Notes of Meeting of Committee of General Counsel held on September 10, 1981” (“concerned with degree” at 1, “worth a damn” at 5) (min12a00); T.E.B., “Report of Meeting of Committee of Counsel held September 10, 1981, at Chadbourne, Parke, Whiteside, Wolfe in New York City,” (“science becomes diluted” at 2, “special projects research” at 3) (swm96b00); and Mike France, “Inside Big Tobacco’s”.
over all aspects of their development by legal departments and outside counsel. This control extended from the initial identification of researchers through publication of results. These efforts—which reached a peak in the 1960s to 1980s—are identified and discussed below.

**Identifying Likely Researchers**

Tobacco attorneys were ever on the alert to identify and evaluate researchers sympathetic to the tobacco industry as potential funding candidates. In one instance, B&W general counsel wrote to Shook Hardy counsel Patrick Sirridge, asking him to “check the literature and see if these gentleman (the potential consultants) have written anything in the smoking and health field and let me know. Also, any other curriculum vitae that you think is of interest would be appreciated.”63 The Environmental Tobacco Smoke projects, which will be examined shortly, raised this effort first to an art then a scientific process.

**Training Research Scientists**

Once scientists were accepted into the attorney consultant fold, litigators trained them to insure a proper understanding of the industry’s scientific positions. During at least some periods in the 1980s and 1990s B&W sent newly hired scientists to coaching sessions with corporate counsel on the appropriate positions. Even senior management scientists underwent this schooling. Jerry Wigand, former B&W vice-president of research and development, testified in deposition that in February 1989, three months after being hired, he had been sent for three days to the firm’s Kansas City, Missouri, headquarters. While there, Shook Hardy attorneys coached him on “the company line on smoking and health, and addiction.” The coaching included instruction that for every possible disease “the evidence in the public health domain had not satisfactorily proven causation.” Studies that attempted to demonstrate links between tobacco smoke and cancer “were fraught with errors.”

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Furthermore, nicotine was not addictive. Wigand further testified that B&W had relied upon these opinions in lawsuits. It maintained the position on addiction “because part of [the company’s] legal defense was that smoking was a free choice.” In a similar scenario, when Dr. Scott Appleton became Brown & Williamson’s director of scientific and regulatory affairs in 1991, lawyers from Shook conducted his orientation and explained the industry’s position on addiction.64

Thwarting Unhelpful Research and Articles

The tobacco industry also criticized and suppressed support for research that did not support its position. In one example Ernest Pepples, B&W’s vice president for law wrote a memo about German scientific reaction to a landmark Japanese article that had demonstrated increased lung cancer in smokers’ nonsmoking wives. The memo concerned Dr. Adlkofer, scientific director of the German Verbandt. Adlkofer planned to write an article in support of the Japanese paper because he believed it accurate and understood that—notwithstanding its public position—in private the tobacco industry also believed so. Although the memo did not provide details, Pepples eased corporate management concerns by adding that even though Adlkofer held very strong opinions on the matter, a few days later Adlkofer called to say the article was off. Nor was this the only time attorneys thwarted Adlkofer’s plans. A decade later Philip Morris attorneys successfully blocked his desire to conduct a lifetime animal study of environmental tobacco smoke. Although a prior shorter study had shown no irreversible lung changes, German industry officials agreed that a lifetime study would likely not be as favorable and canceled the project.65


65 J. Wells memorandum to E. Pepples, July 1981 (pvq12a00); and “Smoking and health research activities in Europe,” December 19, 1990 (lmx59b00). The Jones Day Report provides additional illustrations of this tactic. “Legal counsel for some of the major tobacco companies apparently engineered a plan to fund research intended to “embarrass” Dr. E. Cuyler Hammond. A 1971 note from C.B. Wade (RJRT) notes that ”Dr. Sterling is the man Shinn and Hardy were working with on this project to embarrass Hammond and raise questions in the scientific community.”
**Influence Research Design and Publication.**

Lawyers also influenced the design of certain research projects. Industry documents confirm at least two cases of attorneys steering research away from areas likely to be unfavorable to the tobacco industry. In one they successfully guided the research to a worse design, while in another they coaxed it away from potentially damaging research. In the latter instance Jacob & Medinger attorney Timothy Finnegan visited a research consultant because their planned project had the potential for adverse results. According to B&W general counsel J. Kendrick Wells:

> Tim believes he has persuaded them to take a new thrust with their research. The new thrust will have questionable value but no negative. Sullivan and his people are enthusiastic but of questionable sophistication. Tim expects to receive a revised proposal and he will contact you then.\(^{66}\)

In considering the design of studies, attorneys looked very favorably on proposals that deflected attention from tobacco. In one illustration David Hardy’s requested special project or central file funding for Doctor Mancuso to assist his study of a population involving various causes of death. Hardy liked this project because Mancuso had expressed disagreement with a government scientist’s tobacco research methods and thus appeared likely to discount tobacco causation.\(^{67}\)

**Terminating Research Displaying the Potential for Adverse Results.**

In examining the adverse evidence available to plaintiff counsel, Jones, Day attorneys looked at the legitimacy of claims by plaintiffs' lawyers that manufacturers failed to adequately test cigarette additives. The memo notes that rebutting charges of misconduct at trial might be difficult and complex, given documents such as that written by a Lorillard employee. In it, the author contends:

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\(^{66}\) Lisa Bero, et al., “Lawyer Control,” 244; and J. K. Wells memorandum to K. Pepples, July 24, 1981 (dgu87h00). Even the American Medical Association feel victim to this tactic. As related by Alexander Spears of Lorillard, the AMA program being funded by the tobacco industry “dealt largely with acute effects of nicotine and there were very, very few projects that were very relevant, and had any relevancy really to smoke per se and cardiovascular disease.” Jones, Day Report, 180 (nek96b00).

\(^{67}\) David R. Hardy to Thomas F. Ahrensfeld, et al., January 10, 1972 (rpv66b00) (accessed August 14, 2014).
"[I]n 1984 the Committee of Counsel thwarted the industry scientists' desires to assure the safety of the product by testing ingredients adequately." When asked by *Business* Week reporter Mike France about this document, RJR and the Tobacco Institute declined to comment. They advised him that any public remarks might jeopardize future attempts to claim that it is protected by attorney-client privilege. Another attorney affiliated with the industry, while refusing to discuss the content of the memo, told France that it was a draft. “An investigation into its allegations revealed that the committee never blocked ingredient testing.” One month after the 1998 *Business Week* article appeared, RJR issued a press release, part of which discussed the memo. They contended that the still-undisclosed memo did not state the Committee of Counsel “thwarted” testing, but did admit it mentioned “some company support for the concept.”

Another, earlier event is less subject to ambiguity. Initial results of an early 1980s inhalation study involving 10,000 mice disclosed increased but still not statistically significant lung cancers among the tobacco smoke inhalation group. Attorneys from Jacob Medinger quickly had halted the project. The closure occurred following the first in a series of experiments, despite CTR having already spent $12 million. Shortly afterward the CTR issued a press release announcing that the experiment had not found squamous-cell carcinomas, the kind usually seen in human lung cancer. The press release did not mention that they had found double the expected rate of other lung cancers in the mice.

Other RJR biological studies also became the source of heated debate between lawyers and scientists. Initially, company chairman Bowman Gray, Jr. supported research involving rabbits. One former RJR scientists recalled that they were “young and idealistic, and we were going to change the

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world… Our goal was: if there is a problem, let’s try and fix it….” Initial Study results then suggested that emphysema in the rabbits might be due to smoke’s action on lung surfactants. Shortly thereafter RJR had halted the study for the announced reason that nasal tubes were causing infections in the rabbits, thus making the results unreliable. One former RJR scientist, however offered another motive for the shut down: “The decision… was made because Reynolds did not at that time want to be collecting information that might be detrimental to itself—which would be telling the public what its product does. Ignorance is bliss.”

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**Editing Proposed Manuscripts**

Special project grantees kept tobacco lawyers abreast of their research and publication progress. Attorneys reviewed virtually all in-house documents before publication. Outside consultants often let lawyers review draft manuscripts before submission. Attorneys did not always wait for the offer. A letter accompanying a $350,000.00 check to UCLA professor of oncology, Martin J. Cline, requested review of any press release concerning the research prior to its release.

On October 25, 1984 B&W corporate counsel, J. Kendrick Wells III wrote to BAT British solicitor H. A. Morini regarding a troublesome draft paper by Dr. L. C. F. Blackman, executive director of R&D for BAT. Well’s letter demonstrates the extent attorneys will go to ensure articles for public consumption do not contain damaging admissions. The proposed paper presented industry’s views on the “controversy” surrounding the health effects of smoking. As originally written, it contained a reasonably full description of the evidence that smoking causes disease. It used quotations from various scientists and reports to support the position that a causal link was still not proven. For Wells, however, even acknowledging the evidence concerning a causal link went too far. Wells’ letter contains a copy of Blackman’s 33-page paper, with 45 specific comments, including

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71 Lisa Bero, et al., “Lawyer Control,” 244; and *Business Week*, “Did Big Tobacco’s.”
line-by-line recommendations regarding necessary deletions or inclusions. Every page has comments, however most are too long to be written in the margins and are itemized in the letter. The letter recommends that a specific reference to renowned British epidemiologists Doll and Sir Richard Peto be deleted. Furthermore, any reference to Doll and Peto should be handled with great care. Wells’ editing removed all pretense of balanced presentation from the article, turning it exclusively into a propaganda piece. The letter additionally intimates that this review was not atypical, attorneys routinely reviewed documents prior to publication.\textsuperscript{72}

\textit{Repeating Problem Research}

Occasionally, despite CTR’s best efforts to either modify the opinion or withdraw support for publication, researchers funded by CTR had published findings containing opinions in opposition to tobacco industry positions. Such was the case with Carl Becker, a researcher at Cornell University. He published a series of studies concerning glycoproteins in tobacco smoke, the second of which the CTR funded. This second study, published in 1977, concluded that tobacco smoke glycoproteins “may be important to the pathogenesis of cardiovascular and pulmonary diseases associated with cigarette smoking.” Its publication prompted communication between CTR’s scientific director, Hockett, and his boss, attorney president Addison Yeaman. In their correspondence Hockett lamented that he was not able to have CTR’s funding acknowledgement deleted from the publication. The article forced industry attorneys to hire a scientist to repeat the experiments in hopes of discrediting them. Unfortunately for them, however, Becker had been correct. In their failed attempts to obtain contrary results, they spent $283,777 between 1976 and 1981. Ironically, by 1977 BAT in-house scientists had already replicated Becker’s findings.\textsuperscript{73}

\textsuperscript{72} J. Wells to H. Morini, October 25, 1984 (dgt86b00) (accessed August 15, 2014); LCF Blackman, “The Controversy on Smoking and Health” (cvr02i00) (accessed August 15, 2014). A detailed listing of the original text with a comparison to the editorial comments is contained in Table 9.1 of Stanton A. Glantz, et al., \textit{The Cigarette Papers}, 377-385.

\textsuperscript{73} Robert C. Hockett memorandum to Addison Yeaman, October 31, 1977 (CTR request to delete credit line too late) (fbu53e00) (accessed August 15, 2014); Timothy M Finnegan (Jacob & Medinger attorney) to William W. Shinn (Shook,
Controlling Scientific and Medical Experts

Manufacturing science was easiest with controlled scientists. Keeping scientific consultants focused and in line required constant vigilance. In their management practices, tobacco attorneys used both carrot and stick approaches. Tobacco attorneys had the ultimate carrot: almost unlimited financial backing. For the most helpful scientists, funding could run into the millions of dollars. Nor did the researchers have to wait months or even years for the funding, as often occurred with government grants. Even normal turnaround times could be expedited. For example, in 1972 Hardy requested fast, and increased, funding for what he characterized as an “administratively tenured” Harvard researcher, Doctor Carl Seltzer, to keep him from being terminated by Harvard University and to ensure his Department head would agree to allow him to stay on as a consultant after his pending retirement. Seltzer warranted such immediate attention due to his importance as a “Harvard” researcher. As Jacobs commented at a 1981 Committee of Counsel meeting: “If you have a doctor, you have to keep him busy or he will lose interest, Sterling has been enormously helpful.” Other scientists were kept happy with small checks.74

With the vast sums of money available to tobacco attorneys, finding willing consultants was never a problem. Yet the search was not easy. Litigation attorneys had two primary requirements in developing and using evidence, whether witnesses or documents such as medical articles. First, prior to investing time and money, lawyers wanted assurance that the evidence would be supportive of their position. Second, and equally important, the evidence needed to have the highest possible credibility. Tobacco industry attorneys managed these requirements to perfection for almost fifty

74 David R. Hardy to Thomas F. Ahrensfeld, et al., June 13, 1972 (prp66b00) (accessed August 21, 2014); and Francis K. Decker, “Notes of Meeting of Committee of General Counsel held on September 10, 1981,” 3 (min12a00).
years. As the Jones, Day report conceded, “Although industry funded a number of other “outside” research projects, it did so only when it received clear advance assurances of a “favorable” outcome.” Credibility was a tougher issue. By the 1970s, most eminent medical scientists had no desire to tarnish their reputation with tobacco tar and smoke. The treatment of three researchers, Gary Huber, Domingo Aviado, and Gori, epitomizes the carrot approach to supportive consultants. They are described individually below.

Dr. Gary Huber

Dr. Gary Huber represented the perfect eye of the storm for Jacob’s and Hardy’s “special projects.” When Huber, then of Harvard, solicited industry funds, he exceeded both requirements. As the lawyers well knew, at trial you cannot obtain better expert witness credibility and support then from a Harvard University professor who holds the view that "the number of people at potential risk from tobacco consumption is extremely small relative to the very large number of people who now smoke." As Bill Shim of Shook Hardy exclaimed, when monies were given to Harvard Medical School the industry received a major public relation "plus." With the presence of the two critical litigation elements, it did not matter that Dr. Huber's research led him to what plaintiffs would characterize as a ridiculous conclusion that "smoke appears to make (lung) cells more effective" rather than less.”

Over a number of years—first at Harvard, then as professor of medicine at University of Texas at Tyler—Huber exposed rats and pigeons to cigarette smoke in an exploration of its short and long-term effects on the body. The Harvard connection began in 1972, with a five-year grant of $2.8 million for Huber's research program focused on determining whether smoking produced

75 Jones, Day, “Corporate Activity Project,” 16.
emphysema in animals. When the project failed to induce emphysema—something tobacco companies had already induced in 1977—leading tobacco attorney Edwin Jacob, of the Jacob, Medinger, Finnegan & Hart law firm, extended the project for three years with additional funding of over $2 million. The amount represented the largest single grant by the tobacco industry to any university for health research.77

Throughout his career Huber was dogged with accusations that tobacco attorneys controlled his research. These accusations are understandable given Huber’s close working relationship with tobacco attorneys rather than scientists. For instance, he became a close friend of Arthur Stevens, General Counsel of Lorillard. Shook Hardy attorneys, including Hardy and Shinn, became his liaison with the tobacco industry. By 1979 he felt closer to Shinn than the two Harvard deans with whom he most closely interacted. That same year, Huber asked Harvard for a committee critique of the scientific aspects of his smoking and health program. This may have been due to the increasing strains he was experiencing from Harvard authorities. The tension first became pronounced in 1978. That year Huber may have expressed to Stevens or Shinn his displeasure about Harvard’s laboratory arrangements for the project. Huber’s industry attorney managers certainly expressed their displeasure about the arrangements to Harvard, cutting off funding due to an alleged breach of contract.78

As Huber memorialized in several memorandum to the file during 1979, the partnership ended because Harvard no longer desired to host a tobacco “special project,” and had failed to provide what Huber considered adequate facilities. Huber’s memos detailing his truculent meetings

78 Arthur J. Stevens to David R. Hardy, Esq., October 31, 1972 (qce51e00), William W. Shinn to Thomas F. Ahrensfeldt, et al., February 9, 1978, 3 (fxo19j00); Gary L. Huber to Mr. Lee Stanford, (Shook, Hardy and Bacon), May 17, 1979 (tpx51e00); and Gary L. Huber to Henry C. Meadow, March 26, 1979 (lsp78e00). Huber kept tobacco attorneys fully advised of his turbulent meetings with Meadows and Tosteson. Gary L. Huber to Arthur J. Stevens, Esq., June 5, 1979 (gpx51e00).
with Henry C. Meadow, Dean for Planning and Special Projects, Harvard Medical School, and Daniel Tosteson, Dean of the Harvard Medical School, made this abundantly clear. In separate meetings with Meadows and Tosteson, Huber learned of their deep umbrage over the program. At one meeting he learned a colleague’s appointment was not processed because “his career had been linked so closely . . . to the industry effort.” Huber’s status at the school does not seem to have been as provisional. It appears that Huber’s superiors were willing to continue Huber’s relationship with the university, so long as he terminated all connections with tobacco lawyers. One meeting with Meadows appears to have been particularly rancorous. Meadows informed Huber:

They [the tobacco industry] are not welcome! They are not welcome anywhere on Harvard University’s campus. Tosteson has told me . . . to get them the hell off our campus. And the sooner the better. No one in this administration wants the tobacco industry at Harvard.” Two weeks later Tosteson advised Huber that he had no problem with Huber or his research problem. Rather, he did not want Harvard associated with the tobacco industry. “There is no question, that from the point of view of health, the tobacco industry should best cease to exist as an industry. I think to encourage through advertising persons to smoke . . . is a subtle form of murder.”

Although the tobacco industry technically terminated the contract, they were not displeased with Huber, continuing to fund him at other locations into the 1990s. Huber reciprocated the devotion. After leaving Harvard and into the early 1990s he continued writing articles and letters to the editors expressing doubt about the health hazards of tobacco and questioning government conclusions.

79 Business Week, “Did Big Tobacco’s”; Gary L. Huber to Henry C. Meadow, March 26, 1979; Dr. Huber, memo to files, May 19, 1979 (“Career so closely linked”) (jpx51e00); Gary L. Huber, memo to the file, May 11, 1979, (hell off campus) (jpx51e00); Dr. Huber, memo to the file, March 15, 1979 (kpx51e00); and Dr. Huber, memo to the file, May 24, 1979 (“form of murder”) (nuy81e00). Huber did not listen to either Dean, having been seduced by tobacco attorneys he thought of as close friends. That summer Huber thanked Stevens for his comments, writing “they have considerable meaning to me” and expressed the hope that the two men would have “an opportunity to share further information or the nature contained in the communication on the Tosteson meeting with those who re in a position to bring change in our society.” Gary L. Huber to Arthur J. Stevens, July 20, 1979 (jx51e00). 80 The Tobacco attorneys may have wanted to continue retaining Huber due to his professional connections, including his service as a National Delegate to the American Heart Association, membership in several National Institute of Health Committees, senior editor to the journals Chest and Heart and Lung, and contributing editor to several other
Huber eventually became a whistle blower. In a late 1990s interview with *Frontline* reporters, he recounted a visit by prominent South Carolina plaintiff's attorney Ron Motley. Motley had provided Huber with documents demonstrating that tobacco firms knew about smoking’s causation of emphysema even before they approached Huber. In a subsequent interview for the PBS program *Frontline*, Huber expressed dismay at how he had been tricked. He described how attorneys were not initially involved. Then, shortly after beginning his program Hardy became his “keeper.” Subsequently, company-paid attorneys attended every discussion he had with company executives and scientists. In the interview Huber claimed that although he never trusted the attorneys, he had no knowledge of tobacco company fraud, and stated that he too was a victim.\(^81\)

It is difficult to fully accept Huber’s assertion of complete innocence. In his interview with FBI agents, Huber related that Hardy had implicitly admitted to him industry’s sponsorship of research to delay regulation. Furthermore, the memos he wrote in 1979, while struggling to continue his program at Harvard, express repeated friendship with the attorneys. He kept the tobacco attorneys closely advised of every meeting with Harvard officials, even cataloguing their diatribes against the industry. If Huber, as he stated, did not “[t]rust them from day one at Harvard” and was still so easily duped, he would have been better served by listening to Dean Meadow’s almost fatherly advice at the close of the Harvard program. After Huber expressed his belief to Meadows that the attorneys had a sincere interest in his personal well-being, Meadow responded, “You are wrong. Can’t you see how much harm Mr. Shinn has done you? He has hurt your career, he has wasted your time and your life, and he is not your ally in finding the kind of solution that is needed

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\(^81\) Lowell Bergman and Orlana Zill, “The Government’s Criminal Case,” (Gary Huber Interview).
for you and the tobacco industry.” If he had been asked about this meeting during his Frontline interview, Huber may well have agreed with Meadows. Still, Huber was partially correct in his statement to Meadows. So long as he continued to convey the tobacco industry line, tobacco attorneys had a sincere interest in his “financial” well-being.82

*Domingo M. Aviado*

Medical practitioner Domingo M. Aviado represents a different category of tobacco consultant. Although not as well connected as Huber, he provided numerous and consistent services to Shook, Hardy, other tobacco litigation counsel, CTR, and TI. In return, the law firms ensured he received substantial monetary support.

Aviado’s continued availability as a consultant and trial expert witness for litigation counsel hit a potential roadblock in 1977. At an August 4, 1977 meeting of tobacco counsel at Philip Morris Offices—also attended by Covington & Burling attorney Austern—one participant described the problem. Allied Chemical had offered Dr. Aviado a multimillion dollar laboratory. The ensuing discussion centered around the possibility that if Aviado accepted the offer he might not be available to testify. The group apparently resolved the problem with a simple solution. They provided three quarters of the necessary funding for a new and enlarged consultancy practice.

Aviado opened his consultancy practice, Atmospheric Health Sciences, in Short Hills, New Jersey on February 11, 1980, with invoices billed through Shook Hardy. During the first year Aviado immersed himself in the tobacco and lung cancer literature and performed numerous services on behalf of the law firm. He prepared critiques on five chapters of the 1980 surgeon General’s Report, *The Health Consequences of Smoking for Women*. He also wrote letters to the editor, including one published by *The New England Journal of Medicine* critiquing an article about the bronchopulmonary

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82 Lowell Bergman, et al., “The Government’s Criminal Case,” Gary Huber Interview; and Gary Huber, memo to the file, March 15, 1979 (You are wrong) (kpx51e00).
effects of smoking on nonsmokers. He spoke at local and state hearings and presented a paper on
the pharmacology of nicotine at the University of Pennsylvania Medical School that was then
rewritten for publication. He further assisted by seeking out other experts and labs willing to work
on subjects of interest to Shook Hardy. Finally, he helped in several tobacco and silica related Shook
Hardy projects. During this year he submitted sixteen reports to Shook Hardy, all of them tobacco-
related. In his spare time, Aviado also worked for five other tobacco and industry law firms, as well
as corporations and the federal government. Aviado complained about the government work—
being a consultant to the Clean Air Scientific Advisory Board for the EPA, he griped, took a
disproportionate amount of time compared to the income. This complaint was perhaps have been a
subtle hint that the law firms should be subsidizing even outside activities such as this.  

Gio Batta Gori

Gori’s interactions with the tobacco industry demonstrate that even government employees
could be seduced. At the same August 1977 meeting were Aviado’s potential loss came up in
discussion, the group later addressed a draft article by Gio Batta Gori, describing it as unprofessional
and requiring a complete rewrite. Interestingly, however, they did not dismiss the paper out of hand.
Rather, they noted that NCI had cleared the manuscript for publication: but Gori was willing to
forego publishing it if there were serious objections from tobacco.  

The reason for the tobacco attorney interest—notwithstanding the paper’s poor quality and
the extraordinary offer to forego publication—related to Dr. Gori’s position. He was not an industry
consultant, but rather chaired the Tobacco Working Group (TWG) a project chartered by the
Department of Health, Education and Welfare. The group had been charged with developing a less
hazardous cigarette. Throughout the group’s existence, tobacco representatives participated only as

83 J.C.B. Notes on Health Matters Reviewed at Meeting of Counsel, Philip Morris, August 4, 1977 (isp66b00); and
84 J.C.B. Notes on Health Matters.
observers. Although some insiders raised questions about the ramifications of even this minimal participation, industry attorneys believed political sensitivities precluded industry’s withdrawal from at least minimal participation. However, since they were unlikely to effectively influence the research and direction of TWG, the Committee of Counsel instructed company science directors to distance themselves from the project. The Committee described a three-point plan to affect the distancing: 1) When asked an opinion they should invariably respond that the “scientific director does not accept the premise that smoking is harmful;” 2) seek to eliminate “propaganda” oriented proposals such as “cessation clinics;” and 3) counsel re-examined past letters to the TWG to ensure clarification letter were not necessary for “misstatements.”

It appears the industry succeeded in limiting TWG’s public statements about the hazards of tobacco, having at least one press release cancelled. Gori also sent one or more papers he authored to the tobacco industry for comments. After the federal government terminated the TWG on August 12, 1977, Gori remained with NCI until 1980. At that time he left to become director of the Franklin Institute’s Health Policy Center and a paid consultant to the tobacco industry. In the new millennium he became the editor of *Regulatory Toxicology and Pharmacology*, the official journal of The International Society of Regulatory Toxicology’s, and frequent publisher of industry supportive toxicology articles.85

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Tobacco industry attorneys did not simply recruit compliant individuals. They also developed strategies to influence research output from biomedical scientists; and it is important to understand how these were carried out in practice. The various ways by which they achieved their desired results are described below.

On the Other Hand: Using the Stick

From the 1970s through the 1990s, tobacco attorneys displayed little hesitancy in punishing uncooperative scientists. This retribution extended to outside consultants and company scientists, as well as any third parties within reach. For outside scientists seeking funding, tobacco counsel required cooperation even prior to funding. CTR rejected projects submitted by investigators that had histories of conducting adverse to tobacco’s position.86

Once funded, scientists’ “keepers” required project conclusions in line with tobacco’s public stance on smoking and disease: either there is no health issue, or the issue is in doubt. In 1974, Dr. Freddy Homberger ran afoul of this requirement. Early that year his tobacco-funded study of smoking Syrian hamsters had generated excess cancers. During the summer, his attorney manager, Edwin Jacob, along with Hockett, then the scientific director of CTR, traveled from New York to Homberger’s Maine summer home to discuss the findings. Jacob asked Homberger to call the malignancy by a technical name that would not be understood by many as cancer—pseudoepitheliomatous hyperplasia. At the time Jacob had told Homberger that he would “never get a penny more” if the paper was published without changes. The three agreed on a compromise of “microinvasive” cancer, meaning a microscopic malignancy. Despite Homberger’s compromise, his lab never received further funding.87

86 Lisa Bero, et al., “Lawyer Control”; and W. L. Waite memorandum to H. R. Kornegay, September 19, 1978 (Researcher seeking money believes nicotine is addictive and tobacco causes 300,000 premature deaths per year and “wonders if this is why we might not be interested.”) (rwm66b00).
Corporate scientists faced similar reprisals for independent behavior. At times, instructions not to publish results of research were the least of the punishments. Philip Morris associate senior scientist Victor DeNoble learned this to his regret. He initially received approval in January 1983 for publication of his findings concerning nicotine’s effect as a weak reinforcer in rats. Shortly thereafter several lawyers had “showed up and started reviewing the research.” In July he was told he had to withdraw the paper. DeNoble then received notice that his laboratory was being closed. When he attempted to publish the paper after leaving Philip Morris, he received a letter reminding him about his lifetime confidentiality agreement.  

Although DeNoble could not say definitely why Philip Morris elected to cancel his experimentations, a document written by Shook Hardy attorney, Patrick Sirridge, suggested that Victor DeNoble’s lab had been shut down because its findings hurt the tobacco industry. “This kind of research is a major tool of our adversaries on the addiction issue. . . The irony is that industry-sponsored research is honing the tool. In the final analysis, the performing and publishing of nicotine-related research clearly seems ill-advised from a litigation point of view.”

Tobacco attorneys’ reach extended even to federal agencies. In 1982 Shook Hardy lawyers received documents from a source at Harvard University that included two National Institute for Drug Abuse (NIDA) progress reports of ongoing animal and human nicotine research. Both studies, one human and one animal, demonstrated that nicotine had a defining characteristic of an addictive substance—when given the opportunity, the subjects would administer nicotine to themselves. Within a week, the Tobacco Institute was lobbying its congressional allies to stop NIDA from funding studies of nicotine’s addictive traits. Shortly afterward, President Reagan’s drug czar visited

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NIDA laboratories. While there he suggested to NIDA’s acting head that if the agency had so much money that it could waste it on nicotine, then perhaps it could function with a smaller budget.  

You Have Problem Documents? Hide/Misuse/Destroy

The benefits of attorney-controlled medical research resulted from the ability to hide research through two court-sanctioned secrecy privileges and the ability to destroy documents not subject to an extremely limited attorney-designed document retention program. As a result, tobacco companies relied upon tobacco attorneys to oversee the concealing and destruction of an increasing number of damaging scientific documents throughout the closing decades of the century.

The problem partially arose because, by the 1980s, even tobacco researchers could no longer discount the causal connection of tobacco smoking to life-threatening diseases. Increasingly, corporate and consultant research resulted in documents potentially devastating to tobacco’s litigation defense. Tobacco counsel—both in concert and representing individual companies—began developing plans to protect these documents from discovery by plaintiff attorneys. The plans included use of attorney privileges, restrictive document retention policies, and, where necessary, simple destruction of documents. One Philip Morris researcher, William Dunn, had written explicitly about this, noting that attorneys were likely to require “clandestine” approaches to research that even remotely considered nicotine as a drug. The lawyers feared even tacit acknowledgement of the drug aspects of nicotine: “[i]therefore although permitted to continue. . . we must not be visible about it.” If results come out wrong, attorneys “want to bury it.”

Even literature review proved too dangerous for public consumption. A little over a year following Dunn’s complaint, the Ad Hoc committee of the Committee of Counsel considered

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90 David Kessler, A Question of Intent, 60-61.
91 See, for example, J.C.B. Notes on Health Matters Reviewed at Meeting of Counsel, Philip Morris, August 4, 1977 (isp66b00); J. K. Wells memorandum to E. Pepples, September 25, 1981 (yxq54a99); and Alix M. Freedman, et. Al., “Smoke and Mirrors,” A1;
removing the Literature Retrieval Division from the CTR. The proposal included establishing a separate corporation owned by four outside litigation law firms. The committee believed this could provide the documents with better attorney work product protection from disclosure.92

B&W’s struggle in the 1980s to develop measures providing disclosure protection for the research information BAT sent to the United States provides an excellent illustration of the ethical and moral distances attorneys traveled to protect “contentious” documents. On June 15, 1979, B&W general counsel J. Kendrick Wells wrote to fellow corporate attorney Ernest Pepples. At the time, as he informed Pepples, another company lawyer had “kept his eyes out for potentially sensitive material and . . . simply held them in his office.” In the memo, Wells laid out a new proposal for handling the sensitive BAT research documents, a strategy that involved misuse of the attorney work product and attorney client privileges.

The [Southampton R&D and laboratory] material should come to you under a policy statement between you and Southampton which describes the purpose of developing the documents for B&W and sending them to you as use for defense of potential litigation . . . Continued Law Department control is essential for the best argument for privilege. At the same time, control should be exercised with flexibility to allow access of the R&D staff to the documents . . . The general policy should be clearly stated that access to the documents and storage of the documents is under control of the Law Department and access is granted only upon approval of request . . . One category (sensitive) would be passed along to Dr. Sanford . . . The policy should explicitly make Dr. Sanford the agent of the Law Department with regard to these procedures.93

Three months later Wells still struggled to finalize the procedures. The research department had objected to the initial proposal as too cumbersome. Additional problems arose because the cost sharing agreement with BAT likely specified “the acquisition of scientific research for general

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92 W. L. Dunn interoffice memo to T. S. Osdene, November 3, 1977 (“bury it”) (vlu82i00) (accessed August 13, 2014); W. L. Dunn interoffice memo to R. B. Seligman, March 21, 1980 (“clandestine” and “not be visible”) (xcg77a00) (accessed August 13, 2014); and Francis K. Decker to Joseph H. Greer, September 18, 1981 (dic22a00) (accessed August 14, 2014).

business purposes. [General business records are not shielded by the attorney work product privilege.]” Wells’ revised procedure suggested sending the research to a Dr. Esterle under a formal arrangement that Dr. Esterle was assigned as your agent for the acquisition of scientific materials of litigation.” In addition “appropriate paper work should be established with BAT, including any amendments to the cost sharing agreement to establish that documents of a certain nature are prepared for B&W in anticipation of litigation.  

Since BAT research remained a problem in 1984, these proposals may not have been implemented. In May of that year, shortly after the filing of the landmark second-wave smoking products litigation case Cipollone v Liggett Group, Inc., B&W, BAT, and Shook Hardy attorneys held a conference on US products liability litigation. Wells summarized the conference deliberations in a June 12, 1984 memo to the file. The discussions extended to ongoing research at the two companies, including declarations that BAT attorneys should become more involved and certain projects restructured. “To summarize the status of the discussions,” Wells continued, “it is fair to say that BAT legal are informed about the danger of admissibility of BAT statements on smoking and health in U.S. products liability litigation.” At the meeting, at least one attorney admitted he had only learned of a project that had possible implications for litigation two weeks prior to the conference. The lawyers agreed that British research having implications for US litigation needed much closer attorney attention “through every step of the activity.”

Although before the conference BAT attorneys had been reluctant to become so intimately involved in scientific work, they likely now took action. By early 1985 BAT senior scientists had assisted in establishing what they hoped was a secure conduit for sending research materials to the United States. A series of letters over the three opening months of 1985 explained the pathway. The

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94 J. Kendrick Wells, III memorandum to Ernest Pepples, November 9, 1979 (snh22j00) (accessed August 15, 2014).
first, written on January 9th, informed researchers of a phone call indicating that “Contentious” information was to be sent “. . . to a legal man called Maddox . . . with a cover letter from [BAT] saying that Millbank had asked that he receive it.” The term contentious almost certainly meant legally damaging, rather than scientifically contentious.

The follow-up correspondence provided no further explanation for the procedures other than as a measure to keep legally damaging documents from being discovered by tobacco litigation plaintiffs. The next day Hardwick received written verification that all reports for worldwide distribution should be sent to Robert Maddox, at Wyatt, Tarrant, & Combs, a Kentucky law firm located three blocks from B&W headquarters. Three weeks later Hardwick provided forwarding procedures to the research staff. Significantly, the forwarding cover letter template did not provide any forwarding instructions, simply stating that Millbank—a BAT office—asked that Maddox receive the documents.96

During the same period that B&W attempted to hide British scientific documents, Wells had also expressed concern about American research. His memo of January 17, 1985 outlined a plan for “deadwood” that he discussed with Earl Kohnhorst, vice president of RD&E. During the conversation, Wells informed Kohnhorst how he had labeled a number of behavioral and biological study documents as “deadwood.” These included mouse-painting studies demonstrating the

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96 M. J. Hardwick memo to A. L. Heard, et al., “Material for Brown & Williamson,” January 9, 1985 (“legal man”) (chp56b00); R. J. Pritchard to M. J. Hardwick, January 10, 1985 (gcy50a99); M. J. Hardwick memo to The Communications Group, et al., January 30, 1985 (yqy95a99); and Peter Hanauer, et al., “Lawyer Control,” 237. A communication in 1988 between Lovell, White & King attorney Andrew Foyle and BAT managers concerning Buerger’s disease correspondence provides another example of how tobacco attorneys casually abused the attorney client privilege. “I referred earlier to our desire to create a modus operandi to ensure that legal professional privilege is not lost. Because correspondence on the subject of Buerger’s disease exchanged between you and your colleagues in other companies might not be privileged, it is important that contact between the scientists should be routed through the lawyers. In addition, you should ensure that any internal memoranda written on the subject of Buerger’s disease in relation to the current investigations should be captioned ‘Privileged and Confidential.’” A. W. Foyle to R. E. Thornton, March 21, 1988 (mov66b00). In addition to constrictive document forwarding practices, came proposals for more rigorous public statement constraints. An early February letter from R. G. Baker, a senior scientist at a BAT laboratory, to D. A. Schechter, a BATUS, Inc attorney, in Louisville, Kentucky, discussed the need to set up guidelines for affiliated companies outside the United States to follow when making public statements. R. Baker to D. Schechter, February 4, 1985 (adx56b00).
carcinogenicity of tobacco tar and other studies discussing the chemical composition of Canadian tobacco. Wells suggested that the documents be removed during a scheduled move of RD&E to a new building.  

The BAT document plan’s success appears questionable. A year later Wells was still studying the issue and trying to limit the number of documents sent to the United States. He further expressed concern about receiving information helpful to plaintiff: such material should be kept to the minimum. For example, he expressed a desire to exclude items discussing tobacco smoke toxicity and nicotine addiction. In stressing this desire, Wells obviously recognized that BAT research implicated cigarettes in significant health issues such as the toxicity of tar and the addictive nature of nicotine. His solution? “The only way BAT can avoid having information useful to plaintiff found at B&W is to obtain good legal counsel and cease producing information in Canada, Germany, Brazil and other places that is helpful to plaintiffs.”

Company counsel also had another tool to ensure nondisclosure of tobacco research—corporate document retention policies. In the guise of general business practice they could destroy research deemed obsolete or not useful in two to five years. In a 2002 deposition for the United States racketeering case against the tobacco companies, former BATUS attorney Schechter admitted that these policies safeguarded documents from discovery by plaintiff. In response to being asked if “one of the benefits of limiting such retention [of documents] was that documents would not fall into the hands of plaintiffs or the public or the newspapers?” Schechter answered, “that was the purpose of both the mental copy rule and the [document retention] program as a whole.”

On occasion, however, attorneys deemed as insufficient document shielding through attorney privileges or destruction in accordance with corporate retention policies. They then recommended the outright destruction of dangerous documents as soon as they surfaced. At a meeting with B&W in 1981, Shook lawyer Richard Northrip recommended such a course. During these discussions he recommended testing tobacco additives in-house, rather than contracting with outsiders, because “[i]f company testing began to show adverse results pertaining to a particular additive, the company control would enable the company to terminate the research, remove the additive, and destroy the data.” When asked about the document by Wall Street Journal reporters, B&W officials maintained it ignored the advice. Shook Hardy attorneys declined to comment.

The significance here, however, is not whether the destruction ever occurred, but rather the easy willingness of an attorney to suggest it. Moreover, other attorneys did not express umbrage over the suggestion. A five-page memo from Wells to his subordinate Ernest Pepples provided the likely reason why the company did not follow his advice. Wells objected to the proposal, but not because it involved destruction of documents. Rather, he did not believe it would be effective in shielding the companies.100

Substantial, systematic document destruction also occurred at INFOTAB, the clearinghouse set up by the major tobacco companies for information. The major companies originally had formed the organization in 1978 to serve as a global repository for smoking-related publications, articles, and legislative information. In 1991 the Tobacco Document Centre (TDC) replaced INFOTAB. A 1998 letter from Ron Tully, former director of TDC, to an industry attorney, detailed knowing, flagrantly improper destruction of documents at both INFOTAB and TDC. Tully had written this letter in

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response to an industry investigation into his alleged improper financial transactions while director of TDC. In addition to denying any improprieties, Tully counterattacked his accusers by revealing his board had authorized document destruction activities. In the letter, he related being asked to prepare a paper justifying “systematic destruction of pertinent documentation . . .” with the aim being the removal “. . . of all documents which could be viewed as ‘problematic’, damaging, or useful to plaintiffs in any ongoing industry litigation.” He further revealed that he authorized the destruction of close to a million pages—the equivalent information of approximately 4,000 books—over seven years, with the reduction of INFOTAB papers to the bare statutory minimum. In his last week at TDC he had “spent most of [his] time dealing with roughly 3,000 key documents . . .” Tully also strongly hinted that he had other damaging information, which could be released if the investigation was pursued.

But, once any can of worms is opened, it always amazes people what happens to crawl out. In the case of TDC, there are still many cans to open, and many worms to be dissected!101

The TDC document destruction suggests that lawyers’ reach in document destruction extended worldwide. As noted earlier, Wells complained about corporate production of “information in Canada, Germany, Brazil and other places that is helpful to plaintiffs.” At least in Australia, American attorneys attempted to solve this problem.

A new millennium Australian lawsuit—Rolah Ann McCabe v. British American Tobacco Australia Services Limited, (2002)—shed light on some of these activities. In his testimony for the Australian suit, as well as before United States Department of Justice attorneys, senior Shook Hardy partner Robert Northrip confirmed lawyers aided an Australian tobacco company in preventing detrimental company documents from being disclosed in tobacco litigation. Shook Hardy, with Northrip as lead counsel, took over responsibility for BAT’s Australian interests—Australian

101 Ron Tully to Dr. Marion Funck, General Counsel, Reemtsma GmbH, September 25, 1998 (tdi08d00).
tobacco company W.D. & H.O. Wills (Wills)—in the mid 1990s from BATUS attorney David Schechter. Prior to turning over responsibility for monitoring the Australian company’s document management, Schechter had made several trips to Australia "to reduce the likelihood of unhelpful documents being written or, if they were written, to make it less likely that they would become public." Northrip had continued the practice. According to the attorney, his firm had “advised Wills in relation to document management to ensure that damaging documents would not be discovered in litigation in Australia or elsewhere.” The documents included those that Northrip characterized as a “knockout blow” for plaintiff counsel. The document management practice they installed included not only measures to hide potentially damaging documents, but also wholesale destruction of sensitive files. Mr. Northrip thus agreed with Victorian Supreme Court judge Geoffrey Eames’ statement that Wills destroyed thousands of sensitive documents.102

New Legal Challenges: Nicotine Addiction Emerges as a Central Issue

The publication of the 1986 and 1988 Surgeon General’s reports forced tobacco attorneys to defend against a new front that opened in the war against smoking. Although directed at smokeless tobacco, the 1986 Report of the Advisory Committee to the Surgeon General on Smokeless Tobacco provided substantial evidence about the addictive properties of tobacco. Two years later, in a 618-page report on nicotine addiction, Surgeon General C. Everett Koop had declared that nicotine is a “powerfully addictive drug.” “It is now clear that . . . cigarettes and other forms of tobacco are addicting and that actions of nicotine form the pharmacologic base of tobacco addiction.”103

Although the tobacco industry publicly criticized the 1988 report, it did so cautiously. Statements directly disputing the 1988 report might have led to adverse consequences in smoking

and health litigation. In order to preserve the industry’s crucial litigation position that smoking is a free choice, statements criticizing the findings could not provide more credibility and prominence to the science supporting the Surgeon General’s conclusion. As a result, tobacco lawyers vetted all statements about the 1988 Surgeon General’s report. In a November 1987 letter to TI, Shook Hardy attorney John S. Johnston cautioned that everyone should avoid debating the Surgeon General’s findings on their merits, since “this would only lend credibility to the research.” Instead, Johnston recommended that “[w]e can refute [the] concerns of the American public without directly assessing the underlying research.” Johnston’s advice maintained the decades-long tobacco attorney tactic of creating doubt, while at the same time sidestepping dialogue on smoking and health.104

In keeping with this recommendation, TI paid clinical psychologist Theodore H. Blau, Ph.D. and Stephen M. Raffle, M.D., Assistant Clinical Professor of Psychiatry at the University of California, San Francisco, to appear on July 29, 1988 at a Congressional hearing concerning the Surgeon General’s nicotine addiction conclusions. In his Shook Hardy vetted statement, Blau did not venture into the underlying science. Rather, he argued that using the term addiction when discussing cigarette smoking trivialized the problem of drug addiction in the United States. Furthermore, it was misleading and potentially harmful to the American public. He believed the report’s findings had confused addiction with habit. To him, the distinctions between the terms “addiction,” “habit,” “compulsion” and "dependence" have had become blurred during the past several decades. He cautioned that the word "addiction" was used widely and loosely to describe many habits and everyday behaviors such as coffee drinking, jogging, love, and cigarette smoking.”

Like Blau, Raffle made a similar lawyer-approved statement. In his statement, Raffle focused on what the Surgeon General’s Report did not include, rather than its conclusions. “Clinically, cigarette smoking does not result in addiction-like behavior,” Raffle argued, because users do not require medical management during intoxication or withdrawal. At the hearing, neither Blau nor Raffle revealed that the tobacco industry had paid for their testimony. Subsequent testimony in a lawsuit disclosed that Raffle had no little relevant expertise, having no peer-reviewed research articles concerning nicotine.105

Contrary to these public statements, the tobacco industry had not required the Surgeon General’s assistance to learn about the addictive nature of nicotine. A quarter century earlier Addison Yeaman—at the time General Counsel of Brown & Williamson—declared, “We are, then, in the business of selling nicotine, an addictive drug effective in the release of stress mechanisms.” Prominent British tobacco researcher Sir Charles Ellis admitted as much when he wrote that same year about the soothing ability of cigarettes: “This reaction starts in the brain and leads to the release of a succession of chemical substances (hormones). . .”106

At a 1972 gathering of scientists on a Caribbean island for a seminar sponsored by the tobacco industry, Philip Morris psychologist William L. Dunn had explained why individuals smoke, delicately alluding to nicotine’s addictive nature: “The primary incentive to cigarette smoking is the immediate salutatory effect of inhaled smoke upon body function.” As he further explained, cigarettes are a delivery system for nicotine. Fellow PM scientist Al Udow had agreed with Dunn, writing, “it is likely that greater numbers smoke for the narcotic value that comes from the nicotine.”

106 Addison Yeaman, “Implications of Battelle Hippo I & II and Griffith Filter,” Internal Memo, July 17, 1963, 4, (xrc72d00) (accessed August 14, 2014); and Sir Charles Ellis memorandum to G. F. Todd, Esq., 29th May, 1963 (This reaction) (wps56b00);
That same year RJR research scientist Claude Teague had highlighted the addictive nature of nicotine in a memo describing nicotine as a “potent drug with a variety of physiological effects.” “Happily for the tobacco industry,” he continued, “nicotine is both habituating and unique in its variety of physiological actions. . .”

Six years later, Dunn announced findings further pointing to nicotine’s addictive nature. As nicotine in cigarettes went down, individuals smoked more, suggesting, “a tar and nicotine quota may be operative.” That same year a BAT-sponsored conference concluded: “Serious smokers smoke to prevent withdrawal symptoms.” As Paul Mele—a former research associate of DeNoble’s—quoted a Philip Morris executive during a 1999 FDA investigative interview: “We all know its addictive. I’d shove it in my vein if I could.”107

In 1979, British American Tobacco Company (BAT) scientists brainstormed on how to use the addictive nature of nicotine in product development. Their late summer memo summarizing the discussions used the words “habit” and “addictive” to describe nicotine. However, the overall thrust of the memo stressed the dependent condition of smokers. In alluding to addiction, they utilized statements like “who at one time were wholly “addicted” to cigarettes,” “dependence on the smoking habit,” “it initiates a dependence on the confirmed smoker,” “We are searching explicitly for a socially acceptable addictive product,” “the high profits . . . are directly related to . . . the customer [being] dependent upon the product,” and “one must question the ethics . . . of the new habituation process.”108

108 Anonymous, “Key Areas – Product innovation over the next 10 years for long term development,” 28th August, 1979 (jys76b00).
That same year BAT scientists learned that American companies did not discuss nicotine and addiction. In October, British tobacco company scientist D. G. Felton had visited tobacco operations in the United States. While there he experienced first-hand the reluctance of American companies to consider conducting research relating to nicotine addiction. Attorneys from Shook, Hardy & Bacon or Jacob and Medinger accompanied him through most of the tour. During the tour, both CTR administrative officials and Jacob Medinger attorney Tim Finnegan had expressed to him a “general nervousness in the U.S. Tobacco Industry . . . in working on the effects of nicotine, because of the risk of demonstrating nicotine dependence or addiction.” In the latter’s opinion, work on addiction posed legal risks. In the memo, Dunn further noted his suspicion that since Philip Morris was in fact conducting such studies, “all the major companies are undertaking in-house work of this nature, while maintaining a collective attitude of non-involvement.”

Finnegan was correct in being nervous about what research would reveal. Tobacco lawyers were well aware of the dangerous potential that tobacco industry research might conclusively prove the addictive nature of nicotine. For over thirty years they steered research away from examination of the issue. In 1984 Ernest Pepples, now B&W’s general counsel, wrote to BAT’s Deputy Chairman, complaining about certain planned sessions at a BAT research conference. He particularly objected to nicotine addiction references in one session paper:

[I]n developing . . . the position that a “simple” addiction model cannot explain smoking behavior, the report seems to concede that many potential criteria for addiction identification are met by smoking behavior. For example, the report urges the position that the primary motivation for smoking is ultimately tied to a pharmacological "psychoactive" function of nicotine. Some of the scientists who consult with B&W in connection with health litigation would not agree with this approach. Accordingly, the report is inconsistent with the scientific position on which B&W may make its defense in the New Jersey cases.” Pepples suggested solution was for BAT to increase the involvement of attorneys in planning scientific research. “If such matters as the "Functional Significance" document and the Conference binders, enclosed herewith, are not already routinely vetted

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109 D. G. Felton to P. Sheehy, Esq., undated (general nervousness) (all the major) (omp24a99).
with BATCo. lawyers, you may want to consider involving them more closely in both the conceptual and the drafting stages of these projects.  

Legal counsel did not confine their efforts at restraining nicotine addiction research to corporate and consulting scientists. As previously mentioned, tobacco legal counsel, through TI and President Reagan’s drug czar, directly interfered in NIDA’s nicotine addiction research. This interference likely delayed both research and regulation.

The FDA only began serious consideration of tobacco regulation in 1992, under new Commissioner David Kessler. Upon his appointment in October 1990, Kessler had arrived at the FDA eager to enforce the law. Initially, tobacco took a back burner to other regulatory measures because of a long-standing opinion that cigarettes did not fall within FDA’s jurisdiction. Then, in the fall of 1992, David Adams, a creative FDA employee “freelance artist of the law” offered Kessler a means of bringing the industry within FDA’s jurisdiction. Nicotine, he argued, is a variable in tobacco, not an unalterable ingredient. Its content can be increased or decreased. It is merely a “question of intent.” With at least a colorable argument for FDA’s right to regulate, Kessler authorized an investigation. Over the next two years FDA agents found several informants, but made little progress. When asked directly, the companies denied they “raised nicotine levels above what is found naturally in tobacco.” Jeffrey Wigand, former vice president of research at Brown & Williamson, provided government officials with the first extensive look inside tobacco research on addiction. Hired in 1989 to work on the development of a safer cigarette, Wigand was fired in 1993 and the project terminated. Several months later B&W had sued Wigand. Seeking to terminate his medical benefits, the company claimed Wigand’s disclosure of some terms of his separation

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111 David Kessler, A Question of Intent, 60-61.
112 David Kessler, A Question of Intent, 3, 62-3, 175 (raised nicotine levels).
agreement to a colleague had breached its terms. Wigand grudgingly settled, signing a life-long confidentiality concerning his work.

Angry over the lawsuit, Wigand contacted Lowell Bergman, senior producer for CBS "60 Minutes." When information leaked about Wigand’s discussions with CBS, an FDA employee called, to ask Wigand for assistance in the investigation. After watching tobacco executives testify at Representative Henry Waxman’s Congressional hearing, Wigand readily agreed. The seven executives—who included his former boss, Tommy Sandefur, now CEO of Brown & Williamson—had testified that they believed tobacco smoke is not addictive: a statement Wigand knew was a lie.

Thus commenced Wigand’s secret meetings with the FDA. At first he was known simply as “Research.” After several tentative contacts Wigand agreed to provide information, so long as he met directly with Kessler and was not asked specifically about his work at Brown & Williamson. During a long conversation with Kessler, and then also with staff members, “Research” explained how ammonia could be used to help release nicotine into smoke. Furthermore, nicotine levels could be manipulated through hybridization and genetic engineering.

This was the break Kessler had sought. During the course of several meetings, Wigand explained in detail the nicotine manipulation story. Brown & Williamson Tobacco had hired a genetic engineering firm called DNA Plant Technology to develop high nicotine plants they labeled Y-1. He suggested they look for Y-1’s Portuguese language patent in Brazil. Through Wigand’s assistance, the FDA located not only the patent, but additional evidence demonstrating how tobacco companies made cigarette nicotine stronger and more addictive. By summer’s end the FDA obtained hundreds of documents containing unambiguous evidence that cigarettes are drug delivery devices. FDA investigators also tracked down Y-1 type seeds imported into the United States and planted in southern farms. Equipped with this knowledge, ten FDA investigators descended upon Brown & Williamson offices in Louisville, KY for interviews. Without disclosing their knowledge of Y-1, the
agents asked about genetic engineering and plant breeding. Company officials initially responded that the firm had not undertaken any genetic engineering research. Somewhat later in the discussions, they clarified the answer to say that although the firm had not conducted such experiments itself, more than one private laboratory had received funding for research. At a subsequent meeting, B&W admitted they had developed Y-1, but eventually found it had only marginal benefits, so had discontinued the project.\footnote{Stanton A. Glantz, et al., The Cigarette Papers, 100; Kessler, et al., A Question of Intent, 214-18, Lowell Bergman et al., “The Government’s Criminal Case.”}

Wigand also assisted the Justice Department in its case of fraud against the tobacco companies. Within a year of providing assistance to the FDA investigation, Wigand began providing crucial help to the Justice Department investigation. Through his testimony, federal attorneys could explain the documents and demonstrate their importance to the case. Thus, he became one of the Justice Department’s most significant sources of information. The judiciary also recognized Wigand’s key role in the federal case. One month after his deposition, when B&W attorneys attempted to again question him, a Washington, D.C. district judge quashed the subpoena for six months while the Justice Department debriefed him. Although B&W had only sued Wigand for his public statements, they undoubtedly suspected he was behind the Justice Department and FDA’s newly focused investigation.\footnote{Lowell Bergman et al., “The Government’s Criminal Case.”}

By this time the tobacco industry had commenced virtually buying former FDA attorneys to assist in its defense. Richard Cooper, a former chief counsel for the FDA and now a partner in the respected Washington law firm Williams & Connolly, represented RJ Reynolds. Former FDA attorney Gene Pfeiffer represented Brown & Williamson. Two other former FDA attorneys, Arthur Levine and Donald Beers, now with Arnold & Porter—another prestigious Washington law firm—represented Philip Morris. They also hired Congressional attorneys, including Mike Barrett, former...
staff director for Congressman John Dingell’s oversight committee. Over a five-year period they paid Barrett more than $2.5 million for what he described as virtually nothing. When the industry tried to reduce the agreed to fees, he refused, telling them his acceptance of the retainer had limited his future professional options. In his discussion of former FDA and other lawyers representing tobacco, Kessler believed that at least some of them were honorable. He further recounted that in later years they appeared almost embarrassed by their former representation.

Based on tobacco’s response to Kessler’s efforts, it must have been hard to tell they were honorable at the time. That year, Arnold & Porter mounted “an intensive legislative effort to persuade members of congress of the dangers inherent in the FDA initiative [to regulate cigarettes].” In addition, Philip Morris General Counsel Steve Parrish presented a four-part “Tobacco Strategic Attack Plan” in answer to the FDA’s 1994 push to regulate cigarettes:

1) Frontal Assault – open manufacturing plants to investigators, provide briefings, run aggressive ad campaigns, and preempt local regulation;

2) Surgical Strikes – File law suits such as the libel suit against ABC for its show describing the “spiking” of nicotine in cigarettes;

3) Allied Attacks – Having friendly third parties conduct attacks without obvious connection to the tobacco industry; and

4) Air cover – Raise consumer ire through the National Smokers Alliance, a group founded by Philip Morris. They considered this their NRA.

Despite the tobacco industry’s obstructions, in 1996 the FDA declared it had the authority to regulate tobacco products. Three years later, with the rising potential for the marketing of “safer” cigarettes, the FDA requested the Institute of Medicine (IOM) to examine the science for “safer”

115 David Kessler, A Question of Intent, 143, 169, 174, 205, and 239.
116Sea Island Presentation Draft, April 11, 1994 (yie87e00) (accessed September 12, 2014); and David Kessler, A Question of Intent, 169-170.
cigarettes. In October 1999, the IOM announced the creation and goals of the newly formed Committee to Assess the Science Base for Tobacco Harm Reduction. The twelve members of the Committee included experts in fields ranging from toxicology to epidemiology, with a few of them being at least partially controlled by the tobacco industry. The committee received additional support from IOM staff, liaison members of other IOM boards, and nonvoting consultants.117

In their 2013 article “Tobacco Company Efforts to Influence the Food and Drug Administration-Commissioned Institute of Medicine Report Clearing the Smoke: An Analysis of Documents Released Through Litigation,” Members of the Center for Tobacco Research and Education, of the University of California, San Francisco, along with a colleague from Germany, provided a close look at how industry, with significant attorney assistance, attempted to subvert the IOM Committee proceedings. With the formation of a committee that included industry consultants, Philip Morris had recognized it had an opportunity to sway the scientific assessment and regulation of “safer” cigarettes. They believed the IOM report would have high-stakes regulatory implications. In coordination with outside law firms and their consultants, PM officials had immediately developed implemented strategies to participate in the committee’s decision-making process.

Indeed, Philip Morris’s Worldwide Regulatory Affairs (WRA) and Worldwide Scientific Affairs (WSA) divisions had employed lawyers and consultants in one effort to gather data and submit proposals on how the companies should attempt to influence the committee. In response to the request for proposals, two Arnold & Porter attorneys, Arthur Levine and Jeff Richman, wrote Mark Berlind, senior assistant general counsel of WRA, detailing their telephone conversation with

117 Crystal E. Tan, “Tobacco Company Efforts,” 1-3. “The IOM is part of the National Academies (which consists of the National Academy of Sciences, The National Academy of Engineering, the Institute of Medicine, and the National Research Council), elite self selected professional organizations whose purpose is to provide scholarly advice to policy makers.” Crystal E. Tan, et al., “Tobacco Company Efforts,” 2-3.
Dr. Kathleen Stratton, IOM’s study director. Levine reported that during their conversation Stratton outlined the committee structure, funding source (the FDA), the staff membership, including its leader Dr. Sullivan, and schedule of committee meetings. Levine also obtained information about the degree of allowable industry participation during every stage of the process. Although IOM committees typically did not permit industry participation, Stratton understood it was unavoidable in this instance. She anticipated the Committee would encourage the tobacco industry to provide it with information, either through testimony or submissions. Stratton also told the lawyers that since peer reviewers are not subject to conflict of interest rules, members of industry could serve in that capacity. IOM subsequently used a Covington & Burling attorney as a peer reviewer.¹¹⁸

Thus, when the IOM study staff invited the companies to provide information on exposure and disease markers, clinical trial design for safety and efficacy, and implications for initiation and cessation, the industry was ready—and chose both means of communication: testimony and document submission. Six Philip Morris representatives appeared before the Committee: Richard A. Carchman, vice President of WSA), Hans-Juergen Haussman Executive Manager of Bio research at INBIFO, George Patskan Philip Morris Director of Product Integrity, Richard Solana, and two other senior scientists. Philip Morris scientists and attorneys—including two associate general counsels—reviewed and edited their presentations.¹¹⁹

The conglomerate also provided written materials. Perhaps the most significant was *Analytical Determination of Nicotine and Related Compounds and Their Metabolites*, a 772-page treatise on nicotine analysis that Philip Morris had recently commissioned and underwrote. Industry scientists, consultants, and funded researchers prepared most of the book’s articles. Although the monograph noted that the tobacco company helped fund the book’s publication and some contributors were

industry employees or consultants, it failed to reveal the full extent of overwhelming corporate involvement. In particular, the book made no mention of the extensive editing by company scientists and lawyers, or PM’s agreement to purchase five hundred copies of the book in order to assure its publication.\textsuperscript{120}

In 2001 IOM published the Committee’s findings in \textit{Clearing the Smoke}. The article’s authors were not convinced the available evidence permitted “drawing cause-and-effect conclusions, and the IOM may have come to the same conclusions without the influence of the tobacco industry.” However, they noted that industry officials expressed pleasure about the report, which took a much gentler position on the difficulty in creating “safer” cigarettes than most other government reports.\textsuperscript{121}

In any case, the IOM Report did not impact FDA regulation. The effort had been stymied one year earlier by a U.S. Supreme Court ruling. By the bare minimum of five to four, the Court ruled the FDA did not have authority to regulate tobacco. Tellingly, for tobacco’s “the science on tobacco is still in doubt proclamations,” Sandra Day O’Connor’s summarization of the majority’s opinion conceded that smoking “is one of the most troubling public health problems facing our nation today.” Seemingly almost pleading for Congress to take action, she concluded with the pronouncement that “tobacco use. . . poses perhaps the single most significant threat to public health in the United States. . . [yet] it is plain that Congress has not given the FDA the authority it seeks to exercise here.” Tobacco attorneys had once again held the lines.\textsuperscript{122}

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Managing Environmental Tobacco Smoke Regulations

Even before Kessler’s FDA began serious consideration of regulating tobacco, the industry had geared up to fight mounting regulation restricting where individuals could smoke. The accumulating evidence about the health consequences of environmental tobacco smoke (ETS) provided the impetus of the regulations. In response from about 1988 to the mid 1990s tobacco lawyers created an international ETS consultancy program to fight local, state, national and international regulations as well as the underlying science that supported the constraints. Through the program attorneys sought to manufacture science supporting the industry’s position, thus providing a “scientific backdrop” to dispute the regulations.123

By the commencement of tobacco’s worldwide ETS program, anti-smoking activist-sponsored initiatives demonstrated growing momentum. One of the first regulations had already occurred almost twenty years earlier—not in the United States, but in tiny Singapore. In 1970 the city-state banned smoking in theaters, cinemas, and buses. That same year, Trans World Airlines (TWA) became the first airline to offer non-smoking sections aboard every aircraft. Two years later, the Surgeon General’s Report expressed increasing concern about public exposure to tobacco smoke. This created a backlash: within a year, angry tobacco executives succeeded in having Surgeon General Dr. Jesse Steinfeld fired. They had less success in Arizona, which in 1973 became the first state in modern times to restrict smoking in public locations like elevators, libraries, theaters, and buses.124

Tobacco executives began feeling increasing pressure. Smoking bans appeared in states including Minnesota, as well as countries around the world, including Italy and Thailand. In 1978

123 Monique E. Muggli, et al., “Science for Hire,” 303; J. Drope, et al., “Tobacco Industry Efforts,” 588. These articles provided two of the first close examinations of tobacco’s ETS program; in the process revealing many of the documents used in this section.
marketing research firm US Roper advised TI that growing concern about secondhand smoke presented “the most dangerous development to the viability of the tobacco industry that has yet occurred.” They recommended a long-term antidote of “developing and widely publicizing clear-cut, credible, medical evidence that passive smoking is not harmful to the non-smoker’s health.”

Perhaps in direct response to the recommendation, the tobacco industry greatly increased its efforts to provide a contrasting viewpoint to anti-smoking rhetoric and science. The reply initially took two forms. Six international companies, including B&W, PM, and RJR established INFOTAB, a non-profit international association headquartered in Switzerland. Its strategic objectives included preventing “unreasonable restrictions” on tobacco operations and preserving smokers’ right to smoke wherever they chose. The companies particularly sought to have early warning of anti-smoking initiatives around the world. In the United States, the Tobacco Institute took a more direct approach, launching an advertising campaign aimed at disrupting the anti-smokers movement.

As the threat swelled, attorneys similarly amplified their control of the response. Whenever possible, tobacco attorneys sought to implicate other substances for diseases potentially caused by tobacco. In 1981 Shook Hardy attorney Patrick Sirridge proposed such a project to the Committee of Counsel. He recommended a study of the “sick building syndrome” through Dr. Theodor Sterling. Endorsing Sterling was easy. In his previous work smoking was considered and determined not to be a problem. In this project, Sterling intended to collect building data on substances other than tobacco smoke. Approval came quickly for the $207,913 research. On December 9, 1981 Arnold Henson, senior vice president and general counsel for American Brands, wrote Sirridge to inform him of CTR Special Projects approval of the investigation.

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127 Patrick M. Sirridge to Thomas F. Ahrensfeld, November 20, 1981 (vkq20e00) (accessed November 15, 2014); and Arnold Henson to Patrick M. Sirridge, December 9, 1981 (aoo96b00) (accessed August 15, 2014).
These efforts faced mounting obstacles. Bad news for the tobacco industry continued almost unabated in the 1980s. Although not disposed to speak with any certainty, the 1982 Surgeon General’s Report suggested that second hand smoke might cause lung cancer. Epidemiological research on the effects of ETS further heightened the focus of indoor air pollution discussion on tobacco. The 1984 study came from Japan, where Takashi Hirayama for fourteen years had followed the health of 92,000 nonsmoking wives of smoking husbands. As a group, the wives of smoking husbands experienced an increased risk of lung cancer, with a clear dose response relationship. The group with highest exposure—husbands who smoked one pack or more a day—had a 90% greater risk of lung cancer than wives with nonsmoking husbands. Although consultants for TI and attorneys immediately ridiculed the study, privately they admitted German and British scientists “believe Hirayama is a good scientist and that his non-smoking wives publication is correct.”

A year later, Covington & Burling partner John Rupp approved TI’s formation of the Indoor Air Pollution Advisory Group (IAPAG), with Sorell Schwartz as its chairman. Schwartz was not an unknown commodity to Rupp. Even prior to IAPAG’s formation, Covington & Burling had paid Schwartz and his Center for Environmental Health and Technology to consult for TI. In correspondence Rupp described the newly formed IAPAG membership as university-affiliated scientists actively involved in secondhand smoke discussions and activities with available TI assistance. He hoped the organization might act as a counter advocate to prior Surgeon General’s reports and the increasing call to regulate ETS. In his self-corrected written trial testimony, Rupp claimed the organization’s sole function was to “provide a fair and objective assessment of the ETS science that existed at the particular time.” It “had as its objective to fund better science than had

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been done – to produce answers that would gain wide respect within the international scientific community.”\(^{129}\)

In contrast to Rupp’s many statements, Schwartz’s 2004 written testimony claims IAPAG initially worked exclusively with Rupp, who served as a buffer between Schwartz and TI. Contrary to Rupp’s description of IAPAG as an autonomous agency, Schwartz portrays TI as desiring “unabashed advocacy” because they were “paying the bill.” He liked the way Rupp had run the program and felt TI was simply trying to manage IAPAG’s appearances as public relations opportunities.”\(^{130}\)

Two years after the Hirayama study, the Surgeon General no longer remained equivocal. The 1986 report found smoking not only addictive, but also harmful to nonsmokers. As he wrote in his forwarding letter to President George H. W. Bush:

> Based on the current report, the judgment can now be made that exposure to environmental tobacco smoke can cause disease, including lung cancer, in nonsmokers. It is also clear that simple separation of smokers and nonsmokers within the same airspace may reduce but cannot eliminate nonsmoker exposure to environmental tobacco smoke.\(^{131}\)

The Report’s publication represented a dramatic failure for the tobacco industry, which had attempted to influence the findings. The TI-ETS Advisory Group had discussed the endeavor at its April 1986 meeting, where Attorney Rupp advised the group that IAPAG’s chairman, Sorell Schwartz, had proposed a series of small symposia for this effort. As a program committee member of the American College of Toxicology, Schwartz could obtain its sponsorship of the meetings. Each session would include presentations by potential contributors to the 1986 Surgeon General’s

\(^{129}\) John P. Rupp, “United States’ Written Direct Examination of John P. Rupp (As corrected by the witness), United States of America V. Philip Morris, et al., undated 20 (fair and objective) and 31 (objective to fund) (ghp11b00)


The tentative agenda included presentations from four Surgeon General Report authors—Buist, Hoffman, Hiller, and Wu—as well as a number of frequently used IAPAG consultants: Philip Witorsch, Nancy Balter, Sorell Schwartz, Mark Reasor, Vincent Castranova, and Salvatore DiNardi. With the symposia being “non-public with no press and no reports,” the meetings provided an “opportunity in a semi-private setting of presenting the industry view of ETS to possible contributors to the Surgeon General’s report.”

Although proposed by Schwartz, Rupp controlled the event. A memo seeking his approval suggested scheduling the symposia “as soon as possible so as to involve key scientists who may be contributing to the Surgeon General’s report before they get too deeply committed to the approach that the SG rather clearly has in mind.” In discussing those plans, TI executive William Kloepfer noted: “The idea is that they [Surgeon General report authors] learn more about the uncertainties of the relevant science, and we learn more about the thrusts we can expect them to make. A pretty good investment, we think.”

In its announcement of and invitations to the events, the American College of Toxicology made no mention of tobacco sponsorship. Despite the effort to keep tobacco’s involvement hidden, the American Lung Association (ALA) learned of its backing. As reported in a June column of the Washington Post, the ALA contacted the report authors, disclosing industry’s involvement. Three authors immediately cancelled their participation.

The December 1986 release of the Surgeon General’s report on involuntary smoking sent strong tremors throughout the tobacco industry. Attendees of a three-day Philip Morris conference

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132 Charles Green interoffice memorandum to Dr. Alan Rodgman, June 6, 1986 (non-public and opportunity) (jis31d00); anonymous memorandum to Mr. Rupp, March 31, 1986 (as soon as possible) (ecs63b00); and William Kloepfer, “Draft Remarks,” April 1986 (pretty good investment) (ksk65b00) (accessed September 23, 2014).
133 Kim Klausner, et al., “Create a Bigger,” 22. A memo concerning the May TI Advisory Group meeting further reveals the extent to which Rupp controlled the event: “if more of the SG’s report authors pull out, Mr. [John] Rupp [of Covington & Burling] plans to cancel the discussions.” Charles Green interoffice memorandum to Dr. Alan Rodgman, June 6, 1986 (pis44d00) (accessed September 23, 2014).
on environmental tobacco smoke recognized the urgency of action. The report could result in a tidal wave of new regulations with a resulting “devastating impact on sales.” Describing the report as of “watershed significance,” they concluded, “we can’t stem the tide [of regulation] without addressing this report” At the workshop, Rupp summarized industry’s position in six words: “Where we are --- in deep s#!t.” Looking back at the report in his opening remarks for a 1988 ETS seminar, TI Legislative consultant Charlie Whitley declared,

“We in the industry immediately recognized the impact and intent of this new report. It had the potential to become the same kind of impetus for more regulation and restriction that the 1964 report was on active or direct smoking.”

Although pre-Report stratagems failed, the industry did not hesitate in what it considered a fight for survival. Over the next ten years it allocated an enormous amount of money, time, and resources in an effort to denigrate the underlying scientific basis for the contention that secondhand smoke harms nonsmokers. By the late 1980s, notwithstanding substantial pushback from industry scientists and some management, attorneys exercised extensive sway over the ETS effort, fully controlling the consultants’ program. This Phillip Morris example is likely typical of the battle in most tobacco firms:

...[T]here is a dispute within PM [Philip Morris] between the scientists and the public relations people about who should spearhead the drive. Scientists think it should be scientists—PR [Public Relations] people think it should be lawyers. So far, lawyers are winning.

Cigarette manufacturers, with Rupp in the lead, immediately took several steps. They sent a Freedom of Information Act (FOIA) request requesting documents and materials pertaining to the report. When the office of the Secretary of Department of Health and Human Services withheld a

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134 “Project Down Under Conference Notes,” June 24, 1987-June 26, 1987, 4 (We are in), 5 (Can’t stem the tide), and 8 (devastating impact)( ftp36b00) (accessed April 21, 2015); and Kim Klausner, et al., “Create a Bigger,” 24 (recognized the impact).

number of documents, Covington & Burling filed an appeal. Along with the request, special projects embarked on research to emphasize all possible confounders in ETS and health research. They also intensified research and publications on other indoor air pollutants, seeking to demonstrate ETS’s comparatively small effects.\textsuperscript{136}

The next step involved obtaining credible counter voices to the Surgeon General. Already by 1988 TI could boast of retaining for ETS consultations fourteen academic scientists at prestigious universities and medical schools, having the primary task of commenting on the flawed nature of ETS studies and the lack of conclusive evidence. Legal counsel placed especial importance on criticizing independent ETS studies and having consultants publish counter studies. Japanese scientist Hirayama appeared at the top of their list for vilification. The most significant censure of Hirayama came from a Covington & Burling project initiated in 1991. The criticism, written by Dr. Peter Lee, a self-professed long-term British consultant to the tobacco industry and head of P N Lee Statistics and Computing Ltd, came in the form of an assessment of Hirayama’s participant classifications. Yet Lee did not undertake any of the research forming the foundation for his opinions. Two Japanese scientists—with Doctor E. Yano being the principle investigator—conducted the investigation and interviews underlying Lee’s article. When Yano did not agree with several article drafts written by a scientist employed by Covington & Burling, that scientist wrote tobacco industry executives: "After considerable effort working with Dr. Yano at Teikyo University,\textsuperscript{136}

\textsuperscript{136} Kim Klausner, et al., “Create a Bigger,” 22-3. The industry’s reaction to EPA’s 1993 report classifying secondhand smoke as a Group A carcinogen highlights one manner in which attorneys might have used the documents if they had been obtained through the FOIA request. In its deliberations, EPA had relied upon a new methodological approach. Swidler & Berlin attorney Leonard Miller wrote Tom Borelli, Philip Morris’s Director of Science and Environmental Policy, recommending that Philip Morris focus its criticism on that methodology: “Meta-analysis is a new technique, used only a few times ... by EPA or the courts. My understanding is that there are few, if any, guidelines for the proper use of meta-analysis and possibly even some question about whether it is a scientifically valid method of analysis. ...[Y]ou might wish to encourage an evaluation of meta-analysis-without direct reference to the ETS risk assessment. ...[E]xpanding this effort to an academic forum, an industry forum (drinking water, power utilities, etc.), and perhaps the EPA Risk Assessment Commission ... Even though this effort might not focus on ETS. ...it could be of great use in the future .... in a regulatory proceeding.” Leonard A. Miller to Thomas Borelli, February 2, 1993 (xyc77a00) (accessed September 18, 2014); and J. Drope, et al., “Tobacco Industry Efforts,” 591-2.
we feel it is time to recommend that Mr. Peter Lee [a PM consultant] be asked to submit the Japanese spousal study research for publication in the British Medical Journal.” Eventually, Lee became the sole author of the publication, with Yano being briefly mentioned in the Acknowledgements. Although the study used the data collected by Yano, he was not consulted on the ultimate publication, nor did he provide his consent for use of the data.

Lee’s article differed substantially from the conclusions Yano had reached about the data. Lee concluded that Hirayama’s research was flawed by misclassification bias, declaring: "There is no direct evidence that workplace environmental tobacco smoke (ETS) increases lung cancer risk." In 2005, Dr. Yano wrote a repudiation of the Lee study, asserting: "[i]n addition to the misrepresentation of the facts demonstrated in his own paper, Lee did not report some of the important findings obtained in the project. . ."Using all the data from this project changes the conclusion of the [Lee] report," he wrote, strengthening Hirayama’s findings.137

Along with the academics, twenty-three consultants “whose businesses are to market their scientific expertise” were available for assistance and research. Yet, industry insiders still lamented about how government officials and outsiders still believed their scientists lacked credibility.138

Two “toxic substance” science experts who will appear throughout this dissertation as consultants to attorneys were likely among this group— epidemiologist and biostatistician William J. Butler and toxicologist and industrial hygienist Dennis Paustenbach. Both provided ETS consultation services to tobacco industry lawyers, with Butler maintained the larger presence in tobacco ETS science. While the two scientists played only minor roles in tobacco science, they both

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138 Samuel D. Chilcote, Jr. (President of the Tobacco Institute) to Frank E. Resnik (Chairman, Philip Morris USA), October 25, 1989 (and attached Public Smoking Programs of the Tobacco Institute - Consulting Scientists on ETS and IAQ (irt92b00) (accessed September 19, 2014).
made careers of assisting companies with toxic substance difficulties. Thus, they will appear again in later chapters of this dissertation. In the 1990s Butler worked for Paustenbach at ChemRisk, then formed his own company, Environmental Health Analysis, Inc.—at times continuing to assist Paustenbach’s group. Following his brief work with tobacco, Paustenbach became a major figure in industry’s toxic substance science. By 2015 he had provided expert testimony in over 400 depositions and two-dozen trials. He and his company will appear in several of the latter chapters of this paper.139

Paustenbach’s initial tobacco interview appears to have occurred in early May 1989, when D. J. Doolittle and C. Coggins met with him to discuss the biological activity of ETS at the request of J. Furr of the Womble-Carlyle law firm. Subsequent activities by Paustenbach and Butler included participation in a 1989 ETS Symposium and critiques of EPA risk assessments, its 1989 assessment for ETS among them.140

Their assistance continued into the 1990s, since both were listed as scientific consultants as of August 1990 in the Tobacco Institute, Inc.’s response to interrogatories in the Department of Justice case. In October of that year, Butler submitted a critique to the EPA, focusing on the hazard

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139 Dennis Paustenbach Biography, Cardno ChemRisk Website, http://www.cardnochemrisk.com/index.php?option=com_content&view=article&id=82&Itemid=10, accessed January 23, 2015. This Dissertation will make use of depositions in several of its chapters. These forms of testimony under oath take place during the discovery phase of civil law suits, when both sides are allowed to seek out information from the opponents. They can last from as little as one hour to several days in length. Thus, they provide superb historical references for events subject to litigation. As a twenty year litigation attorney the author took or defended what he considers a considerable number of expert depositions—over two hundred depositions—yet still substantially less than Paustenbach’s.

140 TI ETS/IAQ Consultant Activity 1988 – 1990, 1 and 9 (ayw83b00) (accessed August 12, 2014). Also see W. J. Butler, "Re-Analysis of Data Provided in Fontham et al. (1994): No Increase in the Risk of Lung Cancer Associated with Adult ETS Exposure," OSHA Docket H-122, Ex. 537, 1996. The 1989 McGill University Symposium was organized and run by the tobacco industry. The symposium appears to have been simply a means to publish the presentations of the participants. According to A. Whist of Philip Morris, the goal was to “produce an impressive document that would have the potential of neutralizing two reports that are scheduled to be released near the end” of 1989. Those reports, an EPA prepared risk assessment of ETS and a detailed assessment of ETS by Professor Spitzer of Rockefeller University, “would cause substantial damage [to the tobacco industry] unless they are somehow countered.” A. Whist, Interoffice correspondence to R. W. Murray, August 8, 1989 (dnn34e00); and United States Environmental Protection Agency, Health Effects of Passive Smoking: Assessment of Lung Cancer in Adults and Respiratory Disorders in Children Public Review Draft – Comments of Independent Scientists, October 1, 1990, tabs 4 and 33 (sbe40e00).
identification step of EPA’s risk assessment process for ETS and lung cancer. He argued that the epidemiological information available to EPA was not sufficient to meet the necessary conditions for an epidemiological association between an agent and cancer. In 1996, he also provided comments to California’s Office of Environmental Health Hazard Assessment final draft of its assessment of the “Health Effects of Exposure to Environmental Tobacco Smoke.”

It is not clear whether tobacco companies paid Paustenbach for additional work. His article entitled “Important Recent Advances in the Practice of Health Risk Assessment: Implications for the 1990’s, Regulatory Toxicology and Pharmacology” made RJR’s January 15, 1990 Weekly Highlights of new publications. They especially liked his opinions concerning thresholds and a modification of how regulatory agencies should express risk estimates. His 1994 article on epidemiology and risk assessment became part of tobacco’s resource book. In the article Paustenbach and coauthors questioned potential biases in epidemiology when considering low level risks such as ETS, suggesting that better quality assurance of epidemiology is needed because “when it comes to epidemiology, risk assessment and regulation, the emperor has no clothes.”

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141 “Response of the Tobacco Institute Inc. to Plaintiff’s First Set of Interrogatories, United States of America v. Philip Morris, et al., (U.S.D.C. D.C. District) Civil Action No. 99-CV-2496, February 6, 2001, 9 (quw36b00). D. J. Doolittle Memorandum to G. T. Burger, “Re: Highlights for 2 Weeks Ending May 12, 1989” (Paustenbach interview) (zmb73d00). Interestingly, in Paustenbach’s 1990 curriculum vitae he lists his presentation at the McGill University Symposium, but does not list the Tobacco Institute as a client. Dennis J. Paustenbach, Ph.D., DABT [curriculum vitae] (ssm92d00). Paustenbach’s 2013 CV does not include clients. Neither curriculum vitae includes his comments submitted to the EPA in 1990. Dennis J. Paustenbach Curriculum Vitae, January 31, 2013 (in author’s possession); William J. Butler to Project Officer-Environmental Tobacco Smoke, October 1, 1990 (Enclosing Butler’s comments to “Health Effects of Passive Smoking – Assessment of Lung Cancer in Adults and Respiratory Disorders in Children,”) (kdf98h00). In this critique, Butler correctly wrote that to the extent possible an individual epidemiological study should control for bias, confounding, and the possibility of chance. However, Butler failed to mention that these factors are most important for individual studies and much less so for EPA’s meta-analysis that considered combined numerous studies. When studies are accumulated, biases and confounders tend to be quickly diluted or slant the analysis toward the negative, a position favorable to industry. Butler also submitted a tobacco industry funded critique to the EPA in 1996. In it he castigated one of the primary underlying studies for the agency’s proposed rule. According to Butler, the underlying bias of this study became a "source of artificially inflated statistical estimates that incorrectly indicate a positive association between adult ETS exposure and lung cancer." Philip Morris U.S.A. Comments on: Carcinogenic Effects of Exposure to Environmental Tobacco Smoke Excerpt: ETS and Lung Cancer, April 1, 1996, Section IV: CAL/EPAs Treatment of the Epidemiologic Data Overlooks a Number of Important Issues, 16-17 (in author’s possession); and W. J. Butler, "Re-Analysis of Data.”

142 William S. Simmons RJR Interoffice Memorandum to Dr. G. R. DiMarco, January 15, 1990 (in author’s possession); Dennis J. Paustenbach, “Important Recent Advances in the Practice of Health Risk Assessment: Implications for the
Paustenbach and Butler's infrequent appearances in the internal document depository resemble that of many other experts working on ETS. Like Paustenbach and Butler, numerous tobacco consultants wrote articles, letters to the editor, and attended attorney organized conferences, which were then published with funding from the tobacco industry. As one Covington & Burling memo admitted: “We ask our consultants to cover all substantial scientific conferences where they can usefully influence scientific and public opinion.”

It is almost impossible to calculate the amounts paid to most consultants in this area, since many were paid directly by Covington & Burling and other law firms, often without review by special projects. Records of payments to Paustenbach and Butler might not have reached tobacco company files and consequently would not have been produced in discovery. Attorney consultants were often paid only for work performed. As a 1990 Covington & Burling memo describes: “[O]ur consultants are not on retainer, and therefore are not paid unless and until they actually perform work. As a result, a strong list of available consultants does not in fact mean the creation of unnecessary costs; it does mean wider choice and greater flexibility.”

Covington & Burling and other law firms did not directly organize and manage most ETS consultants. Rather they formed and frequently worked through a series of non-governmental organizations (NGO) designed to appear independent and unrelated to tobacco. The Indoor Air

144 Covington & Burling London, “Report on the European consultancy programme. Philip Morris Incorporated,” circa March, 1990, (irs36b00); and Monique E. Muggli, et al., “Science for Hire,” 311. The ETS program also specifically targeted journal editors. As one commentator reported, “These strategically placed editors allowed the industry to influence the international academic literature.” Joaquin Barnoya, et al., “The Tobacco Industry’s Worldwide,” 74. Consultant journal editors were useful for obtaining publication of both articles and letters to the editor. This later measure constituted an important means of generating controversy around opposing scientific research. Attorneys limited individual scientist letter to no more than three or four a year to limit overexposure. In one case they paid a consultant $10,000.00 for one 8 page letter partially drafted by industry attorneys. D. Hanners, “Scientists Paid to Write.”
Pollution Advisory Group (IAPAG) was soon joined by a number of similarly misnamed organizations. For example, tobacco counsel provided the necessary recommendation to fund another early organization, the ACVA Atlantic study, which later became Healthy Buildings International. Attorneys used the study to demonstrate that ETS had an insignificant impact on indoor air quality. In making the suggestion for the study, they could be fairly confident of the outcome, since during the study’s design period, ACVA requested that the Tobacco Institute choose the twelve homes in each of three separate areas of the country as research sites.\[145\]

Due to the growing pressure being placed on industry by anti-smoking coalitions and the credibility gap perceived for any industry-funded study, cigarette manufacturers also advanced their agenda through varied strategies, which on the surface often appeared unrelated to smoking. Perhaps the most involved and intricate strategy involved the call for “sound science” and the cessation of the “politicization of science” by what they characterized as anti-corporate organizations. The program commenced in February 1993 when Philip Morris and the public relations firm APCO Associates launched a “sound science” agenda, with $320,000 of funding for startup operations. The plan primarily consisted of the unacknowledged establishment of “The Advancement for Sound Science Coalition” (TASSC), with a self-stated description as “a not-for-profit coalition advocating the use of sound science in public decision making.”

Covington and Burling closely participated throughout this effort. Soon after commencement of the program, Covington and Burling attorney Charles Lister stressed the importance of obtaining non-tobacco support for a successful deception as to the program’s purpose:

No one would take seriously a meeting even partly sponsored by PM in which EPA was more than one example among several. In any event, our

points can be made more effectively and persuasively if EPA is discussed within a larger context.

Consequently the program involved recruiting other industries and using other issues to obscure tobaccos role in urging governments, agencies and professional organizations to use a version of “good epidemiological practices” to make it virtually impossible to conclude second hand smoke—and other toxins—caused disease.146

Immediately after its formation, TASSC mailed over 20,000 recruitment letters—100 to key scientists. PM hid its involvement by using APCO to make suggestions about secondhand smoke and TASSC. This subterfuge worked so well that long-time tobacco consultant Gary Huber, at the time a Professor at the University of Texas Medical Center, contacted Shook Hardy attorney Tony Andrade to suggest the organization might be helpful to the tobacco industry. Andrade, also unaware of the connection, forwarded the recommendation to Philip Morris, which alerted him to PM’s involvement.147

Strategies designed to increase concern in the United States about the lack of “sound science” in health studies were not limited to establishing, funding, and guiding TASSC. Through Covington & Burling, industry also arranged for consulting scientists to write articles about the “politicization” of science. Selected articles took the form of academics “reminding” their colleagues of the deficiencies in other works and urging them to avoid politicization of scientific research.

146 Lister C. Memorandum to Dr. Reif, February 2, 1993 (No one would take) (suj87e00) (accessed September 23, 2014); and Elisa Ong and Stanton A. Glantz, “Constructing ‘Sound Science’ and ‘Good Epidemiology’: Tobacco, Lawyers, and Public Relations Firms,” American Journal of Public Health 91, no. 11 (November 2001): 1749-1757, 1749-50; and Devra Davis, When Smoke Ran Like Water – Tales of Environmental Deception and the Battle Against Pollution (New York: Basic Books, 2002): 214-9. As Ong describes in her article, “[T]he ‘sound science’ movement is not an indigenous effort from within the profession to improve the quality of scientific discourse, but reflects sophisticated public relations campaigns controlled by industry executives and lawyers whose aim is to manipulate the standards of scientific proof to serve the corporate interests of their clients.” Elisa Ong et al., “Constructing ‘Sound Science,’” 1749.

147 Elisa Ong, et al., “Constructing ‘Sound Science,’” 1750. Although PM decided by 1995 that TSSC was not an effective tool to affect anti-smoking legislation—with the consequent closure of the Coalition and website—a similar website is now run by attorney Steve Millroy, an adjunct professor at the Cato Institute, a libertarian think tank that has received funding from the tobacco industry. The website continues TSSC’s original work in debunking the science behind health and environmental activists.
These compositions included Canadian John Luik’s *Pandora’s Box: The Dangers of Politically Corrupt Science for Democratic Public Policy*, which attacked a recent EPA report classifying ETS as a human carcinogen. The article did not disclose that Luik had corresponded regularly with a cigarette official about the paper’s content and publication. Nor did it mention his collaboration with Covington and Burling in preparing a brief presentation concerning the paper for an industry-organized symposium in Japan.¹⁴⁸

In 1994 Covington and Burling alerted PM to another strategy’s potential for advancing their plan to constrain science. This involved using the Chemical Manufacturers suggested “Good Epidemiology Practices” to further tobacco interests. He stressed that the Chemical Manufacturers’ Association announced goals of making laboratory and epidemiology practices more rigorous were essentially identical to PM’s. Although disappointed in the vagueness of the Chemical Manufacturers’ Association recommended “Good Epidemiology Practices,” Covington & Burling attorney Charles Lister believed they could form the basis for a European Commission policy document. Thomas Borelli, director of science and environmental policy for PM, agreed. Such a change in policy by the European Commission, he realized, could challenge the less than helpful methodology of IARC. In adopting of the strategy, PM’s scientific personnel urged that its network of outside counsel be proactive in their efforts to remold epidemiology practices.¹⁴⁹

That June PM, its attorneys, and public relations firms began drafting a “Good Epidemiology Practices” resolution. After the seventeen Guiding Scientific Principles drafted by APCO proved too vague to be useful, Shook Hardy attorneys prepared “Good Epidemiology Practices” guidelines for the “Executive Committee” of TASSC. It promoted specific

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recommendations designed to reduce the number of positive health studies. For example, the guidelines stressed that odds ratios of 2 or less are questionable, even with a highly significant statistical association. A Philip Morris consultant then restructured Shook Hardy’s resolution to appear more like a scientist’s version of guidelines. Although the guidelines made no mention of ETS, if implemented they would effectively protect against ETS regulatory action, since it had relative risks of 1.2 and 1.3, respectively, for lung cancer and heart disease.

From 1994 to 2000, seemingly independent GEP conferences conducted by several different organizations all had connections to Philip Morris. These included two seminars conducted by Federal Focus, a non-profit research and educational foundation, which received $200,000 from Philip Morris in 1993 and up to $610,000 in 1994. Covington and Burling also collaborated with Federal Focus’ chairman, Jim Tozzi, to develop eight criteria for epidemiology. In an effort to change scientific standards of proof, these standards suggested that even relative risk associations of 2.0 to 3.0 are weak. Tozzi then urged their inclusion in all guidelines for epidemiology studies.150

_Tobacco’s International ETS Operations Abound_

The most significant ETS program managed by attorneys began in 1987, when Philip Morris initiated its Covington & Burling funded and operated international ETS agenda. It began in North Atlantic nations, with consultants located in Scandinavia, Canada, France, and other countries. Shortly thereafter, the system extended to Asia and South America. By late 1988 the scheme included 81 scientists. Funding started at $2.5 million per annum, increasing to over $3 million per annum in the early 1990s.

In an internal description of the project, Covington & Burling attorney Helmut Gaisch announced the strategy’s objective as the identification and development of scientists willing to “go beyond the establishment of a controversy concerning an alleged ETS health risk . . . to [also] disperse the suspicion of risk” (emphasis in original). Philip Morris characterized the consultants internally as “Project Whitecoat.” The scientists in the program published data “demonstrating” that passive smoking is not harmful, ventilation solves any potential problem, or tobacco smoke is insignificant compared to other indoor pollutants. In virtually every publication, industry’s role in the research was either minimized or not disclosed.151

At a Europe ETS coordination meeting on February 29, 1988, Rupp described the “Whitecoat” program’s selection process and modus operandi. They sought consultants in three classifications: 1) university staff; 2) professional consultants; and 3) members of a chartered research institution. Covington & Burling “appropriately encouraged [consultants] to prepare papers, participate in scientific societies with relevant areas of interest, and take active roles in scientific conferences.” Much of the anticipated research and opinions covered generic indoor air quality, at times even excluding ETS pollution. Whenever possible and advisable, consultants provided statements and testimony to governments and the media, as well as supported and encouraged scientific conferences friendly to the industry.152

From the very beginning, attorneys played a central role in determining which scientists and projects to fund. Attorneys made many of the initial contacts, cautiously approaching potential recruits with the assistance of the Weinburg Group. At this stage the interviewers did not mention

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tobacco, but sought genuine opinions of environmental hazards. The lawyers desired only “true believers,” not individuals simply out to make a buck. The process then proceeded to requesting curriculum vitae to rule out anyone with obvious anti-smoking proclivities.\footnote{J. Drope, et al., “Tobacco Industry Efforts,” 590; and Joaquin Barnoya, et al. “The Tobacco Industry’s,” 70.}

In their rush to organize the scientist networks, coordination at times suffered. In one telex Gaish complained of a telephone call he had received from a scientist expressing amazement that a Dr. Weinberg from Washington DC had called him with the exact questions previously asked by Gaish’s group. Gaish further remarked that his group had previously introduced the scientist to Shook Hardy and this new call made the scientist uneasy about all the attention from America. Gaish cautioned that they should not be falling over each other. Agreement must be reached on who would contact whom.\footnote{Helmut Gaisch telex to JP Rupp, November 16, 1987 (yqu78e00) (accessed September 19, 2014).}

Covington & Burling attorneys and agents permeated all facets of the program. According to a note written by BAT scientist Sharon Boyse, they even planned to “set up an office in London [specifically] to coordinate their European activities.” The author noted:

> Philip Morris strategy is perhaps questionable in some respects e.g. involvement of lawyers at such a fundamental scientific level; disadvantages in perception of what will only be perceived as a 'pro-industry' group of scientists.

She then further explained: “[consultants] general direction of research... would then be ‘filtered’ by lawyers to eliminate areas of sensitivity.” Later in the note, Boyse briefly described the worldwide extent of Covington & Burling’s reach.

> Not only are Philip Morris active in the US (via John Rupp of Covington & Burling) and the UK and Europe (via David Remes), but other Covington & Burling lawyers have also been commissioned to coordinate PM’s ETS activities in the Far East, Australia, South America, Central America, and Spain.
In her conclusion, Boyse reiterated her comments about the “excessive” attorney involvement, calling it questionable “at this very basic scientific level.”

Another BAT official approved of the plan. He believed only attorneys could manage a scientific program being carried out for public affairs reasons.

The only option is to select a group of people who have expertise in both the scientific arena and public affairs arena, and who can be trusted by the industry to manage the programme in accordance with their wishes. The only such groups of people are U.S. lawyers.

Rupp certainly agreed with this assessment. As Rupp reported to Wells, having law firms conduct the process and run the program provided several benefits. “[I]t is important to have a law firm play the role of organizing because the firm can, in the process of organization and horse-shedding individual scientists, avoid product liability problems. The law firm also can serve as a buffer between the companies and the consulting scientists, providing both distance and some opportunities for work product protection. Also, Covington is opening an office in London.”

The subsequent attorney run program began a rigorous recruitment process that provided pliant consultants who largely agreed that smoking and health questions still remained uncertain. They were then taught how to espouse the specific correct public opinions. This occurred in “horse-shedding” training programs organized by C&B. Trainers taught the correct reply for every possible media question about health and ETS. In addition to the live training sessions, recruits also received attorney-prepared training materials.

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157 J. Kendrick Wells, III memorandum to Nick Cannar, October 10, 1988 (Important to have a law firm) (psr96a99).

158 J. Kendrick Wells, III memorandum to Nick Cannar, October 10, 1988 (horse-shedding) (psr96a99); John P. Rupp, to J. R. Basso Dastuque, December 4, 1991, (training sessions) (wbb00a99) (accessed September 19, 2014); John P. Rupp to Richard Davies, July 15, 1992 (kdz38a99); John P. Rupp to Eugenio Rengifo, January, 20, 1992 (training materials) (gbb00a99) (accessed September 19, 2014); and BATCo Regional Public Affairs Training Conference, October 22-27, 1990 (gzn14a99). In their day long presentations Rupp and Boyse provided almost all of the information concerning appropriate scientific positions on ETS, health, and strategies. Rupp’s presentations included “recap of scientific issues.
The project’s benefits included having consultants with easy and early access to IARC studies and reports. This proved fruitful when in 1993 Philip Morris asked C&B to uncover information on an IARC ETS study. The request urged having the consultants “use whatever internal and external resources they may have or may know about to help us get more information on the IARC study as quickly as possible.” Through information about the study’s design, questionnaire, progress and authors’ scientific viewpoints gathered by the consultants from IARC investigators, Philip Morris compiled information about possible weaknesses in the study for later critiques. This effort proved successful when tobacco spin resulted in the media reporting that the study demonstrated no risk, even though it reported an increase of 16% in the risk of lung cancer for nonsmokers exposed to environmental smoke.159

As occurred domestically in the United States, many of the activities sponsored by C&B and other worldwide law firms operated through NGO fronts, the most prominent being the Association for Research of Indoor Air (ARIA), formed in 1988. Like many other organizations fronting for tobacco, ARIA received its funding through attorneys, in this case, C&B. Although the term “research” figured prominently in ARIA’s name, C&B did not form the association to conduct research, but rather to create the appearance of research. ARIA began a trend in which the procedures Philip Morris used in sponsoring domestic “independent” organizations was followed by international groups: “A small nucleus of committed, trusted and capable scientists” managed the group, acting as “locomotives” for projects.

At a presentation of Philip Morris’ initiatives on ETS to the UK tobacco industry, ARIA members provided contrasting views of how the association fit within tobacco company strategy. The first suggested that Aria was “totally independent,” with “no formal contact between the individuals (not to be named consultants) and the industry.” ARIA’s coordinator then eased any misapprehension the industry representatives might have received about true “independence.” To further allay any fears, he provided a schematic of how ARIA and other programs fit within Philip Morris’s ETS initiatives and explained why C&B controlled the program. It is provided below:

![Figure 1: Philip Morris ETS Activities](image)

It was suggested that the position of Covington and Burling allows the member of each group to remain independent of the industry, though all know that it is tobacco money that is funding the exercise . . . [Member’s roles included being] active in learned societies, to attend relevant meetings as observers or contributors and to provide reports on interesting papers.160

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In building layers of concealment for its consultants, C&B used ARIA to form other, more formal societies such as the Indoor Air International (IAI), an association registered with the Swiss government in 1990. While the IAI’s formation appeared independent on the surface, its initial director and journal editor, Dr. D. F. Weetman, had a direct relationship with Philip Morris. Shook Hardy attorneys also provided guidance for the fletching organization. By 1991 IAI published a newsletter in Switzerland and had sponsored conferences in France and Portugal. Near-term plans included several additional meetings with governmental agencies, universities and independent societies in France, Italy, Hungary, the Czech Republic, Greece, the Netherlands, Thailand, and Slovakia. The members also participated in numerous other Indoor Air Quality associations, providing feedback to the tobacco industry.

The IAI—soon to be known as The International Society of the Built Environment—first published its journal, *Indoor and Built Environment*, in January 1992, using papers from a May meeting organized by the IAI. University of Sydney School of Public Health professor David Game and colleagues have examined the articles contained in the journal from its commencement until February 2004. They believe “there is serious concern that the tobacco industry may have been unduly influential on the content of the journal.” In their article describing the investigation, they note the close ties between the editorial board and tobacco interests. In contrast, websites for The International Society of the Built Environment and *Indoor and Built Environment*, as well as the printed journal, do not mention their relationship with tobacco companies. In addition, the journal’s instructions for authors do not contain a requirement to declare conflicting interests. With this lack of transparency, it is not surprising that Game found:

61% (40/66) of papers related to environmental tobacco smoke that were published in *Indoor and Built Environment* in the study period reached conclusions that could be judged to be industry-positive. Of these, 90% (36/40) had at least one author with a history of association with the tobacco industry.
The article concluded with the observation that industry and its lawyers expected the journal to publish overall positive and important results. In Game’s opinion these “expectations were in large part fulfilled.”

During this same period, Philip Morris created The Center for Indoor Air Research (CIAR), similarly with close participation from C&B attorneys. As with ARIA, industry attorneys attempted to claim independence for the group. In a 1993 letter to Dr. Paul Sadler, Scientific Affairs Manager for Imperial Tobacco Limited, Rupp declared, “CIAR does not attempt in any way to influence the substance of its grantees.” However, after reading later portions of the letter, one can understand the reluctance of many to accept this claim:

In sum, while one might wish it otherwise, the value of CIAR depends on the industry’s playing an active role (1) in identifying research projects likely to be of value and (2) working to make sure that the findings of funded research are brought to the attention of decision makers in an appropriate and effective manner. CIAR is a credible and effective vehicle for conducting research that is needed to buttress the industry’s position.

From 1989 until 1999 CIAR funded at least 244 published studies. During just one year of this period (1995), Philip Morris and R.J. Reynolds paid CIAR combined dues of $6.3 million. CIAR finally ceased operations on November 16, 1998 in accordance with a settlement agreement between tobacco companies and US state attorneys general.

Perhaps CIAR’s most expensive study—and the most egregiously biased reporting—came in an examination of airline ETS for the Scandinavian airline SAS, research ostensibly organized by CIAR, but funded through C&B. While minimizing the appearance of tobacco’s control, Philip Morris, along with other tobacco companies, funded the work, conducted the experiments, and

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161 Helmut Gaisch correspondence with Geoff Bible and attachments, June 26, 1991 (nff42d00); David Game, et al., “Environmental Tobacco Smoke Research,” 804-5 and 808.

controlled publication through its attorney agents. At the study’s conclusion tobacco interests deleted significant information unfavorable to industry’s position before delivering the report to the airline. The published version further downplayed the risk, ignored health implications contained in the data, and promoted the benefits of ventilation.163

Rupp selected and coordinated the scientists undertaking the study. They included toxicologist Torbjorn Malmfors, statistician Daniel Thorburn, and occupation hygienist Arne Westlin. At the time of their selection all three served as tobacco ETS consultants. Malmfors, Philip Morris’ chief consultant in the Nordic countries, had previously helped form and direct the tobacco industry’s EGIL group (Swedish acronym for “Expert Group for Indoor Air”). Rupp maintained his consultation and advice throughout the project, from attending the initial review meeting that made changes to the study design through his editing drafts of the report. C&B provided the coordination specifically so the consultants did not appear to be working directly for tobacco companies.

One example of this editing is handwritten notes (author unknown) from a January 1989 meeting at Covington & Burling attended by Philip Morris corporate officials and Rupp. The notes expressed concerns about “inappropriate explanations,” which only served to emphasize high levels of exposure to secondhand smoke in a draft report. The group apparently called for deletion of the information before delivering the results to SAS. Rupp and tobacco scientists reviewed and edited at least two more drafts before approving the finalized report. The editors struggled over a major problem contained in the data: separation of passengers did not solve the ETS problem on airliners. One proposal to resolve this problem was making the “report more positive by including some advices for improvements of the ventilation regime . . .” In the end they excised the offending data.164

164 Covington & Burling, “Invoice for Center for Indoor Air Research,” May 30, 1989 (wys78e00) (accessed September 10, 2014); Mary Pottorff and Stig G. Carlson memo “In Flight Air Quality Tests (IFAG) on Nordic-Based Airlines”
As noted in ARIA members’ presentation to the UK tobacco industry, like US consultants, international consultant responsibilities included attendance at seminars and conferences to present tobacco’s ETS position and discover potential regulatory initiatives. Many of the “independent” conferences they attended were fronts for tobacco interests that had been organized under the auspices of attorneys. One of the most widely publicized of these conferences, the 1989 ETS symposium at McGill University in Montreal, Canada, was attended by both international and United States scientists, including Paustenbach. In all, approximately 30 industry consultants attended the conference. Organized on a fast track basis, Rupp described it as being “designed to help address some situations in the short term.” These “situations” included a new the EPA ETS risk assessment, a Rockefeller University ETS study, and the California hearing on ETS. “The hope is that the symposium will produce substantive argumentation to counter anticipated short-term problems.”

Other C&B organized conferences soon followed. The 1990 Lisbon conference “Indoor Air Quality and Ventilation in Warm Climates” maintained the public appearance of independence, despite its tobacco organization:

Our European consultants have organized and will conduct a major scientific conference in Lisbon next month on indoor air quality in warm climates. More than 100 scientists from throughout the world will attend, including some from the Asian consulting group. The conference is sponsored by a Portuguese university and two international scientific groups—all quite independent of the industry, and all made possible by our consultants.

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\(^{165}\) Covington & Burling London, “Report on the European consultancy programme,” (designed to help) (rs36b00); and J. LaRiviera memorandum to P. Dunn, et al., October 6, 1989 (mff07b00).
By late 1989 C&B attorneys could point to thirty-four conferences that consultants either had attended or were scheduled to attend from December 1988 to November 1990, most related to indoor air and health. According to a C&B memo of the period:

We ask our consultants to cover all substantial scientific conferences where they can usefully influence scientific and public opinion. They also attend many other conferences on their own, as part of their ordinary scientific activities. The conferences we ask them to attend are selected after approval from [Philip Morris Europe scientist] Dr. Gaisch and with the advice of a small group of consultants, who serve as an informal scientific steering group.\(^{166}\)

The international effort did not stop with Europe, instead expanding worldwide. In Australia, Philip Morris used U. S. lawyers to assist in an effort to prevent workplace smoking bans. Much of the groundwork came from the indoor consultancy company Healthy Buildings International (HBI). In 1987 HBI formed its Australia branch with help from a Tobacco Institute of Australia (TIA) seeding grant. As part of the early effort a team of consultants including HBI officials and John Rupp organized ETS seminars and presentations to journalists in Sydney and Melbourne during the week of February 23, 1987.

Initially, HBI used C&B to administer its Australian branch’s work. The law firm then consolidated HBI’s bills, passing them on to Philip Morris. Philip Morris’s policy at the time was to “avoid direct involvement with consultants actively working with either C&B or SH&B. Work with these consultants should be thru respective law firms.” Thus, by invoicing to C&B in this manner, HBI truthfully denied that it ever directly received money from Philip Morris.\(^{167}\)


Following TIA’s initially successful advocacy program, then devastating loses in the courts and public opinion, lawyers assumed control. With the takeover, lawyers managed the organization and assumed many of the staff positions. Like the last gasp of the Roman Republic, a triumvirate ruled: in-house counsel; Alyton Utz, TIA’s counsel; and Shook, Hardy attorneys, who had regularly corresponded with TIA since the early 1980s. Both before and after the takeover Bob Northrip of Shook Hardy required TIA to respond quickly to his advice. On several occasions he emphasized his opinions by traveling to Australia. Shook Hardy attorney Greg Fowler, also moved to Australia for a year, providing Northrip with consistent in country assistance.168

Rupp quickly developed similar programs in both South America and Asia. By years end in 1989, C&B South American and Asian consultants attended the McGill University conference and contributed to its subsequent book. One year later C&B (likely Rupp) proposed annual retainers totaling $205,000 for nine South American consultants. They also requested additional funding to support South American research and other projects.

In Argentina, C&B primarily used one consultant, Dr. Carlos Alvarez, to lobby the legislature and President. According to Rupp, as a personal scientific and technical advisor to President Menem, Alvarez “played a very useful role” in obtaining a veto over anti-smoking legislation. Other C&B reports suggest Alvarez also lobbied other Argentine officials and journalists and possibly the WHO Latin American Regional Office. Rupp believed Alvarez’s value justified paying him a retainer of $50,000.00 per year.169

Tobacco interests maintained involvement in Asia throughout the decade. As occurred in Latin America, Asian tobacco consultants maintained a close relationship with academic

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organizations, in this case the Asian Association of Occupational Health, in which several held high ranking positions. In 1992 C&B contributed $35,000 of tobacco money for a Toxicology Forum Meeting. In 1995-6 Shook Hardy paid industry consultant $235,000 to organize “Chinese projects” that may have included seminars. CIAR cohosted a July 1997 international workshop on risk assessment and good epidemiological practices in China. (At the time the CIAR financed studies included those specially reviewed by tobacco industry lawyers.) One year later The Weinberg Group, a tobacco public relations consultant, sponsored an Asian seminar.170

Conclusion

Federal judges, the media—including business oriented papers and magazines—as well as academic commentators and researchers—have all castigated the tobacco industry and its attorneys for their decades long cover-up. Yet, it is possible, although perhaps unlikely, that no prominent tobacco lawyer ever violated a state bar association ethical rule. Certainly very few, if any, were sanctioned by the courts or disciplined by their state bar for misconduct related to the defense of the tobacco industry. Even today the Shook Hardy website touts their victories in the “Engle progeny” of tobacco liability cases. But, are formal legal professional ethics the real issue? After all these years of warnings can or should plaintiffs today receive compensation in all or even some of the cases?171

Perhaps better questions are: Were the actions of the tobacco attorneys moral and just in a universal sense? Should attorneys be subject to a requirement of morality in addition to their own ethical duties? Have attorneys used tactics similar to tobacco for other toxic substance? Tobacco lawyers will undoubtedly call up the inviolate attorney mantras of “everyone deserves a defense” and “it does not matter what I think, what matters is providing my client with the best evidence possible at trial.” What goes unsaid in

171 Shook, Hardy & Bacon website, online at http://www.shb.com/, accessed on September 30, 2014.
these mantras are the vast sums of money received from clients with exceedingly deep pockets and the resulting untold harm experienced by individuals and the American public health system.

As a lawyer with over twenty years experience in mass tort litigation on both sides of the table, I can testify that the past actions of tobacco lawyers, while excessive, are not that surprising. As the following chapters will demonstrate, none of them are particularly atypical. Yet the moral question is difficult to answer. I have no ready-made solution. Attorneys, including myself, are correct in arguing that everyone deserves representation and we must, on occasion, put our own beliefs on hold to provide adequate counsel. Morality and justice cannot be decided simply in black and white terms. In many circumstances actions are better expressed in shades of gray. Even these are not universal but personal in each of us. I will continue raising these questions of morality and justice in the following chapters.

I close this chapter with the words of the Jones Day Tobacco lawyers, who knew the effects attorneys were having on tobacco science. Internally, they even acknowledged the conspiracy, describing their collective management of the malfeasance:

Almost all of the activities discussed throughout this Report can arguably be fit under the rubric of acts taken in furtherance [sic] of a civil conspiracy to suppress, omit, or misrepresent smoking and health information. Each statement or occurrence, standing alone, can be explained, but when the statements [sic] and occurrences are stacked seriatim, the combined effect is substantial.\textsuperscript{172}

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CHAPTER 6

THE ASBESTOS EXPERIENCE

Introduction: The Chronology of Asbestos Hazards Public Knowledge

During the Great Depression, companies using silica were not the only parties facing increased costs due to occupational lung disease. Approximately a decade after medical doctors, government officials, and asbestos industry management clearly understood that close work with crystalline silica could cause silicosis. They also knew that silica was not the only substance that caused severe lung disease. By the early 1930s most individuals knowledgeable about occupational lung diseases understood that close work with raw asbestos fibers in the manufacture of asbestos textiles could cause severe asbestosis, often resulting in death. Yet nearly forty years went by before the occupational medicine professional community in the United States fully accepted the hazardousness of asbestos.


2 Asbestos is a fibrous mineral found in many serpentine or amphibole rocks. There are three commercially viable forms of asbestos; amosite and crocidolite come from amphibole rock, and chrysotile comes from serpentine rock. Chrysotile also normally contains a small amount of tremolite, an amphibole form of asbestos. Amphibole fibers are straighter than chrysotile and also last in the lung longer than chrysotile. Asbestos causes both nonmalignant and malignant (carcinogenic) diseases, with most research showing that the amphiboles are usually more carcinogenic. The nonmalignant disease, asbestosis, is a fibrosis of the interior of the lungs, primarily in the lower portions of both lungs, caused by the mechanical scarring of the deep lung tissues by the fibers. Pleural thickening and calcification of the lining of the lungs can also occur. The malignant diseases include lung cancer and mesothelioma. Mesothelioma is a cancer of the lining of the lungs that usually causes death by suffocation. Asbestos works synergistically with smoking to greatly multiply the risk of lung cancer above the risk of either individual cause. During the twentieth century and especially the middle decades, thousands of products contained asbestos, including insulation, construction materials, gaskets, brakes, cloth, paper, oil drilling mud, plastics, kitchen potholders, and even dish cloths and kindergarten modeling clay. Various books contain information about asbestos and its associated diseases. Two well-known treatises provide clear details about asbestos and its diseases: Irving J. Selikoff, et al., Asbestos and Disease, and Paul F. Holt, Inhaled Dust and Disease, 68-
In 1931 Anthony J. Lanza—the public health official whom we first encountered in Chapter 4, and the then-current assistant medical director of Metropolitan Life Insurance Company—

informed Johns-Manville and Raybestos Manhattan management that eighty-seven percent of their asbestos textile workers with over fifteen years experience had scarring of the lungs. Even more disturbing was his finding that forty-three percent of workers with less than five years experience had similar scarring. There were also indications of bystander susceptibility to asbestosis, since a number of watchmen and shipping clerks at the mill had asbestosis. Although, as shall be explored later, the report was not published for another five years—and company attorneys edited the final report—this finding was not unique. During the 1930s numerous British, European, and American clinicians similarly reported asbestosis among indirectly exposed workers.³

Yet, while at least one industry study was delayed, independent hospitals and government agencies also cataloged a growing number of asbestosis cases. For example, during the early to mid 1930s John Donnelly, a physician at the Mecklenberg Sanatorium in Huntersville, North Carolina, wrote two articles describing both the progressive nature of asbestosis and the lack of controls in workplaces. His 1932 article succinctly provides his conclusions about the hazards of asbestos.

[E]xposure to the inhalation of this dust [asbestos] for even a comparatively short time is a definite and serious industrial hazard, has been too frequently indicated to be open to doubt. The fact that the condition when once acquired is permanent and more or less rapidly progressive is most important from a public health viewpoint. It also seems to be the consensus of opinion, not only among writers on the subject, but also among the asbestos workers, that the protective devices now in use in many plants are most inadequate.⁴

³ David E. Lilienfeld, “The Silence: The Asbestos Industry,” 793. These events are further explored later in this Chapter. Numerous authors have described them. See for example, David Ozonoff, “Failed Warnings,” 139-218; Robert N. Proctor, Cancer Wars, 113; and Barry I. Castleman, Asbestos: Medical and Legal Aspects, 517 (Castleman testifies extensively for plaintiff counsel in asbestos lawsuits).
By the late thirties, with the moral and financial support of the Roosevelt administration, numerous states began establishing industrial hygiene departments within their governments. Shortly thereafter, several of these state agencies, including those in North Carolina and Pennsylvania, became more active in investigating the asbestos hazards. The North Carolina State Bureau of Health commissioned the better known of these studies—a comprehensive asbestos study of textile workers—yet the Pennsylvania study was first and provided possibly even more compelling data concerning the widespread hazards of asbestos. The Pennsylvania report, issued three years before the better-known North Carolina report, also examined textile mills. It found numerous work sites with exposure levels well in excess of five million particles per cubic foot (mppcf), several of which were even well in excess of twenty mppcf. The average exposure level in areas of the textile mills handling asbestos was just under 5 mppcf. In what would have been of great significance if it had become widely known, the investigators also found very high exposure levels in work activities for other types of asbestos usage. These activities included several of great significance for the construction and automotive industries, including sawing asbestos tiles and shingles, preparing asbestos insulation material, and grinding asbestos clutches, all far exceeded the 5 mppcf exposure level. Asbestotic individuals comprised twenty five percent of the non-control group and one third of the over thirty exposed group. Over one third (15) of the negative group were under the age of thirty, whereas none of the positive group were under thirty. This study does not appear to have received much attention from industry, likely due to the investigators’ reluctance to recommend a safe concentration. “Nor is it possible from our findings to establish the maximum safe concentration of asbestos dust in the air.” Without such a recommended level, industry could not
publicize its conformance with an alleged recommend safe exposure level, nor claim it was taking actions which assured the health of its workers.  

Waldemer Dressen, a U.S. Public Health Service physician, led the North Carolina study, which became known as the “Dressen” report. In his 1938 description of the study he indicated that of the workers in the study with 100 million particle years of exposure to asbestos, half had asbestosis. Twenty percent of the workers with between fifty and ninety-nine million particle years of exposure had asbestosis, while none of the thirty-nine workers with less than fifty million particle years of exposure had asbestosis. From this information Dressen concluded that ten years’ exposure to 5 mppcf of asbestos containing dust carried a very significant risk of asbestosis. He recommended a provisional safety threshold of 5 mppcf. Thus was born the first suggested United States maximum exposure level for asbestos.

Yet, Dressen cautioned that the study was not definitive and did not provide assurances of disease prevention. His recommendation guardedly stated “5 million particles per cubic foot may be regarded as the threshold value for asbestos-dust exposure until better data are available.” Furthermore, he cautioned that the values “must be viewed with caution because they are based on small numbers of workers. This was particularly true in the groups with more than 10 years’ employment,” since even workers with less than 25 million particle year’s exposure had significantly higher rates of cough than unexposed office personnel. In addition, at least 2½ % of workers with 25 million particle-year exposures had asbestosis. At a threshold level of 5 mppcf, this was only five year’s exposure, less than a quarter of an individual’s work life. Thus, Dressen’s study actually

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5 William B. Fulton, et al., *Asbestosis: Special Bulletin No. 42*, 14, 15, 18, 19, and 28 (quote at 28). I have found no citations to this study in any industry-generated publications.
demonstrated that the recommended threshold level was not a “safe” level of asbestos exposure, for even individuals exposed throughout their work life to levels well below this value were at significant risk of contracting asbestosis.  

Throughout the 1940s, even as industry and governmental agencies publicly supported the 5 mppcf exposure level, awareness of the potential hazards of asbestos products, as opposed to raw fiber, continued to grow among both governmental agencies and industry. For example, the Pennsylvania Department of Labor and Industry issued a Safe Practice Bulletin in 1942 concerning usage of asbestos in the wire industry. General Electric, a member of the Industrial Hygiene Foundation, owned the studied plant. Asbestos came into the plant not as raw fiber, but as yarn and batting. The Safe Practice Bulletin emphasized that dust equipment was necessary even for these types of plants, which—unlike the formerly studied textile mills—did not use raw asbestos. In addition, the Bulletin emphasized that workers should take precautions, such as taking showers at the conclusion of their shift, so they did not carry asbestos fibers home. These government studies and reports, however, provided little impetus to change the recommended exposure level for asbestos. Over thirty years would pass before the Federal government took action to reduce the workday exposure level below Dressen’s admittedly provisional and inadequate level.


cancer was not a prevalent disease among the general population. Thus, journal editors found such case reports interesting. One year later physicians K. M. Lynch and W. A. Smith reported on a case of lung cancer in a man with asbestosis. The following year the editors of the *American Review of Tuberculosis* published a similar case report in which medical doctors D. S. Egbert and A. J. Geiger wrote that “the irritating effects of the inhaled asbestos particles may in this case have been a significant factor in the development of the primary lung cancer [and] seem sufficiently plausible to be worthy of consideration.”

Other reports followed, particularly in Germany. In 1938, German pathologist Martin Nordmann reported on two additional clinical observations of lung cancer related to asbestos. In 1941 Nordmann and a coauthor further reported lung carcinomas in white mice following inhalation of asbestos dust. Based on these studies, German medical writers reached consensus on the linkage almost immediately, with Germany in 1943—even amid the turmoil of World War II—declaring lung cancer to be an occupational disease when linked to asbestos exposure. Other European countries followed shortly thereafter. Within three years, the company doctor of an asbestos plant in London wrote that the link of asbestos and cancer was sufficiently strong to justify the introduction of better control measures. British doctors generally reached consensus by 1950, shortly after a report by British industrial medicine physician Edward Merewether, in which he found that at autopsy 13.2% of asbestosis cases had lung cancer.

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10 Industry knew about Nordmann’s findings the same year his report was published in Germany. By this time the Industrial Hygiene Foundation, an industry trade association discussed in the previous chapter, was collecting worldwide medical articles for occupational dust diseases and publishing abstracts of them in its Digest that it sent to all members; see Daniel C. Braun Deposition on May 1, 1980 in *Harold W. Hoover, vs. Johns-Manville*, abstract included on page 96, volume 2 of Defendants Exhibit 1 (Exhibit 1 cited on page 10 of the deposition). Nordmann was also discussed in Philip E. Enterline, “Changing Attitudes and Opinions Regarding Asbestos and Cancer 1934-1965,” *American Journal of Industrial Medicine* 20, no. 5 (1991): 688-692. Information about Britain and the London company doctor is from Geoffrey Tweedale, “Asbestos and its Lethal Legacy,” 2; and E. R. A. Merewether, *Annual Report of the Chief Inspector of
Similar information had also been independently developed in the United States. In the late thirties and early forties Leroy Gardner, a medical doctor and Director of Saranac Laboratories (whom we previously met in chapter 4), conducted numerous animal studies similar to those of Nordmann. Several asbestos manufacturing companies and their insurance company funded these studies. As early as 1942 Gardner determined that inhaled chrysotile fibers could induce malignant neoplasia in mice. By then he had also collected information on eleven cases of human lung cancer and two cases of what would become known as mesothelioma. Unfortunately, neither he nor his successors published much of this work. The work that was finally published provided either little or no information concerning cancer. Similarly, unlike prior symposium papers, the 1952 Saranac Laboratory sponsored symposium proceedings, which included extensive reports on the relationship between asbestos and cancer, were not published.\(^\text{11}\)

Thus consensus arrived much more slowly in the United States. Many doctors were wary of the foreign studies, particularly those published in Germany during the Nazi era. As William E. Smith, Assistant Professor in the Department of Industrial Medicine of New York University, stated in 1952, “The experience described by Nordmann and Sorge does not afford evidence that asbestos dust is capable of provoking tumours.”\(^\text{12}\) One year later Smith wrote a paper evaluating the claims for occupational factors in lung cancer, which typified the confused state of United States medical

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\(^\text{11}\) In 1949 Schepers visited the Saranac Laboratories for three months and was provided access to Gardner’s records. Schepers eventually became the Director of the Saranac Laboratories in 1954 and a plaintiff witness in the 1980s. Gerrit W. Schepers, “Chronology of Asbestos Cancer Discoveries: Experimental Studies of the Saranac Laboratory,” *American Journal of Industrial Medicine* 27 (April, 1995): 593-606; and Geoffrey Tweedale, “Asbestos and its Lethal Legacy,” 2-3. As is described later in this Chapter, industry lawyers kept this information from becoming public. Saranac Laboratories is the same former tuberculosis facility discussed in Chapter 4. By 1932 Gardner was its director. He died in 1946. For a description of the early work of the Saranac Laboratories, see Edward R. Baldwin, “Saranac Lake and the Saranac Laboratory for the Study of Tuberculosis,” *The Milbank Memorial Fund Quarterly Bulletin* 10, no. 1 (January 1932): 1-16.

\(^\text{12}\) William E. Smith, “Lung Cancer with Special Reference to Experimental Aspects,” *A. M. A. Archives of Industrial Hygiene and Occupational Medicine* 5 no. 5 (March 1952): 212.
opinion concerning asbestos and lung cancer. In the asbestos section of the paper, he initially noted Merewether’s findings of a much higher lung cancer rate among cases of asbestosis than among cases of silicosis. He indicated that this raised the question of “whether asbestosis predisposes to carcinoma of the lungs.” Even so, he believed that there were too few cases to provide convincing evidence. He seemingly acknowledged a causal relationship when he went on to implicitly suggest that the virtual disappearance of cancer among individuals employed in the British asbestos industry since the Merewether report was the result of better ventilation. However, he did not explicitly acknowledge that the disappearance of cancer with falling exposure rates provided strong evidence that asbestos caused the cancers. Ironically, he then noted that he had visited Merewether prior to the conference at which he was speaking “and learned that additional cases [of lung cancer] had occurred,” thus leaving open the question about whether better ventilation was sufficient to reduce any risk. His concluding thoughts on asbestos were simply that cases of lung cancer in asbestos workers “will require careful analysis with reference to the population at risk,” especially since other countries such as Finland, with shorter periods of asbestos usage, did not show evidence of increased lung cancer in the asbestos worker populations.14

Smith’s presentation of this paper at an international medical conference also demonstrated the distance between British and American doctors in accepting the linkage between asbestos and lung cancer. During the discussion phase of the presentation, a British doctor remarked that he had seen several specimens of “asbestos cancer of the lung” in the lower lobe at a medical lab in Wales. He asked Smith whether most such cancers were in the lower lobe. Smith did not appear interested

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in following up on the Welsh findings. He did not dispute the characterization of “asbestos cancer of the lung,” but simply responded that he was not aware of a special localization.\textsuperscript{15}

Still, during the 1940s and 1950s, a few American medical authorities recognized the potential hazards of asbestos, as well as other environmental toxic substances. These doctors attempted to call attention to the increasing list of reports linking asbestos and lung cancer. Wilhelm Hueper, M.D., of the National Cancer Institute, was foremost among these renegades. After immigrating to the United States from Germany in 1923, he had initially worked for Du Pont before being laid off “for economic reasons.”\textsuperscript{16}

Hueper subsequently began work on his magnum opus, \textit{Occupational Tumors and Allied Diseases}, a book based both upon his own experiences and his extensive reading of the medical literature. He published the book in 1942. In it, Hueper outlined the current evidence of asbestos lung cancers and provided several factors that were suggestive of an occupational causation. He concluded his examination with a look at “Medico-Legal and Social Aspects,” calling the evidence “sufficiently serious, and the number of persons exposed to the suspected causative agent large enough to indicate a thorough and extensive clinical, statistical, and experimental investigation of the incidence and causative interrelation of asbestosis and pulmonary carcinoma.” He repeated these same warnings throughout the following decades.\textsuperscript{17}

\textsuperscript{15} \textit{Ibid.}

\textsuperscript{16} Wilhelm Hueper’s work for Du Pont and his observations of misconduct there were previously discussed in Chapter 3 at pages 53-55.

\textsuperscript{17} Hueper was the first American researcher to understand the systemic meaning of experimental and clinical work on occupational and environmental cancers; see Wilhelm C. Hueper, \textit{Occupational Tumors and Allied Diseases} (Springfield, Ill.: Charles C. Thomas, 1942), 403-405, and the quote is at 405. The following year this work was summarized in a Bulletin of the American Society for the Control of Cancer. Wilhelm C. Hueper, “Cancer in its Relation to Occupation,” 63-69; Wilhelm C. Hueper Deposition on June 16, 1977 in \textit{Eleanor Miller vs. Raybestos-Manhattan, Inc.}, Civil Action No. 76-899, U.S.D.C., W. D. Pa., 21-22. Hueper Papers Collection, MSC 341, Archives of the National Library of Medicine, Bethesda, Maryland.12. A detailed survey of Hueper’s work is found in Christopher Sellers, “Discovering Environmental Cancer,” 1824-1835.
By 1948, Hueper’s pioneering efforts in occupational and environmental cancers achieved results. That year he was appointed the founding director of the Environmental Cancer Section of the National Cancer Institute (NCI). He remained at this post until 1964. Throughout this career at the NCI Hueper stressed the importance of occupational carcinogens, including asbestos, to the growing level of cancers in society. For example, in an address before the Cancer Prevention Committee in New York on June 15, 1949, Hueper used asbestos as an example of how carcinogenic substances can shift the normal sex ratio of cancer incidence. He told the audience that Merewether’s investigations provided many of the answers about asbestos and lung cancer. Although at the time the lung cancer male-female ratio was 5:1, in his study of asbestos workers, Merewether found the sex ratio to be only 2:1. In his 1951 article on occupational disease hazards, Hueper again made specific reference to the excessive incidence of lung cancer found by Merewether in his cases of asbestosis. Hueper used this example to demonstrate that plant physicians, by careful examination of the excessive rate of cancer among special groups of workers, frequently obtained clues concerning lung cancer causation.  

His 1955 monograph on environmental causes of lung cancer went even further, noting the sustained case reporting of lung cancer in asbestotic patients during the 1930s through the 1940s. He also noted that epidemiologic studies had found higher incidence of lung cancer among asbestotics. In discussing this possible connection, Hueper acknowledged that several doctors were skeptical of asbestotics having an excess risk of lung cancer, including Lanza, Vorwald, Warren and Cartier, with two more undecided. But a larger number—seventeen—believed the causal link either established or highly probable. One year later he published a chart of cancer deaths from occupational carcinogens. Asbestos, with 112 known deaths, was third on the list, following only radioactivity (625 cases) and

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chromates (140 cases). In further support of his opinion, Hueper noted that in autopsies of asbestotics, thirteen point two (13.2) to twenty (20.0) percent had lung cancer compared to the general population’s rate of only point eight (0.8) to two point four (2.4) percent.\(^{19}\)

A few other occupational medicine doctors followed Hueper’s lead. In a 1952 American Medical Association journal, noted occupational doctor William E. Smith described his visit to the United Kingdom to survey their findings on occupational cancers. Over five pages of his twenty-page paper specifically covered asbestos. His initial sentence concerning asbestos described how the British Inspector of Factories had noted lung cancer in 13.2% of the recorded asbestosis cases over the past twenty-three years. Doctor S. Roodhouse Gloyne, of the Institute for Social Medicine in Oxford, permitted Smith to cite information he had gathered from asbestos worker lungs sent to him for examination. Some 14% of the asbestos workers with asbestosis had lung cancer. Even 8.3% of another group of 169 asbestos workers without pneumoconiosis had lung cancer. These rates were a startling contrast to the 1.36% rate estimated for the general population.\(^{20}\)

May R. Mayers, chief of New York’s Division of Industrial Hygiene Medical Unit, similarly sounded the alarm. In a 1952 presentation before a Workmen’s Compensation course at New York University, he noted the growing understanding of the relationship of asbestos and cancer:

Silica and Asbestos are of special importance among the dusts which fall into this category [Pneumoconiosis]. The environmental conditions for their development are essentially similar. Nevertheless, silicosis is not, apparently associated with, or productive of lung cancer, whereas asbestosis very probably is.

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\(^{19}\) Wilhelm C. Hueper, *A Quest into the Environmental Causes,* 36-37 (As noted earlier, Lanza worked for the industry’s insurance company and headed the industry friendly IHF medical committee; Vorwald did considerable work for industry; and Cartier was medical director for a group of Canadian asbestos mines.); and Wilhelm C. Hueper, “Environmental Causes of Cancer,” 141.

In the same presentation he also cautioned about the inappropriateness of using threshold or maximum exposure levels for carcinogens.\(^\text{21}\)

As Dr. Smith’s report suggested might be the case, British doctors were more willing to make the connection when they spoke in the United States. The best example might be noted British medical researcher A. I. G. McLaughlin’s 1955 Saranac Lake “Occupational Chest Disease” Conference discussion of the status of research into dust diseases in Great Britain also fully supported Hueper’s position on the relationship of asbestos and lung cancer. “The connection between asbestosis and cancer of the lung is becoming clearer, and in one series of 100 autopsies on asbestosis cases there were 25 cases of cancer of the lung,” McLaughlin declared. He also noted reports of asbestosis appearing in other occupational groups such as pipe laggers and insulators.\(^\text{22}\)

That same year a significant development occurred that had the potential of being a turning point in asbestos knowledge. Richard Doll—considered by some to be the leading epidemiologist of his era and later knighted by Queen Elizabeth—demonstrated that British asbestos workers suffered high rates of lung cancer. As with many other articles linking asbestos and cancer, the asbestos industry (in this case Turner and Newell of Britain), rejected its own doctor’s request to publish his and Doll’s article. When Doll published the findings on his own, company attorneys unsuccessfully attempted to have the journal editor reject the article, presumably because it might have hurt their profits.\(^\text{23}\)

\(^{21}\) May R. Mayers, “Industrial Cancer of the Lungs,” *Compensation Medicine* 4 (March-May 1952): 12, 14-15. Mayers also gave a talk on occupational diseases to the 146th annual meeting of the Medical society of New York. This talk, which also listed asbestos as a probable carcinogen, was published in May R. Mayers, “Occupational Disease Diagnosis,” *New York State Journal of Medicine* 52, no. 19 (1952): 2381-2385. Given the intended audiences of these journals—individuals concerned with workmen’s compensation and all New York doctors—it is highly likely that attorneys and doctors for most companies, as well as the IHF staff, read at least one of them.


This study, while considered definitive in Europe, did not have an immediate impact in the United States. Henry Stokinger, a well respected industrial hygienist, offered one possible explanation in the 1956 *Annual Review of Medicine*. In this article he stressed that, unlike what occurred in European countries, asbestos did not necessarily cause lung cancer in the United States. While he acknowledged that in both Europe and the United States, sixteen percent of individuals autopsied upon dying from asbestosis to date had lung cancer—a much higher percentage than normally found at autopsy—this in itself was not sufficient proof. Rather, he looked at the claimed distinctions in the rates of cancer between British and American asbestos workers, as well as what he considered a possible explanation for the distinctions. “It is of more than passing interest that the higher rate of cancer in asbestos workers in England is not paralleled in the United States or in Canada, according to Lanza,” Stokinger noted. “The cause for this difference may lie in the type of asbestos…” Stokinger apparently accepted the Canadian mining industry’s claim that chrysotile, the form of asbestos found in Canada and predominantly used in the United States, did not have the same hazardous characteristics as did crocidolite or amosite.

An epidemiologic study published less than two years later seemed to buttress Stokinger’s position. Two medical doctors, Daniel C. Braun and T. David Truan, both of the Industrial Hygiene Foundation, conducted this study for the Quebec Asbestos Miners Association (QAMA) and published it in 1958. In their report, Braun and Truan indicated that they had studied every asbestos miner employed in 1950 with five or more years of exposure. They examined the death
rates of this 6000 man cohort over five years and found nine confirmed and three suspected lung
cancers. The nine confirmed cancers were only slightly above the expected rate of eight lung cancers
for Quebec Province.\textsuperscript{26} Ironically, only one year before, the June 1957 \textit{Industrial Hygiene Digest} had
summarized a Danish report, which had found that 22 of 31 long-time insulators, a group whose
exposures were likely substantially lower than that of miners, had abnormalities of the lung.\textsuperscript{27}

The \textit{Archives of Industrial Health} accepted the paper for publication—likely because the editor,
innovative and controversial industrial hygienist Herbert E. Stokinger, did not believe in an
association of asbestosis with lung cancer. As he wrote to the authors:

I, myself, was particularly pleased to learn the main conclusion of the paper
was against the association of lung cancer with asbestosis, for I had come to a
similar conclusion on obviously less information but was afraid to say so for
this reason. I am enclosing a review which contains a few sentences that I have
marked in this connection that appears in the Annual Review of Medicine,
Volume 7, 1956. You will recall at this time evidence greatly favored the
positive correlation of lung cancer on exposure to asbestos.\textsuperscript{28}

The Braun Truan Report and Lanza’s opinions remained effective into the 1960s. For
example, in 1960 the \textit{Archives of Industrial Health} published yet another case report of an asbestos-
related cancer. In his review of the medical literature the article’s author, John Anderson, M.D.,
missed many of the European cases and the Saranac Lake animal research. In his discussion of
causation, he noted that Hueper felt very strongly about the connection between asbestos and
cancer, but, like many American doctors who were not familiar with European research or
confidential Saranac animal research, Anderson was unwilling to make a “final assessment of the
relationship between asbestosis and lung carcinoma.” That same year an author of a medical

\begin{flushright}
\textsuperscript{27} Industrial Hygiene Foundation, “Literature Abstracts,” \textit{Industrial Hygiene Digest} 21 (June 1957), 16.
\end{flushright}
textbook referenced the Braun Truan study in support of the position that “asbestos had not been found to be carcinogenic.”

The mid 1960s proved the turning point for medical opinion on this issue in the United States. In 1963, Thomas F. Mancuso, M.D., former chief of the division of industrial hygiene, Ohio State Department of Health, and a coauthor published an epidemiologic study of workers at a textile plant linking asbestos and lung cancer. Although this paper provided significant evidence of the linkage between asbestos and lung cancer, the enormous epidemiologic study of insulation workers published shortly thereafter by Irving Selikoff, M.D., of New York’s Mount Sinai Hospital, quickly overshadowed it. At the 1964 international conference on the “Biological Effects of Asbestos,” held under the auspices of the New York Academy of Sciences, Selikoff presented the results of a study covering approximately ten thousand insulation workers. Selikoff found not only a large incidence of asbestosis in the cohort, but also a very significant level of lung cancer. Two decades after German health researchers had reached this conclusion, and almost a decade following a similar conclusion in Great Britain, U.S. specialists finally sensed the tide had turned.

Numerous other papers presented at the Conference also documented the hazardousness of asbestos. Twenty-three of the sixty-five Conference presentations dealt with asbestos and cancer. Twenty of the papers supported the relationship of asbestos and cancer, with the remaining three expressing no opinion. For most doctors, this conference settled the question about whether

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asbestos causes lung cancer: an overwhelming number of researchers had independently established causal links between asbestos and lung cancer.\textsuperscript{31}

Since most of its members were attending the New York Conference, the International Union Against Cancer convened a working group on asbestos and cancer during the conference sessions. The working group, which included numerous government, academia, and industry experts from nine countries, published opinions stating that both carcinoma of the lung and mesothelioma were demonstrably associated with exposure to asbestos dust. The group also noted that its findings “suggest that a more serious and widespread hazard from exposure to asbestos dust may exist than is widely appreciated.” It recommended in part that epidemiologic studies be extended to the insulating industry, the asbestos products industry, and other locations where asbestos was regularly used, including certain factories and the building industry.\textsuperscript{32}

Shortly after the Conference, a groundswell began building for lowering the asbestos maximum exposure level (at this time the exposure level was called the Threshold Limit Value (TLV). This may have resulted from New Jersey State Department of Health physician E. Lynn Schall’s New York Conference presentation. Schall offered a powerful argument for lowering the recommended level of asbestos exposure, after first reviewing the history of the TLV. He wrote that in adopting this level the American Conference of Governmental Industrial Hygienists (ACGIH) had only relied upon some animal studies and one epidemiologic study, that of Dressen in 1938. Schall then argued that Dressen’s study was both flawed and preliminary. The flaws were many and varied. The types of dust sampled included rock dust; a contaminant that greatly increased the overall particle count. The counts also varied dramatically between different operations. About

\textsuperscript{31} The articles and papers are contained in “Biological Effects of Asbestos,” \textit{Annals of the New York Academy of Sciences} 132, no. 1 (December 31, 1965): 1-766. For the importance of the Conference see Philip E. Enterline, “Changing Attitudes and Opinions,” 693.

months prior to the study, approximately 150 workers had been replaced. The researchers did not examine the majority of these individuals because they could not be found. The plants also had an abnormally large number of individuals who had worked less than five years—333 of 511—seventy-three of whom had worked for less than a year. Furthermore, even Dressen regarded the number as only a working figure. He had indicated that the values “must be viewed with caution because they are based on small numbers of workers. This was particularly true in the groups with more than ten years’ employment.” Schall argued that this fact was of special importance because, as Merewether had previously discovered, even with a smaller concentration of dust, fibrosis could still develop fifteen to twenty-five years later. Finally, after almost thirty years had passed following Dressen’s research, occupational medicine professionals had undertaken the research that the Report rightfully acknowledged was necessary. The results confirmed that, as Dressen had realized might be the case, his suggested exposure limit was woefully insufficient. 33

Asbestos Science: The Handmaiden of Litigation

Why did this understanding take so long to gain traction in the United States? In his 1991 review of asbestos and cancer, Philip Enterline, at the time a well respected Emeritus Professor of Biostatistics at the University of Pittsburgh Graduate School of Public Health, suggested this was due to several reasons, including, among others, the unpopularity of Germany among Americans after World War II, the adverse attitude of American leading experts such as Lanza and Vorwald, and the lack of experimental evidence. Although Enterline did not suggest that Lanza’s or Vorwald’s opinions might have been affected by their industry sponsored work, he did suggest one additional related factor for the delay in his conclusion: “…since the asbestos industry probably exercised some control over research… findings unfavorable to the use of asbestos were clearly not in their

interest.” Enterline, for reasons that are later explored, left unsaid the probable outcome of this situation: if a finding was not in the economic or litigation interest of industry, it either would not be published or would be modified to suit the funding agency. Historical evidence suggests this is exactly what happened.

Indeed, with the considerable help of its lawyers, throughout much of the twentieth century industry managed to keep the most critical information about the hazards of asbestos, including its own studies, from becoming accepted by either the occupational medicine community or the public. The deception began almost as soon as employee asbestos cases began appearing and involved most, if not all of the major companies, with Johns-Manville attorneys taking the lead. From the medical side Anthony Lanza, Leroy Gardner, and the Industrial Hygiene Foundation played key roles in this process.

Lanza, in particular, provided great assistance to industry’s defense of asbestos litigation. As discussed previously, Lanza had joined the Public Health Service, as a young physician in 1907, becoming Chief Surgeon to the Bureau of Mines in 1914. After leaving the Public Health Service in 1920 as a recognized pneumoconiosis expert and being hired in 1926 by Metropolitan Life, Lanza began directing industrial hygiene surveys for asbestos companies as part of his duties. Although Lanza subsequently expressed concern about the health of workers and their families and claimed to “have no axe to grind,” these duties quickly brought him into the litigation science fold.

In this administrative position, Lanza became closely involved with corporate attorneys at least by 1929. This assistance continued throughout Lanza’s career at Metropolitan Life and included all phases of litigation and workmen’s compensation. In November of that year he sent a bibliography on asbestosis to Mr. Allan Wardwell, attorney with the Wall Street law firm of Davis

35 A. J. Lanza memoranda of February 27, 1935, to Dr. Armstrong, Third Vice President of Metropolitan Life, obtained at Johns Manville Repository, Denver, Colorado and in author’s possession.
and Polk, Johns-Manville’s outside corporate counsel. The following year he wrote to Wardwell about the proposed definition of asbestosis for the workmen’s compensation schedule then being codified in New Jersey. However, the closest connection came through a Metropolitan Life multi-year investigation into health hazards in asbestos mines, mills and fabrication plants in eastern Canada and the eastern seaboard of the United States, which commenced in 1929.30

One year later, Frank Pedley, a Metropolitan Life physician, wrote an editorial in a Canadian medical journal about asbestos. In the article, he made the claim, “no cases of specific disease have been reported among asbestos workers in the Province of Quebec.” Pedley may have been right in a strictly technical sense, but his statement was certainly disingenuous. That year he sent an unpublished report to his superiors that belied the hopeful tenor of his editorial. In it he described that almost 50% of the miners he had recently examined had asbestosis: “of the 54 men examined at Thetford Mines [in Quebec Province] 24 were diagnosed by x-ray as suffering from pneumoconiosis (asbestosis), of these, 4 were diagnosed as probably first stage asbestosis, 8 as early stage, 9 as definite first stage and 3 as second stage.” Despite this finding—which may have come following the editorial—Pedley did not attempt to correct his journal statements. This failing was but a presage of events to come.37

Lanza completed the initial industrial hygiene survey for Johns Manville around 1931. He sent the report to Wardwell, who also received a similar report for the Canadian mines and plants prepared by Lanza’s superior, E. McConnell, M.D. Although Lanza’s survey found asbestosis in the


vast majority of the long-term workforce, Lanza informed Wardwell that he “understood that this report is confidential and it will be given no publicity by us except with the consent of the firms concerned.” In early 1932, probably as a result of the survey, Lanza recommended to Johns-Manville officials that they seek inclusion of asbestosis under the workmen’s compensation laws. As the minutes of the meeting held to discuss the issue described: “[Lanza] is very strongly of the opinion that asbestosis should be made compensable… He feels that this is the only protection which the industry has… permitting the disease to remain outside the compensable class lends encouragement to unethical lawyers and physicians to work up claims.” Lanza understood that lawsuits posed a much higher risk for the insurance company’s assets than did workmen’s compensation cases.38

Even while Lanza worked on the full investigatory report and urged Johns Manville to seek inclusion of asbestosis in Workmen’s compensation, he also—as he was required to undertake as an insurance company employee—assisted Johns Manville attorneys in the defense of their employee asbestos related lawsuits. Following the 1932 meeting, Lanza helped Johns-Manville’s general counsel, Vandiver Brown, in obtaining agreement from F. V. Meriwether, a U.S. Bureau of Mines doctor, to review x-rays for pending asbestosis lawsuits arising out of the Manville, New Jersey plant. In a letter following up on Lanza’s contact, Johns-Manville’s Vice President for Mines and Productions informed Meriwether of the confidentiality requirements for the readings:

You will readily appreciate that we desire the readings to be held in strictest confidence and that no unnecessary publicity be given to the fact that you are making these readings for us… we wish, if possible, to prevent the results of our efforts being used against us either in the pending suits or in any suits which may be brought against us.

38 The no publicity quote is from A. J. Lanza letter to A. Wardwell, March 20, 1931, Laura Bialy Exhibit 11; E. J. McConnell letter to A. Wardwell, July 9, 1931 Laura Bialy Exhibit 13. The only protection quote is from Minutes of July 15, 1931 meeting between Lanza, McConnell, and several Johns-Manville officials, Laura Bialy Exhibit 14; and David E. Lilienfeld, “The Silence: The Asbestos Industry,” 792.
After Meriwether agreed to the conditions, Lanza informed Meriwether that Brown would coordinate the logistics of the readings. Following this initial effort, in 1933 Lanza met at least twice more with Johns-Manville officials and attorneys about pending litigation and research designed to assist in the company’s defense. Further meetings in 1934 resulted in Lanza agreeing to approach Philip Drinker, a Harvard University professor in Industrial Hygiene whom we first encountered in Chapter 4, for dust control recommendations. The attorneys believed Johns Manville would be in a better litigation position if they could say the company had received recommendations and followed them.39

In 1934, Dr. Lanza submitted a preliminary draft of the proposed survey article to Johns Manville and at least one other company for comments. Johns Manville General Counsel, Vandiver Brown coordinated the industry response. The comments “suggested” numerous changes to the manuscript, of which Lanza incorporated all, or virtually all. Most of the comments related to industry’s concerns about workers’ compensation claims, and litigation, and their desire to have a published article to assist counsel.

Thus, the Johns Manville attorneys and other company agents requested that certain aspects of the original report favorable to industry that had been removed from the publication draft be retained, as well as elimination or modification of other sentences less favorable. For example, they requested that Lanza explicitly draw a distinction between silicosis and asbestosis, with the later being portrayed as less serious. Two other important sentences in the draft failed to find their way into the final manuscript: “However, it is possible for uncomplicated asbestosis to result fatally;” and

39 S. A. Williams to F. V. Meriwether, February 26, 1932, Laura Bialy Exhibit 16; A. J. Lanza Letter to F. V. Meriwether, March 24, 1932, Laura Bialy Exhibit 17; and David E. Lilienfeld, “The Silence: The Asbestos Industry,” 792-793. Even while Lanza was assisting Johns-Manville attorneys in defending asbestos cases, he remained skeptical of their claims that Canadian asbestos was fairly innocuous. As he wrote in 1937 to another asbestos expert, “I have always had the feeling that [the Canadians’] argument was motivated by self-interest rather than to make a scientific contribution.” A. J. Lanza to Bowditch, 13 Dec. 1937, cited in Geoffrey Tweedale and Jock McCulloch, “Chrysophiles versus Chrysophobes,” 246.
“It is possible that asbestosis may cause pneumoconiosis more readily than the silica (SiO2).” The references also failed to include the most significant studies to date, those of Dr. E.R.A. Merewether in England, even though they were well known to Lanza.

The subdued publication, along with its failure to clearly disclose other relevant publications, well fit into the industry’s desires. As the president of Raybestos-Manhattan wrote to Brown, “I think the less said about asbestos, the better off we are . . .” Brown replied, “I quite agree with you that our interests are best served by having asbestosis receive the minimum of publicity.”

The key understanding that should be taken from these events is not that Lanza, Meriwether, or Drinker were doing anything ethically inappropriate for this period, but that almost all of this work was being conducted at the behest of attorneys, and designed, at least in part, to develop defenses for lawsuits, rather than to advance medical science. As requested by company attorneys, the medical personnel kept confidential the only significant non attorney-directed research activity, the original survey. Given the legal ability of attorneys, through their attorney work product privileges, to withhold such information from either the public or the courts, we may never know how many other similar circumstances there are of public officials assisting companies in their lawsuits and then not using the information to further either medical science or appropriate regulatory action. The “matter of fact” actions of all parties to these events certainly seem to point to such activities being generally accepted and perhaps commonplace during the 1930s. As a result, published reports of these and other company sponsored activities must be considered very cautiously, for they certainly only provide the information which the companies wanted to be

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released, that is, information that would be helpful in improving their profits by limiting and defeating lawsuits or other similar costly events.

The fear of “unethical lawyers and physicians,” and thus reduced profits for Metropolitan Life, might have been what led Lanza to reach the conclusion that he should not only keep the survey confidential, but also assist in deceiving Johns Manville employees. In 1933, in response to a Johns-Manville physician’s question about informing employees of the asbestos hazard, Lanza reportedly responded, “I doubt if the hazard is sufficient to justify warning posters. . . . This is especially true in view of the extraordinary legal situation.” Apparently the potential of eighty-seven percent of long-term employees suing Johns-Manville was more important to Manville’s insurance carrier representative than informing younger employees about the significant risk they faced from working with asbestos. For neither the first nor the last time, the legal aspects of the problem influenced the medical response.41

With the long-delayed 1935 publication of Lanza’s Johns-Manville industrial hygiene surveys, this deception grew to include the misleading of medical professionals and the public. After receiving the galley proofs of the study, Brown became concerned about the report’s characterization of asbestosis as a serious disease and discussed proposed changes with one of the company’s outside counsel. Within a week the lawyer sent Brown a marked-up copy of the study, remarking that “one of our principal defenses… has been that the scientific and medical knowledge has been insufficient until a very recent period to place upon the owners of plants or factories the burden or duty of taking special precautions against the possible onset of the disease to their employees.” Brown forwarded these comments to Lanza with his confidence that “I trust that you will give his comments and suggestions, as well as those mentioned in my letter, your most serious consideration.” Brown then provided the classic attorney covering statement: “I am sure that you

41 S. A. Williams letter to A. R. Fisher, August 29, 1933, Laura Bialy Exhibit 25.
understand fully that no one in our organization is suggesting for a moment that you alter by one jot or tittle any scientific facts or inevitable conclusions revealed or justified by your preliminary survey. All we ask is that all of the favorable aspects of the survey be included and that none of the unfavorable be unintentionally pictured in darker tomes than the circumstances justify.”

The most substantive change requested by the lawyers reincorporated a sentence that had been in the original draft but left out of the final paper. This sentence described asbestosis as a milder disease than silicosis. Initially, Lanza had believed that this sentence was, in fact, true. However, by the time the final paper was ready for publication, he was well aware of British reports of asbestotics dying much earlier than silicotics. Thus, he had left it out of the final paper. Notwithstanding this knowledge, when the paper was published as a Public Health Report, the sentence was again included. In agreeing to its reincorporation, Lanza likely believed that the original sentence, as modified in the final paper, “Clinically, from this study, it appears to be of a type milder than silicosis,” was literally accurate, if not wholly forthcoming. Nor, apparently, did he believe the attorney involvement in the changes detracted from the authority of the article. A year later he sent a reprint of the article to Wardwell, noting, “the Public Health Reports publication gives this piece an authoritative and dignified presentation.”

Lanza also assisted Johns-Manville’s lawyers and its depression-era profitability by expressing opinions that minimized the risk faced by employees of United States companies that used asbestos. He did this by claiming that British reports about asbestos hazards were not relevant to the United States because of differences in the asbestos being used in the two countries and the dustier

42 V. Brown letter to A. J. Lanza, December 21, 1934, Laura Bialy Exhibit 55.
conditions found in British factories. Indeed, in his 1936 article, he wrote, “it is possible that the English factories may be more dusty than ours.” The actual facts were somewhat different. Eighty percent of the asbestos in Britain in 1930 came from Canada, the same location that United States companies obtained most of their asbestos. Furthermore, Merewether had already indicated that the same type of machinery was used in both Britain and the United States, but dust control equipment “seems to be more generally used [in Britain] than in American factories.” Since, unlike the United States, by the 1930s Britain had regulations controlling dust levels in British plants, Merewether was likely correct in his assessment.44

However, although Lanza’s actions kept some important asbestos hazards information from being made public and helped downplay foreign medical reports, published asbestos disease studies and case reports continued to increase during the early 1930s. By the mid 1930s the brake industry, significant manufacturers and purchasers of asbestos textiles for brake linings, decided they could no longer ignore the increasing number of case reports linking asbestos and other diseases. They were particularly concerned about the claimed linkage between asbestosis and tuberculosis. On November 9, 1936, Leroy Gardner of Saranac Laboratories met with Lanza, Brown, and other executives to discuss the possibility of animal studies. Nine days later the parties signed a contract for Gardner’s laboratory to conduct animal experiments on the effects of asbestos dust for almost a dozen companies. Brown established stringent confidentiality conditions for the experiments:

It is further our understanding that the results obtained will be considered the property of those who are advancing the required funds, who will determine whether, to what extent and in what manner they shall be made public. In the event it is deemed desirable they shall be made public, the manuscript of your study will be submitted to us for approval.45

Gardner began the experiments in 1937. The interim results placed Johns-Manville and the other sponsors in a quandary. The asbestosis found in the animals was considered mild; however, 81.8% of the animals had developed lung cancer. One sponsoring company commented, “we feel that the reference to the question of cancer susceptibility should be omitted from the report since it is inconclusive.”

This work also put Gardner in a quandary. While he understood the work was confidential, Saranac Laboratories required a continued stream of funding. Without the Laboratory providing information about its range of ongoing work to companies, funding would not be forthcoming. Thus, as had occurred in 1933, the lure of publication and publicity for his laboratory resulted in Gardner being reminded by Brown that his work was confidential. When the Saranac Laboratory Annual Report of 1938 mentioned the Laboratory’s “asbestosis” work, Brown wrote about it to Raybestos-Manhattan’s President, Sumner Simpson, including copies of the report and an additional article on pneumoconiosis written by Gardner. Simpson quickly responded that Gardner did not appear to be living up to the agreement not to publish without prior consent. Gardner was subsequently reminded that the corporate attorney written contract he had signed did not permit publication. Fortunately for Gardner, despite the undesired publicity, the companies continued funding the study for several years, but without a completion date. The final report was only in draft stage when Gardner died in 1946. It was finally published, without the cancer reference, after Brown told Lanza to “handle the matter,” although one study participant claims this was not the result of sponsor pressure. A subsequent Saranac study, which also found increased cancer among asbestos-exposed mice, was never published.46

46 During this period Gardner continued his work for mining companies, including some asbestos companies. One of the reasons companies continued using Gardner, despite his missteps, might have been his willingness to closely collaborate with attorneys from these companies. Examples of this were previously provided in Chapter 4. In yet another example of this, on May 6, 1942 Ivan Sabourin, chief counsel of Johns-Manville’s Canadian mining subsidiary, sent a medical report to Gardner that he (Sabourin) had “taken the liberty of drafting for you . . . for the benefit of my
Thus, by the mid 1930s, asbestos product manufacturers and miners, like the silica industry, were concerned about the growing body of evidence demonstrating the hazards created by the dust of their product. The increasing asbestos disease lawsuits and workmen’s compensation claims undoubtedly caused even greater concern. Therefore, when members of the silica industry suggested that action was needed to eliminate their litigation crisis, asbestos-manufacturing companies took notice. When two hundred fifty industry executives met at the University Club of Pittsburgh for a “Symposium on Dust Problems” on January 15, 1935, at least one asbestos representative was in attendance. Several prominent attorneys, as well as Lanza, Sayers, Pendergrass, and Drinker, spoke to the assembled representatives. Asbestos giant Johns Manville’s representative, attorney Vandiver Brown, kept a low profile at the meeting, as he later acknowledged, not wanting to attract the public attention that was then being directed toward silica exposures.

As Brown’s subsequent memo clearly demonstrates, although he wanted to keep a low profile, he also wanted to help develop the direction and program for a proposed industrial hygiene foundation. At the meeting, Brown began by pointing out “that the members of the Asbestos industry did not care to be associated in the minds of the public or of employees with those industries whose problem was silicosis and for this reason [he] felt that there might be some opposition to having a representative of the Asbestos industry working with them.” However, he agreed to serve on a temporary committee, subject to veto by his company or the asbestos industry, so long as “cooperation could be worked out without an undue amount of publicity.” At the close of the meeting seven attendees, including Brown, were elected to form a temporary committee tasked with considering the options and report back to the groups on their recommendations.  

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47 Vandiver Brown, “Memorandum re: Mellon Institute of Industrial Research Symposium on Dust Problems – Pittsburgh,” January 15, 1935, Johns Manville Repository, Aurora, Colorado and Author’s personal collection. (This memorandum has been used and authenticated in a number of asbestos lawsuits.) Ironically, two years previously, on...
Although Brown may have stayed in the background at the initial meeting, once the symposium group decided to form the new industrial hygiene agency, subsequently known as the IHF, he quickly publicized its formation. Shortly after the new foundation’s first meeting, Brown wrote to the editor of *Asbestos*, an industry trade magazine, extolling the IHF’s usefulness to industry.

Although the [IHF] is approaching various problems relating to air hygiene from an unbiased viewpoint, it is nevertheless the creature of industry and is the one institution upon which employers can rely completely for a sympathetic appreciation of their viewpoint. Its aim is to take an honest effort to appraise the evil, to advance scientific knowledge of many of its doubtful aspects and to suggest remedies, both preventative and curative. As such, it deserves and should receive the unqualified support of all members of industries faced with a dust hazard.  

Similar to what occurred within the silica industry, much of the asbestos industry responded.

The IHF was inexpensive to join. According to a past President of the IHF, any company with an interest in occupational health could join. Even small companies could afford admission, since the Foundation dues operated on a sliding scale based upon the number of employees. From the 1930s through the 1960s the Foundation maintained a United States membership of about 144 companies, with small turnover each year.

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48 Vandiver Brown letter to C. J. Stover, 4 December 1936, in David Ozonoff, “Failed Warnings,” 197; While there is little information available about his specific actions, as a member of the IHF Legal Committee Vandiver Brown was well situated in the 1940s to assist in the control of lawsuits and ensure that crucial information became available to companies in their defense of worker compensation claims. Daniel C. Braun Deposition on December 9, 1976 in *Evelyn Rodeman, et al. v. Combustion Engineering, Inc., et al.*, Case No. C72-390, et al., USDC for the Northern District of Ohio, Eastern Division, 39. Brown was likely the individual who ensured representation of asbestos companies on the initial Temporary Committee formed at the symposium to develop the guidelines for the new industry trade association, the Industrial Hygiene Foundation. Vandiver Brown, “Memorandum re: Mellon Institute of Industrial Research Symposium on Dust Problems – Pittsburgh”; and David Rosner and Gerald Markowitz, “Workers, Industry, and the Control,” 35.

Little changed in the asbestos industry’s position as the 1940s progressed. Brown remained as a central figure in asbestos research through the 1950s. Often, Metropolitan Life forwarded x-ray interpretations directly to Brown, rather than company doctors. Through its Legal Committee and the various Legal Bulletins, the IHF kept companies fully informed about the various legal defenses available to them for asbestos and other work related diseases. Additional bulletins and digests provided abstracts of the increasing number of studies and case reports discussing toxic substance hazards and the growing recognition of a possible link between asbestos and cancer. Although companies could use the reports to consider changes in their operation, corporate attorneys also found them knowledge about them very useful, since the studies and reports threatened to limit industry’s traditional defenses to workers compensation claims and lawsuits. Efforts to provide counterarguments to these studies may also be the reason the Legal Committee often suggested speakers and programs for joint medical and legal meetings and seminars sponsored by the IHF.  

By the early 1940s, the IHF was distributing a significant amount of information to its members concerning asbestos and other hazards. In his 1977 deposition the former President of the IHF, Daniel C. Braun, M.D., listed the benefits of the IHF as embracing monthly and annual publications, courses and meetings, and numerous services including toxicological, industrial health, air sampling, and engineering. Closed-door medical meetings that the IHF held for its members supplied one easy method by which the information was distributed. These meetings, such as the one planned in 1948 “for physicians associated with the asbestos industry” were occasionally mentioned in the IHF bulletin sent to member companies. In 1950 the IHF and the University of Pittsburgh School Of Public Health even jointly sponsored a postgraduate course for company

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medical directors and physicians on Pulmonary Disease and Employment. The IHF supplied digests included abstracts of most of the then current medical and scientific articles concerning silica, asbestos, and a number of other hazardous materials. Although the actual records detailing the IHF’s advice to individual members were destroyed in 1979—a period when plaintiff counsel had begun seeking records about industry knowledge—given the state of knowledge at the time, it seems likely that by the 1940s the IHF advised individual members similar to the previously described advice that the Pennsylvania Department of Labor gave to General Electric concerning asbestos product hazards and safety precautions, including the necessity of dust control equipment and other worker precautions, including showers before leaving work so dust was not carried home.

Despite this available information, there is but slight documentation of any company making sincere attempts to even comply with the voluntary standards suggested by Dressen. In a 1976 interview, Dohrman Byers, a knowledgeable and long-experienced industrial hygienist, described the problem facing anyone seeking to improve industrial health standards. At the time, in addition to the IHF, a few state agencies were conducting industrial hygiene surveys. Almost all of them were voluntary because state attorneys believed they would lose if an agency attempted to seek court approval of an inspection. Even when inspected, most companies “promptly ignored it most of the time…” Federally sponsored surveys of a number of shipyards during World War II also

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52 Robert L. Holtz, Ed., Occupational Disease Prevention. Many company records also may have been destroyed during this period. Although Westinghouse claims the proposal was never implemented, in 1980s advice similar to tobacco attorney initiatives, Jeffrey J. Bair, an attorney for Westinghouse, recommended that most correspondence and industrial hygiene files be discarded. His reasoning for this recommendation came from the increasing litigation and the “smoking gun” documents found in many of the files. As he noted in his conclusion to the review of documents, as a result of the potential for increased litigation, “well reasoned and conceived document retention and destruction programs for departments such as Industrial Hygiene, and in fact the entire Corporation, are imperative.” Jeffrey J. Bair and C. W. Bickerstaff, “untitled and undated review of Westinghouse Documents,” 03239699 to 03239720, Westinghouse Archives, Pittsburgh, Pa; and Jeffrey J. Bair memorandum to C. W. Bickerstaff, February 9, 1988, 03239696 to 03239698, Westinghouse Archives, Pittsburgh, Pa.
53 This was another common trait of companies using either silica or asbestos. Robert L. Holtz, Ed., Occupational Disease Prevention, and Charles D. Yaffe, “Interview of Dohrman H. Byers on May 17, 1976,” Annals of the American Conference of
highlighted this problem. In his study of wartime shipyards, Drinker first inspected the Bath Iron Works shipyard in Maine during 1942. There he found conditions that presented “a real asbestos hazard.” Hoping that the conditions at Bath Iron Works were not typical, he scheduled inspections at four other East coast shipyards. While the various yards varied in their conditions, they all exceeded the recommended five-mppcf exposure limit by wide margins. However, due to the recent dramatic war-related increase of workers and their high turnover, only a limited number of asbestosis cases were found. Of course, given the limited time period involved and the frequent departure and turnover of workers during World War II, this would be expected. Unfortunately, after the limited finding of asbestosis, few, if any, U.S. shipyards changed work practices until the 1960s.54

Shipyards were not the only facilities to receive the attention of health officials during World War II. The federal War Production Board tasked W. G. Hazard, director of the Industrial Hygiene Division of the New Jersey Department of Health investigator to look into reported health problems at a New Jersey Johns Manville plant. He asked Metropolitan Life to conduct a survey. Hazard closed the investigation after they reported back that the problem had been adequately controlled for a number of years, even though their own surveys showed a continuing problem throughout the war.55

In contrast to this limited government involvement, most health surveys remained in the hands of industry. Throughout, World War II, as well as after, IHF staff continued surveying members’ plants upon request. The first in-plant dust investigation had occurred as early as 1938.

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During the first eight years of its existence, dust exposure investigations totaled over forty percent of 
the IHF investigations. One third of the total investigations involved plants working with silica or 
asbestos, one quarter with mining or processing ore, and one quarter with chemicals, with the 
remainder in miscellaneous categories. In 1946 alone they conducted ten dust investigations. The 
IHF announced in 1953 that it had conducted more than 600 "special studies" for its member 
companies. Of course, given companies desire of secrecy due to the potential of lawsuits or 
workmen’s compensation claims, it is unlikely that companies would have allowed the IHF into the 
plants without both parties understanding of the necessity of confidentiality. Since the investigations 
were conducted confidentially, the IHF did not publicize the findings. Nor have I found any 
evidence that the results were anonymously passed on to state industrial hygiene offices.56 Still, in 
light of these investigations and those by states such as Pennsylvania, by the late 1940s the IHF— 
and, with a high degree of probability, most of the asbestos products industry—knew that many of 
the activities involving asbestos in industry could create exposures sufficient to require precautions 
be taken to protect their workers.

Although most of the investigations from this period may have been destroyed, at least one 
1940s investigation demonstrating this likelihood has survived. This 1947 report for North Carolina 
textile manufacturers, which surfaced through asbestos litigation discovery, clearly documented just 
these asbestos hazards. Since most of the study’s sponsors were members of the Asbestos Textile 
Institute (ATI), an industry trade association founded in 1944 to promote the usage of asbestos 
textiles, it was that institution which reviewed the work. In the report to the ATI, the IHF’s head 

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56 Industrial Hygiene Foundation. *History of Industrial Hygiene Foundation* (Pittsburgh: Mellon Institute, 1956), 2-5, 12-14, 
18; T.F. Hatch, W.C.L. Hemeon, M.S. and F.R. Holden, “Findings from Foundation Plant and Laboratory Research, 
Foundation Archives, Carnegie Mellon University, 27-32, 27, 28, 31, and 32; and Barry I. Castleman, *Asbestos: Medical and 
Legal Aspects*, 727. IHF appears to have given honest internal advice to the industry. At least one of the reports, a 1947 
investigation of asbestos textiles, criticizes one of Lanza’s investigations for not reviewing older x-rays to determine if 
there were changes, prior to reaching conclusions about the rate of asbestosis. Plaintiffs Answers to Metropolitan Life 
engineer, William C. L. Hemeon, provided information both on the magnitude of the asbestosis problem in several textile mills and on methods of dust control. Furthermore, in the report Hemeon demonstrated that industry knew about the limitations of Dressen’s report. In particular, Hemeon expressed little confidence in the five-mppcf threshold limit value then in place.

The information available does not permit complete assurance that five million is thoroughly safe nor has information been developed permitting a better estimate of safe dustiness. It is nevertheless of the greatest importance either that such assurances be sought or a new yardstick of accomplishment be found for accurately measuring any remaining hazard in the dust zone below five million for the elimination of future asbestosis depends on the degree of control effected now.  

Hemeon suggested that a review of x-rays at the North Carolina textile factories—which were required to take annual x-rays of textile workers—would help determine if the five million exposure level was safe. In addition, he suggested that a general x-ray and medical survey of workers in one or two plants with a long history of good dust control would be of great value. There is no record of a follow-up investigation, nor did anyone publish Hemeon’s report. The reluctance on the part of the ATI may have been partly due to Hemeon’s discovery of a twenty percent rate of asbestosis among workers exposed to dust levels of only two mppcf. This was not information calculated to warm the heart of industry attorneys. As Enterline personally knew and properly observed in his 1991 article, publication was not in industry’s interest, since it would have eliminated one of its key asbestos litigation defenses.

Consequently, neither the industry nor the IHF took any action to either release the information to other researchers or to systematically study the health problems associated with asbestos. One individual associated with the IHF, Theodore F. Hatch, did note in a paper presented to the IHF membership that in many industries there was no systematic knowledge about the

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58 W. C. L. Hemeon, Report of Preliminary Dust Investigation for the Asbestos Textile Institute, 20; and David Ozonoff, “Failed Warnings,” 198. Enterline’s personal experience will be further examined later in this chapter.
correlation between lung damage and the dust exposure of particular activities. He further acknowledged a need for more extensive information. Yet, he did not suggest that the IHF gathered information be made either publicly available or correlated.

These in-plant dust studies had begun as early as 1938 and included major projects in the 1940s. Although the IHF’s self-published history repeatedly emphasizes how these surveys and investigations assisted companies by “fostering healthful working conditions . . . [and] advancing harmonious labor relations,” there is also no evidence that any of the other IHF studies for specific companies were ever published. Nor does the history provide any details about the IHF’s work in the area of asbestos or its diseases. This lack of publication concerning plant conditions and workers’ health, even on an anonymous basis, raises the question about whether the surveys were for the purpose of advancing medical science and protecting the workers or rather to protect the companies from workers’ claims and lawsuits. The fact that almost every company which made or sold asbestos products has now been sued, and none has used IHF surveys from any period to demonstrate in court that they met what they believed were the appropriate standards, suggests with high probability that almost all of the surveys found instances of high levels of dust in work areas.59

Throughout the 1940s the asbestos industry sought to control this growing knowledge about asbestos. Although both numerous in-plant surveys and at least two animal studies were then ongoing, industry representatives did not discuss their knowledge in forums where it could become public. For example, information about annual meetings of the IHF was published in an industry-controlled journal, *Industrial Medicine and Surgery*, yet its 1950 synopsis of the Symposium contains not

59 T.F. Hatch, et al., “Findings from Foundation Plant,” 27-32, 27, 28, 31, and 32; The quote “fostering healthful working” is from Industrial Hygiene Foundation, *History of Industrial Hygiene Foundation*, 12. This quote also suggests that another purpose of the surveys might have been to keep workers satisfied that they were being protected; see Barry Castleman, *Asbestos: Medical and Legal Aspects*, 727. During the author’s twenty year experience as both defense and plaintiff counsel in asbestos litigation, he is not aware of any additional IHF studies being put into evidence in his or other cases.
one comment about asbestos. This was the case despite the fact that one of the speakers, Arthur Vorwald, was in charge of the laboratory that had informed brake manufacturers about the substantial number of lung cancers that developed in mice during inhalation studies examining the effects of the inhalation of asbestos. Since industry conducted its investigations and research confidentially and rarely released survey information to the public, medical professionals not privy to insider information had limited, if any, knowledge about research that was funded by companies or about the true conditions in the plants.

This control of information was vital to industry’s main concerns, defending workmen’s compensation occupational disease claims and lawsuits and making a profit. Release of either the true conditions in plants, as shown by Hemeon, or the results of the animal tests entailed the potential enormous consequences at trial or adjudication. The failure of many legislators to recognize the true nature of silica and asbestos diseases remained industry’s greatest defense to workmen’s compensation claims even into the 1960s. As late as 1963, the IHF’s legal committee was informing industry attorneys that for dust diseases such as asbestos “frequently disability does not occur until many years after the termination of employment or exposure,” and at the same time recommending that they continue to seek legislation that allowed workers to file claims for only two or fewer years following employment.⁶⁰

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⁶⁰ Industrial Hygiene Foundation, “Industrial Hygiene Foundation Annual Meeting-November 15 and 16, 1950,” Industrial Medicine and Surgery 20, no. 1 (January 1951): 38-39; Three years later the same journal suggested that a typical question during the joint medico-legal panel of the 1953 Conference was not about the hazards of dust disease, but rather “What steps should be taken to secure general cooperation among compensation attorneys, industrial physicians and commissions to eliminate the racketeering lawyer and the unethical medical witness?” The medical panel at that same Conference included talks on “stress disorders,” “Problems associated with older workers,” and “Compensation for Residual Disability.” Industrial Hygiene Foundation, “Industrial Hygiene Foundation Annual Meeting-November 18-19, 1953,” Industrial Medicine and Surgery 23 (January 1954): 20; and Theodore C. Waters, Current Status of Compensation for Pneumoconiosis, Legal Survey Bulletin No. 4 (Pittsburgh: Industrial Hygiene Foundation of America, Inc., 1963), 16-17. At an early 1940s meeting between Charles H. Roemer (President of asbestos product manufacturer Unarco) and Johns-Manville President Lewis Brown and his brother, Vandiver Brown, one of the brothers told Roemer that Unarco managers were fools to notify employees about asbestosis, stating that Johns-Manville saves a lot of money by not telling them. Charles H. Roemer Deposition on April 25, 1984, In the Matter of Johns-Manville, Et al. v. the United States of America, U.S. Claims Court Civ. No. 465-83C; Barry Castleman, Asbestos: Medical and Legal Aspects, 401.
The 1949 leak of Saranac Laboratory information about the industry-funded mice inhalation study demonstrates the extent to which corporate attorneys went to keep industry information confidential. Upon learning of this leak, Johns Manville attorney Vandiver Brown traveled to South Africa in an attempt to retrieve a document that mentioned Gardner’s work. The incident involved a thesis written by Gerrit W. H. Schepers, M.D., ScD. At the time Schepers, who would in 1954 become Director of the Saranac Laboratories, was in the United States as a Commonwealth Fellow from South Africa. In 1949 he had spent three months at the Saranac Laboratories as a representative of the South African government while also pursuing post-graduate studies. In his thesis for a New York University diploma, Schepers had mentioned Gardner’s animal work on asbestos and cancer that he had learned about while at the laboratory. After his oral examination, Lanza, then a professor at NYU and a member of Schepers’ graduate committee, took Schepers to see Brown, at the time the chief attorney for both Johns-Manville and the asbestos consortium that had funded Gardner’s research. At the meeting, Schepers learned that Brown had been given the original of his thesis. Brown also asked Schepers for any copies to which Schepers replied that a copy had been sent to the South African government. Brown immediately flew to South Africa to retrieve the report from the Department of Mine officials. Gardner died in 1946 with the report still unpublished.

Following Gardner’s death, the Quebec Asbestos Mining Association (QAMA)—largely controlled by Johns-Manville—agreed to a proposal by Dr. Arthur Vorwald, Gardner’s successor to study cancer in rodents, in which he hoped to prove cancer incidence would not be increased after asbestos exposure. The study received intense observation and control from Johns Manville attorneys. The attorneys inserted precise language in the contract requiring confidentiality, with prepublication review and comments of any publications.

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Vorwald sent an interim report to QAMA in May 1952, after the mice had been exposed to asbestos dust inhalation for fourteen months. In the report, Vorwald indicated that, while the results were not statistically significant, there was a trend toward greater tumors in exposed animals than controls. He intimated that continued exposure would demonstrate asbestos carcinogenicity. Unsurprisingly, the study was never published and no final report has been found.62

Thus, the activities of Johns-Manville, the IHF, and other asbestos companies from the 1930s to the early 50s demonstrated the very danger of manipulation of science by those with strong economic interests that Hueper had warned against in 1943.

Industrial concerns are in general not particularly anxious to have the occurrence of occupational cancer among their employees or of environmental cancers among the consumers of their products made a matter of public record. Such publicity might reflect unfavorably upon their business activities, and oblige them to undertake extensive and expensive technical and sanitary changes to their production methods and in the types of products manufactures. There is, moreover, the distinct possibility of becoming involved in compensation suits with extravagant financial claims by the injured parties. It is therefore, not an uncommon practice that some pressure is exerted by the parties financially interested in such matters to keep information on the occurrence of industrial cancer well under cover.63

With no discussion, there was little call for changing the asbestos exposure standards. Throughout the 1940s, 50s and 60s, an informal body of hygienists, the American Conference of Governmental Hygienists (ACGIH), worked with industry experts to provide guidelines on exposure limits to numerous toxic substances, including asbestos. The ACGIH had been formed as an independent entity in late 1937 and early 1938 at the urging of the United States Public Health Service. Its purpose was to promote industrial hygiene and coordinate activities between the states and federal government. The first meeting was held in June 1938. Shortly thereafter, the Committee

62 J. F. Woodard, Esq. to Ivan Sabourin, Esq, December 1, 1950, Box 09-0034, Johns Manville Archives; Vandiver Brown to J. P. Woodard, November 15, 1950, Box 09-0034, Johns Manville Archives; Vandiver Brown to J. F. D. Rohrbach (President Raybestos-Manhattan), April 19, 1948, Box 04-01866, Johns Manville Archives; and Barry Castleman, Asbestos: Medical and Legal Aspects, 67-68.
on Threshold Limit Values began to function, but it was not formally established until 1941. Establishing such exposure limits or guidelines had already attracted the interest of industry, which sought to keep the guidelines as high as possible. Although there is no direct evidence that this occurred for asbestos, the 1946 standard simply followed the Dressen’s figure of five mppcf of total dust, without further consideration. What is important to bear in mind is that, given the general lack of new publically available statistics, exposure limits for asbestos did not change until 1968. In retrospect, relevant professional experts found that the failure to tighten the standards of this period resulted from a lack of evidence due to industry’s control of the data. During the 1980s a committee of the Industrial Medical Association acknowledged that the failure of either groups like the IHF to make their investigations public or of industry to follow up on the initial investigations greatly hindered efforts at establishing scientifically based exposure limits for asbestos.

No one seriously questions that factual data derived from human experience are by far the most reliable basis for TLVs [Threshold Limit Values]. The only significant source of such data is industry. Unfortunately, data of this kind are rarely available to ACGIH – either because industry has not taken advantage of its unique opportunity to develop them or because they remain hidden in industry’s files for any one of several reasons.

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In the early 1950s, U.S. industries involved in asbestos production and utilization faced their greatest crisis to date. Case reports on asbestos related lung cancer were becoming increasingly prevalent. Abstracts contained in the IHF files include a number of 1940s and early 1950s German articles concerning asbestos and cancer of the lower lobe of the lung. In 1949 the official journal of the American Medical Association contained a short article about the relationship of asbestos and lung cancer. One year previously the British Medical Journal had acknowledged a growing impression of a linkage between lung cancer and asbestos, listing reports from France, Germany, and Scotland.

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64 David Ozonoff, “Failed Warnings,” 193-6; and Committee on Industrial Hygiene and Clinical Toxicology of the Industrial Medical Association [AAIPS], in M. E. La Nier (ed.), Annals, 141, quoted in David Ozonoff, “Failed Warnings,” 199.
Less than two years later, after Merewether’s detailed report of asbestosis and cancer, British doctors generally accepted the linkage. Another German case report in 1954 discussed asbestos being found in the tumor tissues of what, by its description, was most likely mesothelioma.\(^{65}\)

By 1950, company insiders also fully understood that asbestos presented a much greater hazard than was commonly known. In 1950 the medical director of Canadian Johns-Manville, a subsidiary of the largest United States asbestos manufacturer, wrote to his president that “there seems to be increasing proof that asbestos fibers do cause lung cancer ... this whole subject could cause our companies unlimited embarrassment and untold expense if labor leaders made use... of the subject.”\(^{66}\)

Since even companies without an internal medical department could join the IHF and then rely upon it to keep them confidentially informed about the medical literature, there seems little doubt that all companies either understood the hazards of asbestos or chose to not examine the evidence. Beginning in 1937, as noted above, companies had received this information from the Industrial Hygiene Digest, sent exclusively to all member companies. It provided companies with a detailed review of the current medical, legal, chemical, toxicological and engineering literature. For example, the January 1950 Digest abstracted an article concerning asbestosis found at necropsy. That issue also observed that reports found carcinoma of the lung in the studied asbestotics at seven times the incidence of the general population. In addition, the IHF and its member companies had

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66K. Smith to Lindell, 28 January 1950 cited in David E. Lilienfeld, “The Silence: The Asbestos Industry,” 795. Ironically, that same year a Raybestos lawyer demonstrated the manner in which lawyers used industry-managed asbestos medical science in their favor. At a deposition of Hueper for a case involving a Raybestos brake lining plant worker with asbestosis and lung cancer, the attorney asked Hueper to justify his opinion about the linkage of asbestos and lung cancer, since the American medical literature did not have any publications showing that asbestos and lung cancer were linked. The attorney apparently did not mention the asbestos animal studies conducted for brake manufacturers, including Raybestos, in the 1940s that demonstrated lung cancer; nor did he mention the abstracts of international asbestos/lung cancer articles contained in the Industrial Hygiene Digests that were received by all members of the IHF. Wilhelm C. Hueper Deposition on June 16, 1977.
access to their own investigation data, such as that reported by Hemeon and Gardner. By 1952 the legal department of Johns-Manville was even recommending that a warning label be put on its bags of asbestos fiber. However, in meetings with government officials—such as the one with NCI staffers, including Hueper, in 1955—industry officials lied. After that meeting the NCI concluded that “there have been no asbestos-cancer animal experiments.” Because they lacked the industry-held data, federal officials, unions, and even standard setting bodies such as the ACGIH did not realize the full extent of the hazard.\(^\text{67}\)

When the IHF received requests for information from outsiders, they simply passed them on to the companies. For example, in 1955 J. Kane of the International Association of Heat and Frost Insulators and Asbestos Workers Union wrote to the IHF asking about the occupational disease risks in the insulation trade. Rather than either providing the information IHF officials simply passed the letter on the Johns-Manville for response.\(^\text{68}\)

In a scenario reminiscent of the silica industry’s efforts of the 1930s, the asbestos products industry’s program of silencing critics, co-opting experts, and keeping research confidential largely succeeded in the United States during the early to mid 1950s, with industry’s position dominating the public and medical dialog. As with silica, asbestos was viewed as a disease under control. A manual published by the Magnesia Insulation Manufacturers Association (MIMA) (an asbestos insulation products trade association) indicated that asbestos “offers no hazard to the worker.” Moreover, although reports of lung cancers among asbestotics were in increasing, the lack of United States public recognition for the linkage kept asbestos companies from having to defend against lawsuits involving cancer.\(^\text{69}\)

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\(^{68}\) Barry Castleman, *Asbestos: Medical and Legal Aspects*, 731.

One example of the effects of this control was published in the 1956 *Annual Review of Medicine*. In this *Annual*, the well-respected industrial hygienist Herbert Stokinger completely accepted the asbestos industry’s position. He began by noting that to date sixteen percent of individuals autopsied upon dying from asbestosis had lung cancer. He continued, however, by dismissing its importance: as already noted, he relied on Lanza to argue that “It is of more than passing interest that the higher rate of cancer in asbestos workers in England is not paralleled in the United States or in Canada… The cause for this difference may lie in the type of asbestos...”\(^\text{70}\) The important point here is that Stokinger indicated he received his information from the very insurance employee who knew that there were other indications and reports, not publicly available, that strongly suggested asbestos caused lung cancer. We can only speculate what Stokinger might have said if Lanza had passed on his or Gardner’s work with the asbestos companies or had provided Stokinger with the information contained in IHF digests.

Yet given the scope and range of the new information being published, including Doll’s 1955 British report and Hueper’s continual reviews of occupational cancer, simply withholding information was deemed insufficient. In addition to DuPont’s efforts to discredit Hueper, there is also some inferential evidence that the asbestos industry tried to stop publication of his papers. For example, in 1952 Hueper presented a paper on occupational pulmonary cancer at the Seventh Saranac Lake Symposium. Although it was customary for the proceedings to be published with funding received from industry sponsors, in 1953 the Symposium’s chairman, Vorwald, informed Hueper that the proceedings would not be published since “he had difficulty obtaining the necessary funds from the sponsoring organization for printing the proceedings.”\(^\text{71}\)


Despite their pervasive influence among prominent writers, industry leaders recognized that Hueper’s opinions could not go unchallenged. They thus sought ways to undermine his influence and decided to take affirmative action. The Asbestos Textile Institute, the same group for whom Hemeon had prepared a report, became the lead agency in this effort. At an ATI meeting in 1956, Kenneth Smith, M. D., Johns-Manville’s Medical Director, suggested dealing with Hueper by embarking on “a program of investigation and publicity to counteract the unfavorable publicity presently directed to the asbestos industries as a result of the work of Doctor Hueper.”

In his attempts to achieve greater evidence about asbestos, Hueper inadvertently provided the industry with the means to undermine his influence. In 1954 Hueper went to Canada in an effort to publicize the fight against occupational cancer. At the first Canadian Cancer Research Conference, he described asbestos as being among his industrial carcinogenic substances. During the meeting, the statistician for the National Cancer Institute of Canada expressed concern about cancer in the Quebec asbestos mines. He told Hueper that he planned to conduct a survey of the area. Hueper offered to help. Unfortunately for Hueper, the North American asbestosis manufacturers seeking to curtail Hueper’s influence in government owned some of the largest mines. They immediately took action to forestall a Canadian governmental study by offering to conduct their own. Employees of the IHF undertook this Quebec Asbestos Mining Association (QAMA) funded study. In time it became infamously known as the previously mentioned Braun Truan report.

The QAMA investigation was not the only survey proposed during this period. Approximately one year after the commencement of the QAMA study, both Braun and Schepers presented proposals to the ATI for a similar study. They both also told industry that textile mills had a significant hazard of lung cancer. Braun, for example, wrote to Hugh Jackson, manager of Johns-

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72 David Ozonoff, “Failed Warnings,” 204.
73 Barry Castleman, Asbestos: Medical and Legal Aspects, 113.
Manville’s Industrial Health Program on August 23, 1957, thanking him for providing information about an upcoming ATI meeting and informing him that a literature survey revealed “that the possibility of an association between lung cancer and asbestosis is much more likely to exist in asbestos factories than in mining operations.” On October 6 of that same year Schepers gave a three hour speech at an ATI meeting, seeking approval for of a proposed survey of their operations and informing them “that they, as asbestos producers, stood accused by the medical literature of manufacturing products that can cause cancer and that they were the only ones that could obviate this tragedy to their employees and customers by taking proper steps to safeguard them from asbestos dust exposure.” The ATI rejected both proposals.

The program was considered ill-advised at this time due to its implication that a relationship existed between asbestosis and carcinogenic development, a condition which, to date, has not been established although it has been given rather widespread publicity in the press.\(^\text{74}\)

QAMA was already taking great efforts to ensure that its ongoing survey did not simply add to this widespread publicity. From the very beginning, company lawyers were intimately involved with the Braun Truan study and likely directed most, if not all, of its facets. The IHF sent the first letter concerning this potential investigation to Ivan Sabourin, General Counsel for both Johns-Manville Canada and QAMA. It sought to arrange for a preliminary visit by Braun to Canada. Sabourin was one of three Johns-Manville employees with whom Braun held discussions prior to the commencement of the study. These discussions occurred in Canada over the span of three days starting on February 19, 1956. The first meeting, held at Sabourin’s home, occurred that first day. Although Johns Manville’s medical director, Kenneth Smith, was present, during the meeting Sabourin provided the “broad outline of the need for an epidemiological study, the interest of the

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Quebec Mining Association in such a study.” Sabourin was also apparently very familiar with the IHF’s medical research facilities, since he, rather than Braun, provided the group with an outline of the facilities of the IHF. Sabourin was also present during the February 20 meeting with the City Health Officer from Montreal, as well as the subsequent lunch with Verdun’s City Health Officer and afternoon meetings. Subsequently, Sabourin attended most, if not all, of the three-day meetings. During the last day, Braun had a summarizing meeting with Sabourin, after which they visited a local physician “to clear up a point which Mr. Sabourin had missed at the Wednesday evening dinner meeting.” Four months later IHF’s managing director sent Sabourin a letter seeking confirmation of the study’s acceptance and asking if the first part of July was a satisfactory starting date.

These events plainly point to the attorney, Sabourin, being the prime mover of the study—but even more important, his activities did not cease once the IHF received approval for the study. Although Sabourin may not have been quite as closely involved in all of the details of the subsequent study, as the investigation progressed, Braun met and talked with Sabourin on numerous occasions. Sabourin also arranged many of Braun’s meetings with other individuals, as well as Braun’s social calendar during his Canadian visits. Furthermore, upon completion, the report and twenty copies were sent, not to the head of QAMA, but directly to Ivan Sabourin.  

Clearly, Sabourin was not only the prime mover but also the driving force behind the study. The study’s ultimate purpose was not science or medicine, but legal.

The final report purported to show that cancer rates among miners was little different from that of Quebec Province as a whole. This result, however, was misleading. At the time medical professionals knew that the high rates of cancer found in cities significantly raised the overall cancer

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rates of Quebec Province. The rural sections of Quebec Province (such as the area in which the miners lived and worked) had appreciably lower rates of cancer. Indeed, Braun and Truan’s study of miners had found a death rate among them due to lung cancer that was two and a half times higher than that found in rural Quebec. More importantly, they found an even higher cancer rate in the lungs of deceased asbestotic miners. Autopsies determined that four of thirty-two miners who died of asbestosis also had lung cancer. In their initial report, the authors acknowledged the consistency of this highly significant rate with other studies that had examined the link between asbestosis and lung cancer. They then attempted to explain it by suggesting that perhaps asbestosis was being under-diagnosed at the asbestos mines. Still, they concluded, “the results suggest that a miner who develops the disease asbestosis does have a greater likelihood of developing cancer of the lung.”

Unfortunately, but not atypically, the report in this form was never published. Following QAMA’s review of the report, Sabourin met with Braun to produce a “condensation” for publication. The authors subsequently agreed to delete all reference to the relationship of lung cancer to asbestosis or rural sections of Quebec. Sabourin obviously understood the precipice awaiting the industry if the original report had been released. It not only confirmed Hueper’s opinions, but might also appreciably lessen the likelihood of successful company outcomes in asbestos lawsuits and workmen’s compensation hearings.

Asbestos industry officials acknowledged the problems that this reduction in the report caused. They also recognized its necessity for litigation purposes. For example, Medical Director Smith wrote to the QAMA attorney about a condensed version he received:

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76 Saxon Graham, Madeleine Blanchet, and Thomas Rohrer, “Cancer in Asbestos-Mining and Other Areas of Quebec,” *Journal of the National Cancer Institute* 59, no. 4 (October 1977): 1140; Barry Castleman, *Asbestos: Medical and Legal Aspects*, 114-115. It is extremely important to correctly pick or determine the control group when conducting an epidemiologic study. Increased disease rates in the studied group can only be determined when risk factors are controlled and the group is compared to the appropriate background level in another comparable group that does not have the specific risk factor being studied.

We have noted the deletion of all references to the association of asbestosis and lung cancer in this condensation. While we believe that this information is of great scientific value, we can understand the desire of the Q.A.M.A. to emphasize the exposure of the asbestos miner and not the cases of asbestosis...It must be recognized, however, that this report will be subjected to criticism when published because all other authors today correlate lung cancers and cases of asbestosis.

In a confidential note, written shortly after the report was completed, John Know, medical director of Turner & Newell—a large British asbestos firm—agreed that the study was flawed and actually demonstrated a higher incidence of cancer. Indeed, Know wrote that Braun and Truan “adopted a criterion of exposure that was bound to reduce their overall incidence of carcinoma, asbestosis, or anything else which affected a small proportion of long duration workers.” On the other hand, the chief counsel for Canadian Johns-Manville praised the article. He thought it had “wonderful public relations value.”

As previously noted, Herbert Stokinger, the editor of the Archives of Industrial Health, published the paper, describing his pleasure in reading the conclusion of the paper. Since he did not think there was an association of asbestosis with lung cancer and was searching for confirmation of his belief, he may not have closely examined the paper’s methodological problems.

A few public health officials, however, were highly critical of the report. In a letter to the Director of the NCI, Hueper complained about harassment, using the Braun Truan report as one of his primary examples. In it he indicated that for “quite some time” he had gained an impression that his difficulties in investigating environmental hazards resulted from “extra-governmental influences” that were more concerned with “practical economic implications” than with protecting the

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78 Letter from K. W. Smith to I. Sabourin of December 20, 1957 quoted in David E. Lilienfeld, “The Silence: The Asbestos Industry,” 797; Barry Castleman, Asbestos: Medical and Legal Aspects, 117; Ivan Sabourin letter to QAMA of June 22, 1958, Laura Bialy Exhibit 3200; The asbestos mining industry was also aware that by 1957 Saranac Laboratories had reported to the industry on an additional seventy cancers and four mesotheliomas among Canadian miners. Saranac Laboratories were not permitted to publish the results. G. W. Schepers, “Re: ‘Changing Attitudes and Opinions,’” 465.
American people from environmental cancer hazards. In this fifteen-page letter, Hueper listed numerous examples of harassment. He specifically noted that he had been requested to accept the findings of the Braun Truan report, when he had specific knowledge of at least thirty lung cancer cases found by Vorwald, and Schepers that the IHF study did not include. He went on to explain exactly why he could not accept the Braun Truan report’s findings:

I would ... consider it scientifically dishonest, if I would accept as correct, as suggested to me, the recently published allegation of [the QAMA/IHF study] concerning the frequency of asbestosis cancer of the lung among Canadian asbestos workers because I know that the total number of such cases is much higher than ... those cited by these authors who worked under the sponsorship of the American asbestos industry... My friends at the Ministry of Labor, Quebec, which has handled cases of asbestosis cancer, moreover, have assured me that [the authors of the IHF study] have not contacted them for information in asbestosis cancer. Under these circumstances I feel that I am not only under no obligation, but in fact also I would commit a scientific offense if I would honor the statement of [the IHF report] as anything more than specially manufactured scientific merchandise of shoddy quality.  

Although there is no direct evidence, this memo certainly raises the possibility that, in an effort to silence him, industry representatives had direct contact with Hueper’s superiors. Other companies, such as du Pont, as we have already seen, had no qualms about doing so.

Three years later, Hueper wrote a six-page memo again describing the asbestos industry’s intransigence during the 1940s and 1950s. In response to what appears to have been a question by the Associate Director of Field Studies at the NCI, about the feasibility of holding a workshop on occupational cancer hazards, Hueper indicated that the two prior conferences of 1948 and 1955 had

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80 Wilhelm C. Hueper to John R. Heller, dated June 8, 1959, 1, located in MSC C 228, Box 17, Archives of the National Library of Medicine. Around the same time Hueper also spoke similarly to an organized labor group concerning the Braun Truan report. Wilhelm C. Hueper, “Organized Labor and Occupational Cancer Hazards,” (undated), 20-21, Hueper Papers Collection, MSC 341, Archives of the National Library of Medicine. During the 1970s a report funded by NCI and based upon interviews with over eighty individuals examined the beginnings of the National Cancer Institute and the war on cancer. It found a “revolving door of industrial and government experts had operated since the earliest efforts to deal with cancer nationwide;” see Devra Davis, The Secret History of the War on Cancer, 14. As was noted in Chapter 3, legal harassment of scientists that offer opinions different from that desired by industry has not been confined to Hueper. See for example Miriam Shuchman, “Consequences of Blowing the Whistle in Medical Research,” Annals of Internal Medicine 131, no. 12 (20 June 2000): 1013-1015.
not been followed by any meaningful action by industry. Moreover, at the two prior conferences, representatives of industry invited to the conferences “boldly refused to divulge definite figures and facts related to occupational cancer hazards and known to them.” (One of those facts actually misrepresented by the asbestos industry was their claim that no animal studies had been undertaken.) The industry had not only continued “this spirit of non-cooperation and public irresponsibility,” but even attempted to deny the existence of hazards in order to escape any liability to their workers. Hueper then pointedly described how several companies (including Quebec mining companies) had tried to repudiate the existence of hazards “by having investigators of their choice prepare and publish epidemiologic information based on incorrect, defective and biased data.” This was a direct reference to the Braun Truan Report. He finally complained that to date the only epidemiologic study on asbestos was the miner study [Braun-Truan report], despite the fact that “…50 per cent of the employees of an asbestos brake lining plant in Pennsylvania who came to autopsy with asbestosis of the lung had also cancer of the lung.” (This was a Raybestos plant.) The records at the National Library of Medicine unfortunately do not contain any response to Hueper’s memos, so we cannot further speculate about the actions or motivations of Hueper’s superiors. However, the Surgeon General did forbid him to undertake an epidemiological study unless he could get industry’s approval. 81

In a 1962 article, Hueper again emphasized the problems with the Braun-Truan study. In this paper he explained that the statistical analysis presented the largest problem. Canadian cities had a much higher cancer rate than other sections of the country. By including the city data in the expected rate of cancers, Braun and Truan had provided a base rate of cancer elevated far above that

81 Wilhelm C. Hueper to Dr. M. B. Shimkin, dated May 9, 1961, located in MSC C 228, Box 17, Archives of the National Library of Medicine; Surgeon General information in Wilhelm Hueper, “Unpublished autobiography draft,” 139; and Wallace Werble, “The News This Issue: Research Notes,” Drug Research Reports 4, no. 17 (September 13, 1961): 6, located in MSC C 228, Box 17, Archives of the National Library of Medicine.
found in the general rural areas of the country that resembled the mining district. Hueper concluded the paper by further stressing the deceiving nature of the report; “according to the data provided... by Braun and Truan, their conclusion is patently incorrect and grossly misleading and results in obscuring the existence of a markedly elevated lung cancer rate for members of this working group.”

Nor did Hueper confine his criticism to letters and articles. While speaking at Irving Selikoff’s landmark 1964 New York Conference on Asbestos, Hueper yet again lashed out at the asbestos industry. It was predictable, he stated, that “no large-scale observations on the incidence, morbidity and mortality rates of asbestosis and asbestos cancers have been published from the giant American asbestos industry.” He found it “regrettable” that “the original plan of having a recent epidemiologic survey on these aspects of asbestos production in Canadian mines and mills to be undertaken under the aegis of the National Cancer Institute of Canada was not adhered to and that this study was carried out as an industry-dominated venture which yielded highly controversial negative results.”

Other participants at Selikoff’s 1964 Conference also took particular aim at the published Braun-Truan Report. In perhaps the most pointed analysis, industrial hygiene expert Thomas F. Mancuso noted two obvious major flaws in the study. First, the study was seriously diluted due to 66% of the workers being under the age of forty-four. Given the long latency period of lung cancer, the young age of the workers and the short follow up did not allow sufficient time to accurately

analyze lung cancer rates. He also noted that the group studied was a “survivor group” which
excluded their contemporaries who had already died.\textsuperscript{84}

Still other authors took particular aim at the voluntary standard, questioning why it was still
being used. Hueper noted that even non-occupational exposures might be hazardous. As previously
discussed, Schall’s talk critiquing the threshold limit value and its sole evidentiary support, Dressen’s
report, received particular attention from the attendees.\textsuperscript{85}

Industry lawyers did not stand by passively after this major assault on their one published
study. Following the Selikoff’s Conference, the ATI attempted to quell the furor with implied
threats. Their attorneys sent letters to both Selikoff and the New York Academy of Science that
warned them of the dangers in “innocent but unwise treatment of research data in public
discussions.”\textsuperscript{86} However, it was too late to put the asbestos back into the bag.

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With these new studies, conferences, and critiques, the asbestos company attorneys were
reduced to fighting rear-guard actions. With increasing asbestos hazards research and knowledge

\textsuperscript{84} Thomas F. Mancuso, “Discussion Following Asbestos and Neoplasia: Epidemiology Session,” \textit{Annals of the New York
Academy of Sciences} 132, no. 1 (December 31, 1965): 589-602. Mancuso also coauthored articles with Hueper, including
Hueper’s 1952 discussion of occupational cancer in which the authors had briefly described the correct way to conduct
surveys for cancer in occupational groups, specifically mentioning asbestos workers as one of those groups; see Wilhelm

Exposures to Asbestos,” 184-192. Section IV of the Conference, \textit{Human Exposure to Asbestos: Community Studies} provided
numerous authors expressing concerns about the hazards of asbestos outside of the work place. \textit{Annals of the New York

\textsuperscript{86} The quote is from in Cadwalder, Wickersham & Taft letter, October 26, 1964 cited in Jock McCulloch and Geoffrey
Tweedale, “Shooting the Messenger: the Vilification of Irving J. Selikoff,” \textit{International Journal of Health Services} 37, no. 4
(2007): 621. This article also examines the historiography of books that have lambasted Selikoff. The three most
prominent histories are by Jacqueline Corn, British medical historian Peter Bartrip, and Rachel Maine. Jacqueline Corn
is the wife of noted and prolific asbestos defense expert, Morton Corn. Her book was sponsored by W. R. Grace, one of
the largest former manufacturers of asbestos fireproofing. Peter Bartrip’s books were funded by asbestos contractor
ACandS and New York attorneys for British asbestos company Turner and Newell. In his books Bartrip found Selikoff
to be both a key medical pioneer and a charlatan. Bartrip’s books read similar to legal briefs. This was undoubtedly
helpful to the defense attorneys during Bartrip’s subsequent litigation testimony. Rachel Maine’s book was sponsored by
the Winthrop Group, a group of consultant historians who write commissioned histories and conduct litigation support
for U.S. corporations. In their article, McCulloch and Tweedale demonstrate how each of these books failed to fully
examine the historical record.
came ever-increasing lawsuits against the major companies. For the first time in 1966, Wayne Stephenson, attorney for an asbestos insulation worker Claude J. Tomplait filed a lawsuit against the manufacturers of the asbestos products he worked with. Eventually this case was settled with five companies for $75,000.00. Tomplait lost the trial against the sixth defendant because he could not remember at which jobs he had used its products. While that case was lost, in a subsequent coworker’s case, the jury returned a verdict for the plaintiff, Clarence Borel, in the amount of $79,436.24. The dam of silence and confidentiality had burst. Shortly thereafter a plaintiff asbestos insulator, for the first time, won a case as a user of asbestos products from the manufacturer. 87

Since that time the courts have been flooded with tens of thousands of personal injury lawsuits against asbestos manufacturers as well as numerous suits from property owners for asbestos products used during the construction of their buildings. 88

In the fight against this wave of lawsuits, the IHF provided considerable assistance to the defense lawyers. In its medical series bulletin, The Pneumoconioses, the IHF put the best light on current research by carefully selecting the information included. For example, the bulletin noted that reports linked both lung cancer and mesothelioma to asbestos but also cited one recent British study that held out hope of lung cancer risk being eliminated with good housekeeping. The bulletin, however, failed to indicate what it considered the safe level of exposure or what good housekeeping would entail. Overall, the bulletin read like a primer for attorneys to use when fighting legal and workers’ compensation claims by workers. For example, it stated:

[I]t is advisable to reiterate the observations of many physicians, namely, that the X-ray picture should never be used to estimate the presence of the extent of impaired pulmonary function or disability. Many cases with X-ray evidence

88 By 1982 16,500 unresolved lawsuits for asbestos disease had been filed against Johns-Manville. Additional cases were being filed on an average of 500 per month. Barry Castleman, Asbestos: Medical and Legal Aspects, 243. In addition, by 1987 numerous class actions and individual property damage cases had been filed, most asking for millions of dollars in damages to property from asbestos. Author’s professional experience.
of third-stage asbestosis (the most advanced stage) have been known to carry out their usual work and live fairly comfortable lives for several years. On the other hand, no case of definite disability has been seen unless there was the typical X-ray pattern of asbestosis.89

A review of the medical articles authored by employees and consultants to the IHF during the 1960s and 1970s reveals research efforts more in keeping with providing support to litigation efforts than attempts to protect workers or determine the full nature of asbestos’s disease potential. For example, Paul Cartier, Medical Director of the Thetford industrial clinic for the Canadian asbestos mines in the area, and Paul Gross of the IHF, published an article in 1962 concerning a case involving alleged non-asbestos fibrosis of the lungs. Since the individual had worked with asbestos, most doctors likely would have expected this case to be the result of asbestos. Gross and Cartier specifically wanted clinical doctors to be aware of other, unknown, causes of fibrosis. While the worker had been employed in occupations involving exposure to asbestos for many years, Cartier did not believe the man had sufficient exposure for asbestosis. Like industry doctors who dismissed acute silicosis, he also believed the disease progressed too rapidly for asbestosis. The authors, however, were unable to determine the cause of the disease. In conclusion the authors stated: “This case illustrates the danger of making the diagnosis of pneumoconiosis on presumptive evidence. It also points up the desirability, if a diagnosis of pneumoconiosis is to be a reasonably sound one, to be in possession of all essential details of the industrial dust exposure.”90 If the word silica had been substituted for asbestos, the entire article would have seemed appropriate in the 1930s. As had previously happened with silicosis cases, by calling for increased skepticism about asbestos diagnoses, even in cases of acknowledged asbestos exposure, the authors provided critical support to defense attorneys in almost all asbestos lawsuits.

IHF employees, including Gross, also wrote articles demonstrating that “asbestos bodies”—protein covered fibers found in lungs that were being used by plaintiff counsel to demonstrate asbestos exposure—did not always contain a core of asbestos. The IHF researchers determined that other fibers could also cause asbestos bodies. They suggested the bodies should be renamed “ferruginous” bodies, since they did not necessarily contain asbestos fibers. While this fact was extremely useful for defense of lung cancer lawsuits, it had questionable importance for occupational health, other than to show that other fibers might also be hazardous. Gross followed up this determination with an article demonstrating that urban dwellers had large numbers of fibers in their lungs. Since these fibers in his estimation appeared to be from wood smoke, defense counsel could now contend that even lung fiber counts would not help a plaintiff counsel in establishing a connection between asbestos exposure and his client’s lung cancer.

Even when the researchers found a connection between asbestos and lung cancer, they developed a theory that exonerated pure asbestos. For example, in 1967 Gross reported on a rodent inhalation experiment he had conducted with chrysotile which found a substantial incidence of lung cancer, including at least one mesothelioma. While he acknowledged that this study confirmed experimentally the epidemiologically “surmised” relationship of asbestos and lung cancer, Gross “presumed” that the trace findings of heavy metals in the chrysotile caused the cancer, rather than any direct effect of the asbestos. This opinion provided a basis for defense attorneys to still dispute

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Asbestos bodies had been used since at least 1957 to diagnose an individual with asbestos related cancer; see Wilhelm C. Hueper, “The Pathological Significance of the New Environmental Disease Panorama Resulting from Modern Industrialism,” Presentation at the Fifty-Fourth Annual Meeting of the American Association of Pathologists and Bacteriologists, April 12, 1957, Washington D.C., Hueper Papers Collection MSC 341, Archives of the National Library of Medicine.

the cause of lung cancers in individual cases, despite the findings of numerous studies linking asbestos to lung cancer. If the culprit was heavy metals, rather than asbestos, then in any individual case, the plaintiff might have contracted lung cancer from exposures other than asbestos. 93

The IHF’s 1960s production of medical science materials useful to industry attorneys culminated with its 1968 book, *Industrial Hygiene Highlights*, edited by Lester V. Cralley, Chairman of the IHF’s Engineering Committee and an employee of ALCOA. This book provided perhaps the best demonstration of the IHF’s willingness to downplay the asbestos hazard, even after the medical consensus about the hazardousness of asbestos that was reached shortly after the 1964 Selikoff conference. The book, comprised of chapters written by individual authors, began with the long-held and largely respected industrial hygiene mantra that there is a dose-response relationship between poisons and injury; “risk becomes negligible when exposure falls below a certain tolerable level…” thus implying that any substance can be used so long as care is taken. This mantra takes into account the ability of the human body to fight off small doses of a great many materials.

However, with regard to cancer, which can begin with the mutation of one cell, the mantra has limited usefulness. 94


The epidemiologic section of the book took notice of increased attention being paid to the health effects of asbestos. The author, Lewis Cralley, a high official in the United States Public Health Service, who also had a close relationship with the IHF, wrote that he remained unsure of the relationship between asbestos and cancer. He cited Sir Richard Doll’s 1955 findings of increased lung cancer among asbestos textile workers as the impetus for this interest, along with “a number of studies which tended to support the earlier findings.” He then observed, without much discussion, that during the mid 1960s five major asbestos conferences had convened. The author identified the major result of these conferences to be an understanding “that little had been done to characterize the environmental exposures of asbestos workers in terms of types of asbestos used, metals and minerals associated with asbestos ores, and additives in processing.” The chapter went on to observe: “Some researchers are of the opinion that the value set by American Conference of Governmental Industrial Hygienists of 5.0 million particles per cubic foot of air (mppcf) of total particulates i.e., both respirable fibers and motes, is too high. On the other hand, available criteria for deriving current standards to prevent injury to health are still minimal and insufficient.” After emphasizing that many of the cases of asbestosis were in workers initially exposed prior to World War II, when exposures were “generally quite high”, the author conceded that the asbestos TLV should be reviewed with an eye toward “consider[ing] the merit of eventually” basing the number on the amount of respirable asbestos fibers in the air. The author remained confident that reducing the exposure levels to those that “can be achieved with current good industrial hygiene control practices” would result in a decrease of lung cancer. Given the state of knowledge concerning asbestos at that time, most of these opinions seem fanciful at best. In keeping with Hueper’s and Beyer’s opinions about Public Health officials, Cralley did not suggest that a comprehensive epidemiological investigation be undertaken for United States asbestos manufacturers, even though as of 1977 not a single such investigation had been published. One possible explanation for Cralley’s
reticence to critically examine the country’s asbestos usage might be his long and close association with the IHF. Not only was his brother Lester the chairman of an IHF committee, but Cralley had also participated in numerous IHF studies.95

The toxicology chapter of Industrial Hygiene Highlights provides still another example of information and opinions useful in defending against asbestos lawsuits. This chapter provided minimal information about asbestos, all of it minimizing any potential health effects. The chapter first cited an IHF study that did not find asbestosis in rats, even after heavy doses of chrysotile for sixty-two weeks. While the author admitted that the dosage produced a high incidence of lung cancer, he highlighted the trace quantities of nickel, chromium and cobalt on the asbestos, clearly implying that the metals might have caused the cancers. The next reviewed study had not found a correlation between duration of asbestos exposure and breathing tests in workers, again implying a lack of causation between asbestos and disease. A third study determined that the oils sometimes associated with amphiboles had a weak carcinogenic response on mouse skin. From this one admittedly weak association—far less of an association than that found by a number of doctors for asbestos by 1950—the chapter’s author suggested that oil might play a role in cancer. Finally, the author discussed one more potential litigation defense issue, citing another IHF finding that not all coated fibers in the lungs were asbestos; some of them were aluminum silicate.96 By highlighting these studies and ignoring the numerous studies throughout the 1960s demonstrating the hazards of asbestos, this chapter clearly fit well within industry’s desire to demonstrate that asbestos was fairly innocuous: almost anything but asbestos might cause cancer.

95 Lewis J. Cralley, “Epidemiological Studies of Occupational Diseases,” In Industrial Hygiene Highlights, eds. Lester V. Cralley, Lewis J. Cralley, and George D. Clayton, 11-14; Andrew D. Hosey, “Interview of Lewis J. Cralley of October 22, 1975,” Annals of the American Conference of Industrial Hygienists 7 (1984): 61-67; Wilhelm C. Hueper Deposition on June 16, 1977, 18. (Selikoff’s investigation of asbestos insulators had been undertaken at the behest of the union. The IHF may have conducted other investigations, but if so, none have surfaced. As noted earlier, Hemeon had suggested such an investigation in the late 1940s, but the companies involved in the surveys did not approve his request.)
96 Emil A. Pfitzer, “Toxicology,” In Industrial Hygiene Highlights, eds. Lester V. Cralley, Lewis J. Cralley, and George D. Clayton, 244-270.
A final chapter in the book—devoted to new and recurring health hazards—attempted to bolster the industry position that recognition of asbestos hazards was a recent phenomenon. The final two pages of the chapter discussed asbestos under the title “Old and continuing problems.” There, the author opined, “we are just beginning to realize the extent of the problem from the use of asbestos.” This was a critical point for defense lawyers in negligence cases involving silicosis, asbestosis, or lung cancer. If the hazards to the specific job classification had not been studied or reported when the workers were being exposed to silica or asbestos, then the companies contended that they were not negligent. However, only two sentences later the author admitted, “the problem of asbestosis has been studied extensively since the 1920s when a definite relationship was established between the fibers and the disease.” Apparently, however, the author did not believe this was the case with lung cancer. He implicitly cast doubt on asbestos’ relationship with lung cancer by mentioning two unsolved problems regarding asbestos. These problems were the question about whether the metals associated with asbestos were the actual cause of lung cancer and the finding that not all “asbestos bodies” contain asbestos. ⁹⁷ Both of these problems provided potential crucial elements for the defense of asbestos litigation. In sum, the asbestos information and opinions contained in *Industrial Hygiene Highlights* provided asbestos defense attorneys with a wealth of material to cast doubt on the association of any plaintiff’s disease with asbestos.

Throughout the 1960s, the IHF, its employees, and agents did not limit their activities to developing an asbestos litigation-defense friendly scientific literature. Beyond publishing *Industrial Hygiene Highlights* and numerous medical articles, they also sought to subvert both governmental and quasi-governmental health agencies and standards. The best example of industry’s attempt to control the agenda, co-opt the experts, and manufacture science during the 1960s, came from the United

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States Public Health Service. There, as previously noted, Cralley worked closely with both Johns Manville and the IHF. During the 1960s he conducted a study of asbestosis in Johns Manville employees. When asbestos investigative reporter and author of the first mass media articles chronicling the beginnings of asbestos litigation, Paul Brodeur, asked him in 1968 if the study included lung cancer or mesothelioma, he responded that he was only interested in asbestosis. Brodeur then asked him if mesothelioma could occur with small exposures to asbestos, to which Cralley responded, “in his opinion the association between asbestos and mesothelioma was not proven.” Brodeur apparently left the next obvious question unstated: If the association was not proven, why was it not a part of Cralley’s study?

Cralley may not have even been that interested in asbestosis. When scientist William Johnson joined the newly established National Institute of Occupational Safety and Health (NIOSH) in the early 1970s, he examined the mortality data on asbestos textile workers that his predecessors, including Cralley, had accumulated. He was shocked at the data: “Just from the most cursory look at those data, almost anyone would know that there had been a tragedy of immense proportion in many, if not all, of those factories. Why, the men working in them were dying of asbestosis and cor pulmonale – a form of heart failure that often accompanies the disease-right on the job! Men in their fifties! And some only in their forties!” In a later interview Johnson remained cautious in his description of any purported government/industry cooperation in a cover-up. He would not say that the Public Health Service had covered up evidence, but he made a point to say he did not know if the study’s authors were stupid, criminally negligent, or may have been concerned about their right to enter the plants being cut off, implying that at least one of these scenarios

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applied. Johnson further stated that even as the study was being conducted, everyone knew that many of the workers had advanced asbestosis or cancer.99

As late as 1972 at least some officials of the United States Public Health Service could still be viewed as conforming to the 1930s view of them held by Clara Beyer, confidential aide to Labor Secretary Frances Perkins, as industry lackeys. That year, even as many asbestos manufacturers were removing asbestos from their products, an article in Food and Cosmetics Toxicology comforted its readers with the news that US Public Health Service officials “are now satisfied that asbestos can be used safely in modern industrial plants.” The information came from Cralley. Industry attorneys could take comfort that at least some government officials still understood their viewpoint.100

Actions of the IHF with regard to asbestos policy during the 1960s also provided assistance to industry personal injury attorneys. Foremost among these were the actions directed toward the voluntary threshold limit value for asbestos. Gross became a member of the TLV Committee of the ACGIH in 1964. In 1965, he became chairman of the insoluble respirable dust subcommittee, the committee that established the Threshold Limit Value for asbestos. He remained active in the committee until 1983.

Although no direct evidence has been forthcoming that Gross worked directly for and with corporate attorneys while conducting committee business, several factors point strongly to this probability. First, throughout the 1960s and 1970s Gross performed research for Johns Manville,

100 Ibid; Clara Beyer reference from David Rosner, et al., Deadly Dust: Silicosis and the Politics, 126. Hueper had similar experiences with officials in the Public Health Service. In 1952 the Surgeon General removed him “from any connection with occupational cancer epidemiology and forbade [him] to contact State Health Departments and industries in such matters.” Wilhelm C. Hueper Deposition on June 16, 1977, 13. Hueper also complained in his draft autobiography that throughout his career at the NCI, the Division of Industrial Hygiene, PHS “consistently tried to interfere with [his] efforts to investigate occupational hazards in man.” He attributed this harassment to the Division’s desire to “maintain cordial relations with industry (management).” Wilhelm Hueper, “Unpublished autobiography draft,” 137-139. Incredibly, Johns-Manville apparently believed that factories were not the only places asbestos could be used safely. In 1971-2 it sold at least 250,000 pounds of modeling clay—containing more than 50% asbestos—to New York schools. Ruby I. Compton and Barry I. Castleman to S. John Byington, April 27, 1977, Box 04-1851, Johns Manville Archives (seeking a consumer ban on asbestos products).
including a never-published confidential animal study on brake dust. Second, in 1969, as a member of a U.S. Public Health Service Committee he secretly provided draft copies of a Committee report to asbestos companies. Robert de Treville, at that time President of the IHF, wrote a memo concerning the report asking for industry comments, explaining: “we will attempt to see that needed corrections are introduced by Dr. Paul Gross, a member of the Committee.”

Yet, neither of these activities demonstrates the full extent of Gross’ willingness to assist the asbestos industry to manufacture science. As part of a National Academy of Science (NAS) Committee, Gross provided both written documents and verbal reports about the ongoing committee study of asbestos in drinking water to Raymond L. Erickson, attorney for the Reserve Mining Company. This relationship was only disclosed when memoranda that Erickson wrote to his superiors about the discussions were provided to the government, pursuant to an early summer 1976 court order in litigation between the federal government and Reserve Mining. The memoranda recorded that Erickson “conferred with Dr. Paul Gross on May 5 and May 8, 1976, with respect to his participation in the subcommittee… Dr. Gross will be providing us with more information as to each of the individuals on the subcommittee in addition to copies of their draft reports which are identified and described generally in the attached two outlines.” The memo also indicated that “Dr. Gross will be providing us with the draft reports to be submitted by the remainder of the subcommittee members and we should have some opportunity to assist Dr. Gross in evaluating the contents of those reports.” When asked by a Science reporter about the incident, Gross attributed

101 Gross was not the only asbestos industry consultant on the ACGIH TLV Committee. Dr. Arthur Vorwald, who as Director of the Saranac Laboratory agreed to publish Gardner’s non-cancer related research but withheld his animal cancer studies, served on the committee from 1951 until 1956. While on the committee, Vorwald continued to conduct research for asbestos companies, some of which he did not publish. Barry I. Castleman and Grace E. Ziem, “Corporate Influence on Threshold Limit Values,” American Journal of Industrial Medicine 13, no. 5 (1988): 546-7; Robert T. P. deTreville memorandum to IHF Fibrous Dust Study Sponsors of September 10, 1969, Industrial Hygiene Foundation Archives, Carnegie Mellon University.

his conduct to his mistaken impression that the committee proceedings were open to the public. However, even after learning that Erickson would not be allowed to attend any meetings, he still provided Erickson with further information. In a later memorandum, Erickson reported that Gross had informed him at one point that further documents could not be provided. However, “Dr. Gross felt he could read to me the draft conclusions pertaining to the health aspects of the subcommittee’s report.”

Gross took all of these actions after initially indicating on his NAS pre-selection bias statement—required prior to any appointment to NAS committees—that he had no conflicting consultancies. When asked about this, he explained that his filing was proper since, although he had previously testified for Reserve Mining, he was not currently on retainer to Reserve Mining. Reserve Mining’s attorneys apparently did not agree; they continued to list him as a consultant on their court filings.

Although Gross initially denied that he had been influenced by the attorney or had known about the confidential nature of the documents, and other participants believed he was being objective during committee discussions, these activities raise an inference of doubt about his independence. This inference is only strengthened by Gross’ reading another participant’s draft conclusions to Erickson over the phone subsequent to the time he admitted understanding the confidential nature of the committee documents. At the bottom line, even if Gross’ explanation of his activities is accepted, this incident clearly demonstrates the lengths to which attorneys will go for an edge in medical science relevant to litigation.

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103 Nicholas Wade, “NAS Committee on Asbestos,” 662.
104 The “Bias Statement” asks scientists to declare, among other things, who they have consultancies with and whether they have made any public pronouncements on the issues in question. Nicholas Wade, “NAS Committee on Asbestos,” 663-4.
In addition, even if Gross correctly characterized his conduct, what does this say about industry’s access to—and ability to influence—regulation and voluntary standards? If Gross felt comfortable conducting confidential asbestos research for companies, while at the same time not disclosing its implications for disease when asbestos exposure limits where set at the ACGIH, and was willing to provide confidential government reports to industry attorneys, it seems highly probable that he would not quibble about providing ACGIH information to industry attorneys or take the position they desired in committee discussions. Yet, he continued to participate in ACGIH meetings. The importance of Gross’ influence at the ACGIH to industry only diminished in 1970, when the Federal government became intimately involved in industrial hygiene issues.

As the 1960s progressed a number of concerns had acted in concert to eventually secure federal action for occupational diseases. During this period, the increasing prevalence of asbestos disease was not the only area of anxiety for the nation’s workforce. During the 1960s the nation’s businesses and industry saw a reversal in the long-term gradual reduction in workplace injuries. From an injury frequency rate of 23.1 injuries per million man-hours in 1930, it had dropped to 12.0 in 1960. However, throughout that decade there was a steady increase, until the rate stood at 15.2 in 1970.105

By 1970, many labor and governmental leaders believed the injury rate was intolerably large. By that year injuries requiring sick leave were totaling 3% of the work force, with a loss of 100,000 man-years of production. Although later evidence suggested that the rise in injuries was either cyclical or, at best, evidence for an upward trending increase was mixed, Congressional committees in particular expressed concern about this development. The Labor Committee of the House of Representatives wrote that “this upward trend shows no signs of change,” while the Labor

Committee of the Senate called for immediate action: “The knowledge that the industrial accident situation is deteriorating, rather than improving, underscores the need for action now.” Even the author of the conservative American Enterprise Institute’s analysis of the Act admits that the trend was “a troubling development for which no underlying causes have yet been identified.”

Perhaps due to the voluntary nature of occupational diseases and their, at times, long latency period, less was known about occupational diseases. Even Selikoff’s massive study of 10,000 insulators had not been accomplished with business support, but rather through the union. However, it was estimated that only 25% of workers exposed to health hazards were adequately protected. At least one doctor testified about asbestosis: “It is depressing to report, in 1970, that the disease that we knew well 40 years ago is still with us just as if nothing was ever known.”

However, even with this growing understanding of the problem, safety and health did not leap to the forefront of labor union concerns until the late 1960s following calls for environmental and pollution legislation. Union leaders such as Jack Sheehan, former legislative director for the United Steelworkers of America, believe that the Occupational Safety and Health Act would not have passed in 1970 “without the aggressive precedent of the environmental movement in this field.” The November 1968 Farmington, West Virginia mine explosion that killed seventy-eight miners—and the growing movement to aid victims of black lung—also spurred calls for action by numerous individuals and organizations, including consumer and labor activist Ralph Nader. By 1970 an occupational safety and health bill became organized labor’s top legislative priority.


108 Susannah Zak Figura, “OSHA through Time: an Insiders’ Portrait,” Occupational Hazards 58, no. 4 (April 1996): 37-44, 37; John Mendeloff, Regulating Safety (Cambridge, Massachusetts: The Massachusetts Institute of Technology, 1979), 17-18; and Thomas O. McGarity, et al., Workers at Risk, 33-34. Interestingly, writers from both the right and left political perspective appear to essentially agree on the main outline of these causal events; see Charles Noble, Liberalism at Work:
In 1970, these calls for action induced quick results from Congress. Every witness before the House subcommittee holding hearings agreed about the need for safety and health legislation. The initial House and Senate versions of the act passed with overwhelming bipartisan support, 383 to five in the House and eighty-three to three in the Senate. The Act was signed into law by President Nixon on December 29, 1970 and became effective 120 days later. At the signing ceremony for the Occupational Safety and Health Act President Nixon called it “one of the most important pieces of legislation… ever passed.”

The Act contained 34 sections with numerous provisions for safety and health in the workplace. It provided not only for inspections of workplaces, but also established the National Institute for Occupational Safety and Health with research and education functions, as well as a Workmen’s Compensation Commission to “undertake a comprehensive study and evaluation of state workmen’s compensation laws.” The Act required each employer to provide his employees with a job that is “free from recognized hazards that are causing or likely to cause death or serious physical harm.” Perhaps the most crucial portion of the Act allowed OSHA agents to enter businesses “without delay.” Since the time of the first state industrial hygiene agencies in the early part of the century, inspectors had to make appointments to visit businesses. Now for the first time, inspectors could examine the actual working conditions of factories and plants.

Of equal importance, the Federal government could now conduct research and determine appropriate levels of exposure to toxic substances, rather than rely upon information from businesses whose profits depended upon keeping costs to a minimum. As authorized by the Act,

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during the winter of 1972 the Labor Department compiled and published in the *Federal Register* existing federal and national consensus standards to serve as a base for the agency’s activities.\(^{111}\)

Yet, in an election year, and as part of an administration that sought to nurture good relations with businesses, OSHA’s initial efforts were severely criticized. The Watergate scandal did not only involve the burglary of the Democratic Party offices, but also exposed the desire of Republicans for OSHA to buttress businesses’ support for the administration. During the investigation of the break-in, a memo from the first administrator of OSHA surfaced which put another light on the Republican administration’s true feelings about the legislation. In this memo, George C. Guenther appeared to assure the President that he would not enforce the law and suggested that OSHA not propose any controversial standards in the near future. Although Guenther later claimed that the memo was only a statement of his views and was not put into effect, at least one commentator has suggested that Guenther “promised to slow rulemaking in exchange for industry contributions to the committee to Re-Elect the President.”\(^{112}\)

Even if Guenther’s characterization of the memo is accurate, many commentators agree that, at least in its initial years, the agency failed in most respects to live up to its potential. In the first five years only three new health packages were adopted: asbestos, fourteen carcinogens, and vinyl chloride. Even the initial asbestos standard only came about due to an AFL-CIO petition. In response, an Emergency Temporary Standard of a 5.0 f/cc exposure limit was put into effect on December 7, 1971. This limit was confirmed in the final standard promulgated in June 1972. Since

\(^{111}\) Susannah Zak Figura, “OSHA through Time: An Insiders’ Portrait,” 38.

that time, asbestos has been increasingly closely scrutinized, with OSHA repeatedly lowering the allowable level of exposure.\textsuperscript{113}

* * *

Following repeated litigation discovery disclosures of company knowledge and continued advances in medical research, asbestos lawsuits grew exponentially in the 1980s. Companies scrambled to find new defenses. Subsequently, many companies such as Johns Manville, Keene Corporation, Armstrong World Industries, Pittsburgh Corning, and Owens Corning Fiberglas, who used the two most hazardous forms of asbestos—amosite or crocidolite—, declared bankruptcy. The remaining firms that used or sold asbestos primarily used two scenarios in defending their cases. First, in every case they claimed the plaintiff’s disease was caused not by their chrysotile product, but by amosite or crocidolite. In addition, even if their disease might have been caused by chrysotile, any exposure to their product was too small to have contributed to the disease. The evolution also extended to plaintiff cases, as workers with different occupations and types of exposure began filing lawsuits. In addition, property owners began suing for removal of the asbestos products in their buildings.

With this evolution in the lawsuits, came a progression in the manner in which companies used and created science. The bankruptcy of virtually all manufacturers of amphibole-containing products left manufacturers using chrysotile as the sole defendants in virtually all asbestos cases.

Officials of the country of origin for chrysotile (Canada) and scientists hired as experts for industry lawsuits argued that chrysotile was different. Its health effects were much less than those for amphibole asbestos. With proper precautions it could be used safely. The literature relating to this issue is mountainous, and, with the ongoing litigation in the United States, seemingly growing exponentially.

As the asbestos personal injury and property damage cases mounted in the 1970s and 1980s, litigation support increasingly drove the science. Every company, particularly those with only chrysotile products, strove to distinguish their product as not being responsible for the disease. Manufacturers of chrysotile containing products began emphasizing to worried purchasers that their products only contained “white asbestos” (chrysotile), a substance distinct from the amphiboles crocidolite and amosite, the asbestos types that caused mesothelioma, a deadly cancer. This distinction is somewhat misleading. Many mines that produce chrysotile are near amphibole deposits with the resultant product being a mixture of the two. Others, including almost all Canadian asbestos mines, have deposits that are contaminated with the amphibole tremolite. 114

Although the mechanism of carcinogenicity remained obscure, much research became centered on the dimensions of fibers and their biopersistence, both areas of potential litigation defense to manufacturers of chrysotile asbestos products. One theory emerged that only long, durable fibers caused cancer. At trial, attorneys claimed that longer, more durable amphibole fibers caused an individual’s cancer, not the shorter, “curlier,” and less durable chrysotile fibers. 115

Various segments of the industry also strove to influence medical opinion by concerted public relations efforts. On December 4, 1970 Johns Manville and other companies formed the

114 Geoffrey Tweedale, et al., “Chrysophiles versus Chrysophobes,” 239-259; and author’s professional experience.
Asbestos Information Association/North America (AIA) at its New York headquarters. The AIA’s stated goals included providing a means to rebut “irresponsible and uniformed criticism” of the industry and its products and to “propagate the benefits and indispensability of asbestos through advertising, publicity, and speeches.” Within a year its budget was $300,000, spent primarily on monitoring conferences and papers, with follow up activities (“lines of action”) as needed. These follow-up activities included attempting to smear Irving Selikoff and to distinguish chrysotile from other fibers. Assisting in asbestos lawsuit defenses remained an additional unstated goal of the AIA.

Two articles written by Enterline, an eminent epidemiologist first mentioned at the beginning of this chapter, perhaps best illustrate this activity. In 1978 the AIA commissioned a paper by Enterline in which he opined that the asbestos cancer hazard was not recognized in the United States until 1964. A 1991 Enterline paper also included this same opinion, with one important caveat: “…the asbestos industry probably exercised some control over research, and findings unfavorable to the use of asbestos probably exercised some control over research, and findings unfavorable to the use of asbestos were clearly not in their interest.” Following the 1991 article, Egilman and Hardy wrote letters to the editor to which Enterline responded. In his retort, Enterline offered, as one explanation for the failure of Gardner and Vorwald’s to publish their cancer findings, the possibility that the studies did not clearly demonstrate cancer. Although both Hardy and Schepers—who were both at Saranac during the relevant periods—disputed this, the point here is not whether or not the studies were clear, but that they were not published and apparently not shown to Enterline when he was asked to write the editorial opining the industry’s lack of knowledge about cancer risk until 1964. This opinion, far from “propagating the benefits and indispensability” of asbestos, had only
one logical purpose: to assist in the defense of asbestos lawsuits by showing that asbestos companies were not negligent in their sales of asbestos products.

Unfortunately, Enterline’s 1978 article did not stop at failing to include industry research. It also provided misleading information, particularly concerning Hueper. Although he credited Hueper with linking asbestos and cancer in 1943, Enterline pointedly cited to a government publication by Hueper, in which he had failed to induce cancer during animal studies. Furthermore, by citation to another governmental publication, Enterline suggested that Hueper seemed to agree that German studies might have been unclear. Thus, Hueper was made to seem unsure during the 1950s about the linkage between asbestos and cancer. In presenting this evidence, Enterline ignored Hueper’s sustained, very public proclamations throughout the 1940s, 1950s and 1960s that asbestos caused lung cancer. If he had so desired, Enterline could have cited at least ten widely circulated publications of Hueper’s concerning this opinion. In fact, even Hueper’s 1956 monograph clearly associated asbestosis and lung cancer. Enterline’s failure to cite the additional articles, or even fully explain the 1956 article, raises the possibility that the AIA or its attorneys asked him not to do so, and specifically provided him with the only two cites of Hueper’s work that cast any uncertainty on Hueper’s long held (and otherwise clear) opinion concerning asbestos and cancer.

In 1984, funding from the Canadian government, the Quebec government, and Canadian asbestos mining interests established another primarily public relations agency, the Asbestos


Institute. Its stated purpose was (and remains) to "maximise (sic) the use of existing resources in a concerted effort to defend and promote the safe use of asbestos on a global scale." By 2001 it had received $54 million from its sponsors. During that period the Asbestos Institute sponsored conferences, undertook public relations initiatives, worked closely with industry attorneys, and disseminated “scientific” information, all touting the “safe use” of chrysotile. The following three examples of these activities demonstrate the character of their activities. In 1987 the Institute President reminded the readers of The Economist that in Selikoff’s study, “asbestos victims did not only inhale white asbestos but were exposed mostly to amosite asbestos,” thereby implying that it was the alleged majority exposure to amosite that caused their diseases. The Institute also apparently organized an International Seminar on Safety in the Use of Chrysotile Asbestos, held in Havana, Cuba during 2000. At the Seminar, a British medical doctor argued that tremolite caused the cancers in Canadian miners, but much of it was removed in the milling process. In 2003 the Asbestos Institute publicized a new study that concluded chrysotile is safe, based upon a theory that short fibers are innocuous.118

In the same year as that of the founding of the Asbestos Institute, manufacturers of asbestos building products launched a similar public relations agency in the United States, The Safe Buildings Alliance (SBA). It also promoted the argument that chrysotile was the “safe” asbestos. Public and governmental relations headed the list of its purposes. The Safe Buildings Alliance coordinated closely with trial and regulatory counsel, keeping them updated about every governmental initiative

that could affect litigation strategies and evidence. One of its major goals was to convince building owners that they did not need to remove asbestos products, particularly spray-on fireproofing and sound proofing, used in the construction of their buildings. Major funders for the SBA included former makers of asbestos spray-on products for buildings, W. R. Grace, National Gypsum, United States Gypsum, and Celotex. The SBA’s primary activities included lobbying in Washington, D. C., preparing public relations informational bulletins, and organizing scientific meetings and conferences.\footnote{Safe Buildings Alliance, \textit{Asbestos in Buildings: What Owners and Managers Should Know} (Washington, D.C.: Safe Buildings Alliance, 1989); and Geoffrey Tweedale, et al., “Chrysophiles versus Chrysophobes,” 251.}

As the 1980s drew to a close, both industry and public health advocates prepared large conferences to advocate their positions. Both conferences were attended by leading scientists. Both evaluated the hazard posed by asbestos in the environment. Both agreed that asbestos could be deadly, causing scarring of the lungs and cancers. That, however, is where agreement stopped. Other than the half of the audience composed of lawyers, almost no one attended both conferences. At the first conference, held in 1988 at Harvard University and sponsored jointly by the Safe Buildings Alliance and a realtor association, the doctors and scientists, most of whom were industry consultants and expert witnesses—or soon would be—explained why asbestos in buildings was not a cause for concern. Shortly after the Harvard Symposium, articles appeared in \textit{The New England Journal of Medicine} and \textit{Science}, extolling the virtues of chrysotile while lamenting that its detractors painted it with the same brush as the amphiboles. Although at least two of the authors, J. Bernard L. Gee and Morton Corn, frequently appeared as expert witnesses for industry asbestos trial counsel, including myself, neither article disclosed their industry connections. Subsequently, industry defense counsel enthusiastically used both articles at trial.\footnote{Brooke T. Mossman and J. B. L. Gee, “Asbestos-Related Diseases,” \textit{New England Journal of Medicine} 320 (June 29, 1989): 1721-1730; Brooke T. Mossman, J. Bignon, M. Corn, A. Seaton, and J. B. L. Gee, “Asbestos: Scientific...}
Irving Selikoff hosted the second conference in New York in 1990. The scientists and doctors at this conference—many of whom were plaintiff attorney consultants or soon would be—explained why a third wave of asbestos disease approached due to the asbestos used in construction throughout the 1960s and 1970s. Plaintiff counsel attending this conference likely prepared lists of potential new client categories.\textsuperscript{121}

In addition to public relations efforts and new review articles, industry attorneys were also receiving trial assistance from Canadian researchers. From 1966 into the 1990s, J. Corbett McDonald, a professor at McGill University in Montreal, Canada, conducted a massive study of 11,000 Quebec miners and millers. This study emanated from the Institute of Occupational and Environmental Health (IOEH), which was launched in 1966 by the Quebec Asbestos Mining Association (QAMA). Although McDonald denied that the IOEH was an industry initiative, it was the main recipient of QAMA research funding, receiving over $500,000 for research between 1966 and 1972. In funding the study QAMA intentionally took a page out of the tobacco playbook, mentioning with admiration tobacco industry’s research efforts.

A first and unanimous recommendation was the carrying out of the epidemiological survey proposed by Dr McDonald. The consensus of opinion seemed to point out that the QAMA should take into its hands the ways and means to conduct the necessary research instead of doing it through universities or letting it fall in the hands of the Government. As an example, it was recalled that the tobacco industry launched its own program and it now knows where it stands. Industry is always well advised to look after its own problems.

During the later stages of the study McDonald appeared as an expert witness for asbestos products manufacturers who were being sued by building owners for removal of the asbestos in

\textsuperscript{121} Geoffrey Tweedale, et al., “Chrysophiles versus Chrysohpobes,” 239-40. Having attended both conferences, I can confirm the importance of lawyers at both of them. At both conferences plaintiff and defense attorneys comprised a significant segment of the audience. Most of the audience discussion questions came from either attorneys or medical/scientific experts who were being provided significant funding by either plaintiff or defense attorneys.
their buildings. In his testimony McDonald argued that chrysotile only caused mesothelioma when it was contaminated by amphiboles, such as tremolite. He further contended that proper milling and screening could and did remove the tremolite from most Canadian chrysotile used in manufacturing.\textsuperscript{122}

By the late 1980s and 1990s McDonald and others argued that chrysotile only caused mesothelioma when it was contaminated with amphiboles. McDonald and other McGill scientists, as well as their sponsors, argued that proper milling and screening could and did remove tremolite from most Canadian asbestos used in manufacturing.\textsuperscript{123}

The British Occupation Hygiene Society (BOHS) published the completion of McDonald’s cohort study of eleven thousand Quebec miners and millers in a 1997 issue of its journal \textit{Annals of Occupational Hygiene}. The journal also invited one of the study’s authors to write a guest editorial. In keeping with the intense antipathy of industry and the necessities of litigation defense, Canadian epidemiologist and biostatistician Doug Liddell used the editorial to launch an attack on the “menace” and “intense malice” of the Mount Sinai physicians—a group he characterized as “The Lobby.”\textsuperscript{124}

Just as had occurred in the 1930s silica crisis, in an effort to control or neutralize the leading medical authorities, asbestos litigation defense attorneys (including myself) were continuing to contact and arrange consulting agreements with many of the near legendary figures in asbestos. Those contacted and used by defense counsel in litigation included such legends of early cancer research as Sir Richard Doll and Christopher Wagner. Similar to Sir Richard Doll, Christopher


Wagner held a hallowed position in asbestos lore, in Wagner’s case for his landmark South African study in 1960 which definitively identified mesothelioma—normally an extremely rare disease—as having a close causal association to asbestos, and being particularly prevalent among crocidolite workers. Following Wagner’s initial findings, the mining companies had withdrawn support for the research and suppressed a survey report, but the research was published in a British medical journal. This publication attracted so much criticism from the mining industry that it became difficult for Wagner to conduct research in South Africa. He subsequently immigrated in 1962 to Great Britain where he joined the government’s Pneumoconiosis Research Unit in Wales. 125

Wagner’s subsequent writings and experience in litigation starkly illustrates some of the methods used in those contacts (and contracts) and their subsequent effects on science. In essence, Wagner, a giant among the asbestos occupational medicine field, became a poster child for the manner in which attorneys can influence occupational science. 126

When Wagner first came to prominence more than twenty years earlier as the “discoverer” of the asbestos/mesothelioma relationship, he had believed that asbestosis was not a necessary prerequisite to diagnose an asbestos related mesothelioma or lung cancer. He had also believed that all forms of asbestos caused lung cancer and mesothelioma. For example, in 1962 Wagner had opined that well developed asbestosis was not necessary for establishing the etiological role of asbestos in lung cancer. At the 1964 New York Academy of Science Asbestos Conference, he had even presented evidence linking Canadian chrysotile with mesothelioma. Even as late as 1979, Wagner had endorsed IARC’s (and Wagner’s) finding that chrysotile caused mesothelioma. One year later, in response to a letter from Johns Manville, he wrote that his rat experiments produced a few

126 Much of the following material concerning Christopher Wagner is taken from Geoffrey Tweedale, et al., “Chrysophiles versus Chrysophobes,” 252-53; and Jock McCulloch, “Saving the Asbestos Industry,” 612.
mesotheliomas from chrysotile.\textsuperscript{127} Thus, over a period of twenty years Wagner’s opinions remained consistent: he believed all forms of asbestos caused mesothelioma and lung cancer.

This all changed during the late 1980s. From 1986 until 2001 Owens-Illinois, the former manufacturer of a leading type of steam pipe asbestos insulation (Kaylo), made regular payments to Wagner through an asbestos defense legal firm. While it is not entirely clear why he was retained, it was likely to keep Wagner from testifying for plaintiff attorneys and to have access to him for testimony and authoring medical articles about asbestos. The total sum paid to Wagner probably surpassed $300,000, a significant amount at that time, particularly in Britain where research wages were low. Wagner’s monthly retainer often exceeded $6,000.\textsuperscript{128}

Wagner’s views about asbestos risks changed no more than four years after his first payment from Owens Illinois. That year he testified that even heavy exposure to chrysotile did not cause mesothelioma. Since Kaylo—the asbestos pipe insulation material manufactured by Owens-Illinois and its successor manufacturer, Owens Corning Fiberglas—contained amosite, attorneys also convinced him to testify that not even all amphiboles are alike. At a 1990 London deposition he not only testified that chrysotile does not cause mesothelioma, but also that he was unsure about amosite. He stated that he would only go so far as to say amosite can “probably with very heavy dosage” cause mesothelioma.\textsuperscript{129}

Wagner expressed similar views throughout the 1990s. His service on the independent Health Effects Institute’s Asbestos Research Literature Review demonstrates just how far he had

\textsuperscript{127} W. C. Hueper to Dr. H. L. Stewart, dated August 6, 1962, MSC C 228, Box 17, Archives of the National Library of Medicine. In this letter Hueper wrote that he agreed with Dr. Wagner’s opinion that asbestosis is not necessary to diagnose asbestos related lung cancer; J. Christopher Wagner, “Epidemiology of Diffuse Mesothelial Tumors: Evidence of an Association from Studies in South Africa and the United Kingdom,” \textit{Annals of the New York Academy of Sciences} 132, no. 1 (December 31, 1965): 575-578; Bogovski, P., J. C. Gilson, V. Timbrell, and J. C. Wagner, eds. \textit{Biological Effects of Asbestos} (Lyon: International Agency for Research on Cancer, 1973); Geoffrey McCulloch, “Saving the Asbestos Industry,” \textit{612}.


\textsuperscript{129} Author’s personal experience.
diverged from mainstream medical opinion. Among the eighteen panel members chosen from across the spectrum of opinions, Wagner was the sole dissenter from the panel’s 1991 finding that chrysotile causes mesothelioma. These views only became stronger as the decade advanced. In early 1997 he wrote a letter to the editor of the *American Journal of Public Health*, reiterating his position that crocidolite caused the vast majority of the mesotheliomas.\textsuperscript{130}

By the late 1990s, however, Wagner lamented his decision to work with industry attorneys. When interviewed by a prominent public health historian in 1998, Wagner expressed regret “that he had allowed himself to be compromised.” During the interview, Wagner “complained about how the asbestos industry set out to frustrate scientific discovery and how science had been hijacked by lawyers and the press—so much so that he expressed regret that he had ever worked on ARD.” Even then he did not disclose his consultancy, which only emerged during legal discovery in 2001, shortly before Wagner’s death.\textsuperscript{131}

Although these activities of industry lawyers, consultants, and public relations representatives helped reduce payments in asbestos lawsuits, the mainstream international medical profession did not follow their lead. By the late 1990s most medical authorities worldwide had become convinced of the risks of chrysotile. The Canadian government, the Canadian mining industry, and American asbestos product manufacturers stood almost alone in advocating the “safe usage” of chrysotile. For example, in 1997 a Helsinki multidisciplinary panel concluded that while less potent, chrysotile still


\textsuperscript{131}Geoffrey Tweedale, et al., “Chrysophiles versus Chrysophobes,” 252-53; J. Christopher Wagner, “Asbestos-Related Cancer,” 687-688. The Wagner quote is from Jock McCulloch, “Saving the Asbestos Industry,” 612. While defending Wagner at an early 1990s deposition at which Wagner was testifying for my client as an expert witness, I personally experienced Wagner’s willingness to modify his opinion as needed. At the time the author, there to defend Doctor Wagner at the deposition, was surprised but very pleased at how accommodating Wagner was to the author’s position, when questioned about whether there were health effects distinctions between crocidolite and amosite—amosite being a key component in my client’s primary asbestos product, Kaylo. At the time, the author was not aware that Wagner was under retainer by Kaylo’s earlier manufacturer, Owens Illinois.
caused mesothelioma. In 1998 a monograph published under the auspices of the U.N. Environmental program, the World Health Organization and the International Labor Organization concluded that “[e]xposure to chrysotile asbestos poses increased risks for asbestosis, lung cancer, and mesothelioma in a dose-dependent manner. No threshold has been identified for carcinogenic risks.” In 2001 The World Trade Organization agreed that there is no safe limit to asbestos, all types are carcinogenic and “controlled risk” during manufacture, usage and disposal is unachievable. 132

The next two chapters will examine the history of science for two other toxic substances, chromium and benzene. Chromium science in the late twentieth and early twenty-first centuries demonstrated the increasing sophistication and ability of industry related scientists—working closely with attorneys—to produce the evidence necessary for trial. Although benzene science has not had as clear a smoking gun to attorneys, it has displayed the same characteristics as both asbestos and chromium science. Finally, I shall return to industry’s asbestos and silica science in the twenty-first century, examining its continuing evolution.

CHAPTER 7

A TALE OF TWO CANCERS: THE CHROMIUM EXPERIENCE

Introduction

It was the best of science, it was the worst of science, it was the age of
wisdom, it was the age of foolishness, it was the epoch of belief, it was the
eoepoch of incredulity . . .

(With apologies to Mr. Dickens)

This saga is a history of two cancers, lung and stomach, involving hexavalent chromium
research events on two United States coasts, East and West, and two countries, the United States
and China, between the 1940s and 2015. The occurrences comprise efforts to reduce hexavalent
chromium pollution and compensate for exposures to it. Like the classic Charles Dickens novel, this
story involves suspense, intrigue, and love (of money). Unfortunately, unlike Dickens’ story, this tale
is true.

The chromium account is exceedingly complex, involving both litigation in California and
regulatory action before OSHA, the EPA, and state environmental agencies. Several of the
underlying events occurred in and near Shanghai, China. Like the history of tobacco, chromium
provides an archetypal example of how attorneys have influenced medical research, literature, and
regulatory activities. The story reaches a climax in the Los Angeles area with two lawsuits involving
ingestion of chromium; the Hinkley and Kettleman cases—Hinkley being made famous in the
movie Erin Brockovich, released in 2000. Both of these cases involved chromium in the towns drinking
ground and drinking water. Many of the documents disclosing attorney roles and the shaping of the
chromium scientific literature for litigation and regulation came to light in these lawsuits. Studies and
actions that occurred during regulatory proceedings before the California Agency, OSHA and the
EPA further illuminate the role of attorneys in shaping medical science. Finally, a public health advocate article, which resulted in a lawsuit filed by EPA against Elementis Chromium, demonstrates how scientific studies funded by the companies can be misused and distorted in the peer-reviewed literature.¹

Chromium Background

Chromium is a naturally occurring metal. Since its first commercial usage in the early nineteenth century, chromium and its salts have been used in an ever-widening array of applications in the chemical industry, artistic and anti-corrosion paints, electroplating, welding, and alloys. It is used in steel manufacturing, leather tanning, and the production of dyes, pigments, plate metal surfaces, and alloys. It is also a major component of the pesticides used in pressure-treated wood. Although trivalent chromium, often called Cr(III), is a natural necessary nutrient, hexavalent chromium, or Cr(VI), is produced primarily through industrial processes and enters living cells more readily than Cr(III), causing a variety of ailments including vomiting, convulsions, ulcers, kidney and liver damage, and cancer. The National Toxicology Program of the Department of Health and Human Services has labeled the hexavalent chromium compounds calcium chromate, chromium trioxide, lead chromate, strontium chromate, and zinc chromate as “known human carcinogens” since 1980. The Agency for Toxic Substances and Disease Registry (ATSDR) has pronounced Cr(VI) to be one of its “Top 20 Hazardous Substances.” OSHA estimates that one million U.S. workers have regular hexavalent chromium exposures.²


The level of hazard presented by Cr(VI) in drinking water is more controversial. When orally ingested in sufficient quantities it can penetrate tissue and organs throughout the body. Although converted to the safer Cr(III) by stomach acid, the speed of the conversion is uncertain.3

The Early History

Chromium has long been known as hazardous. Its acute toxic properties became evident shortly after discovery of the metal in 1797 by French chemist Louis Vanquelin. The first American deposits of chromite were discovered just north of Baltimore in 1827, with production of potassium bichromate commencing in Baltimore by 1845. The textile industry became the first major utilizer of the element’s mineral forms, as pigments. During the second half of the century, electroplating with chromium also became common.

Doctors quickly recognized some of its dangers. By the late 1800s they identified veteran workers by the hole in their septum. Health complaints riddled chromate plants. Early medical experiments confirmed several chromium-caused diseases and conditions. One 1877 experiment determined that stomach and intestinal effects appeared rapidly in animals after chromate solution injections in the stomach and intestinal tract. These early case reports and experiments suggested further toxic effects on the kidneys, liver, and perhaps the heart.

Chromium joined the ranks of suspected carcinogens in 1890 when Glaswegian doctor David Newman wrote a case report describing the adenocarcinoma of a 47-year-old worker, previously exposed to chromate pigments. Determining his work history was not difficult for the

the Chrome Coalition provided outlines of testimony to OSHA suggesting that “OSHA’s exposure profile recognizes only a fraction of the potentially exposed workforce.” If this statement is correct—and not just a tactic to limit regulations due to economic infeasibility—at the time millions of additional workers were being exposed to chromium under regulations that all parties agreed did not protect against cancer. Kathryn M. McMahon-Lohrer to Docket Office, Docket H054A, OSHA, attachment B, paragraph I.A., dated January 3, 2004 (sic 2005), OSHA Docket H054A, Exhibit 40-12-1.

doctor. His work exposure became readily apparent from the large, rounded twenty-year old perforation of his septum, a condition shared by many of his fellow chromate laborers. For the next forty years the European medical literature reported occasional cases of lung cancer among chromate workers, while even corporate doctors acknowledged widespread disablement due to acute poisoning. In 1911 a doctor notified the local work inspectorate in Germany of two lung cancers at a chromate production plant. In December 1936, following additional reports, German health officials accepted lung cancer as work-related among chromate workers. That same year, Teleky first drew attention to the possible connection between chromates and cancer of the digestive track, when he observed five digestive tract cancers among forty-four deceased chromate workers. Since then epidemiology studies have occasionally commented on small increases in digestive tract cancers.4

Nor was this knowledge confined to Europe. Officials at German chemical giant I. G. Farben had sent at least one review article to the largest United States producer of chromium, Mutual Chemical Company (Mutual). The case reports elicited little immediate reaction other than the letter's recipient, Herbert Kaufman, manager of the Jersey City plant and son of the Company's...
president, requesting to “have correspondence concerning this general topic addressed directly to the writer.”

Three years later, an American Medical Association’s publication of a letter to the editor instigated slightly more action. A Jersey City, New Jersey doctor wrote the Journal of the American Medical Association’s editor asking whether his patient’s work at a chromium factory might have been the cause of his lung cancer. The editor replied affirmatively, referring the doctor to five journal articles. Having apparently read the JAMA article, Herbert M. Kaufman, Mutual’s president, asked both plant managers about the issue. Harry Heller, his Baltimore facility’s co-manager, responded that he knew of the articles, noting “confidentially” that although the incidence was still low, company employees had more lung cancer than the general populace. Kaufman’s return letter cautioned that the company should not comment on the issue, but plant managers should improve the work environment.

Throughout the 1930s German clinicians continued finding chromate industry lung cancers. In 1940 a German doctor reported a further thirty-eight cases of lung cancer in Germany. During the early stages of World War II, Wilhelm Hueper, always interested in occupationally related cancers, took notice, discussing the clinical aspects of the disease during his review of the European literature.

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5 E. Pfeil, “Lungentumoren als Berufserkrankung in Chromarbetrieben,” Deutsche Medizinische Wochenschrift 61 (July 26, 1935): 1197 (The author’s eleven page copy is from ICO v. Honeywell (ICO); H. J. Kaufmann to I.G. Farbenindustrie, January 15, 1936, (ICO). The author tracked down the documents at the National Library of Medicine after reading the Mutual chemical chromium history—which will be discussed shortly—in Benjamin Ross & Steven Amter, The Polluters – The Making of our Chemically Altered Environment (New York: Oxford University Press, 2010). The authors of this book may have sent the ICO documents to the National Library of Medicine. When this author requested the documents from the Library the researcher had difficulty finding them. Upon production, the Library researcher indicated that their retention by the Library was unusual, since the library did not normally collect or keep evidence from legal cases.

6 W. H. Longley letter to the editor (and reply), “Chrome Dust and Lung Cancer,” Journal of the American Medical Association 111, no. 7 (1938): 645; H. M. Kaufmann memorandum to H. J. Kaufmann and H. Heller, August 31, 1938 (ICO); H. Heller memorandum to H. M. Kaufmann, September 1, 1938 (ICO); and H. M. Kaufmann memorandum to H. Heller and H. J. Kaufmann, September 2, 1938 (ICO).

As concerns about the health of workers in plants and shipyards increased during the later stages of World War II, the U.S. Employment Service cautioned prospective employees about unhealthy conditions at Mutual Chemical Company plants. This warning brought about an investigation by the Industrial Hygiene Division of the Public Health Service. Ever ready to help businesses, the USPH undertook the study not to learn about the hazards, but rather “primarily for the purpose of overcoming the disinclination of men to work in Bichromate plants and in the hope that they would be able to assure the USES that there is no need for the latter to continue to caution prospective referrals concerning working conditions.” By the end of 1945 Kaufman received reports of the investigation’s initial findings of a problem, confirming at least one additional lung cancer at the Jersey City plant.8

Mutual’s owner evidently decided on a dual track response to these events, expressing concern about the “new” knowledge, while at the same time developing a workmen’s claims defense strategy. He hired Hueper in 1946 as a consultant to address the “new” knowledge problem. Unaware that IG Farben had been keeping Mutual advised of their experience, Hueper recommended an analysis of the German experience and an epidemiological study of the plants. Following receipt of Hueper’s report, Omar Tarr, a vice president at Mutual, toured Germany’s chromium plants. There he learned that German manufacturers considered the relationship of chromate with cancer a proven fact. Upon his return, and possibly as a result of his findings, company officials hired the Saranac Institute as a consultant for chromium health issues. Its

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8 G. A. Benington memorandum to O. F. Tarr, December 22, 1945 (ICO) (primarily for the purpose); and Benjamin Ross, et al., The Polluters, 68.
director’s death in October ended the short relationship, with Anthony Lanza taking over as replacement.

This track overlapped—and may have been subservient to—the second track dealing with the compensation claims. Baltimore insurance attorney Theodore Waters began advising Mutual at least by late 1946, when, in collaboration with Lanza, he began organizing Mutual’s defense in chromium lung cancer claims. As noted previously, Waters brought vast experience to the collaboration, having helped the Industrial Hygiene Foundation and corporations shut down most claims for silicosis. The writings by Lanza and Waters during this period are very circumspect, likely because of the potential of disclosure in litigation, but both met on February 1, 1947 with Mutual executives to discuss an unidentified problem likely related to chromium workers compensation.\(^9\)

Although the full influence of Waters is not known, the next action by Mutual also suggests they relied upon his experience in mitigating silicosis claims. Apparently upon Lanza’s recommendation, Mutual decided to conduct an epidemiological study of the plant, although by now it was obvious they had a substantial lung cancer problem. They may have been attempting to limit the areas of the plant from which claims could be approved. Their choice to conduct the study, Willard Machle, was an industrial hygiene consultant who had worked for a decade at Robert

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\(^9\) Benjamin, et al., *The Polluters*, 68-9. Hueper was brought on board immediately. However, by the time of the hiring of Anthony Lanza and the Saranac Institute, Theodore Waters may have been brought on board as a result of the compensation claims. Waters was very familiar with Gardner and Lanza, since both experts spent considerable time assisting in the defense of silica and asbestos disease compensation claims. Given the findings of Machle discussed below, the motives of Mutual are hard to understand. After Tarr’s visit to Germany, Mutual must have known what any reputable study would find. Ross suggests that they were attempting to post-date the discovery in order to bolster their defense of claims. Benjamin Ross et al., *The Polluters* 68-70, 193(footnote 32). This is possible. It is also possible that given Machle’s prior association with the company oriented Kettering Laboratory, they hoped he could be influenced to provide an ambivalent report. The importance of compensation claims to Mutual should not be underestimated. Mutual had experienced “a flood of petitions” in the 1930s after a compensation board awarded $900.00 to an a former employee suffering from a perforated septum and “throat involvements.” Jerome Dohan memorandum to G. A. Benington, February 3, 1947 (ICO). In a note attached to a letter to Machle, Mutual’s president, George A. Benington discussed organizing a unified medical service with other manufacturers bearing “in mind that a Jersey City physician might carry more weight if and when he appears before the Compensation Board than would a physician from Newark or some other city.” George A. Benington to Willard Machle, August 25, 1948 (ICO). Mutual also determined to only accept “valid claims” that were within the one year statute of limitations. O. F. Tarr memorandum to A. G. Noble, July 28, 1948 (ICO).
Kehoe’s Kettering Laboratory, a medical research establishment well known to be friendly to businesses. The result, known in advance by Mutual’s hierarchy, confirmed the high lung cancer rate. With the assistance of a second doctor, Frederick Gregorius, Machle then expanded the investigation to include the nation’s other five chromium production plants, owned by four additional companies. The results, published in August 1948, provided further confirmation of the extreme toxicity of chromium. The report confirmed an exceptionally high rate of lung cancer for chromate workers, with most plants having rates ten to forty-three times that found in other comparable industrial groups. The average lung cancer death rate at the factories exceeded that the general public rate by twenty-five times.\(^\text{10}\)

In reaction to this alarming revelation, the Public Health Service’s Industrial Hygiene Division (IHD)—ever ready to assist industry—proposed yet another study. James Townsend, the division’s chief, made the suggestion to industry representatives at an Industrial Hygiene Foundation meeting in Pittsburgh. One aspect of the study held the potential to advance science; the study’s scope expanded to include investigation into neighboring community exposures, possibly in reaction to Hueper’s work. Shortly thereafter Omar Tarr and chief counsel Waters met with Townsend and Bloomfield to discuss the November 18\(^{\text{th}}\) plan. Likely concerned about additional lawsuits, Waters specifically asked for deletion of all off-site (community) investigations, a request Townsend promptly granted. Planning stretched into the new year, with Lanza, perhaps at the recommendation of Waters, being retained by industry as its liaison to the IHD. In January New Jersey health officials sought to conduct separate sampling of the New Jersey facility but were convinced to accept USPH’s survey and air sampling for which Mutual was provided over one week’s advance notice.\(^\text{11}\)

\(^{10}\) Willard Machle, et al., “Cancer of the Respiratory System,” 1114-1115, 1121. In his study, Machle found 42 out of 193 deaths among chromate workers due to lung cancer, when only three would be expected in other reference industries. Also see Sverre Langard, “One Hundred Years,” 192.

\(^{11}\) O. F. Tarr to A. K. Van Wirk, December 27, 1948 (ICO); O. F. Tarr to William Buroda, December 30, 1948 (deletion of community study) (ICO). Waters played a central role in these activities. Theodore Waters to C. George Krueger,
At the same time, Mutual took action to upgrade its environmental controls—undoubtedly in an effort to eliminate future claims and retain its workforce, now that employees understood the danger. With Tarr in control, Mutual developed plans for a new plant, intended to replace both of its older facilities. A retained engineer incorporated features designed to reduce chromium exposures both inside the plant and in the surrounding community. Health and environmental controls amounted to more than 10% of the total construction costs, an enormous sum in an era of little regulation. Yet, given the rising rate of lung cancers, and the likely necessity of similar actions by other producers, it was an amount eminently justifiable to shareholders. The new plant opened in 1951.12

Although no longer employed by Mutual, Hueper continued pursuing answers about chromium’s toxicity. Notwithstanding objections by the IHD, at Hueper’s instigation NCI funded a study of a chromium plant in Ohio being conducted by Thomas Mancuso of the Ohio Health Department. In 1951 the limited epidemiological study further confirmed the high rate of cancer among chromate workers. Mancuso and Hueper’s conclusion in the first of two articles displayed

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the typical conservatism of medical researchers, with a simple statement that epidemiologic, biochemical and histologic evidence all suggested dust from insoluble chromium compounds “may play a causal role in the production of lung cancer.” Both papers raised troubling issues. First, trivalent chromium appeared to also pose a limited risk. Second—and potentially more dangerous to chromium manufacturers’ bottom line—the cancer hazard might not be limited to workers, but also extend to the surrounding community.  

Although he participated as fully as in the first paper, Hueper’s name did not appear on the second article because of objections Mutual agents made to his superiors. Hueper had provided Tarr with a courtesy copy of the yet to be published second paper, who in turn gave it to Lanza. Lanza sought deletion of what he saw as problematic conclusions. When Hueper refused, Lanza complained to the Public Health Service. In response, the new head of the IHD told Hueper to remove his name. In July Tarr told Hueper that Lanza was behind the demand. Hueper subsequently removed his name from the manuscript, but not one to back down from a fight, complained to the Surgeon General. In response to Hueper’s mettlesome (as viewed by many in the Public Health Service hierarchy) nature—and perhaps at Lanza’s or Waters urging—the Surgeon General shut down Hueper’s entire outside program, ordering Hueper to cease all field work, contacts with state and local health agencies, and work on chromium. At Congressional hearings Lanza denied any misconduct, while Hueper’s superiors claimed purely administrative motives in curtailing Hueper’s activities. The written evidence belies both of these claims.

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13 T. F. Mancuso and W. C. Hueper, “Occupational Cancer and Other Health Hazards in a Chromate Plant: A Medical Appraisal I. Lung Cancer in Chromate Workers,” Industrial Medicine and Surgery 20, no. 8 (August 1951): 368-363, 363. As early as December 13, 1948 Krueger was advising Tarr that he had received requests from USPH for information on a number of hazardous industries, including chromates. Tarr surmised this was from Hueper. O. F. Tarr to A. E. Van Wirt, December 13, 1948 (ICO).

14 At first Hueper did not seem to realize his program’s closure came from Mutual’s complaints. As late as November 1953 he sent a friendly letter to Tarr asking for continued short-term cooperation and informing him of the department’s request that he discontinue “further work on the chromium problem” within six months. W. C. Hueper to O. F. Tarr, November 30, 1953 (ICO). Benjamin Ross et al., The Polluters 69-71. While the evidence suggests general concern on the part of Mutual officials, this anxiety—with the possible exception of Tarr—appears to be directed more
Still, by 1952 Harriett Hardy could announce that the subject of lung cancer among chromate workers was receiving “considerable attention” in the United States and abroad. She specifically noted Mancuso’s Ohio findings of fifteen the expected rate of lung cancer among chromate workers, as well as Baetjer’s similar Baltimore findings.\(^\text{15}\)

**Water Contamination Issues**

The “considerable attention” Hardy had described in her article focused almost exclusively on airborne chromium exposures. Notwithstanding scattered reports of digestive tract cancer, public health and other medical practitioners gave little thought to water borne contamination. Still, legal cases on groundwater contamination are not new. European court proceedings commenced as early as 1349. In the United States the World War II’s industrial expansion, together with the breakneck growth of synthetic chemicals, resulted in numerous episodes of groundwater contamination, with concomitant governmental activity. At least five states (New York, Michigan Virginia, California, and Maryland) reported substantial incidents.\(^\text{16}\)

The unique environmental, industrial, and cultural aspects of southern California brought it to the forefront in post-war efforts to control groundwater pollution of chromium and other substances. In 1946 the State Health Department issued new regulations intended to provide enforcement of existing but neglected laws. As was done for both asbestos and silica, industry mobilized to control the debate and declare the problem solved. The effort commenced by ensuring

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\(^\text{16}\) Benjamin Ross et al., *The Polluters* 107-109.
that the legislature investigated the issue, with a pro-business Republican assemblyman, Randal Dickey, chairing the responsible committee. They then established a new trade organization, the California Association of Producing Industries, to take the lead in arguing that the pollution was the result of household sewage.

In the spring of 1949, the committee report first noted that public health officials, scientists, and the public all were concerned about toxic chemicals. The report stressed, however, that this concern was misdirected. It insisted that industry was extremely important to California and direct threats to public health from groundwater pollution almost always came from sewage rather than industrial waste. While there was “the remote possibility of poisoning by some of the chemical wastes,” few were hazardous to the public health. Subsequent proposed legislation only allowed the state to intervene in industrial pollution after a finding of imminent hazard to human health. After the Governor denounced the bill and huge crowds attended the next committee hearing, a compromise was reached which allowed local governments to basically continue operating as they had before.

A year later the State Water Pollution Control Board commissioned a collection of data on contaminants. The Board’s choice to collect and present the information, Jack McKee, was a new sanitary engineering professor at Caltech, having formerly been a partner in a Boston engineering consulting firm. While at Caltech, McKee maintained a consultation business with municipal and industrial clients. In his subsequent compilation of the data, McKee often did not include studies on cancer-causing substances such as chromium, because the studies did not determine “limiting or threshold concentrations.” If no safe dose was known then how could a standard be proposed? Thus, his entry for chromium did not mention lung cancer among plant workers. After observing that the U. S. Public Health Service had set a chromium-drinking standard, McKee dismissed it.
“Recent observations,” he sniffed, “tend to discount the fears of the U.S.P.H.S. and the foregoing statements relative to the physiological effects of hexavalent chromium.”

Reappraisal: The Convergence of Chinese and California Water Pollution Activities

That dismissal eventually changed, thanks to seemingly unrelated 1950s events in two small rural communities half a world apart. The activities in these locations became the opening act in efforts to control drinking water hexavalent chromium contamination in the United States. In 1952 Pacific Gas & Electric Co. (PG&E) began mixing chromium with water to prevent rust at a pumping station in Hinkley, a small rural desert village near Barstow, California.

During the final year of the decade, in a rural district of the northeastern Chinese province of Liao-Ning, the JinZhou Alloy Plant began processing chromium ore. With China striving to industrialize, officials initially paid little attention to environmental problems, such as the plant’s release of chromium-contaminated wastewater into a nearby dry riverbed and piling tons of solid waste outside of the plant. Six years later, with the plant at full-scale production, Doctor JianDong Zhang, a biomedical researcher exiled by the government to this remote area, took his first samples of the wells in two nearby villages, confirming villager claims of severe chromium contamination in almost a third of the wells. By the end of that year some of the well water looked yellow. Sampling confirmed the contamination had progressed to some of the wells in three more distant villages.

Ironically, in the same year that Zhang first conducted his survey at JinZhou, villagers in Hinkley began noticing problems with their well water. Ten days before Christmas, PG&E’s local

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17 Benjamin Ross et al., *The Polluters* 112-3.
station chief, Richard Jacobs, examined the greenish deposits—chromium causes greenish deposits—in Hinkley farmer John Speth’s toilet bowl, then lied; telling him that PG&E had nothing to do with it. Speth died of stomach cancer in 1974.20

Following the pollution discoveries, investigations in China and California took widely diverging tracks. By the early 1970s the chromium-laced plume reached even more distant villages in Liao-Ning province. Zhang conducted health surveys of residents in five contaminated villages near the JinZhou plant from 1965 to 1974. He uncovered a variety of ailments associated with hexavalent chromium (Cr(VI)) exposure, including mouth ulcers, diarrhea, abdominal pain, and vomiting. Zhang began publishing series of articles, in local medical journals, culminating in a 1987 article that linked Cr(VI) to higher rates of stomach cancer. Finally, likely in response to Zhang’s investigations, the Chinese government erected a concrete barrier around the plant’s perimeter.21 But in California, PG&E took no action: it did not disclose its findings until 1987. Before turning to the conclusions of these two stories, however, let us first follow the regulatory trail.

**Chromium Regulation and Litigation**

As recommended by the American National Standards Institute in 1943, the mid twentieth century industry established voluntary exposure limit for chromium stood at 52 micrograms in each square meter of breathing zone air (52µg/m²). The standard did not consider the potential of cancer, but rather sought to limit the occurrence of chromium-exposed worker dermatitis, skin ulceration, and nasal perforations previously reported in 1924 and 1928 studies. Upon its the creation in 1971 OSHA, adopted the standard as the only limit available, even though everyone understood that the

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standard was not fully protective. In 1975 the National Institute for Occupational Safety and Health (NIOSH) urged action limiting the allowed exposure to 1µg/m³. One year later, Doctor Morton Corn, then head of OSHA, posted notice in the Federal Register about the agency’s decision to reduce the limit, based upon the conclusion “that a comprehensive occupational health standard is urgently needed to protect employees from the harmful effects of exposure to Chromium 6 . . . [and intends to] the rulemaking proceeding in the shortest time possible.” In the end, years slipped by with no change in the standard, as first other substances took priority, then judicially required individual risk assessments for each chemical standard slowed regulation to a crawl, and finally political backlash created a milieu opposed to almost all regulatory efforts.22

Industry and its supporters encouraged this inaction by suggesting that since they had reduced chromium exposures significantly from historical fog-like levels, the risk of respiratory cancer was significantly reduced or even eliminated. Much of this reassurance occurred through the auspices of the IHF. Among other industry-funded activities, the IHF sponsored three Chromium Symposia in 1977, 1980 and 1986, each specifically designed—similar to the Silica Conference in the 1930s—to demonstrate that industry had solved the problem.23

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23 David Serrone, Ed., *Proceedings – Chromium Symposium 1986*; See, for example, David M. Serrone, “Editorial,” v-vi, v. (Serrone argued that the dose response curve and question of threshold must be based on “current scientific understanding and knowledge.” While pronouncing that “the risk of respiratory cancer has been eliminated,” he then argued that “[r]esearch efforts at all levels . . . are needed so that chromium chemicals can continue to be made and used safely.” (emphasis added) Most other speakers, when they discussed chromium related cancer, expressed similar reservations about its relevance for modern industrial workers. “Epidemiological studies in man confirm the carcinogenic nature of this metal, at least under certain circumstance.” The symposium will provide the opportunity “to discuss whether or not there need to be more stringent regulations governing human exposure to chromium . . .” (emphasis added) William, O. Berndt, “Introductory Remarks,” V. One industry sponsored study presented at the conference suggested that only a very limited number of hexavalent chromium materials “seem to be capable of evoking a carcinogenic response” in rats. Leonard S. Levy and Patricia A. Martin, “An Experimental Investigation into the Carcinogenicity of a Range of Chromium-Containing Materials.” P174-191. An investigation by British Chrome and Chemicals Limited funded researchers found workers employed at the plant since 1961 did not have an increased mortality from lung cancer. “The important results of this re-analysis of data is that, for men employed [since 1961] there is no indication of a continuing risk of lung cancer related to occupation.” “Experience does not support the conclusions . . . that there is a potential excess risk of death from lung cancer among United States of America workers.
The little progress made in the 1980s toward regulations that accounted for cancer came not from OSHA but out of the EPA. In August 1984 the EPA released a Health Assessment Document for chromium. After heated correspondence from industry, EPA officials met with representatives of American chrome manufacturers and utilizers. In light of subsequent actions by the EPA, the meetings likely entailed industry representatives and attorneys contending that past major epidemiological studies held no current relevance since modern plants maintained vastly lower exposure levels. In other words, like occurred with silica, they claimed control measures taken by industry during the past twenty years had solved the problem. However, although industry could point to several limited epidemiological investigations that seemed to back this claim, at least one EPA researcher recognized that the industry-touted studies did not cover sufficiently long periods to determine whether or not the lower levels were safe. Furthermore, risk analysis based upon the Mancuso studies certainly did not suggest a sufficiently low risk for employees at the levels being touted by industry.

Industry’s effort to delay regulation succeeded. In light of the controversy, the EPA decided to delay action until they examined the lower levels of hexavalent chromium exposures resulting from increased control measures taken in the early 1950s. They contracted with Johns Hopkins epidemiologists to study Mutual Chemical’s Baltimore plant, including its more modern facilities, which regulated exposures at levels lower than the Plainsville factory studied by Mancuso. They also determined a need for a more long-term study that also examined confounding factors. As Tarr suspected might be the case in the early 1950s, seven years later the preliminary results demonstrated increased cancer risks throughout the plant. In the meantime EPA proposed a regulation to prohibit

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exposed to the current Permissible Exposure Limit.” P. L. Bidstrup and J. M. Davies, “Epidemiology: Update to 1985,” 192-209, 196, 202. In an almost solitary cautionary statement, EPA researcher Herman J. Gibb, warned that the British results may not be meaningful: “it is possible that workers who have experienced a decrease in exposure have not been followed long enough for cancer to appear” (author’s emphasis). His premonition was soon proven correct. Herman J. Gibb, “Carcinogen Risk Assessment of Chromium Compounds,” David Serrone, Ed. Proceedings, 248-309, 264.
use of chromium in air conditioning cooling towers, stating, “the agency has determined that hexavalent chromium compounds are potent human carcinogens.”

Eventually public health advocates and union professionals resolved to apply external pressure on the recalcitrant OSHA. In 1993 Public Citizen, a nonprofit investigatory social welfare organization, and the Oil, Chemical and Atomic Workers International Union (now part of United Steelworkers) petitioned OSHA to reduce Chromium (VI)’s Permissible Exposure Limit (PEL) to 0.25 µg/m³, less than a half percent of the then current PEL. Seven months later OSHA denied this request, citing “the extremely stringent judicial and statutory criteria” that must be met before issuing an emergency order. In his letter to Public Citizen, President Clinton’s head of OSHA, Joseph Dear, softened the refusal by announcing that OSHA had decided to commence rulemaking proceedings to establish a new PEL for hexavalent chromium, acknowledging that “[t]here is clear evidence... that the current PEL... can result in an excess risk of cancer.”

In the interim, OSHA commissioned a risk assessment to determine how low the standard for airborne hexavalent chromium should be. The contractor based its calculation upon the Mancuso and Hayes studies, concluding they provided a basis for obtaining “relatively consistent” risk estimates. The final report, completed in May 1995, determined that working with hexavalent chromium for forty-five years at the exposure standard then in force could result in 88 to 342 excess cancers, thus indicating that up to 34% of long term exposed workers could die of lung cancer. Yet that number palled in significance compared to the conclusion that even low exposures likely result in a significant number of deaths. Even the NIOSH recommended exposure level or 2 µg/m³, (as CRO3) could result in 1.8 to 8.9 excess cancer deaths. OSHA endorsed the analysis in its November

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24 Bernard D. Goldstein to Dr. J. R. Robinson, December 5, 1984; J. R. Robinson to Mr. William Ruckelshaus, November 7, 1984 (both contained in Lanier Archives. All documents cited as being from the Lanier Archives are also in the author’s possession. In the future they will be cited to as (Lanier)). David Michaels, Doubt is Their Product, 99, 100-101; David Michaels, et al., “Selected Science”; Harry Salem, “The Chromium Paradox,” 1.
1996 regulatory agenda entry, noting “[t]here appears to be no dispute that the current PEL is too high,” even while continuing to delay action.\textsuperscript{25}

By this time, industry was fully engaged in delaying action. A series of industry letters, many likely written or edited by attorneys, urged caution on the part of OSHA until better data became available from the new study being performed on the Baltimore plant by EPA and Johns Hopkins researchers. They declared that delay was essential because this study was “expected to be the most accurate and complete database on chromium exposure and mortality available.” OSHA’s initial response: it would not postpone formal rulemaking. Nevertheless, delays continued. Over the next several years OSHA promulgated a series of unrealized plans to issue a proposed notice: in April 1994, May 1995, November 1995, May 1996, April 1997, October 1997, November 1999, November 2000, and May 2001, each one moving the target date further into the future. Finally, in December 2001, following the change in presidential leadership to George Bush, the agenda simply listed hexavalent chromium rulemaking as a “long-term action” with an anticipated date of rulemaking notice “to be determined.”

Halfway through this process Public Citizen lost patience with OSHA’s reticence to commence rulemaking. In March 1997, Public Citizen asked OSHA for a commitment to a timetable. One month later, the agency set a target date of September 1998, a year and a half away. No longer willing to continue at what they saw as futile attempts to nudge OSHA toward recognizing the devastating consequences of industrial chromium exposures, the group filed a petition for review with the Circuit Court on October 13 challenging the agency’s “unreasonable

delay” in setting a standard that reflected Cr(VI)’s carcinogenicity. The court denied the petition after OSHA stressed its workload and proffered that it intended to issue a notice of proposed rulemaking by September 1999, yet another year later.

As noted above, OSHA failed to meet this—and subsequent—deadlines, failing to act even after Johns Hopkins researchers published their study confirming the high rate of cancer even in the new sections of the Mutual’s Baltimore plant. This study, the largest to date, included detailed information about thousands of employees over a forty-year period and roughly 70,000 contemporaneous measurements of airborne hexavalent chromium in the plant. In addition to the high cancer rate, the researchers also determined that the current PEL level was not even sufficiently low to prevent nasal septum deterioration, occurring at exposure levels less than half the then current PEL.26

Nor could it be said that industry strove to voluntarily lower their exposures in advance of agency action. In a study published in February 2002, Public Citizen found that over the period 1990-2000 the median measurement for industrial chromium exposure remained relatively high at 40.5 µg/m³, with 21.3% of the readings over the then current PEL. Furthermore, the researchers did not detect any significant decline throughout the period.27

With this background in hand, Public Citizen filed its second petition with the Third Circuit in March 2002, again seeking review of the agency’s inaction on a new hexavalent chromium standard. As had occurred previously, OSHA defended itself based on workload and “higher priorities,” including improvement of language in certain existing standards and revision of an

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existing rule on highway signs, conforming it to Department of Transportation rules. It further asked for a delay in obtaining information on occupational exposure to Cr(VI): data it had belatedly asked for on August 22, 2002.

Federal 3rd circuit court judge Edward Becker was not amused. On December 24, 2002 Becker ruled that OSHA’s delay had “exceeded the bounds of reasonableness” (emphasis added) and ordered all parties to appear before a mediator prior to a final ruling. On April 2, 2003, he ordered the issuance of a final rule reducing occupational exposure to Cr(VI). In accordance with the ruling, OSHA published the proposed standard on October 4, 2004, with a docket for comments open until January 3, 2005, and a public hearing convened February 1-15, 2005. After a further extension allowed by the Court, OSHA finally promulgated the new standard on February 28, 2006: although at 5.0 µg/m³, it remained twenty times higher than that requested by Public Citizen and two and a half times higher than that recommended by NIOSH. In the final rule OSHA asserted that it had reduced the “significant risk posed to workers by occupational exposure to Cr(VI) to the maximum extent that is technologically and economically feasible” (emphasis added). 28

Throughout this period, public health advocates were not alone in their efforts to influence OSHA’s regulatory activities. Well before Public Citizen’s first petition, corporate manufacturers and users of Cr(VI)—apprehensive about potentially costly lower exposure limits—conducted numerous activities aimed at forestalling, delaying, or limiting regulatory action, many through their attorneys. Nor were their efforts limited to letters seeking a delay in proceedings. Shortly after the creation of OSHA and the EPA, industry used every means at their disposal—including a wide variety of actions such as focused research, docket comments, congressional and agency lobbying, and lawsuits—to delay or dilute regulations on all substances During the mid 1970s the Industrial

Hygiene Foundation also assumed a crucial role in many of these endeavors. Although lawyers likely worked behind the scenes, many of these early activities involved corporate health and human resources personnel, with limited or no attorney involvement indicated.  

Early efforts appear to have focused on collecting data, closely monitoring agency activities, and discussing health and safety issues. One chromium industry trade association, the Chromium Chemicals Environmental Health and Safety Committee (Chromium Committee), played a key role in these efforts. The Chromium Committee—apparently formed to manage research, monitor governmental agencies, and conduct public relations efforts, similar to tobacco formed groups—worked through IHF. For example, in its August 1975 meeting, the Chromium Committee discussed the inconsistency of methods in the acute toxicity data its members had collected. In addition, as early as 1977 it actively monitored OSHA proposed regulations relating to chemicals and cancer, even when they only indirectly included chromium. Proposed topics for discussion in the Committee included measurement of exposures and treatments of skin exposures.

Even during this early period, the Chromium Committee sought to persuade state and federal agencies that increased regulation was not necessary. In late 1976 through early 1977 the Committee began planning what would become the first IHF colloquium on control of chromium hazards. The organizational letter indicated the purpose of the symposium as developing an industry

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30 Marianne C. Kaschak to Mr. Patrick Gilhooley, November 17, 1980; Industrial Health Foundation, Inc. memorandum to All Members of the Management Committee and attendees of the May 5 and 6 Atlanta meeting; E. F. Foley memorandum to IHF Chromium Chemicals Environmental Health and Safety Committee, December 30, 1977 (informing State and Federal); IHF Letter of Understanding for the IHF Chrome Producers Committee, undated; J. D. Dougherty memorandum to W. S. Ferguson, August 18, 1978. (all documents from Lanier) Two trade groups represented the chromium industry in its regulatory efforts, the CCEHSC and the Chrome Coalition. The CCEHSC’s membership included United States and European chromium producers, while the Chrome Coalition represented United States producers and users of chromium. At least two companies had a representative in each group. After the formation of the Chrome Coalition in 1986 Barnhart appears to have acted as the primary go between, especially with regard to keeping the CCEHSC advised of the Chrome Coalition’s—and its attorneys’—activities. For an example see IHF Chromium chemicals Health and Environmental Committee, “Minutes of the Environmental Subcommittee meeting, October 2, 1991 (Lanier).
position and “informing State and Federal OSHA personnel as to the degree of control already in effect and the adequacy of ... protective procedures.”

By 1979 the Chromium Committee’s behavior began evolving. That year’s Chromates Task Force April 3 meeting plans offered those who desired to prepare for litigation an opportunity to form a separate subgroup. Although group members eventually decided a separate litigation action committee was premature, given the unlikelihood of OSHA regulatory action, the offer clearly demonstrated the increasing involvement and importance of attorneys in industry activities and planning.31

As the possibility of regulatory action increased the Chrome Committee funded research designed to support its positions. On at least one occasion when research did not result in findings supportive of their position, the Chrome Committee resorted to tactics similar to that of the tobacco attorneys. Inveresk Laboratory in Britain held the original contract to study rats exposed to chromium. In 1984 the laboratory found unexpected mortality in one group and 100% mortality in another test of chronic exposure. Although the laboratory contended that it had run the tests as originally advised, the IHF withheld final payment for the study because the payment was not required until “acceptance” of the final report. One of the scientists IHF used to criticize Inveresk’s findings was involved in subsequent studies to replicate the research. Shortly thereafter DeFlora and colleagues wrote a series of articles suggesting the body detoxifies hexavalent chromium.32

In addition to increased research, during the 1980s the industry began taking more of a passive/aggressive approach to their regulatory positions. One letter from Diamond Shamrock to a

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32 David Egilman, “Corporate Corruption of Science,” 170-71 (Egilman writes that the Chrome Coalition funded the scientists criticizing Inveresk’s report, however he likely confused it with the IHF’s Chrome Committee since the Chrome Coalition was not formed until 1986); and Fernando Luigi Petrilli, Anna Camoirano, Carlo Bennicelli, Patrizia Zanacchi, Manna Astengo, and Silvio De Flora, “Specificity and Inducibility of the Metabolic Reduction of Chromium(VI) Mutagenicity by Subcellular Fractions of Rat Tissues,” Cancer Research 45 (1985): 3179-3187 (concluding that “threshold phenomena ... are involved in the initiation of lung cancer by chromium.”), 3186.
California agency in 1985 admitted that there are acute and chronic effects of chromium associated with “overexposure” (emphasis in letter), yet it also averred that the data “indicate[s] the presence of a carcinogenic threshold” (emphasis in letter). They further stressed that their thirteen-year-old plant—with an action level of 12.5 µg/m³—had no record of lung cancer in its thirteen-year existence. Left unmentioned were the limited number of workers involved and the general medical and industry acknowledgement that the latency period for chromium induced lung cancer greatly exceeded thirteen years.

As this adversarial approach grew more engrained attorneys became considerably more prominent in industry actions, including all phases of involvement such as regulatory comments, lobbying, and research. If not before, by the late 1980s industry scientists began including attorneys in the distribution list for the confidential reports they forwarded concerning scientific agency meetings. This increased involvement of attorneys coincided with the formation of the Chrome Coalition in August 1986, also under the IHF management umbrella. Based on both organization’s histories and activities, it appears the Chrome Committee’s member companies formed the Chrome Coalition to represent the industry’s face to regulatory agencies and to broaden funding for industry regulatory efforts by including not only manufacturers but also industrial utilizers of chromium within its membership.33

From 1989 into 1994, senior attorney John L. Wittenborn and his team of lawyers at Collier Shannon Scott (Collier Shannon) gradually took increasing roles in this effort, initially conducting traditional governmental lobbying and legal activities for the Chrome Coalition. These actions included lobbied Congress concerning reauthorization of the Clean Air Act as it pertained to

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chromium, and lobbying/negotiating with both the EPA and OMB to limit chromium regulations, including efforts to declassify trivalent chromium as an air toxic, as well as limit regulations pertaining to chromium in cooling towers, exportation, and land disposal.34

When EPA refused to modify their position on chromium usage in cooling towers, on April 17, 1990 Collier Shannon petitioned the United States Court of Appeals on behalf of the Chrome Coalition. They argued that industry had not been able to adequately comment on the new rule and that it imposed an unnecessary burden on businesses that exported hexavalent chromium. The two parties signed a settlement agreement on December 15, 1992, with the rule being modified on August 19, 1994 to limit the scope of required notifications.35

During these varied endeavors, Wittenborn and his team interacted with both attorneys and scientists in industry and the government. By 1992 Wittenborn was a member of the Chrome Coalition’s Executive Management Committee. In this role many of his firm’s meetings and telecommunications with government scientists concerned the science behind proposed regulations. Often, these contacts were preceded by conferences with industry management and scientists. The team also sought, and possible utilized, outside scientific consultants. Although the documents do not provide a direct connection between Collier Shannon and Paustenbach and colleagues during this period, they were already actively in support of the law firm’s efforts to distinguish trivalent from hexavalent chromium. What is unclear is whether these activities were specifically at the


request of Collier Shannon. During this period Paustenbach’s firm also represented many other clients, at least some of whom had interests in chromium.\(^{36}\)

Collier Shannon attorneys next turned their attention to the OSHA hexavalent chromium docket. Telephone conferences with Chromium Coalition chairman Joel Barnhart increased, as did conference calls and meetings with OSHA personnel. The law firm also hired an outside economic consultant to assist in its handling of an industry survey about chromium usage and handling. By September 1994, Collier and Shannon attorneys realized the early Johns Hopkins University study data suggested OSHA might seek to significantly decrease hexavalent chromium’s PEL. In response, they intensified efforts to delay the OSHA chromium PEL proposal. Attorney Stephanie Siegel played a principal role in this endeavor, participating in an industry conference call that discussed the possibility of obtaining a minimum three-month period to review the epidemiology data from the Johns Hopkins study before OSHA issued a rule. Following the call Siegel reviewed the draft letter request before it was sent to OSHA.\(^{37}\)

\(^{36}\) John L. Wittenborn to Marianne C. Kaschek, December 29, 1989, and January 31, 1990 (Lanier); Marianne C. Kaschak memorandum to Members of the Chrome Coalition Executive Committee, March 11, 1992 (Lanier) (Executive Committee); Collier, Shannon, Rill & Scott Invoices to Chrome Coalition, c/o Marianne C. Kaschak August 28, 1992, December 14, 1992, February 25, 1993, June 11, 1993, July 15, 1993, August 30, 1993, October 13, 1993 (Lanier); William E. Rinehart (President IHF) to Leonard E. Bryant (Chairman CDART Committee) et al., September 30, 1993 (ChemRisk proposal and contract) (Lanier). ChemRisk was already a very familiar face to several Chrome Coalition members. It had been a major consultant to companies fighting New Jersey’s attempts to require clean up of chromium-polluted sites in New Jersey in the early 1990s. During this period they also worked with the IHF—and at least several sponsors including Maxus Energy Corporation—in matters involving the EPA and the Chromium RfD, organizing at least one seminar, and responding on behalf to adverse scientific studies. ChemRisk’s invoices for these undertakings totaled more than $200,000.00. See, for example, Michael L. Gargas fax to Dr. William Rinehart, May 4, 1993; William E. Rinehart to Deborah M. Proctor, August 20, 1993; Mark Harris to Bill Rinehart, February 24, 1993; Michael L. Gargas to Ms. Kaschak, July 2, 1993 (Activity Report on the Chromium RfD project); and Deborah Proctor memorandum to Bill Rinehart, et al., June 29, 1993 (all documents Lanier). The New Jersey activities demonstrate how ChemRisk and its sponsors typically worked in a very similar fashion to the activities of tobacco attorneys. In a letter to the New Jersey Department of Health, Harris claimed that an “independent third party,” the IHF, organized a “Blue Ribbon Panel” based solely upon expertise and experience, with no prior association to the sponsors or the IHF. In reality, not only was the IHF intimately involved with—and paid by the chromium industry—but, in addition, the panel was specifically selected by ChemRisk for their conservative views and were handfed information by ChemRisk, with ChemRisk drafting much of the report. Mark Harris to Leah Ziskin, September 28, 1992 (Lanier); and Dennis J. Paustenbach to William E. Rinehart, October 13, 1992 (Lanier).

The upswing in OSHA activity thus also produced an upswing in Collier Shannon’s participation in scientific matters for the Chrome Coalition. By late 1994 Collier Shannon attorneys increased the hours they spent on Chrome Coalition activities, becoming more deeply embroiled in the decision-making process of creating new science to counter chromium exposure and disease data being received by OSHA. They continued to review and revise letters being sent to OSHA regarding the Johns Hopkins data, as well as advising the Chrome Coalition about the appropriate consultants to reanalyze the data. They also prepared requests for proposals to reanalyze the Hopkins data, while at the same time filed Freedom of Information Act (FOIA) requests to obtain the data.\textsuperscript{38}

Much of this amplified effort comprised advice and projects for Chrome Coalition chairman Dr. Joel Barnhart, the vice president – technical for Elementis Chromium Inc., a producer of chromium. Barnhart attended the Industrial Hygiene Foundation (IHF) Chromium Chemicals Health and Environmental Committee (Chromium Committee) as a representative of Elementis and its predecessor from 1984 until 2003. He also served as chair of the Chrome Coalition for most of its existence following its formation in 1986.\textsuperscript{39}

By late 1995, the chromium industry must have felt besieged. They surely recognized that Public Citizen’s lawsuit brought ever-increasing pressure upon OSHA to begin the process of issuing a new, more protective standard for chromium. In October, Chrome Coalition Ad Hoc Committee member Mike Buczynski met with the Colored Pigments Manufacturers Association (CPMA) in the law offices of Fitzpatrick & Waterman in Secaucus, New Jersey. At the meeting, attorney Harold Fitzpatrick strongly advocated taking an aggressive adversarial approach to

\textsuperscript{38} John L. Wittenborn letters and attachments to Marianne C. Kaschek dated November 12, 1994 and November 28, 1994 (invoices) (Lanier). OSHA responded to the FOIA request by indicating that other than documents of a proprietary nature, everything relevant was in the docket. Adam Finkel to Ms. Jeralene Green, November 7, 1995 (Lanier).

\textsuperscript{39} Joel Barnhart, Ph.D. affidavit dated 11 May 2011, Exhibit A to Respondent’s Memorandum in Opposition to Complainant’s Motion for Accelerated Decision on Liability, In the Matter Of Elementis Chromium Inc. f/k/a Elementis Chromium, L.P., Docket No. TSCA-HQ-2010-5022, United States Environmental Protection Agency.
regulation. A summary of the discussions reported him as recommending an approach in which the groups:

…give OSHA as little information as possible; throw monkey-wrenches in along the way; and most importantly, ‘don’t tell OSHA how to do it right-wait until they do it wrong, then make it work to your benefit in litigation.’ In other words, they should conduct a close variation on the tobacco approach."  

Then came even more calamitous news. At their November meeting in the Washington offices of Collier Shannon, the group learned that the EPA-Hopkins chromium study was finding “a clear dose response relationship with increased risks even at exposure levels below the current OSHA limit.” Since OSHA typically set standards well below the levels found to cause disease in epidemiological studies, such a finding in the final report might result in OSHA setting a standard approaching Public Citizen’s proposal. A standard that low would likely cost chromium manufacturers substantial profits, since if they simply raised profits to offset the increased environmental controls, purchasers would begin searching for alternatives. Even without increased chromium prices, end users undoubtedly would seek to avoid the increased costs of environmental controls necessary to use chromium in their processes. With this potentially devastating news, it is not surprising to find that attorneys were well represented at that meeting, with John Wittenborn, Stephanie Siegel and Chet Thompson (all attorneys at Collier Scott) in attendance representing Specialty Steel, a trade association of steel manufactures. The participants struggled mightily to find ways that might influence regulatory actions. Most of the eight action items resulting from the meeting entailed work exclusively by Collier Shannon. Following the meeting, attorney Siegel set up

a meeting with OSHA’s Adam Finkel to introduce the Chrome Coalition to him and obtain an update to OSHA activities.41

As 1996 began, Collier Shannon lawyers again increased their participation in scientific data generation designed to influence OSHA’s new hexavalent chromium standard; conducting analysis of the hexavalent chromium exposure data that was available from surveys. They also continued seeking the Johns Hopkins data. At the Chrome Coalition meeting of January 18, 1996, Wittenborn presented the results of the survey and analysis to the group.

On month later, on February 13, 1996, the Chrome Coalition Ad Hoc PEL Committee met at the Pittsburgh International Airport to plan a counteroffensive to the Johns Hopkins study. Prior to that meeting, Collier Shannon attorneys reviewed various analytical test methods used for chromium exposures and held conference calls and meetings with a few Chrome Coalition and Industrial Hygiene Foundation members to discuss potential contractors, risk assessment proposals, and guidelines for the upcoming meeting. Just prior to the meeting, Siegel reviewed at least one contractor proposal and sent IHF representative Marianne Kaschak and Barnhart discussion guidelines for use during the meeting. She then attended the meeting via phone as the representative for Specialty Steel Industry. Over the next two days, Siegel conferred at least twice by phone with Kaschak and then took part in in a general Chrome Coalition conference call on February 15. The group decided to recommend ChemRisk to the full association and also raised the possibility of considering an “anti-Mancuso paper.” Throughout these activities she kept Wittenborn advised of her discussions.42

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41 Chrome Coalition Meeting Minutes of November 14, 1995, 1; Chrome Coalition, “Action List resulting from November 14, 1995, Meeting and Joel Barnhart fax to Chrome Coalition OSHA/PEL Ad Hoc Committee, November 24, 1995 (all documents Lanier).

42 John L. Wittenborn letters and attachments to Marianne C. Kaschek dated February 26, 1996 and March 15, 1996 (Lanier); and Marianne Kaschak memorandum to OSHA PEL ad Hoc Committee (with hand written notes), February 13, 1996 (Lanier).
The Coalition turned to familiar figures for help in planning their strategy. Dennis Paustenbach and his staff at ChemRisk had already worked with some of the largest chromium producers in successfully forestalling New Jersey’s efforts to require substantial cleanup of chromium contamination. The second consultant invited to the meeting, William Butler, represented Environmental Risk Analysis. Both individuals were familiar with how to create doubt about scientific studies, having worked for the tobacco industry in its efforts to limit regulation of cigarettes. Notably, Butler played a leading role in tobacco’s efforts to stall passive smoking regulation, spearheading attacks on a second hand smoke study.

The group met with Paustenbach and Butler for the specific purpose of soliciting “proposal[s] for critiquing the OSHA risk assessment techniques and its standard-setting process relative to a proposed Cr+6 PEL.” Predictably, the consultants’ recommendations read like a tobacco industry playbook: reanalyze old studies, obtain the data from the EPA study to “do a proper analysis,” conduct new tests seeking industry desired results, and publish the quick reviews and studies in peer review journals. 43

The meeting notes indicate that Paustenbach was fully onboard with building the evidence necessary to legally contest any regulatory effort. Even before conducting research, he was “quite certain that a positive impact could be made, basically because of the weak database.” Paustenbach felt “very strongly about conducting the analysis and submission of papers that have been peer reviewed into the docket as soon as possible since OSHA would be required to address them in the standard-setting process.” Paustenbach suggested that they review the Mancuso work as well as all other relevant epidemiology studies. This would ensure there was enough in the docket to mount a significant legal challenge.

43 Minutes of Chrome Coalition Ad Hoc PEL Committee meeting of February 13, 1996, including the summary of discussion and recommendations. Contained as an internet attachment to David Michaels, et al., “Selected Science: an Industry.”
The Committee summary revealed that Paustenbach believed:

Johns Hopkins data must be thoroughly analyzed beyond what EPA/OSHA had contracted for, so that the issue of hexavalent chromium exposure is evaluated properly now and that further misconceptions like Mancuso are dismissed.

Paustenbach further mentioned he had spoken to Johns Hopkins lead researcher Genevieve Matanowski about the possibility of ChemRisk’s pro bono confidential involvement to see the study to completion. In this case the coalition could approach the regulators with a program designed to fill the “data gap,” thus entering into a data gap agreement to forestall the rulemaking. Collier Scott lawyer Siegel expressed skepticism about this possibility, since the OSHA activity was so far advanced. In the end: even assuming additional time was not available, Paustenbach offered a comparison to their work in the benzene rulemaking, indicating he expected similar results. He believed Adam Finkel might be willing to offer a compromise agreement as he had done with 1,3-butadiene. The Coalition liked what it heard. It hired Paustenbach through its attorneys—a standard tobacco tactic—providing the potential of shielding the materials through the attorney work-product and attorney-client privileges.

In the negotiations and decision-making concerning the scope of ChemRisk’s work, at least one participant expressed concerns about the potential increased involvement of attorneys and ChemRisk’s push for an adversarial approach to the regulations. At one of the early 1996 meetings

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44 Minutes of Chrome Coalition Ad Hoc PEL Committee meeting of February 13, 1996, Paustenbach likely anticipated that the Chrome Coalition would find a way to underwrite any “pro bono” assistance. Even if they did not, he could build the cost into the contract.
45 David Michaels, *Doubt Is Their Product*, 100-101. Collier Shannon attorney Kate McMahon also attended the next Chrome Coalition meeting that occurred on April 22, 1996, where they likely discussed Paustenbach’s work and a proposed contract. Two days later she attended a Chrome Symposium at which the Johns Hopkins and EPA researchers presented their data on the Baltimore Chrome plant, as well as the risk assessment methodology; see John L. Wittenborn letter and attachment to Marianne C. Kaschek dated May 23, 1996. (Lanier). Since 1996 at least eighteen epidemiological, risk assessment, toxicological studies, or reviews of the health effects of hexavalent chromium have been published by this group. All have discounted or minimized the hazards of hexavalent chromium. During this period Chemrisk also had other clients requesting chromium articles to counter regulations and lawsuits. Thus a number of the articles were funded either by, or jointly with, other entities.
the Chrome Coalition considered having “all correspondence routed through attorneys.” Donald Billmaier, a senior doctor with AlliedSignal, wrote Barnhart to express his “distaste” for that:

> I think it is ridiculous to have the attorneys in the position of screening all correspondence, etc. This reeks of tobacco methodology. I guess one could argue that the tobacco companies have been successful in what they are doing, but they are certainly lacking in trust, and sooner or later everything is going to be made public anyway. Even the lawyer heavy, paranoid CMA does not go to this extreme.

In the same letter he conveyed

> some concerns at AlliedSignal involve the manner in which ChemRisk has presented findings or opinions, and the question of whether, in some cases, an overly adversarial manner has helped or hurt the achievement of the end which is sought. While adversarial or legal proceedings may ultimately be necessary, it should be the chrome Coalition which makes this decision, not ChemRisk. ⁴⁶

Although Barnhart’s specific reaction is unknown, Billmaier’s letter does not appear to have affected either Collier Shannon’s increasing involvement in Chromium Coalition activities or ChemRisk’s contract. The agreement between Collier and Shannon (on behalf of the Chrome Coalition) and ChemRisk envisioned a broad based critique of both the OSHA risk assessment techniques and OSHA’s process for setting the PEL for hexavalent chromium. It specifically incorporated a critical role for the attorneys in the production of the information: shielding the information as attorney work product. The agreement specifically noted its purpose as being

> to provide technical information from the Consultant to CSR&S [Collier Shannon] to facilitate the development of legal advice and attorney work product for the coalition. Accordingly, this agreement shall be construed in all respects to preserve the confidentiality of information, opinion, and data to the extent provided for under the attorney-client privilege and attorney work product.

The contract also contained another provision providing additional insurance that any non-helpful results could be hidden from view. Although the contract gave ChemRisk the right to

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⁴⁶ Donald J. Billmaier fax to Joel Barnhart, May 1, 1996 (Lanier).
publish articles based upon the final report, until the report was finalized they agreed not to disclose any information. Specifically, ChemRisk agreed not to

publish or disclose any of the results of the Work prior to final completion, and receipt and approval by CSR&S [Collier Shannon] of a final report of the Consultant, to be prepared in accordance herewith, unless specific written approval is granted by CSR&S. CSR&S and the Consultant agree to full public disclosure of scientific information contained in the CSR&S-accepted final report developed through this project.

Thus the attorneys had the final say in the report’s completion. Of course, given ChemRisk’s track record of providing clients with the information they desired and its aggressive stance in the proposal—as well as its desire to continue working for attorneys—this provision was likely not necessary. Still, its inclusion demonstrates the effect attorneys have on science through their desire to solicit and publicize only helpful evidence, not the soundest understanding. 47

As 1996 ended, Collier Shannon counsel submitted comments to OSHA on behalf of the Chrome Coalition, as had been sought through OSHA’s general information collection request. These comments, although couched in language of cooperation, proposed procedures, inspections, and information gathering likely calculated to substantially delay any rulemaking. Notably, the letter did not disclose any of the research activities recently instigated by the Coalition through its attorneys, even though the research was specifically relevant to OSHA’s request. 48

Although the manufacturing members of the Coalition expected significant help from Paustenbach’s and Butler’s criticisms of various studies, they recognized that this alone could forestall a marked decrease in the PEL. The main problem remained the findings of elevated cancer risks in virtually every long-term epidemiology study of chromium producing plants over the past fifty years. In addition, none of the studies had sufficient workers in low exposure areas to provide

47 Contract dated September 10, 1996 between ChemRisk, CSR&S, on behalf of the Chrome Coalition, and the Industrial Health Foundation, Inc. (in author’s possession).
48 John Wittenborn and Stephanie Siegel to Docket No. ICR-96-14, November 25, 1996. (in author’s possession).
statistically significant results demonstrating a safe level of exposure. The latest EPA-Hopkins study, with its likely conclusion that even controls put in effect in the 1950s were starkly insufficient, only compounded the problem. Based upon the likely findings, OSHA’s use of the typical straight-line dose response curve for cancer in its rule making might mandate an extremely low PEL. In the face of this evidence, the manufacturers, through the Chrome Committee, decided on a strategy of developing evidence in support of the cancer threshold theory as the best hope of avoiding crippling environmental control requirements.

In the hope of obtaining results that might at least imply a threshold level of risk, the Chrome Committee hired Applied Epidemiology (AEI)—a subcontractor of ChemRisk in other chromium projects—to conduct a study at five plants owned by Coalition members: two in the United States, (Castle Hayne, North Carolina and Corpus Christi, Texas) two in Germany (Leverkusen and Uerdingen, Germany), and one in Great Britain (Eaglescliffe, England). Barnhart would later describe the purpose of the effort as conducting “a large study to better assess and understand the cancer risk associated with exposure to hexavalent chromium at the facilities in the study.” The Committee did not need to conduct an investigation into the background of AEI; its owner was Kenneth Mundt. He would be studying some of the same people as in his previous investigation.

As occurred in research approved by tobacco attorneys, AEI’s proposal and protocol underscored the desire to advance a theory necessary for the defense of chromium: the potential of a threshold for chromium-induced cancer. It further specifically highlighted the major problem of prior studies on this issue: “the relatively small study sizes and short-follow-up periods resulted in a limited ability of [prior] studies to clarify the relationship between modern [controlled] occupational exposures and . . . respiratory cancers in particular.” Despite intimate knowledge that the exposure data differed substantially between the plants—air sampling in the United States and urinary
sampling in Germany—the protocol emphasized the crucial nature of a large study population, “to improve statistical power and the inferential value of the results.”

The chromium plants included in the study seemed to provide a unique opportunity to demonstrate the effectiveness of modern controls. Not only was the cohort larger than the Baltimore facility, the exposures levels were lower and the exposure histories longer. The Baltimore plant workers studied by Herman J. Gibb had exposures to hexavalent chromium on average 5-10 times higher than the concentrations in the German or United States plants in the four plants. The highest average concentration in any of the four plants was less than 10 µg/m³, whereas the lowest concentration at either of the Baltimore facilities was 30 µg/m³. The plants were also newer, so employees also had shorter work exposures. The project seemed to offer real potential for producing a negative study, thereby lending support for a chromium lung cancer threshold. On July 24, 1998, the chair of the Chromium Committee informed OSHA, the EPA, and other regulatory bodies of the study’s commencement.

With approval of the contract, the IHF became the direct managing entity for the project. The limited documentary evidence available concerning the decision making behind the study does not identify any attorney involvement. Attorneys did not attend many, if any of the Chrome Committee meetings. While this does not preclude other law firm or company counsel from being involved, it suggests much less lawyer involvement during the research stage of the project than occurred in tobacco research.


50 Exhibits with charts to Complainant post hearing brief, Docket No. TSCA-HQ-2010-5022.

51 Joint Stipulation of Facts in Docket No. TSCA-HQ-2010-5011. By 2002 Collier, Shannon was coordinating the OSHA docket effort, and may well have been providing management advice throughout the four plant study.
Throughout the course of the contract, AEI provided frequent reports to the IHF and the Chrome Committee. The researchers experienced two major difficulties. The advanced stage of OSHA’s rulemaking activities made quick data collection essential. Thus, Applied Epidemiology, with the concurrence of the Chrome Committee, dropped Eaglescliffe from the project in 1999 when it became obvious the data could not be timely assembled, thereby reducing the statistical power of the project.52

The distinctions between the sampling data in the United States and German plants presented an even thornier problem. With the British plant already removed from the study project managers could not afford any further reduction of the cohort. Thus they needed to develop methodology to reconcile the two forms of measurement: air sampling in the United States and urinary sampling in Germany. As described in the July 1, 2001 report—which sought to justify the slow progress of the project—“the harmonization of urinary with air data proved to be a study in itself.” AEI also reported having commenced drafting the final report, expecting to complete the report and the expert review by October. They suggested a meeting with the Chromium Committee in October for “a special session for discussion of the draft final report.” They also expected the outside reviewers to have submitted comments by then.

On October 25, 2001, Mundt informed the IHF that all analyses were complete, with all but the discussion section of the final report drafted: “A complete draft for review and comment will be distributed to the sponsor and AEI’s review panel by the end of November.” However, delays again pushed back delivery of the draft of the final report until early 2002, when it finally reached the

Committee and the “Scientific Advisory Board:” outside peer reviewers James Stewart of Harvard University, Harvey Checkoway of the University of Washington, and Edwin van Wijingaarden of the University of North Carolina. Even then, following the extended difficulties and delays in “harmonizing” the sampling data, the report’s author did not suggest or even hint at the possibility of splitting the cohort. Nor, apparently, did any of the reviewers make such a suggestion.\footnote{The two reports found in the IHF files provide detailed information about the status and methodologies being used in the study. They do not suggest any consideration being given to splitting the cohort by the method of sampling. Nor do the limited files of this period retained by the IHF contain any reference suggesting this possibility or modifying the protocol in any manner. As of October 2001 Mundt anticipated two manuscripts, one discussing the methodology and sampling protocols and one for the lung cancer results. Marianne C. Kaschak memorandum of July 3, 2001 re: Cohort Mortality Study of Four Chromate Production Facilities – Interim Report (harmonization of urinary) (special session); Applied Epidemiology, Inc.’s Interim Report of October 25, 2001 (complete draft); Kenneth A. Mundt to Marianne C. Kaschak, January 14, 2002 (Draft Report sent); and Kenneth A. Mundt to Marianne C. Kaschak, October 31, 2002 (Final Invoice) (all documents Lanier).}

The group apparently took nine months to review and comment on the report. On October 8, 2002, Mundt emailed the “final” copy of the Final Four Plant Report dated September 27, 2002 to the IHF. The report found an elevated risk of lung cancer from exposure to hexavalent chromium for the high exposure group using standardized mortality ratio analysis. Mundt formally presented the study’s final report to the Chromium Committee during its October 15, 2002 meeting. Far from suggesting that the protocol had serious flaws, it reiterated the investigations’ soon-to-be eliminated strengths:

This study benefited from the multi-site design that provided a reasonable large cohort of post-change [less exposed] chromium chemical workers, along with the corresponding increase in statistical power generally lacking in previous studies of post-change cohorts.\footnote{Joint Stipulation of Facts in Docket No. TSCA-HQ-2010-5011. Applied Epidemiology, “Final Report: Collaborative Cohort Mortality Study of Four Chromate Production Facilities, 1958–1998, Submitted by Applied Epidemiology, Inc. to the Industrial Health Foundation, September 27, 2002,” Docket H054-A, Exhibit 48-I-2, online at http://dockets.osha.gov/vg001/V047A/05/55/31.PDF (in author’s possession).}

Surprisingly, chromium manufacturers did not immediately make the findings public, even though pressure had increased on OSHA to reduce exposure levels. Other industry activities I have reviewed that were designed to influence regulatory bodies seem to suggest that, if the report had
been helpful, the Committee would have released it immediately. This did not happen. In early 2002, less than a month after the filing of the Public Citizen petition with the Circuit Court to require chromium regulatory action by OSHA, Collier Shannon (representing the Coalition) coordinated with CPMA attorneys to intervene in the case. Their intervention made no mention of the report. Rather, they continued a tradition of delaying tactics by developing strategies for motions to seek extensions in time in the lawsuit (there would be at least two such requests). The attorneys spent considerable periods of time throughout 2002 and early 2003 in an ultimately unsuccessful attempt to forestall regulatory activity. As part of their efforts, they sought and prepared expert witnesses that were familiar with preparing studies specifically designed to cast doubt on prior epidemiological studies. Throughout preparation of their case, the attorneys still made no effort to disclose the completed industry study.  

Collier attorneys and agent, scientific consulting firm Exponent, obtained yet more work from the Coalition when OSHA sent out a general request for hexavalent information during the summer of 2002. Wittenborn specifically recommended that Exponent provide most of the responses to OSHA, with Collier Shannon managing the contract to ensure secrecy through the attorney/client privilege. Even before the Coalition made a decision about OSHA’s request, Collier attorneys conferred with consulting scientist Deborah Proctor. Following the meeting, they requested an extension of time for their submission. In November attorneys held at least two conferences with Proctor. At least five attorneys reviewed Exponent’s comments to OSHA, with at least one not only commenting—but also making unilateral revisions. In November, Exponent charged just over $16,000.00 for the final product. Even then, when new information seemed

necessary to convince OSHA not to lower regulations, the 4-plant study remained absent from the submission. The questions thus arise: Did industry want to hide the study’s findings, and if so, why?  

Although technically not managing or directing of scientific research, Collier Shannon’s involvement in other litigation provides a vivid demonstration of the methods attorneys used to hide research for their clients. Throughout at least half of 2003, Collier Shannon attorneys devoted substantial time to determining whether the Coalition could claim an attorney privilege for documents in the possession of its member OXYCHEM that were requested by the plaintiff in this case. They did not simply conduct substantial research into the legal requirements for asserting the privilege in Illinois, but also reviewed, coded and prepared several thousand pages of documents that OXYCHEM intended to produce. These efforts included comprehensive reviews of Mark Stenzel’s and Mike Buczynski’s files, the two individuals primarily responsible for health and safety at OXYCHEM. The research and review covered a significant amount of scientific research conducted by, through, or for the Coalition, including at a minimum both Exponent’s and Harding-Barlow’s work. Although their exact reasoning for the ultimate assertion is unknown, they considered asserting any “colorable privilege claims.” In the case of Harding-Barlow, none of the considerable number of prior documents in the IHF archives suggest that attorneys were involved in the research effort or that its purpose was for litigation—or even specific regulatory—type activities.  

Thus, analogous to what had often occurred in the tobacco industry, this attempt appears to have been a post hoc effort to manufacture attorney work product and attorney client privileges.

56 Joel Barnhart email to J. W, August 29, 2002 (Lanier); Collier Shannon Scott invoices to Chrome Coalition c/o Marianne C. Kaschak, October 21, 2002, December 16, 2002 (Lanier); and Christina B. Parascandola to Marianne Kaschak, May 5, 2003 (Lanier).

Even as the four-plant study remained under wraps, the industry explored other means of influencing OSHA. Exponent’s exposure study comprised but one segment of the Chromium Coalition’s agenda to influence potential EPA and OSHA rulemaking during this period. They knew that not only must new studies be produced, but older, damaging studies required discrediting. Collier Shannon attorneys became increasingly central to these activities. Throughout late 2001 and 2002, Collier Shannon attorneys managed Exponent’s contract to critique the Gibb report, reviewing and commenting on the drafts of the report, as well as its abstract and proposed presentation at a scientific conference. Throughout this process, the attorneys met with Exponent’s lead scientist, Deborah Proctor, and spoke to her in conference calls. Although no detailed records of the meetings and phone calls have been found, it is hard to imagine an attorney making this substantial effort unless the original draft report did not provide the proof needed, requiring significant editing and massaging before being included in the attorney’s repertoire of evidence.\(^5\)

The September 12, 2002 Chrome Coalition meeting provides an excellent example of lawyers gaining increased influence. Four attorneys attended: Wittenborn, Joseph Green, and Christina Parascandola of the Collier Shannon office, all representing Specialty Steel, and Harold Fitzpatrick, of Fitzpatrick & Israels (a New Jersey firm) representing the Color Pigment Manufacturers Association. Committee minutes detailed the critical nature of attorney responsibilities in the Coalition’s activities. First, the participants reelected Wittenborn to head the Clean Air Committee. Collier Shannon attorneys further noted they continued to monitor the EPA air toxics risk characteristics program and indicated they would forward a list of the EPA Metal Assessment panel to the IHF for distribution and review. Next, the group discussed Exponent’s critique of Gibb’s publications and comments regarding the Painesville (Mancuso’s) cohort. Of even

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greater importance, by this time Collier Shannon attorneys had assumed the coordinating role for fighting reductions in the PEL. This function included coordinating responses to a series of questions that OSHA had recently posed to interested parties in the Chromium regulatory workup. Collier, Shannon and a contractor—the group expected that it would likely be Exponent—would answer the questions, which included questions about the EPA-Hopkins study led by Gibb.

Although initially decided that the IHF would manage the contractor, following phone calls of September 20 and October 2, the group decided “that the contract be initiated between Collier, Shannon and Exponent to insure attorney client privilege.”

During this period, Exponent scientists telephoned and corresponded with Gibb. This may have been at the instigation of the attorneys, since shortly before the correspondence they met with Exponent consultants to discuss the next stages of the Gibb critique. The decision to contact Gibb may have been made because either Proctor or Collier attorneys realized the critique was not sufficiently strong. TobaccoWiki editor Anne Landman took the following notes during an April 14, 2008 conversation she had with Gibb concerning Exponent’s communications with him:

Exponent called up and tried to hire me (Gibb) but I’m not interested because I did a study on chromium on workers that became the basis of OSHA’s Personal Exposure Limit (PEL) for chromium. They don’t set a limit on it but once every millennium! One of the best occupational studies on chromate. After I did the study, Exponent was hired to criticize the study. Don’t mind criticism but . . . it would have looked great on them if I went to work for them.

Following oral arguments before the Court, Collier Shannon attorneys likely foresaw the ultimate approval of Public Citizen’s petition—since, in January 2003, Collier Shannon attorneys met internally and with Barnhart to discuss strategy and rulemaking goals. Shortly prior to the

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59 Summary of the Chrome Coalition Meeting of September 12, 2002 (Lanier).
following month’s Chrome Coalition meeting, Wittenborn and Barnhart met to develop the rulemaking strategy. Wittenborn then presented the proposed strategy for development of an industry friendly hexavalent chromium PEL at the Chrome Coalition meeting. A month later the attorneys continued to fine tune the strategy both in house and with Barnhart, while also deciding on which individuals and potential litigation/regulatory support contractors they would invite to the next scheduled meeting. In April, they received a double dose of bad news: the 3rd Circuit Court’s approval of the petition and the announcement that California was “shelving” the “Blue Ribbon Panel” report that had exonerated chromium from causing stomach cancer.\footnote{Collier Shannon Scott invoices to Chrome Coalition c/o Marianne C. Kaschak, February 25, 2003, March 24, 2003, and April 21, 2003 (Lanier); Collier Shannon Scott invoice to Chrome Coalition c/o Marianne C. Kaschak, May 22, 2003 (Lanier). The California report will become exceptionally important later in this chapter.}

Even with all of this bad news, the Chrome Committee did not release Mundt’s findings. They would later attribute this failure to problems with the exposure sampling data. Yet, this was not a new development. Even prior to preparing the initial study protocol, all of the involved parties knew that the exposure data was from air sampling in the US and urinary samples in Germany. This required conversion of the US data into urinary chromium levels, which AEI accomplished after making standard assumptions. As provided for in the protocol, following the data conversion, Mundt’s group divided the cohort into four groups based upon relative exposure levels, then compared the number of persons in each group who had lung cancer with the expected number of lung cancers for a group of that size from the plants local area. The Exposure group with 0-39.9 μg/L chromium exposure had four, with 6.37 expected (the initial report misreported this as 2.97 expected), 40-99.9 μg/L had 4 with 4.20 expected, and 100-199.9 μg/L had 5 with 5.3 expected, the group with an exposure in excess of 200 μg/L had 12 with 5.72 expected.

In later affidavits, Mundt and Barnhart claimed that questions about the sampling data first arose at a September epidemiology conference in Barcelona (attended by Mundt) and informal
discussions with the Chrome Committee just prior to the October meeting. Although others would later claim one of the Science Advisory Board members had expressed similar concerns, the available documents and affidavits do not mention this. In any case, when Mundt forwarded the final report (after attending the Barcelona conference), he made no mention of expressions of concern, and did not propose splitting the report. Apparently he first mentioned dividing the report to the committee members following their expression of concern during the informal discussions.

I confirmed with the Chromium Chemicals Health and Environmental Committee that similar concerns [about the problems associated with combining urinary and air sampling data] and issues about combining the cohorts had been discussed with me by peers at the September 2002 EPICOH conference.

Thus, it appears that only at this late juncture did the participants give any thought to changing the study’s protocol. The reasons given for the change—the German exposure levels being significantly higher with primarily urinary chromium measurements, while the lower US exposures were measured by air sampling—had been facts not only well known throughout the study, but also considered during preparation of the study’s protocol.

With this division of the data, the study no longer met the initial goals of providing the EPA with a large study with significantly greater statistical power. The two separate groups were simply not large enough to detect disease at the lowest chromium levels experienced by a very significant percentage of the cohort. Dr. Barnhart later wrote that at the conclusion of the October 2002 meeting he did not believe the final report demonstrated “a previously unknown risk associated with exposure to hexavalent chromium.” Given the excess cancers found in the original report, this statement must be met with a healthy dose of skepticism. He further maintained: that “the only information from both of these studies [EPA-Hopkins and the Coalition studies] that reasonably
supports a conclusion that hexavalent chromium presents a “substantial risk of injury to health” is that persons subject to higher exposure levels have an increased incidence of lung cancer.”

If in writing this Dr. Barnhart had been discussing the information and analysis ultimately contained in the two separate papers published about the cohort, he would have been correct, since these truncated articles did not demonstrate a risk except among the highest exposed individuals. However the internal report’s charts and data clearly demonstrated a higher cancer risk among workers with long term, but only moderate exposures, including a two-fold increase of cancer among workers who started work at age 35 or older had around 130 µg/m³-years, the estimated exposure of the intermediate exposure group. By simply calling for a division of the study, thus reducing its statistical power, the sponsors magically eliminated this hazard.

In late 2003, an event occurred that eventually dramatically worsened Chromium industry’s relations with the EPA—while at the same time afforded historians a rare look behind the legal veil to view how attorneys fund, support and influence scientific research, regulatory actions, and opinion making. These unexpected results came about because of the IHF’s unique position as a record keeper and a plaintiff attorney’s foresight. The IHF not only hosted two separate chromium groups, but also acted as the groups’ agent in many matters, additionally maintaining Committee and Coalition records, as well as maintaining its own records that recorded group activities.

That year, beset by numerous lawsuits claiming it actively participated in a cover up of research demonstrating the harmful effects of asbestos, the IHF filed for Chapter 7 bankruptcy and closed its doors. Shortly thereafter, the court appointed Trustee assumed control of the files, which included numerous records pertaining to the two Chromium groups. The documents did not remain

62 Joel Barnhart, Ph.D., affidavit dated 11 May 2011, Exhibit A, In the Matter Of: Elementis Chromium Inc. f/k/a Elementis Chromium, L.P.
63 Exhibits with charts to Complainant post hearing brief, In the Matter Of: Elementis Chromium Inc. f/k/a Elementis Chromium.
dormant for long. In April 2004, Barnhart visited the IHF and inspected the cabinets containing the Committee and Coalition’s records. In a 2004 affidavit supporting an ultimately successful petition for return of business records, Barnhart informed the Western District of the Pennsylvania Bankruptcy Court that three file cabinets of the IHF’s file cabinets contained “business records of the Chromium Committee and the Chrome Coalition.” The group likely breathed a sigh of relief when the successful retrieval of the files. Yet, this was not the end of the matter. The Trustee sold the remaining files to plaintiff attorney Mark Lanier who shipped them to storage facilities near his Houston, Texas office. There, additional documents about the activities of the two committees waited amidst a now barely organized volume of materials, some of which dated back to the early years of the IHF. The trade association records then sat unnoticed for the next two years.64

In the meantime, OSHA began collecting comments on its proposed rulemaking. In his October 4, 2004 comments on OSHA’s proposed lowering of the hexavalent chromium PEL, Initially, Barnhart still failed to disclose the new study. Rather, he wrote that he was not aware of any study that considered low dose long-term exposures but the industry believed it had a better way to estimate the risk of exposures:

the rate of Cr(VI) exposure is important. . . . it appears that by lowering the highest repetitive daily dose by a factor of five, the risk was lowered by at least a factor of five even though the cumulative dose remained constant....
We believe . . . consideration of this should be taken when estimating risk at very low exposure levels based on effects at much higher exposure levels.65

The work of the attorneys, the chrome industry, and their contract scientists finally came to maturity during comment and hearing period for the proposed regulation. First, they submitted Exponent’s hypercritical, legalistic fifty-page evaluation of the Gibbs/Hopkins study, prepared

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64 Joel Barnhart, Ph.D. affidavit dated 11 May 2011, Exhibit A, In the Matter Of: Elementis Chromium Inc. f/k/a Elementis Chromium, L.P.; and personal communication with Mark Lanier.
under the supervision of Collier Shannon attorneys. On January 3, 2004, Coolier Shannon attorney Kathryn M. McMahon-Lohrer sent two documents to OSHA: her comments on the proposed rule on behalf of three industry trade groups and an outline of Barnhart’s anticipated testimony at the hearings scheduled to begin on February 1. Her steel industry comments dealt primarily with the extraordinary costs the industry expected to result from the proposed regulations, along with a representation that OSHA’s risk analysis was deeply flawed because the studies had not actually examined steel workers and did not assume there is a threshold for Chromium cancers. Barnhart’s testimony reiterated these last two points, arguing that “the possibility of a threshold-like effect in the relationship between exposure to Cr(VI) and lung cancer suggests that the studies on chromate production workers should not be relied on to establish the PEL.”

During the second post-hearing comment period, McMahon-Lohrer submitted a brief that included a strident attack that industry consulting firm Exponent had prepared on OSHA’s risk assessment. In this attack, Exponent displayed an aggressive adversarial position, a position far removed from normal scientific balanced analysis. Continuing the position taken by industry at the hearing, they castigated OSHA for not considering the possibility of a threshold for the carcinogenic effects of chromium. They claimed that linear dose response risk assessment as is almost exclusively used for cancer, is “unproven.” Application of high dose exposure diseases to lower doses will overstate the risk and application of the chromate production lung cancer estimates to other, larger industries is not supported. They further claimed that particle size must be important (likely because this is one difference they could point to in the two industries) and supported this position by claiming to have conducted electron microscope analysis of certain relevant ores (without any

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Reading this document reminded me of a comment a prosecutor made to the jury after one of my closings at a U. S. Navy criminal trial. “Defense counsel is very skilled but is like an octopus. He has clouded the water with his ink, so you can not see that there is nothing behind it” (I lost the case).

Information about the 4-plant study first came to OSHA’s attention on April 20, 2005. In a post hearing brief on the proposed OSHA occupational exposure rule for hexavalent chromium on behalf of the Specialty Steel Industry of North America, Collier Shannon attorney Kathryn McMahon-Lohrer declared

it recently came to our attention that Environ Health Services (“Environ”), [By this time Environ had completed the purchase of Applied Epidemiology.] the OSHA contractor primarily responsible for risk assessment, has just completed the first phase of a significant epidemiological study of chromate production workers and is near completion of the second phase of this study. This multi-plant study of four chromate facilities in the United States and Germany examines the mortality experience with occupational exposure to Cr(VI).

McMahon-Lohrer further warned that failure to consider these “new” findings would be “arbitrary and capricious,” the standard used by courts in determining whether or not to uphold regulations.

Underscoring McMahon-Lohrer’s implied threat, the Society of the Plastics Industry, Inc. remarked about the “potentially great significance” of the new study. What McMahon-Lohrer likely meant by the first and second phase of the “significant epidemiological study” were the two papers into which the Chrome Committee and the study authors divided the “final” four-plant report. Furthermore, as shall shortly become evident, serious questions can be raised about whether McMahon-Lohrer, as opposed to the Specialty Steel Association, only recently learned of the 4-plant study. There are also...

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questions concerning Collier Shannon’s involvement in the decision to split the report into two cohorts.68

Given the importance of the original study and the two papers to the OSHA PEL rulemaking, a brief discussion of the methodology and findings of these papers is not only relevant, but also necessary. Even if the research underlying the papers was not strictly managed by attorneys—a question I will explore later—the reasoning, statistical analysis, historical perspective, conclusions drawn, and opinions expressed in the papers all provide an understanding of the methodologies used by litigation/regulatory support firms in their support of attorney activities at trial and before regulatory agencies. As shall be seen, the publication of a paper suggesting the two studies were part of a campaign to undermine OSHA’s rulemaking—along with the resulting cross claims—further emphasizes the papers’ importance to the mental processes and methods often used by attorneys and their agents in building evidence for their cases or regulatory presentations.

Publication of the first article based upon the 4-plant study occurred almost congruently with McMahon’-Lohrer’s submission of comments to OSHA, appearing in the April 2005 edition of the Journal of Occupational and Epidemiological Medicine. The authors submitted the paper in July 2004, with it being accepted for publication in October. The data for this paper encompassed only the small American plants cohort. The second article, incorporating the German plant data, became delayed when it received considerably rougher treatment in the hands of peer reviewers for the second journal. After being rejected by the unnamed journal, the Journal of Occupational and Epidemiological Medicine deemed it worthy for a relatively swift publication in April 2006.

Both articles claimed to have determined that modern plant exposure levels likely provided sufficient protection to workers. The first article made several claims about current industrial

practices and its findings. Initially the article opined that—even without consideration of the study being discussed—new processes and industrial hygiene practices being used since the 1960s had “improved the work environment and exposures are now believed to be controlled at a reasonably protective level.” They emphasized that most of the prior studies were of limited use since their cohorts included mostly employees exposed to the high-lime process. While they explained that plants in both the United States and Germany were examined at the same time, they did not explain why they were being reported separately. The authors reported three lung cancer deaths when 3.59 were expected. They admitted that while encouraging, the study did not cover a sufficiently long period to even account for the typical latency period of twenty years for lung cancer. In addition, one of the lung cancer deaths occurred in a non-smoker, an extremely rare occurrence. They concluded by asserting the “absence of an elevated lung cancer risk may be a favorable reflection of the post-change environment. However, longer follow-up that allows for longer latency for the entire cohort will be necessary to confirm this preliminary conclusion.” Thus the report provided little, if any, additional information beyond that all included in Mundt’s previous study of Castle Hayne.  

The second paper, covering the German facilities, took a more aggressive approach, touting its findings as suggesting a “possible threshold effect of occupational hexavalent chromium exposure on lung cancer.” It too argued that earlier studies should not be relied upon due to the use of high-lime processes not relevant to today’s workers. They noted the original 4-plant study included factories in both the United States and Germany, but because of differences in measuring exposures they divided the study into two parts. Distinctions from the 4-plant study did not end at

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69 Rose S. Luippold, et al., “Low-level Hexavalent Chromium,” 381-385; and David Michaels et al., “Selected Science,” 3. While much of the following analysis concerning the two articles that came out of the study is technical and may not specifically concern attorneys, the arguments used by the protagonists and the ensuing maelstrom provide insight into the techniques utilized by Chromium industry consultants and demonstrate how similar they are to tobacco attorney methods.
the division of the cohort. Instead of the three categories of high, intermediate, and low of the original report, they estimated the relative risk of “high levels of exposure, relative to low and intermediate exposure.” They found no consistent trends in lung cancer mortality related to duration of exposure, but admitted, “few study subjects had an opportunity to accrue high cumulative exposures of 20 years or more before the end of the study.” Even at the highest and longest cumulative exposure category reported the SMR was only 2.74, or less than three times the expected rate of cancer. All other reported categories had rates less than expected. They believed the findings were “suggestive of no effect of Cr(VI) on lung cancer until some as yet undetermined level of exposure is exceeded. . . the findings of these studies are generally compatible with the concept of threshold effect.”

Publication of this second article occurred during a flurry of accusations and counter denunciations about the actual findings of the 4-plant study. The controversy arose after three medical scientists—David Michaels, Celeste Monforton, and Peter Lurie—at the Project on Scientific Knowledge and Public Policy (Project on Scientific Knowledge), an undertaking of George Washington University’s School of Public Health and Health Services, read three surprising trade association statements during OSHA’s second comment period for the chromium regulation. The remarks all directed OSHA’s attention to new [apparently unpublished] studies of German facilities demonstrating excess lung cancer mortality only at the highest levels. Yet none of the submissions provided the study data or indicated where they could be found.

“Intrigued” by these comments, the PSKPP scientists searched the Internet, finding Barnhart’s affidavit to the court handling the IHF’s bankruptcy and other documents describing the 4-plant study. These materials led them to attorney Mark Lanier and his newly acquired IHF archives. Two of the scientists traveled to Houston, spending three days reviewing the Chromium

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documents. The research resulted in publication of an article—just three days before OSHA announced the final rule—lambasting the actions of the chromium industry during the rule-making proceedings. The exposé described how the industry’s agents conducted post hoc analysis to challenge study results unfavorable to industry and split the final multi-plant report into two statistically underpowered components. They further described how the original final multi-plant report revealed significant risk to the intermediate as well as the high exposure groups, something neither of the published articles reported. The elevated intermediate risk disappeared in the two articles through statistical tricks. They found the American plant article—published in April 2005—underpowered, primarily due to the limited number of individuals included and a time frame too short to allow for the latency of lung cancer. The watchdog group further declared that the German cohort article—published in 2006 after first being rejected by another journal—went even further in its misrepresentation, combining two exposure strata which resulted in the disappearance of the workers of particular interest, the large group with intermediate exposure.\textsuperscript{71}

Three days later Reuters published a telling report about the Project on Scientific Knowledge article that warrants an extended quote:

The U.S. Occupational Safety and Health Administration is set to rule on that on Tuesday [February 28, 2006]. But in the run-up to the decision the journal \textit{Environmental Health} reported that industry-commissioned scientists withheld data suggesting even small amounts of the known carcinogen, which is used in the steel, aerospace, electroplating and industries, can be deadly. "We think we have an example in which all of the standard elements of scientific distortion are present: hiding behind the lawyers, statistical manipulation, failure to publish ... all that kind of stuff which comes right out of the tobacco industry playbook," said Dr. Peter Lurie, one of the report's authors.

Kate McMahon-Lohrer, an attorney at the firm Collier Shannon Scott and counsel for the industry group Chromium Coalition, vehemently disagreed with the \textit{Environmental Health} report. "That charge is absolutely and completely false and it's outrageous and libelous," she said. In a telephone interview, McMahon-Lohrer acknowledged that hexavalent chromium

\textsuperscript{71} David Michaels, et al., \textit{“Selected Science,”} 5. A Collier Shannon lawyer deeply involved in Chrome Coalition activities submitted one of the comments.
raises workers’ cancer risk at high doses, but said there was debate about the risk from low doses. She denied any industry-sponsored research was withheld from OSHA.

David Michaels, who heads the project on scientific knowledge and public policy at George Washington University and was a senior author of the report, said studies commissioned by a chromium industry group showed even low doses elevate cancer risk. "Industry had commissioned a study which looked at newer facilities where exposures were much better-controlled and that study showed that workers with relatively low exposure to hexavalent chromium had greatly increased risk of lung cancer," Michaels said by telephone.  

Neither chrome industry members nor Collier Shannon chose to sue the article’s authors for libel. While their decision-making process concerning a possible suit is not known, we cannot rule out the importance of the documents recovered from the IHF by Barnhart. Industry attorneys could not have relished the possibility of having to release the documents during discovery.

The activities of the three muckrakers did not end with the published article. They next wrote letters to the editor of Journal of Occupational and Environmental Medicine (JOEM) castigating it for publishing the articles and specifying the flaws in each of the articles. JOEM printed their letter about the first article, along with the industry authors’ reply, in its October 2005 issue. A letter criticizing the German article and the industry authors’ reply appeared one year later.

In the first letter, Alton Dweck—an M.D. and researcher at Public Citizen’s Health Group—joined two colleagues in arguing that the United States data, when considered alone, did not support the article’s conclusion that the cohort suggested an “absence of an elevated lung cancer risk.” In support they provided three arguments. First, the “healthy worker effect” is readily apparent in the study. Second, the short length of the study precluded any meaningful conclusions. Finally, the small size of the study rendered statistically significant conclusions almost impossible. As any epidemiologist should have known the study was too small to detect disease at the relevant plant.

exposure levels. As the watchdog group argued, since the study “could not have detected an increase in lung cancer even at the highest exposure level in the Gibb study, [a level higher than found in the industry study] it is obviously statistically underpowered to detect any increase in cancer at lower exposure levels.”

In their reply, the authors did not take issue with the primary points made by Dweck, and his colleagues. They brushed these off with one brief sentence drawing attention to the article’s admission of “methodological issues of low statistical power and short follow-up.” Rather, in a reply signed by the majority of the original authors, they attempted to turn attention to subsidiary issues such as industry funding, timing of publication, and the “purpose” of publication. They commended industry for “sponsoring research of their own employees.” They further urged readers to evaluate the data at “face value,” while observing that OSHA specifically asked for data like this, ignoring its being withheld for several years. Although the authors’ reply provided no justification for the delay of several years in providing the data to OSHA, they emphasized that the cohorts were separated due to United States exposure data consisting almost “exclusively of air-monitoring results, whereas for the German plants, the most extensive exposure data were biomonitoring (urine and blood) measures, and air-monitoring results were fewer and limited to a few years.” Finally, they reported finding irony in the larger cohort being submitted to a different journal that rejected it only after the OSHA hearings ended.

In the watchdog group’s letter to the editor concerning the German factory portion of the study Michaels and his colleagues reiterated many of the points made in the original 2005 exposé. Criticisms again fell into three categories. First they faulted the authors—almost all of who were also authors on the first paper—and their sponsors for not providing the final report immediately to

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OSHA, rather than waiting to publish it in two journals. Both before and during the comment period, OSHA had repeatedly requested this type of information from all parties, at one point specifically asked if one of the plants with a prior limited epidemiological study had an update or if there were “other cohorts available to look at low exposures.”

Second, the data contained in the two articles did not stand-alone. Rather, the separately published data comprised two segments of a larger study that contradicted the author’s conclusions. Although the study protocol and the final report repeatedly emphasized “the need to maximize statistical power by combining data from the four facilities,” in fact “the investigators subsequently divided this study into two components and published two statistically underpowered studies.” Moreover, when considering the cancer odds ratio, the article’s authors had combined the low and intermediate exposure categories that were contained in the internal report, resulting in the disappearance of the report’s observation of increased risk among the intermediate group, as well as a clear dose-effect relationship. In what the public watchdog scientists called irony, they noted that the article’s authors cited the report’s urinary conversion factor when comparing the German urine exposure levels to OSHA’s permissible exposure level—even while claiming they divided the study due to these irreconcilable differences.

Finally Michaels and his colleagues observed that OSHA had already rejected the paper’s conclusion that the “data suggest a possible threshold effect.” In this rejection, OSHA had underscored the study’s limitations, citing to the small cohort, the limited follow-up, and the few lung cancers and participants in the lowest three groups discussed. OSHA scientists found these limitations “severely limit the power to detect small increases in risk that may be present with low cumulative doses.”

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The authors of the German study, who included Thomas Birk—a scientific manager for Environ in Germany, Birk—disputed each of criticisms in a reply printed immediately after Michael’s letter. They readily admitted delivering the data to industry well before the issuance of the proposed rule, arguing it was not their place to provide it to OSHA. They had no duty to forward the data to regulatory agencies, stating: “it is standard in the consulting industry to undertake client assignments on a confidential basis.” They provided no reason why the IHF—never mentioning the actual sponsors—did not provide the report to OSHA. The consultant group further argued that there was no bias in the manuscript, since they “had the right to publish the results after consideration (without obligation) client comments.” The response further intimated that the original report would not have been appropriate to give to OSHA since it had not been rigorously peer reviewed and therefore was not the “best available evidence” for OSHA, without explaining how the outside “Science Advisory Board” peer review conducted for industry was insufficiently “rigorous.” Nor did they suggest how supplying no evidence to OSHA during the allowed comment period was more appropriate than submitting their industry report.75

The authors further explained that without further contact with the IHF following submittal of the report, they prepared manuscripts, with the process then being accelerated “at the urging of the U.S. Office of Management and Budget.” The time difference in publication was the result of submitting the articles to different journals, with the German paper being held up for review until the end of the OSHA review process and then rejected. They next submitted it to JOEM, whose editors accepted the paper, but only following revision.

The comment by Birk and his colleagues about OMB also deserves further scrutiny, since it suggests likely close involvements of attorneys in this whole process. Neither the paper, nor the response hinted at the authors’ contact with anyone in industry other than the IHF. If they had no contact with industry representatives, how and why would OMB have contacted them? In fact, if they were urged by OMB to complete the articles, they must have had further contact.

Through this period OMB kept public records of its meetings with nongovernmental entities. During the early years of the new millennium OMB officials had four meetings concerning hexavalent chromium: one at the request of Public Citizen and steel worker representatives, one with cement industry representatives, and two with the chromium industry. On June 10, 2004, industry representatives met with OMB to discuss the OSHA hexchrome regulation. Attendees included Harold Firtzpatrick, Kate McMahon-Lohrer, and Joel Barnhart. On October 3, 2005 industry representatives met with OMB to discuss the OSHA hexchrome regulation. Attendees included McMahon-Lohrer, for the steel industry. If OMB had urged quick completion of both articles, as Birk contends, it must have been discussed at the June 10, 2004 meeting, since the second meeting occurred after publication of the first article. Thus, McMahon-Lohrer likely knew of the study no later than ten months prior to her comments to OSHA claiming “recent” knowledge. The similar post-hearing comments presented by Fitzpatrick for the CPMA provide buttressing for this possibility, since he too attended the June 10, 2004 meeting with OMB. When added to the frequent meetings that Collier Shannon attorneys held with Barnhart, the division of the study into two papers as a result of attorney pressure cannot be ruled out.\(^7\)

Returning now to the response of Birk and his colleagues: they addressed Michael’s statement that the two papers were both part of a larger study, by reminding readers that study’s division was described in the two papers. However, they dismissed Michaels’ assertion that the results from the larger study results refuted the paper’s conclusion that the data suggested a threshold: such a claim was “inaccurate and based on information taken out of context.” Further analysis of this comment is impossible, since they did not specify why the accusation was erroneous and taken out of context. Turning next to their reason for splitting the study into two parts, they emphasized that the “several substantial differences between the cohorts complicated the analysis.” They claimed that upon seeing the considerable distinctions, unnamed individuals colleagues attending the Epidemiology in Occupational Health Symposium held in Barcelona in 2002—where the study was publicly presented—“recommended that the results be stratified by country and reported separately.” In addition, the unnamed senior academic epidemiologist who served as an external advisor also recommended division. They further argued that separating the groups did not diminish the two studies because the German study was large and led to the suggestion of a threshold. Thus, they contended that the results in Michaels’ table displaying the overall Odds Ratios of the original internal report were unreliable due to the “substantial differences between the U.S. and German cohorts,” a statement not made in the original report to industry.

Perhaps desiring to appear willing to compromise, they finally agreed with Michaels et al.’s third point that demonstrating a threshold from the study was difficult due to the small numbers. However, they hoped that “presenting such results—even if only suggestive—stimulate additional useful epidemiological research and expand scientific inquiry.”

Back and forth accusations such as these tend to be very confusing. And that is the point. As previously mentioned, this is another technique used by lawyers when good facts are not available:

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77 T. Birk, et al., “Authors’ Response.”
confuse the issue and muddy the waters by creating doubt. The importance of these events over a period of almost a decade revolved upon four key questions 1) Why did industry conduct the study?; 2) Why did they not provide the results immediately to OSHA and the EPA?; 3) Why and how did they change the protocol and is that important?; and 4) Why were two articles published?

The reason for the study was simple. Industry conducted the study because, if OSHA used available epidemiological studies to determine the appropriate hexavalent chromium PEL, it likely meant setting a level very costly to industry—particularly chromium manufacturers—with potentially sharply reduced profits if competitive products and processes restricted manufacturer price increases. Of course, industry’s attorneys could and did draw attention to the alleged weaknesses of the available studies: they examined much higher levels of exposure; they did not have sufficient smoking data; there were gaps in the data; and they had numerous other shortcomings always found in epidemiological studies.

This type of attack was not new. It was the mainstay of cigarette attorneys for sixty years. Epidemiological investigations involve people and real, uncontrollable events. They are not conducted in the highly controlled environments of laboratory studies. Thus all skilled trial attorneys can point to numerous shortcomings in the proof of virtually any epidemiological investigation. To be blunt, if an experienced plaintiff or defense toxic substances attorney cannot find at least two or three weaknesses in any adverse study, he or she should consider another line of work.

However, unlike tobacco at mid twentieth century, here the evidence of causation at high exposure levels was exceedingly clear. Yet, studies on exposures and disease at the newer American, British, and German factories presented an opportunity to create doubt about causation at lower levels of exposure. If higher disease rates were not found, the six plants had just sufficient populations to provide the statistical power necessary to justify declaration of a threshold effect. Nor was this possibility purely hopeful thinking. At least two previous examinations of British and
American plants with fairly low exposure levels hinted at the possibility of lower lung cancer rates than found in older United States plants. The risks were also limited. In the proposed project the majority of workers had exposure histories barely reaching the 20-year latency period for lung cancer, with a large minority falling well short, thus rendering unlikely an increased lung cancer death rate. Balancing the risks and potential rewards, the plants thus appeared to be the best possible source for data to limit restrictive regulation.

In the end, the most telling indication of the importance of this study and the chromium industry’s desire to “get it right” was the contractor picked to conduct the study; Applied Epidemiology Inc., a firm managed by industry consultant Kenneth Mundt. The firm had recently demonstrated its reliability in work that supported Chemrisk, a firm universally known for its unwavering industry oriented litigation support. As a litigation support firm, Chemrisk knew how to build evidence for litigation and regulatory proceedings. Above all else, a study must provide evidence for the attorney’s case. From the very start of an industry toxic substance study, close attention must be paid to the various possible outcomes; building evidence for the client without learning anything undesirable. Thus it was here. The Chromium Committee carefully vetted the initial 5-plant study protocol, which went through more than one revision. In an effort to have a study with sufficient statistical power to demonstrate meaningful results, they intentionally incorporated factories in three countries, even though the chromium sampling techniques varied widely and required normalizing with each other.

The documents do not reveal the extent of either Collier Shannon’s or other attorneys’ inputs during the actual study. However, with the authors’ stating that the contract was confidential, this cannot be ruled out. Even assuming there was no direct involvement, a carefully worded protocol and analysis that is subject to review (as were both the case here) eliminates much of the necessity of close supervision during data collection. Through statistical analysis, data can be easily
massaged, with results changing dramatically depending upon the methodology. In this case, review of the data and rough report was extensive, with substantial industry input. Apparently, even nine months of review and massaging were insufficient to build facts sufficiently credible that they could influence the rulemaking, since both the authors and the Coalition members/attorneys did not believe the finalized study worthy of either publication or submission to OSHA and the EPA.

This analysis consequently leads to the second question: what caused the sponsors to withhold the results from OSHA and the EPA? Justifications from industry and the authors used variations on a theme, depending on the audience. The most basic element of their explanation lay in the authors’ statement that they wanted to provide the “best evidence” which required a “peer reviewed” journal article. Thus submission needed to wait until publication of the results. But, as we shall see shortly, this is not normal practice under EPA rules. This explanation also seems belied by the fact that three academic epidemiologists “peer reviewed” the study’s final draft. If the industry only desired to submit the “best evidence” and their own outside reviewers did not count, why did they waste time in hiring them?

It stretches credulity to suggest they had no faith in their own ability to pick a reviewer and felt no evidence was better than the report. Collier Shannon attorneys had no problem providing OSHA with Exponent’s nitpicking critique of the Gibb study without a peer review. Neither the authors of the final report nor anyone associated with the Chrome Coalition provided any indication why it should be different than the other submissions. The only possible explanation offered is that they believed it did not provide any new information for OSHA and that the data was suspect due to the differences in exposure measurement. Yet even this explanation rings hollow if the study truly showed the likelihood of a threshold effect.

Since the attorneys were in charge of strategic planning for the OSHA rulemaking fight, it seems unreasonable to assume they did not have critical input in the decision to not submit the final
report to OSHA. It is not believable that attorneys, faced with a deadline in comments and data submission for a critical PEL adjustment, would not put forward a study supporting their position, even if it had suspect data, simply because it did not have formal peer review. This is especially true since they submitted Exponent’s tortured—and non-peer reviewed—analysis that quibbled over minor problems in the Gibb study. Although specific proof of this hypothesis is not available, the refusal to submit the study has the hallmark of attorney decisions such as making a strategic decision to withhold potentially bad facts or to reschedule an expert for later in a trial when the initial run through of the testimony does not go well the night before.

This supposition is supported not only by the attorney control of strategic planning, but also by the German facility article authors’ statement that the OMB encouraged them to hurry the articles. Barnhart’s contact with OMB went through Collier Shannon, who was also the lobbying agent for the Chrome Coalition. The simplest explanation is that the attorneys decided the best strategy to exert extra pressure on OSHA was through the Bush administration’s OMB—always eager to discourage regulations—arranging a meeting for themselves and the industry authors.78

It appears therefore that the best explanation for delaying presentation of the study results to OSHA was that the attorneys did not believe the current formulation of the data would positively influence OSHA’s decision in the direction hoped for by industry. Thus, the unnamed “collaborators” at a conference and an unnamed “senior academic epidemiologist” provided a perfect rationale for both delay and reformulation of the data to provide stronger evidence. Three scientists reviewed the draft report prior to its final version. If one of them made this

78 In 2004-6 OMB held five meetings with groups concerning the hexavalent chromium rule. Attorney Kate McMahon attended three of them, twice for the Chrome Coalition (June 4, 2004 and December 14, 2005) and once for the steel industry (October 3, 2005). The other two meetings were with Public Citizen, steelworkers representatives, and David Michaels (January 9, 2005), and the cement industry (August 19, 2004) http://www.whitehouse.gov/omb/oira_1218_meetings_353 (accessed October 10, 2014). When did report go in and how does this fit timeline?
recommendation why did the authors not split the study prior to the final version being submitted? If the analysis as presented in the final version supported the opinions in the two papers, surely they would have simply mentioned that in each paper and not substantially restructured the analysis, while still indicating they split the data due to problems reconciling the exposure measurements.

Yet, the two papers did not pay homage to the original document and its three categories of exposures. This brings us to the third question - Why and how did they change the protocol and is that important? The public watchdog researchers provided the answer to how. The industry authors dramatically altered the protocol’s underlying principles by splitting the cohorts, thus reducing the numbers of individuals with lower exposures to numbers unlikely to show any positive results even at exposure levels found in higher level exposures then found in the Gibb study. The reduction in size also meant finding increased disease was extremely unlikely, since a large majority in each cohort did not approach the necessary latency period for chromium induced lung cancer. Moreover, they not only split the cohorts, they also changed the modified the analysis such that the increased odds ratio for the intermediate group disappeared. In addition, although they pointedly declared that the exposure measurements could not be reconciled, they used the same measurement conversion as used in the final report to compare the German readings to OSHA PEL limits.

The most sensible explanation of the protocol modification is that it provided the “best evidence” not for OSHA, but rather in furtherance of the attorneys’ strategy. That this extra work was undertaken solely at the instigation of the authors is not believable. Interestingly, in their protestation of independence, the industry authors provided hints that they did not work in isolation on the articles. In their response to the “watchdogs,” the authors wrote that they had no further communication with the IHF after submitting the final report. However, the IHF was simply an

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79 Given the latency period of lung cancer and the number of workers with less time from first exposure to the conclusion of the study, the positive results in the final report are both frightening, and likely to increase as more workers enter the danger period of twenty or more years since first exposure.
administrator of the contract. The declaration includes no such proclamation concerning the actual study sponsors and managers, including Barnhart and the attorneys. Furthermore, Barnhart and the attorneys conducted the strategic planning that the study was supposed to support. Any further contacts would have been with these individuals, not the IHF. As discussed above, such contacts did indeed take place.

The desire of the attorneys to have the study in the peer-reviewed literature in anticipation of litigation also offers the best explanation about why the authors approached JOEM to publish the articles. The attorney comments to OSHA concerning the prospective papers are classic tactics in establishing a record for litigation and regulatory actions. Still, they knew that establishing a record of OSHA failing to consider all evidence was not sufficient. They also needed to ensure the seeming reliability of their experts. Following Daubert, expert witnesses at trial are more likely to be excluded if their opinions have not been published in the peer-review literature. Thus, even though the articles’ conclusions were at best weak—admitted to even by the authors—once in the peer reviewed literature they could be more readily relied upon by the experts at trial.

These articles also display other clues that they were prepared with an eye toward potential litigation. In several ways they are very similar to the silica articles published following retention of many experts by defense counsel in the 1930s. Debatable issues are stated as fact. For example, all prior studies are dismissed as irrelevant because of “changed conditions,” praising industry’s advances in controlling exposure levels. The sole conclusion of one abstract is that “data suggest a possible threshold effect,” despite the data not having the statistical power necessary to support that claim. Finally, by reducing the observed categories from three to two, they manufactured the spurious claim of threshold and eliminated the clear dose response relationship found in the original odds ratio calculation.
These faults in the study all lead to questions about why JOEM published the two studies. With its extremely limited cohort, low exposures, and insufficient latency period, the first article simply stated the obvious. The study had neither the statistical power nor the length of time necessary to demonstrate a positive correlation. Watchdog leader David Michaels argued that its publication only occurred due to JOEM being controlled by industry. He is not alone in this claim. JOEM has long been criticized for supporting industry positions, right or wrong. As the official journal of the American College of Occupational and Environmental Medicine, its editorials often leaned towards industry. In addition, JOEM’s past history of biases in selecting and approving articles has created doubts among many public health officials and advocates about its position as a legitimate peer reviewed scientific journal.

In this case, although the two articles might have enhanced the scientists’ reputations for litigation purposes, they were unlikely to increase the journal’s standing in the scientific community. Under the Daubert guidelines the “peer-reviewed” publications increase the probability of a court’s admission of the scientist’s testimony at trial. This stands in sharp contrast to the scientific community, where insignificant studies such as these are soon lost in obscurity and tend to downgrade the prestige of journals that publish them.

The long and difficult exercise to publish the German portion of the study provides support for this argument. The authors first submitted the German study to a different, but unnamed journal. Scientists often try to publish parts of a study in several journals to increase its visibility and impact. Logically, since the German study involved a larger cohort, providing greater, albeit still limited statistical power, they likely attempted to publish it in a higher rated journal. After rejection

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of the study, they turned to the reliable and agreeable editors at JOEM who quickly accepted it for publication.

Other plausible scientific rationales for the publication appear lacking. Given the circumstances surrounding the publications—including one article’s rejection by another journal—JOEM’s desire to assist industry, as a motivating factor seems compelling. Although documentation for this particular incident may be sparse, the circumstantial evidence all point to a concerted effort on the part of industry’s PEL strategists (the attorneys) to develop at least one peer-reviewed study with negative findings for presentation to agencies and for litigation purposes. This is not difficult to accomplish, when, as here, a cohort is reduced in size and has limited latency. As John Bailar, prominent statistician and professor emeritus of epidemiology at the University of Chicago, has written, “One way to obtain a reliably negative result is to design a study with limited statistical power to demonstrate an effect.”

Indeed, in an article describing how to distort science, Bailar even cited to these specific chromium studies as a prime example of how this can be accomplished:

A recent report on health effects among workers exposed to hexavalent chromium reported no effects of consequence. However, the industry-sponsored investigators divided the cohort post-hoc into two subgroups, reducing the power of the study to identify an effect. They also classified the exposures, again post-hoc, into low- and high-exposure groups, making the excess risk in the intermediate exposure group “disappear”, the exposure levels of particular interest in an ongoing federal occupational health rulemaking. Moreover, they failed to share the results with a federal agency, even after the agency asked specifically for exactly this sort of epidemiological data. One must ask about the reasons for such obvious commissions and omission, and whether they are related to the fact that the industry supported the study.

Government scientists and officials appear to agree with the above analysis. They did not embrace these studies; actually OSHA did not even consider the studies sufficiently worthy to

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82 John C. Bailar, “How to Distort,” 221.
consider them. In a recent guide, NIOSH did not mention the studies even though its publication included a risk assessment for hexavalent chromium. In particular, the EPA was not amused by the delay in disclosure of the study and then the method of notification and publication. In 2010 the EPA sued Elementis for failing to submit the original study to it, as required by TSCA. In its lawsuit and case legal briefs, the EPA consistently argued that Elementis knew the study showed intermediate lung cancers, but failed to disclose the information. In response, Elementis tried a novel tactic: in a one hundred eighty degree turnaround from the position industry’s lawyers took before OSHA, its new lawyers now argued to the administrative judge that the study provided no new information. On November 12, 2013, Chief Administrative Law Judge Susan L. Biro ruled in favor of the EPA. She concluded that upon receipt of the report Elementis should have immediately provided it to the EPA, finding that

The SRI in the Final Report includes much more than the statistically significant risk finding at the highest exposures. And when the Final Report is compared to the Gibb Study, there are multiple and significant distinctions in the SRI presented. Respondent’s argument that these differences are “irrelevant” because only “cumulative total exposure . . . is relevant in assessing risk,” has not been supported by a preponderance of evidence in the record.

Elementis has appealed. As I write this chapter, this saga continues.83

California: Hidden Groundwater Contamination and Stomach Cancer

Hinkley, California, located in the Mojave Desert near Barstow, is home to an important component of Pacific Gas and Electric’s (PG&E) Texas to California natural gas line, a compressor station. Built in 1952, the station boosts the pressure of the natural gas, enabling it to reach central and northern California. The heat generated by the compressor flows to cooling towers where hexavalent chromium is added to the water as a corrosion and rust inhibitor. When the water

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83 In the Matter of: Elementis Chromium Inc., f/k/a Elementis Chromium LP, Initial Decision, United States Environmental Protection Agency Before the Administrator (Chief Administrative Law Judge Susan L. Biro, November 12, 2013), 72.
became saturated with solids—like hexavalent chromium—a portion is discharged into holding ponds. During the period 1952 until at least 1966 the company discharged approximately 750,000 gallons/month of hexavalent chromium contaminated water into the pools. By 1966, discharges to the unlined pools included an estimated 65 tons of chromium-based inhibitors. Since PG&E did not line the ponds until 1972, over the years significant quantities of hexavalent chromium leaked from the containment. Leaks and spills associated with the station may have caused local groundwater and air concentrations of hexavalent chromium to reach levels of more than 1,000 times the safe limit for drinking water and more than 50,000 times the safe level for inhalation.84

On December 7, 1987, over twenty years after Hinkley farmer John Speth first observed and notified PG&E of greenish deposits in his toilet water, PG&E officials finally advised California authorities that a routine November environmental survey had discovered 0.58-ppm of hexavalent chromium in a groundwater monitoring well north of waste water ponds associated with its Hinkley PG&E compressor station. Further sampling of over 50 local wells determined that eleven wells had chromium concentrations in excess of the drinking water standard of 0.05-ppm. The plume extended approximately one mile north of the compressor facility. When later asked by a Fox news reporter why this had not been reported in September 1965, when PG&E personnel had found hexavalent levels in the area up to 400 times the current EPA safety standard, PG&E replied that senior management had not been told about the earlier finding.85

Concerned that the public disclosure might bring about lawsuits, PG&E next took several actions aimed at quelling unrest among the residents of Hinkley. Initially they sent a flyer sent to the neighbors of the station, informing them that groundwater contamination had occurred due to the

“small amounts of chromium . . . commonly added by industries to cooling towers to prevent corrosion and scaling.” The flyer recommended they avoid drinking well water, but for other purposes it remained safe. At a subsequent town meeting on April 25, 1988, PG&E officials explained to the citizens—characterized in company official notes as “residents”, “politicos,” and “tort law suits”—that there was “no risk at current levels” and generally, site groundwater was suitable for drinking and agriculture. They further advised the attendees that chlorine in swimming pools “kill[s] any contaminants in the pool, including chromium.”

Following the town and internal company meetings management might have realized the state and local citizens would likely require further reassurance. In possible response to this blackboard notes at one PG&E meeting about the contamination indicated that any potential risk assessment they conducted should support the lack of public health and environmental risk. In a further effort to forestall regulatory and legal actions, the utility also began a program of quietly buying potentially contaminated local property.  

The fears of lawsuits alluded to at the town meeting, in blackboard notes, and through the property purchase program, were realized six years later. As a result of legal assistant Erin Brockovich’s investigation—perhaps conducted at the instigation of a property owner who became very suspicious when offered an extremely high amount for her house—seventy-seven plaintiffs hired Brockovich’s employer, trial attorney Ed Masry. He filed suit against PG&E for damages and fear of cancer from the chromium in *Anderson et al. v Pacific Gas & Electric et al.* (hereinafter Hinkley case)—the first of a series of cases concerning Cr(VI) groundwater contamination by PG&E. Eventually 648 individuals joined the initial lawsuit. PG&E initially attempted to limit the lawsuit by moving to dismiss the major claim of “fear of cancer.” To their surprise, on June 13, 1994 the court ruled that while such claims may not ordinarily be allowed, in this case the public’s interest in pure

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86 Carole D. Bos, “Erin Brockovich” Chapters 2 and 8.
water supply meant that allowing pollution to take place without effective redress was against public policy. The “fear of cancer” claim remained part of the legal action. Following the ensuing summer blizzard of plaintiff motions to compel discovery the parties agreed to arbitrate the case before two retired judges, John K. Trotter and Daniel H. Weinstein. The parties picked thirty-six claims for the initial arbitration. Individual discovery and arbitrations for these claims took nearly two years and resulted in large awards. Shortly thereafter, PG&E agreed to a global settlement of $333 million, while also agreeing to cease its usage of hexavalent chromium and clean up the pollution. 87

The two-year arbitration process affected more than just PG&E and the clients from Hinkley. It also greatly affected the scope and tenor of medical science and research into ingested chromium. As a result of the arbitration, the medical science of CR(VI) drew greatly amplified attention from many parties, including state and federal regulators, plaintiff defense counsel, chromium utilizing companies, the media, and the public. However new research came almost largely from scientists hired to defend the chromium utilizing industries, including PG&E and Lockheed Martin Corporation. Moreover, much of the research was conducted through the auspices of defense attorneys, enabling the research and opinions to be both molded to the attorneys’ desires and hidden unless it proved useful at trial or before regulatory bodies.

During the arbitration period of the Hinkley case, PG&E attorneys employed numerous scientific expert witnesses known for their interests in chromium. They included an Italian (Silvio DeFlora), a Swede (Sveere Langard), a British citizen (Leslee Bidstrup), and two Americans (Steven Patierno, and Ellen O’Flaherty). However, the scientists most useful to the attorneys—and the ones who conducted most of the attorney-funded research—came from the litigation support firm ChemRisk.

PG&E’s attorneys hired ChemRisk in the fall of 1994, and maintained their contract until at least the summer of 1996, to help in the Hinkley case’s arbitration. The attorneys never intended for ChemRisk scientists to be witnesses, but rather to act as “confidential consultants” and generators of peer-reviewed literature that could be utilized by the attorneys’ large stable of expert witnesses. In this capacity, the consulting scientists provided great benefits to PG&E, publishing at least eight articles on chromium, all specifically designed to demonstrate a relative innocuousness of hexavalent chromium. Many of the studies included exposure to different hexavalent chromium water concentrations and carefully controlled acute ingestion of chromium by volunteers. One contract even baldly indicated the experiment’s purpose was to determine that hexavalent chromium levels associated with swamp coolers are negligible.88

During the first Hinkley arbitration, the judges informed the parties that they believed two Mexican and Chinese epidemiologic chromium studies were important, perhaps even compelling, to the case’s resolution because they linked ingestion of hexavalent chromium and cancer. Following this comment, industry attorneys from Haight, Brown, & Bonesteel—perhaps Steve Hoch and Caroline Dec—met with ChemRisk scientists Dennis Paustenbach and Brent Kerger. At the

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88 In deposition testimony for another lawsuit ChemRisk scientist Brent Kerger characterized their work during the Hinkley case’s arbitrations as “confidential consultants.” Thus, since they wanted to keep their consulting “confidential,” it appears they neither anticipated nor desired the connections between their articles and PG&E attorney funding/management to become public. PG&E attorneys also revealed that Kerger was employed as a “confidential litigation consultant” in the Aguayo case during 1998 and 1999. Thus any work, experiments, or research he conducted or managed during this period was similarly confidential. Deposition of Brent Kerger, Volume 1, December 4, 2002, *Aguayo et al., v. Pacific Gas and Electric Company, et al.*, filed March 15, 1995, Los Angeles County Superior Court, 15, 18-19, 23, 25-28, 70-71, and 275-281 (in author’s possession). Nor was this the first time PG&E outside attorneys Ernie Getto, Bry Danner, Robert Wyman, B. J. Kearwynn, and Ernie Getto, are included in that list. Dennis J. Paustenbach, Ph.D., DABT [curriculum vitae], http://legacy.library.ucsf.edu/tid/ssm92d00/pdf. (Accessed November 12, 2014 and in author’s possession). In at least some of the experiments that ChemRisk performed during this period for Latham and Watkins attorneys, the lawyers and they apparently agreed about the results ahead of time. For example, in the contract concerning swamp coolers, the experiment’s purpose is listed as determining that hexavalent levels associated with the equipment is “negligible.” Kerger’s explanation for the stated purpose is that these were already known facts that could be further supported. He did not, however, point to any specific prior evidence. Deposition of Brent Kerger Volume 2, February 18, 2003, 520-521.
meeting, the attorneys informed the two scientists about the judges’ comments. Kerger remembers being told the attorneys would like more information about the studies. As recalled by one former lawyer for PG&E, they told the litigation support firm “to follow up, to see if they could make contact and get some of the underlying data.” Kerger recalls at least a few subsequent meetings he had with the attorneys about the status of his work with regard to these studies. It was Kerger’s understanding that “pretty much every consultation (they) had with them (PG&E’s attorneys) would be considered in their defense.”

Although PG&E had hoped the arbitration agreement would resolve its difficulties, a related event that same year added additional emphasis to the attorneys’ concerns. In March of 1995 Gary Praglin, a tall, lean West Los Angeles attorney who worked on the Hinkley case, filed a lawsuit for residents of Kettleman City, a small town nestled close to the interior hills of the coastal range. Like the Hinkley lawsuit, it alleged PG&E gas line activities contaminated their water chromium. If the two studies were not countered, PG&E payments to residents of the valley might exceed a billion dollars.

Following the meeting ChemRisk employees prepared funding requests then began tracking down the articles’ authors. As explained by Kerger, “We were advisors to the attorneys and to PG&E, scientific advisors, and they informed us of this need for additional information requested

by the judges and we came up with a scope of work in order to fill that request.” Throughout the subsequent research and follow-up, ChemRisk kept the attorneys informed of its progress.91

ChemRisk officials first assigned one of their scientists, Brent Finley, the task of traveling to Mexico to meet with one of the Mexican study’s authors, Dr. Armienta-Hernandez, and any others that “might have some additional background on the published articles that had been based on two different groundwater chromium plumes in different areas of Mexico.” While in Mexico, he also visited the affected sites, but could not obtain much information, finding the authors unhelpful. Kerger and Corbett later spoke to Armienta-Hernandez in an attempt to get data on her background and exposure urine data, which were very different from what ChemRisk had determined in their research. She may have sent the information, but if so, it probably did not explain the differences or was deemed as not useful to the defense of the cases. Thus, ChemRisk did not publish anything about the Mexico work, likely because they could not resolve questions about the study in a manner useful to the Latham & Watkins.92

ChemRisk’s investigation of the Chinese article proved more fruitful, resulting in the initially most important, yet ultimately most damaging, article to PG&E’s defense. ChemRisk assigned the task of tracking down the study’s author, Dr. JianDong Zhang, to an affiliate in Shanghai. Once found at his home in JinZhou, in northeast China, ChemRisk hired the retired, relatively impoverished scientist as a consultant at $250 a month, a very significant amount for the doctor.93

91 Deposition of Brent Kerger, 42-43, 47, 51, 328, 330, 479-82. As occurred with other invoices, ChemRisk sent the bills for these activities to PG&E’s counsel.
92 Deposition of Brent Kerger, 52, 54-56, 77. It appears that the Mexican scientists did not cooperate. They may have realized that ChemRisk was attempting to discredit their study.
93 Peter Waldman, “Study Tied Pollutant to Cancer.” Much of this paper’s narrative about the 1997 “clarification” of Doctor Zhang’s original 1987 article linking ingested hexavalent chromium to stomach cancer comes from documents obtained by plaintiff counsel from ChemRisk in the Aguayo v. Pacific Gas and Electric Company case. There may be other ChemRisk documents related to this episode since, when plaintiff counsel requested the documents, ChemRisk turned them over to its counsel who decided which ones to release. It is unlikely, therefore, that any missing documents are exculpatory. Since the attorneys did withhold certain “attorney work product” documents, there may be other documents illustrating the connections between ChemRisk and them. Deposition of Brent Kerger, 14-17. Note that although ChemRisk later claimed Zhang was afraid of having the government find out he was working with westerners,
In hiring Zhang, ChemRisk had an ultimate goal in mind: disproving the purported link between ingestion of hexavalent chromium and stomach cancer. In a 1995 internal memo, the ChemRisk scientist primarily responsible for this project, William Butler (previously mentioned in the tobacco chapter), foresaw two “products” for PG&E from ChemRisk’s work with Zhang in support of this goal. He proposed directing one effort toward producing evidence forming the basis for trial exhibits in new trials demonstrating “the absence of the association between cancer and groundwater exposure to hexavalent chromium.” The plan envisioned the exhibits being buttressed by the submission of a follow-up report on Zhang’s study—with Zhang as the lead author—to a peer-reviewed journal. Butler requested a budget of $25,000 from the attorneys for the new Zhang article, much of it for 60 hours of his time to “interpret data,” “write reports,” and perform other tasks. Butler included additional time for other ChemRisk scientists and technical staff. He anticipated Zhang receiving no more than approximately $2000.00. Zhang’s contribution was listed not as author, but rather as “research assistance.” Near the conclusion of the memo Butler also groused: “It is at times difficult to convince Dr. Zhang of the importance to us of the specific details of his studies so that we can execute our own analysis (emphasis added).” Butler further indicated that his optimism was based upon data showing that although the study area as a whole had much higher stomach cancer than normal, within the study area the village closest to the source had lower rates of stomach cancer than those further away. 

ChemRisk was openly working with affiliates in Shanghai before contacting Zhang. Deposition of Kerger, 108. Further examples of ChemRisk’s open work with Chinese scientists during this period are contained in the Benzene chapter. Bill Butler memo to Brent Kerger, August 7, 1995 (in author’s possession); and Peter Waldman, “Study Tied Pollutant to Cancer.” ChemRisk’s goal differed substantially from Zhang’s initial goal. In a letter to Ye (translated by Ye) Zhang states, “I will work to the end on the pollution issue of chromium (VI) that we fight together.” As explained by Kerger, although ChemRisk scientists knew that Zhang was interested in conducting further research at the villages, ChemRisk did not consider this possibility because it was not within the scope of the work assigned to them by the attorneys. They may have also been reluctant to see what the data would show. Deposition of Tony Ye, Volume 1, December 12, 2002, 1-291, Volume 2, December 13, 2002, 292-576, and Volume 3, March 11, 2003, 577-683, Aguyao et al., v. Pacific Gas and Electric Company, et al., 534; Deposition of Brent Kerger, 334-5. One explanation for the lack of distance correlation apparently not considered by Butler was the fact that water in the closest villages became so contaminated that it was yellow. Bottled water had to be shipped in. Thus, individuals in the closest villagers drinking substantially smaller quantities of the well water could have resulted in lower rates of cancer. (Cite for yellow water and
Three weeks later ChemRisk staff finished a first draft of the new study. Kerger later maintained that the draft resulted from collaboration Dr. Zhang and the ChemRisk employees, who came “up with an initial outline or set of information that we, meaning myself, Bill Butler and Tony Ye, developed into the final manuscript,” with Butler providing all of the numerical analyses. Recent college graduate and new ChemRisk employee Tony Ye then translated—to his very limited ability—the document into Chinese before sending it to Doctor Zhang, who spoke and understood very little English.\(^95\)

While noting the high cancer rate, the initial manuscript emphasized that each village’s distance from the smelter did not necessarily correlate with cancer rates from the village. The draft only considered physical distances, not the hydrological movement of the groundwater. After reading the translated manuscript, Dr. Zhang agreed there was no positive correlation in the study area between physical distance to the groundwater pollution source and cancer mortality—an opinion in keeping with his original article, which did not claim a direct distance correlation. However, Zhang did not accept the consultant’s conclusion that “lifestyle of the residents and other environmental factors unrelated to chromium contamination” could explain the higher cancer rate in the study area. Through Ye, he asked the firm to replace the assertion with a vaguer one mentioning several possibilities, as well as the need for more research, since the stated conclusion was “only an inference; it is inappropriate to consider it as a cause.” Shortly thereafter Ye wrote to Butler that

\(^{95}\) Deposition of Brent Kerger, 159-60, 267-8; Peter Waldman, “Study Tied Pollutant to Cancer”; Deposition of Ye, 78-9, 324-6; Ye admitted he had trouble “all the time” translating Zhang’s words into English. “As I can recall, during my time at ChemRisk when I translate each of Dr. Zhang’s manuscript, I -- it’s more than three revisions. It's -- it’s normally more than five revisions because the first time, the second time I normally do not get it correct; so it’s take [sic] several times to revise my translation.” “But like what I said, my translation -- normally I find my translation needs to be revised at a later time. That's when I work on Dr. Zhang's article, that's many, many revisions I have made.” Deposition of Tony Ye, 19 and 78. Still, Ye remained as translator throughout the paper preparation, even though ChemRisk knew about Ye’s very limited abilities as a translator. Deposition of Brent Kerger, 323-4.
“Dr. Zhang did not totally agree with us with the conclusion section,” so he (Ye) had “to make a little compromise.”

Documents and testimony by former ChemRisk scientists show that Ye subsequently produced numerous additional typed drafts of the article, all of them first prepared in English. The cover pages of the first two drafts state that it was “by ChemRisk,” with subsequent drafts deleting that reference. More than ten of the early drafts contain handwritten changes by ChemRisk employees other than Ye. The final report’s text and graphics were produced in English on ChemRisk computers three months following the last draft that was apparently translated into Chinese.

This final version of the report—for which no Chinese version has been produced—ignored Zhang’s request to amend the conclusion. Rather, it strengthened prior ChemRisk opinions by identifying lifestyle and other environmental factors as the “likely” cause, rather than suggesting that they “might” be the cause. It further insisted that the higher cancer rates were “not a result of the contaminated water.” According to the paper, neither stomach-cancer and lung-cancer deaths “indicated a positive association with hexavalent chromium concentration in well water.” The last Chinese language draft subsequently produced by ChemRisk in litigation contained neither of these assertions. Nor did the final version mention any of Zhang’s reports written prior to 1987, perhaps because they identified Chromium (VI) as a cause of cancer in the region, even if the distances from

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96 Tony Ye memo to Bill Butler, September 6, 1995. (in author’s possession); Deposition of Tony Ye, 317-19; Peter Waldman, “Study Tied Pollutant to Cancer.” Nor does Ye have a specific memory of Zhang requesting the inclusion of a critical sentence that first appeared in the November draft of the Zhang 1997 article: “Thus, it is apparent that the increased mortality rate was not due to the contaminated water.” Deposition of Tony Ye, 203-210. The “lifestyle distinctions” claimed in the paper have never been explained. In his earlier work Zhang wrote that there is not much difference between socioeconomic status or diet among the different villages. Deposition of Tony Ye, 397-8. It is thus unlikely that Zhang either mentioned them or provided information about them to ChemRisk.
the source did not directly correlate to the incidence of cancer. In one paper Zhang wrote, “nearly 80% of cancer was attributable to environmental pollution.”

Dr. Zhang died in 1999, before anyone other than ChemRisk employees could ask him about the article. In an interview taken around 2005, his son expressed extreme aggravation at the suggestion his father would have retracted his award-winning 1987 findings. He further related that his father was “sure of the relations” between hexavalent chromium and cancer. Young Zhang asserted that he expressed this confidence in part because he assisted in portions of the original investigation.

In December 1995, Ye submitted what ChemRisk characterized as a “clarification” to two journals, the *Archives of Environmental Health* and the *Journal of Occupational and Environmental Medicine*—even though this clearly violated both journals’ written policy against simultaneous submissions. Ye signed the ChemRisk drafted cover letters, stating he was doing it as a favor to Doctor Zhang, and gave his California home address and phone number as contact points. The Study listed Zhang and one other Chinese scientist as authors. Neither the study nor the plain paper cover letter mentioned either ChemRisk or PG&E. ChemRisk employees subsequently explained this method of submission to two factors. First, the study was “not a work product, per se,” of the consulting firm. Second, at least two ChemRisk employees maintained that Doctor Zhang told Ye he only wanted the two Chinese scientists listed as authors. Although they actually preferred being listed, ChemRisk

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97 Deposition of Tony Ye, page 516; Deposition of Brent Kerger, 514; Drafts of the 1997 paper with Bates stamps, TY-0102-0112, TY0089-0097, TY0469-0476, WB-0215-0220, WB-0204-0209, WB-0200-0203, WB-0181-184, WB-0175-0179, WB-0164-0167, WB-0062-0070, TY-0113-0119 (Include complete cites). These documents were all produced by ChemRisk personnel pursuant to subpoenas in California litigation; Peter Waldman, “Study Tied Pollutant to Cancer.” Kerger annotated the part of Zhang’s translated article that attributed 80% of cancers to pollution “BS”, meaning “Bullshit.” Deposition of Brent Kerger, 175-6.

98 Peter Waldman, “Study Tied Pollutant to Cancer.” Ye’s testimony also highlights the possibility of other discrepancies in the drafting process. For example, Ye’s attorney refused to allow his client to respond to the plaintiff Attorney Gary Praglin’s attempts to admit a Chinese draft of the paper and an English version are the same document, but with certain sentences left out of the Chinese version, likely because Ye was not sufficiently adept at translating to determine whether or not the two papers were identical. Nor could Ye explain why Zhang never told him about his name being misspelled in several drafts. Deposition of Tony Ye, 304-16, 333.
scientists acceded to this request because of the potential Chinese political ramifications to Doctor Zhang if foreigners were included on the study. For example, in his deposition Kerger testified that Zhang chose only to list himself and Li as authors. Kerger “respected that request” even though he wanted to be listed as an author. Kerger did not ask to be an author because he “though it would be rude.” This “preference” of Dr. Zhang’s remains controversial, since, as Peter Waldman relates in his *Wall Street Journal* exposé, “Mr. Ye, in a 2005 interview, said he didn’t recall Dr. Zhang ever telling him that.”

At ChemRisk, company officials had originally discussed how to phrase the letter and decided since they would not be listed, Ye should handle it exclusively. They were also the ones who told Ye to use his home address on the cover letter, rather than his ChemRisk address. Thus the letter from Ye to the journals submitting the manuscript simply stated that “Since Dr. JinDong Zhang lives in China and does not speak English, I have been assisting Dr. Zhang with the preparation and submittal of this English manuscript.” In his deposition Kerger incorrectly maintained it would not have been relevant to mention ChemRisk or PG&E in the cover letter or article since, at that time journals had no expectation or requirement for that information.

Upon learning in May 1996 that both journals had accepted the publication, ChemRisk employees directed Ye to withdraw the article from the *Archives*—which was requiring changes to

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99 Deposition of Brent Kerger, 93, 94, 98-100. (Rude quote at 172); Peter Waldman, “Publication to Retract an Influential Water Study,” *Wall Street Journal*, June 2, 2006, A. 10; and Deposition of Tony Ye, 62-3. Kerger has even argued that ChemRisk was not a substantial contributor to the 1997 Zhang article. He argues, that at best, it “was a collaboration that he [Zhang] took credit for solely and we had no problem with that.” Deposition of Brent Kerger, 267-8; In a 2006 interview with *The Scientist*, Kerger almost completely discounted ChemRisk’s contribution to the article. “Was our intellectual contribution significant in light of the contributions of the authors that he wanted to have on the paper? The answer that we came to is no.” “The paper was really just a summary and a clarification of this research that already existed.” Melissa Lee Phillips, “Journal Retracts Chromium Study,” *The Scientist*, June 7, 2006. Apparently, Kerger believes that writing the initial version of each of ten drafts of an article over the course of several months, reanalyzing all of the data, conducting all of the statistical analysis, preparing all of the graphs, and writing a conclusion that the purported author does not agree with, is not sufficiently significant for inclusion as an author.

100 Deposition of Brent Kerger, 191-2, 194-5. (Quote at 192). Kerger also incorrectly argues that although the article did not mention that PG&E—through its attorneys—funded it, it did not matter, since an editor of a journal in 1995-6 did not worry about who funded a brief communication. Deposition of Brent Kerger, 168.
the manuscript—in favor of what they subsequently maintained was the more prestigious journal, JOEM.\textsuperscript{101}

In late spring of 1996 ChemRisk apprised its controllers—the attorneys—of the continuing good news. First, in a letter to Haight, Brown, & Bonesteel attorney Steven L. Hoch—which expressed his appreciation for their continued confidence in ChemRisk’s work—Kerger provided a report on the “Status of ChemRisk Publications on Chromium.” Kerger listed the Zhang 1997 article as one of eight “ChemRisk Chromium Manuscripts in print or accepted by peer-reviewed Journals.” Significantly, it was the only one that had no ChemRisk employees listed as author. On June 5, 1996, ChemRisk Associate Health Scientist Gwen Corbett wrote to five attorneys, including both in-house and outside counsel for PG&E, as well as five ChemRisk employees including Paustenbach, Proctor, and Finley, informing them of the paper’s acceptance by JOEM with no revisions. In the memo she reminded them that Zhang’s original paper “stated that total cancer and stomach cancer mortality was significantly elevated in populations living along the Cr(VI) contaminated groundwater plume.” She then observed that the new paper “clarifies that the cancer death rates (both total and stomach cancers) ‘were not correlated with the degree of exposure to Cr+6’ and that ‘neither stomach nor lung cancer indicated a positive association with Cr+6 concentrations in well water.’”\textsuperscript{102}

Since publication of the article, ChemRisk scientists, while acknowledging that they may not have translated the final report into Chinese, have consistently maintained that Zhang was kept fully informed about the paper’s contents and fully collaborated in its production. Former ChemRisk

\textsuperscript{101} Deposition of Tony Ye, 133. In a further subterfuge—if not outright criminal fraud—ChemRisk officials gave Ye an unsigned note to send to JOEM asking to have the $350.00 publication fee waived because “it would be an imposition of a substantial financial burden on Dr. Zhang who is retired and living in China.” Deposition of Tony Ye, 452-3.

scientist Tony Ye, who acted as translator for Zhang—even though he admitted very limited abilities to translate writing—maintained that the article was published with Dr. Zhang’s “agreement.” For Kerger’s part, although he does not know if there is a Chinese version of the final paper, Ye, Butler, and he “agreed at the start of this project that we would communicate fully and in every way with Dr. Zhang about our thoughts and any – anything else datewise or wordwise or technical contentwise that went into this manuscript, and I believe we followed through on that.” Furthermore, “the end result was always approved by Dr. Zhang in terms of the exact writing.” He did not explain how this could occur with Ye’s admittedly poor ability to translate, or with Zhang’s unsuccessful efforts to amend the conclusion.\textsuperscript{103}

ChemRisk scientists expressed great pride in the manuscript. They had taken a potentially very damaging piece of evidence and turned it into a substantial bolster for PG&E’s public, regulatory, and litigation position. At his deposition, Kerger stressed that in a way, the 1997 Zhang paper was more important than any other scientific paper about carcinogenicity of ingested chromium. This importance came about “because it’s really the only epidemiology treatment that’s out there in the literature of a groundwater contamination plume and its potential cancer effects in a

\textsuperscript{103} Deposition of Tony Ye, 78, 324; Deposition of Brent Kerger, 159, 160, 162, and 197. Excerpts from several of Zhang’s pre 1987 papers, possibly translated by Ye, indicate Zhang believed Chromium ingestion was a significant health and cancer problem. Deposition of Tony Ye, 90-94, 104-5, 515. Interestingly, at his deposition Ye did not remember if he ever told Zhang that PG&E was in litigation over hexavalent chromium water contamination. Deposition of Tony Ye, 99-102, 371-3. Kerger initially maintained that he had told Zhang that they were working for PG&E, who was in chromium litigation, but later he backtracked stating that they had told Zhang of their “interest in understanding the health effects of hexavalent chromium in groundwater and I don’t . . .think it would be of particular interest to him to know the name of the company that was – that was in litigation about it or that it was particularly in litigation. My expectation is that we told him we were interested in understanding chromium VI in groundwater and its health effects. And I don’t recall if I said “PG&E” or “litigation” with regard to our interests.” Deposition of Brent Kerger, 169-70, 217-8. As the paper was in process of being published Zhang signed a statement that “Per our telephone conversation, I totally agree to your editing and expanding of the original manuscript. I think the English translation is accurate and complete.” In his deposition Ye admitted this document was sent to him by ChemRisk to translate and send to Zhang for signature. Deposition of Tony Ye, 265-6. In fact, the English version was not a translation, but the original document. To date, ChemRisk has not produced a Chinese version of the final draft. In his deposition, Kerger admitted that the final version might not have been translated into Chinese. Deposition of Brent Kerger, 159-60, 517. Certainly no documentation demonstrating a translation has been produced. Thus, at the time Zhang signed the statement he likely had not seen the manuscript to which it referred.
population.” Because of this, it was possibly the only study that could be used in regulatory rulemaking or be relied upon be expert witnesses in a court of law. PG&E’s troubles seemed to be over.104

At first scientific and agency opinions seemed to agree. Almost immediately the “clarification,” seemingly written by the original authors, began influencing scientific views. New peer reviewed articles—especially those funded by industry or their attorneys—cited the paper for its conclusion that the Chinese stomach cancer rates were not due to chromium, some even quoting the paper’s position that “lifestyle” was a more important factor. For a few years the paper directly affected both federal and some states’ water policies. The 2000 ATSDR update of its chromium profile included a paragraph discussing the 1997 study. The paragraph concluded by noting that in the 1997 study the authors “commented that these more recent analyses of the data reflect lifestyle or environmental factors, rather than exposure to chromium (VI)” —a position immediately rejected by Dr. Zhang, but included in the final draft of the published paper. OSHA’s 2001 assessment of whether Cr(VI) should be allowed in wood preservative cited to the article.105 Shortly thereafter the EPA and the California Department of Health Services cited the study. The study was also cited by a California Blue Ribbon Panel as supporting a conclusion that there was no need for further regulation of hexavalent chromium in drinking water. Finally, a New Jersey regulatory report written before 2006 cited to a Paustenbach co-written article that relied upon the 1997 article in part for the conclusion that scientists do not know whether hexavalent chromium is carcinogenic when ingested.

104 Deposition of Brent Kerger, 164-5. Interestingly, Kerger discusses the groundwater plume as important in later declarations about the article, but ignored the issue of groundwater movement in the article.
New Jersey’s chief risk analyst, Alan Stern commented that although they were aware of the earlier 1987 study they had not read it “because it’s in Chinese.”

One California agency, however, remained unconvinced. In 1999 California’s Office of Environmental Health Hazard Assessment (OEHHA) established a Public Health Goal (PHG) for total water-based chromium to 2.5 parts per billion (ppb), a significant reduction from the state-legislated required 50 ppb. They based the reduction on concerns raised by a rodent oral ingestion study of admitted limited reliability. They maintained this position despite a meeting with Paustenbach—likely acting on behalf of his attorney sponsors—and his July 17, 2000 follow-up letter to OEHHA Deputy Director George Alexeeff, referring to the Zhang 1997 paper and strongly disputing the need for a reduction in California’s PHG for chromium. In it he cautioned the agency that “[i]t’s important to recognize that the only epidemiological study of humans exposed to chrome VI via drinking water”—citing to Zhang and Li, ’97, the follow-up—“reported no excess in GI cancers even though drinking water exposures to chrome VI were well above the California MCL.”

Paustenbach further informed Alexeeff that one of the water quality tests in China found chromium levels of 20 milligrams per liter of water, 400 times the current California PHG of 50 ppb. In an

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effort to perhaps drive home his point, Paustenbach then reminded OEHHA of the importance of its decision: “Due to the renowned technical expertise of OEHHA, many organizations, expert panels, and juries throughout the United States and elsewhere weigh your guidance documents in their decision-making.”

Even as Paustenbach was attempting to quash OEHHA’s desire to publish a new PHG, publicity about the groundwater controversy exploded through two media sources. In March 2000, the opening of Erin Brockovich, a film about the Hinkley lawsuit, caused an outburst of public outrage. Five months later the Los Angeles Times began a series of articles chronicling the extensive chromium contamination of the Valley’s water groundwater. In reaction to the public’s ensuing outcries, the Legislature acted. In September Governor Gray Davis signed into law requirements to test Southern California water supplies for hexavalent chromium and a further examine of health risks associated with its ingestion.

One month later, California State Senator Deborah Ortiz held the first of what eventually became a series of three legislative hearings focused on the “Health Effects of Chromium VI Contamination in Drinking Water.” Even before these hearings Ortiz had a reputation as a street fighter and champion of the working class and downtrodden. Following her parents’ divorce after her twelfth birthday, she had grown up on welfare. As a member of Sacramento’s city council she


had pushed through several initiatives to help the city’s poorer districts. Considered by all to be blunt and feisty, opponents saw her as purely political, one-sided, and quarrelsome. Elected to the state Senate after an unexpected win in the Democratic primary, Ortiz had maintained her uncompromising desire to fight for liberal causes, including education, the underprivileged, and public health. Her opponents denigrated her methods as grandstands and publicity seeking but acknowledged her effectiveness. In the ensuing hexavalent chromium Senate hearings, she displayed all of these traits.109

As the first short hearing opened on October 24, 2000 Ortiz stressed that its purpose was “not to arrive at final decisions to public policy, but rather, search through the data.” She hoped to acquire an overview of the research concerning hexavalent chromium in water and identify gaps of knowledge requiring further investigation. Over the course of a few hours, four panels discussed hexavalent chromium and its human health implications, chromium in California’s drinking water, San Fernando Valley’s water supply, and public policy options. The panel participants tended toward the public health protective position, including state and local public officials, academicians, and two representatives from a plaintiff law firm, Edward Masry and Erin Brockovich. During the opening panel session, state scientists and academicians provided a short primer on hexavalent chromium and the science of its health implications as well as the uncertainties involved, while reminding everyone of the state law requirement that the OEHHA take a health protective approach in setting PHGs. Federal, state, and local environmental and water officials then discussed the extent of contamination in California, displaying varying levels of concern. Finally, near the conclusion of the

hearing, Masry, Brockovich, and three members of the audience called for all pertinent officials to take action in limiting chromium in the state drinking water.110

One year following the premier showing of *Erin Brockovich*, OEHHA—bombarded from all sides—decided to request an outside opinion for their PHG. In March the state asked the University of California at Davis to convene an expert panel (frequently and hereinafter called the Blue Ribbon panel) to provide advice about the carcinogenicity of hexavalent chromium when ingested.

“Cal/EPA forwarded to the University of California for its consideration names of experts that primarily included those from lists assembled by OEHHA and DNS. Cal/EPA also accepted and forwarded for consideration names from other sources.”111

An unknown university official asked Dr. Jerrold Last, a professor and toxicologist, to chair the panel and pick the six other members. Last first picked Doctor John Froines, head of the environmental program at UCLA. Known as a political radical in his younger days, Froines—a scientist committed to the cautious approach to chemicals—was now a respected toxicologist who had previously worked for OSHA and been the deputy director of NIOSH. Among others, Last also invited Dennis Paustenbach onto the panel—likely following the strong recommendation of Cal/EPA science advisor William Vance, who was lobbied by industry for a ChemRisk scientist to participate. Last considered the problem of Paustenbach’s work for several companies on chromium issues, both in the past and currently, but believed they did not constitute a conflict of interest even though the panel’s charter stated there should be no economic conflict of interest.112 Last later explained why he agreed to include Paustenbach on the panel:

One of the questions that Cal/EPA asked us to consider was whether or not, in what is called toxicokinetics or in the disposition of chromium within the

110 Senate Hearing October 24, 2000.
111 Deborah Ortiz reading of written statement by Cal/EPA Secretary Winston Hickox, Senate Hearing, February 28, 2003, 73.
body, there might be the basis for doing a quantitative risk assessment from animal data, and I felt that Dr. Ellen O’Flaherty—Dr. Paustenbach’s major professor—or Dr. Paustenbach were the two people in the world best equipped with the tools to address these issues. I had invited Dr. O’Flaherty to serve on the panel. She refused because she was currently living in Paris, and it was quite a distance. Dr. Paustenbach, who was my second choice, was willing to serve, and at the time, we felt we needed that expertise.\footnote{Testimony of Jerold Last, California Senate Health and Human Services Committee, Informational Hearing: “Possible Interference in the Scientific Review of Chromium VI Toxicity, April 2, 2003, Sacramento, CA. (Transcript in author’s possession), 18.}

Although unmentioned by Last, Flaherty also declined because she believed her work for PG&E—the same company Paustenbach consulted for—presented a conflict of interest. In an email to Last declining the invitation, she specifically told Last that working for PG&E constituted a conflict of interest:

Dear Jerry, thank you for your invitation to discuss with you my participation in the panel to advise Cal/EPA on how to best set a standard for chrome VI in drinking water. You may not be aware that I have retired and I live in Lyon, France. In addition, since I have served in the past as an expert witness on behalf of PG&E and on behalf of Lockheed in chromium-related legal proceedings, [I] might not be considered entirely appropriate for me to serve on the committee advising EPA on such a high profile issue.\footnote{Deborah Ortiz’s reading of Doctor Ellen O’Flaherty’s communication with Jerold Last, Senate Hearing, April 2, 2003, 41-42.}

Apparently neither Paustenbach nor Last had similar qualms.

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The panel experienced considerable controversy during its proceedings, beginning with the selection process of Froines and Paustenbach. The chromium companies and their attorneys reacted to the panel’s establishment with predictable responses, most of which occurred behind a veil of secrecy. When a plaintiff attorney raised doubts in a courtroom about the legitimacy of the panel’s selection process, PG&E attorneys proclaimed their and PG&E’s innocence. In a document presented to the court in the Aguayo case, Latham & Watkins attorney Ernie Getto told the judge “the suggestion that the committee’s [the Blue Ribbon panel] evaluation of the scientific literature...
pertaining to chrome VI resulted from undue influence on the part of PG&E is unfounded and absurd. PG&E had no involvement in this process whatsoever.”

The facts, however, contradict this statement. Attorneys and their agents were, if not the prime orchestrators, very prominent behind the scenes actors in chromium companies’ substantial activities designed to influence the Blue Ribbon panel. They performed in both their own capacity to prepare witnesses and through surrogates—including the public relations firm Kahn/Powell, the trade association Alliance for Responsible Water Policy, and the litigation support firm Exponent and its subsidiary, ChemRisk.

Kahn/Powell played a leading role in the corporate efforts to mold the UC Davis Blue Ribbon panel’s report. Within five days of the panel’s formation, Eric Newman, an attorney employed by Kahn/Powell, developed a three-prong strategy plan to avert a lowered chromium PHG: 1) reassemble a non-operational public relations front, the Alliance for Responsible Water Policy, as a anonymous face for chromium corporation positions; 2) establish expert contacts. This principally involved “evaluat[ing] litigation experts list” and “explor[ing] assistance in toxicology from litigation identified experts and Dr. Silvio deFlora, Brent Finley [and] Deborah Proctor”—all attorney consultants; and 3) “choreograph the required interactions among the affected community, legislative and regulatory officials, and outside expert advocates in a manner that supports strategic objectives.”

Although Newman’s specific client for this endeavor is not known, at the time his clients included PG&E. His emails concerning these issues went to numerous attorneys, Exponent scientists, chromium corporations, and at least one trade association of OSHA and EPA regulatory fame, the Chrome Coalition. The few available documents concerning the process suggest that

116 enewman@ka-pow.com (Eric Newman) email to censored clients, with cc. to thull@ka-pow.com, “Subject: Chromium Strategic Plan,” April 6, 2001. (in author’s possession)
Newman worked closely with both attorneys and attorney agents—particularly Exponent scientists—during the spring and summer to ensure a “win” for the chromium companies in the Blue Ribbon panel’s final report. ¹¹⁷

The first prong of Newman’s strategy, the Alliance for Responsible Water Policy (the Alliance), became a major element of the chromium group’s surreptitious attempts to control the outcome of the Blue Ribbon panel. The Alliance presented itself to the UC Davis Blue Ribbon panel as an independent, unbiased, “responsible” third party. Yet, throughout the spring and summer it was nothing more than a mouthpiece for the attorneys and the public relations firm. It did not have its own address, phone or fax numbers. Nor did it have a budget or tax return. When an individual called or faxed the association, the phone rang in Newman’s office. The executive director of the association, John Gaston, became a paid consultant and expert witness for PG&E attorneys Latham & Watkins in the Aguayo lawsuit. ¹¹⁸

Newman and Gaston actually began the effort to thwart a new chromium PHG at least a month before OEHHA decided to request an outside opinion. In a February 2, 2001 letter signed by Gaston but written by Newman, the two men requested companies to each provide the Alliance with $30,000. With such “modest contribution[s] from several key players” the two promised to “build a strong alliance to implement a strong pushback on the issue to dispel the voodoo science relied upon to date and to challenge the runaway misinformation campaign that we’re now confronted with on almost a daily basis.” The Alliance and other attorney agents, like Exponent, provided cover for PG&E and other chromium corporation attorneys to influence the Blue Ribbon

panel during the selection process, the public hearing and, ultimately the preparation of the report. In particular Newman, Gaston, and Exponent scientist Deborah Proctor worked closely together.\(^{119}\)

The group directed initial efforts to ensure at least one of their own became a member. As the selection process began, ChemRisk scientist Brent Finley sought ways to have either Proctor or himself selected for the panel. In turn, Newman alerted a wide range of clients, attorneys, and others about the panel formation, indicating he planned to lobby strenuously for “balanced representation” on the board. By this, he meant having Finley, “Deborah Proctor, and/or other folks on the nonalarmist (sic) side of things” selected for the Blue Ribbon panel. Six days later, a relieved Finley emailed Newman with the news that Paustenbach had been selected since Professor Last did not believe his work on behalf of PG&E was a conflict. “So it looks like we got ‘one of our own’ on the panel.”\(^{120}\)

In fact, this group did not confine itself to seeking “selection of one of its own.” It also actively worked toward limiting the influence of the one scientist on the UC Davis Blue Ribbon panel who took a cautionary view, perhaps even precautionary viewpoint, toward regulating chemicals, Doctor Froines. When Froines became Last’s first selection, they feared he might unduly influence further selections. When the remaining selections did not display any indication of Froines’ influence, sighs of relief likely emanated from many in the group. Yet Froines remained a threat as long as he continued serving on the panel. Proctor especially disliked Froines. At one point she commented, “Froines is chair of the panel now? Froines is so full of it.” Newman went even further, actively lobbying California officials to limit Froines’ influence.

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\(^{120}\) Brent Finley email to Eric Newman, “RE: LAT & AP Stories Re: State Action on Chrome 6 Standards,” April 6, 2001. (In author’s possession). In the same April 6 email Finley expressed hope on securing a position on the recently announced NTP 2 year oral cancer animal study for chromium panel for Paustenbach, Proctor or himself. See also Gary Praglin Testimony, Senate Hearing, February 28, 2003, 34.
While there is no direct evidence that Paustenbach took part in this effort, his participation cannot be lightly dismissed. Fellow Exponent scientist Deborah Proctor worked closely with Paustenbach, who provided her with inside information about the panel’s activities. She played the go between with Paustenbach and Latham & Watkins attorneys Ernie Gettto and Cynthia Cwik. In one email report to the attorneys about the panel she provided good news/bad news, informing that “[t]he OEHHA risk assessment will use the findings of the blue-ribbon panel. The panel has six to seven members right now, and Froines is back on.”

As the panel’s work progressed, this effort became increasingly successful. Last seemed to particularly rely upon Paustenbach in preparing draft sections of the report, while excluding Froines from significant first draft input. Froines intimated as much when he formally resigned from the panel in frustration on July 11, 2001. His resignation email voiced his concerns “about the apparent lack of input from other members of the committee and potential issues of objectivity.” His worry focused primarily on Paustenbach and the use of his work as a first draft of a large section of the panel’s report.\(^\text{121}\)

Froines was correct in his assessment of Paustenbach’s influence. Proctor and five other authors, including Paustenbach, had recently submitted a chromium review paper—paid for by another Exponent chromium client—to the *Journal of Toxicology and Environmental Health* for publication. As of the time Paustenbach gave it to Last in late June it had not been accepted, having been sent back for revisions on June 27 and only formally accepted on July 30. This paper became the basis for at least one chapter of the report. According to an email of Last’s, the first draft of the

\(^{121}\) Gary Praglin Testimony, Senate Hearing, February 28, 2003, 11-12, 34-35 (See pages 13-15 for conflict of interest opinion of Paustenbach); and Chip Jacobs, “Dropping Science. Froines first complained about “bias” on the panel in an April 25th email to Alexeoff about the unquestioned acceptance of Paustenbach’s conflict. Shortly thereafter Froines indicated a desire to resign but Last talked him out of it. Portions of Proctor’s email were deleted before being provided to Praglin. Thus, there was likely additional panel information included in the email that the attorneys considered attorney work product. This conduit appears to have been well known to the committee chairman.
chapter was copied “pretty much verbatim from a review Dennis and his colleagues have in press, so we will want to do some revisions to eliminate the verbatim aspect.” Among the reasons Last gave to others for wanting to “eliminate the verbatim aspect” was that he did not “want to look like we’re merely rubberstamping Dennis’s conclusions. They come with baggage.” ¹²²

As the UC Davis Blue Ribbon panel began its open hearings in July, following Froines’ resignation, Paustenbach contacted Chairman Last, offering his resignation because of his “sense that plaintiff counsel (or others) would attempt to discredit the hard work of the panel.” To Paustenbach’s surprise, Last and the panel, apparently in an effort to minimize controversy, agreed.¹²³

Paustenbach’s resignation probably caused little consternation among the attorneys managing the effort. Paustenbach had already accomplished their major goal by preparing the initial draft of the most critical portion of the report and convincing Last to ask Proctor for the soon to be accepted paper that supported Paustenbach’s draft. The attorneys may even have suggested Paustenbach offer to resign in order to quell the potential of greater public focus on the panel. Counsel could now direct their focus on buttressing the viewpoint Paustenbach had included in the draft.

Presentations to the public hearing provided the perfect opportunity to provide testimony buttressing the opinion that the draft report represented mainstream science. In furtherance of this endeavor, litigation attorneys for at least two corporations with chromium water contamination problems became actively involved in monitoring and managing actions related to the panel, including surreptitiously preparing documents and possibly testimony for experts appearing as witnesses for the Alliance for Responsible Water Policy before the Blue Ribbon panel.

Three industry experts—Deborah Proctor, Phil Cole, and Sverre Langard—appeared before the panel calling for the “sound science” of Paustenbach’s draft to prevail. They argued that since there was no “proof” that hexavalent chromium causes cancer by ingestion, further regulation was unnecessary. Neither PG&E nor any other chromium utilizing company publically sponsored any of the presentations. Rather, the “independent” Alliance for Responsible Water Policy “paid” for the scientists to appear before the panel. In actuality, however, chromium defense attorneys and their agents managed and paid all of the witnesses.

The expert preparation process began early in the panel’s term. On May 26, 2001, Proctor wrote to PG&E’s attorneys informing them that with their approval she had contacted PG&E expert witness Phil Cole about funding for his panel presentation manuscript. She further indicated she would ask panel member Paustenbach to try persuading other members to fund Cole’s presentation. If the panel refused to fund his appearance, she would seek funding through Eric Newman. In the end, Newman’s Alliance did provide funding to Cole. Following his testimony before the panel, Cole received a $6,467 check from the Alliance for Responsible Water Policy.\textsuperscript{124}

How, or even if, attorneys directly prepared Cole for his testimony is unknown, but the experience of industry witness Sverre Langard offers one possible scenario for what might have happened. During his appearance at the panel, Langard maintained that he received payment for his time from the Alliance for Responsible Water Policy. However, he sent his bill of $11,500 for the testimony—without mention of the Alliance—to Gibson, Dunn & Crutcher, attorneys for Lockheed, a company with substantial chromium groundwater contamination problems.\textsuperscript{125}

Although the full extent of the attorneys’ preparation of Langard’s testimony is unknown, the few facts available demonstrate considerable involvement and influence. In one example, four days before Langard’s appearance before the panel Gibson, Dunn & Crutcher (Gibson Dunn) attorney Brett Oberst—who represented Lockheed—emailed him. The message in part informed Langard, “Deb Proctor, a scientist at Exponent whom you may know and who has been helping us stay informed regarding the panel meeting, is sending you by fax the missing pages from the analysis by Dr. Froines.” Oberst went on to notify Langard that Last was so interested in his talk that Langard could take an extra ten to fifteen minutes to present his material. The most likely source for these documents and information is Paustenbach, Proctor’s colleague at Exponent/ChemRisk.

Another senior Gibson Dunn attorney, Patrick Dennis, reviewed and edited Langard’s draft presentation with, in his words “minor” comments. The comments included a suggestion that Langard not include any reference to “the potential drinking water standard for chrome VI.” Dennis also provided a cover letter for Langard’s submission of the paper to the panel, which Langard adopted verbatim when he sent the paper to the panel. Langard also sent a cover letter to the panel with his testimony regarding his opinion that the “epidemiological literature does not support an association between Cr(VI) exposure by ingestion (or inhalation for that matter) and gastrointestinal cancer.” As with Langard’s testimony, a Dennis Patrick, Lockheed attorney, provided substantially all of the verbiage.126

As the final component of the attorney-managed strategy, Proctor made the final presentation at the hearing and provided the panel with another copy of her in-press paper. Following the hearing she sent a letter to clients who are unknown because their names have been

blackened out. Since she was under contract to attorneys, it appears likely they were one of, if not
the sole, recipients. In the missive she described how she had shared the general industry approach
with the panel. She then described the panel members’ reactions. “These members have expressed
great interest in our study and have indicated that this information is important to their deliberations
and recommendations to OEHHA. Therefore, the results of the proposed Chrome VI study will
potentially provide real benefits to the client and other industries facing regulatory actions on
Chrome (VI) in drinking water in California.”

OEHHA also maintained an interest in the committee’s activities. Both Jay Beaumont and
George Alexeeff of OEHHA also attended the expert panel hearing at Davis. As a result, Beaumont
learned of Attorney Praglin’s involvement in the litigation and briefly met him. Praglin was
interested in the backgrounds of the committee members and asked them if they had any conflicts.
Both McConnell and Professor Marc Schenker indicated no. Beaumont remembered being surprised
at hearing this, since he knew of Schenker’s consultancies. He later contacted Praglin, requesting
documents obtained through discovery be sent to OEHHA for review. Beaumont recalled receiving
more than one box of documents in response to the request. He also recalled having perhaps one
more phone call, but never provided any assistance to Praglin.

A little over a month later, the UC Davis panel’s report concluded the state did not need a
new drinking water standard. It based its findings upon two primary factors. First, it conducted
critical analysis of the only available rodent study concluding it was not persuasive because many of
the rodents had succumbed early to a virus. Second, it considered other studies; particularly the 1997
Zhang article in deciding there was no proof of cancer. Overall the tone of the final report
contrasted sharply with the California state requirement that the OEHHA take a “health protective

“approach” when considering a PHG, seeming rather to contend that nothing should be done unless
a health problem is fully proven.

Portions of the final version relied heavily upon the review paper provided by Proctor, yet
the report did not include the paper in its references. In a July 2, 2001 email to the panel, Last
informed them that much of the initial draft came from the Proctor article. In the email he noted
that he had “copied the third chapter pretty much verbatim from a review Dennis [Paustenbach] and
his colleagues have in press, so we will want to do some revisions to eliminate the verbatim aspect.”
Marc Schenker, Distinguished Professor of Medicine at the University of California at Davis, wrote
chapter four. It followed the arguments and opinions of the Proctor paper very closely. For
example, Schenker’s description of the original Zhang article reads like a defense witness testimony
at trial, with every possible bias and data gap being catalogued. He concludes that the “association”
of chromium in water to Zhang’s findings was difficult to interpret in light of the problems with the
paper. Following this devastating attack on the 1987 article, Schenker then reviewed the Zhang 1997
paper without a single comment about any shortcomings. This paper, of course—substantially
written by ChemRisk scientists—supported the view taken by the Committee, and, as will be seen
later, contained numerous biases and misstatements. Other sections, as noted in the footnote,
included verbatim passages from Proctor’s article without attribution. Interestingly, Schenker had
previously consulted for the chromium industry, including PG&E, and been listed as an expert
witness for trial. He also still had a box of documents from PG&E in his possession. In many
universities, the direct quotes taken from Proctor’s paper without attribution, if written by a student,
likely would have made the paper subject to investigation for plagiarism. Two unanswered questions
thus arise. First, why would they not provide a citation to the article? Second: were they trying to hide where the report language and opinions came from?120

At a California Senate hearing, Last explained the omission as inconsequential. His explanation raised questions concerning plagiarism standards in the University of California system:

SENATOR ORTIZ: Can I ask why there was no reference or no citing that it was Dr. Paustenbach’s work anywhere in the final report?

120 Deborah Ortiz and Jerold Last discussion, Senate Hearing, April 2003, 20-21; and Gary Praglin Testimony, Senate Hearing, February 2003, 15-22. According to Praglin, Schenker had not disclosed his relationship with PG&E until the close of the Committee public Hearing, when Praglin asked him about it. The following are but two examples of the report’s heavy reliance upon the Proctor et al. paper. The narrative in the panel report indicated by italicized font is identical to the Proctor article language that immediately follows, while the underlined report section is identical to the Proctor paper, except for the intimation that no specific type of cancer was increased, whereas the Proctor article only states cancers as a whole were not increased.

UC Davis Blue Ribbon panel language

Proctor paper language
“In the town of Lecheria, in southern Mexico, approximately 3,000 residents were exposed to Cr(VI) in soil, drinking water, and air due to emissions from a chromite production plant (Rosas et al., 1989; Neri et al., 1982). The plant had been established in 1958 and used “old technology” until 1973. It was closed in 1978 because of public health concerns. Gross environmental contamination was suggested by the solid waste from the plant being used as street fill, and wet solid waste being left in open areas next to the plant. The concentrations of total chromium in groundwater used as drinking water was 0.9 ppm (900 ppb), and the concentrations in air were 0.25–0.39 μg/m³ (Rosas et al., 1989). Concentrations of chromium in urine and hair in the exposed community were significantly elevated as compared to a control population (Rosas et al., 1989). There was no increase in the number of deaths due to cancer among the exposed population (18/947 deaths = 1.9%), as compared to the control population (39/1972 = 2.0%) over the 24 year period studied (Neri et al., 1982).” Deborah M. Proctor, et al., “Is Hexavalent Chromium Carcinogenic via Ingestion? A Weight-of-Evidence Review, Journal of Toxicology and Environmental Health, Part A: Current Issues 65:10, (2002): 710.

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PROFESSOR LAST: This was a material that was published in the literature. It was from a published paper. There’s a limited number of ways you can say Smith and Jones fed rats 2 parts per billion of chromium, and we had been given permission by Dr. Paustenbach to use this material in any way we saw fit. I saw no problem with that.\textsuperscript{130}

Yet, the industry and attorney sponsored subterfuge went even further. One of the primary papers relied upon by Proctor, et al.—and therefore the panel—to support the position that there is insufficient proof to declare Cr(VI) genotoxic when ingested, was written by an expert witness for PG&E, Italian scientist Silvio DeFlora. In reaching their conclusion, the panel directly quoted DeFlora’s opinion that “oral chromium is not genotoxic at doses which greatly exceed the drinking water standards.” Since at least some of the panel members were unaware of De Flora’s service as an expert witness for PG&E attorney during the period of the subject paper’s preparation, the panel did not have the opportunity to consider whether his opinions were biased and conduct further research of their own.\textsuperscript{131}

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ChemRisk scientists rejoiced following the report’s release. Paustenbach likely believed the Zhang 1997 article played a major role in the panel’s decision-making process. He quickly emailed the report to Brent Kerger, his former colleague and one of the scientists primarily responsible for the Zhang 1997 article, with a comment: “Buy a good bottle of wine, pull up a chair . . . and then read this. Then say to yourself ‘Yep, I really finally did something good for society.’”\textsuperscript{132}

The release of the report in August acted as a seismic event in both the regulatory and legal arenas. In November 2001 the OEHHA withdrew its recommendation of a PHG of 2.5 parts per billion of total chromium in water, citing to the panel’s report. In the legal forum Latham & Watkins wasted little time utilizing the report. They spread the word in the legal and judicial community via a

\textsuperscript{130} Deborah Ortiz and Jerold Last discussion, Senate Hearing, April 2003, 20-21.
\textsuperscript{131} David Egilman, 173.
\textsuperscript{132} Peter Waldman, “Study Tied Chromium-6.”
quickly written article by a Latham & Watkins attorney. In a more critical—and ultimately catastrophic—short term effort, they submitted it to the judge in a pending Cr(VI) case, requesting to have the case dismissed.

The article, prepared by Latham & Watkins attorney Daniel L. Martens, appeared in the 2002 spring issue of the American Bar Association’s journal *Natural Resources and the Environment*. It touted, even flaunted, the findings of the California Blue Ribbon panel. Martin emphasized that after examining both animal studies and several epidemiological studies the panel found “no basis” for finding that orally ingested hexavalent chromium is a carcinogen. Martens did not mention Latham & Watkins’ close involvement in activities directed at influencing the panel and the report. Nor did he mention their intimate association with several of the negative studies. He concluded the article by suggesting that the Committee’s report will “leave nothing but sand on which a plaintiff’s expert may attempt to “build his house” in future chromium cases.\(^{133}\)

Yet even as Latham & Watkins gloated in the moment of seeming victory, PG&E’s world began to crumble. During the summer of 2001, Jay Beaumont, a lanky, introspective OEHHA toxicologist, began investigating the actual facts surrounding ChemRisk’s involvement in the 1997 Zhang and Li paper. Upon first meeting Beaumont, I could immediately sense his strong commitment to his office’s mission of protecting the public health. In discussing public health science, he was passionate about the necessity for accuracy and attention to detail in scientific endeavors, while also displaying a firm understanding of the uncertainties inherent in public health science. He believed that protecting the public health required hard decisions based on scientific evidence that is still young and fraught with uncertainties.\(^{134}\)

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\(^{133}\) Daniel L. Martens, “Chromium, Cancer and Causation: Has a Death-Blow been Dealt to Chromium Cases in California?” *Natural Resources and Environment* 16 (Spring, 2002): 264-266.

\(^{134}\) Author interview with Jay Beaumont, in Sacramento, California, August 28, 2013. Beaumont’s scientific articles do not suggest, as some defense attorneys might argue is the case with all government regulators, that he is anti-industry, and slants his opinions toward requiring regulation. For example, see E. M. Bell, I. Hertz-Picciotto, and J. J. Beaumont,
Beaumont’s investigation into the Zhang 1997 paper began as a result of his review of literature for the California PHG. In reading various articles he became concerned about one of the most important papers being used by the panel, Zhang’s 1997 reanalysis. To Beaumont, something just seemed odd about the paper. In August 2001, subsequent to his request, OEHHA’s Deputy Director assigned Beaumont to look further into the 1987 and 1997 articles.

A resultant detailed examination of the Zhang 1997 paper revealed several problems, both in its statistical analysis and in its authors’ connection to ChemRisk. In September, Beaumont duplicated some of ChemRisk’s analysis and calculated a stomach cancer rate of 1.87, a figure he described to other scientists as at odds with the paper’s characterization. In mid September he emailed his superiors that Zhang had a web page citing to his consultation work with ChemRisk’s parent corporation.

Following the Internet trail, he learned that ChemRisk’s clients included numerous hexavalent chemical corporations, among them Elementis Chromium, PPG Industries, American Chrome and Chemicals, and Chemical Land Holdings. He likely did not discover ChemRisk/Exponent’s connection to PG&E, because PG&E’s attorneys had hired them as confidential consultants. Although somewhat unsure of the financial connection, by now Beaumont had strong suspicions about ChemRisk’s involvement in the 1997 paper.

In early October, Beaumont sent OEHHA Director Joan Denton a memo further elaborating on his statistical findings, inaccuracies in the paper, and ChemRisk’s CEO’s (Dennis Paustenbach) participation on the Blue Ribbon Panel. As one example of the inaccuracies, 

Beaumont calculated that in the overall area of the study, the odds of dying from stomach cancer were 69% higher than the province as a whole and 81% higher than immediately adjacent uncontaminated areas. He also noted the authors’ misuse of published data when they claimed they could not compare the study area’s stomach cancer rates to the surrounding province because the information was not available. In contrast to their claims, Beaumont found that the province rates had been published in the same source that the authors used for their rates. Interestingly, if province rates were substituted for the ones used, the stomach cancer incidence proportion rose to a statistically significant 1.8. Following these criticisms, Beaumont concluded the memo with the observation that the 1997 article did not acknowledge any funding, but was probably paid for by “U.S. companies that have a strong financial interest in the health-effects evidence for Cr6.”

Even as Beaumont was unraveling the inaccuracies and litigation connections of the 1997 paper, Latham & Watkins attorneys’ efforts to use the UC Davis Blue Ribbon panel report as support for a motion to dismiss the Aguayo case similarly hit a roadblock. Unaware that Beaumont had already discovered the ChemRisk connection, Latham and Watkins attorneys told the judge that, in light of the panel’s report, everything had changed. They asked permission to amend their motions to dismiss the case. In the fall, the judge allowed them to amend their pleading and

135 Jay Beaumont email to George Alexeeff, August 7, 2001; Jay Beaumont email to Richard Stedman, et al., “Stomach Cancer Excess in the Zhang and Li study,” September 10, 2001; Jay Beaumont email to George Alexeeff, et al., “Zhang Paid by Cr Industry,” September 14, 2001; Jay Beaumont email to Joan Denton, et al., “Cr6, Dr. Zhang, and Panel Conflict,” October 5, 2001; personal communication with Jay Beaumont. (emails in author’s possession); and Peter Waldman, “Publication to Retract an Influential.” Prior to this time Jay Beaumont had coauthored an article that did not find significant risks from chromium in tannery workers. The concluding paragraph of the article provides an interesting contrast in scientific philosophy from both ChemRisk articles and the Blue Ribbon report. “In summary, although this study revealed some elevated risks for certain causes of death among tannery workers, no significantly increased risks were noted for any cause of death thought a priori to be occupationally related. Several limitations of this study, however, are discussed which may have accounted for our negative findings. Considering the limitations of this study, it would be improper to conclude that employment in the leather, tanning, and finishing industry presents no occupational health risks.” F. B. Stern, et al., “Mortality of Chrome Leather Workers and Chemical Exposures in Tanneries,” Scandinavian Work and Environmental Health, 13, no. 2 (1987): 529-531, 531. This distinction is observed in almost all attorney-funded research when compared to public health oriented research. The philosophical underpinnings for the two approaches is further explored in the discussion of Doctor Froines testimony at the California Senate Hearing held on October 24, 2000.
motions, stating he would consider dismissing the case—but, in a move which would prove fatal to PG&E, first allowed the plaintiff attorney, Gary Praglin, to conduct discovery. Over the next several months, Praglin took two dozen depositions and obtained hundreds, if not thousands, of documents, detailing the role of attorneys, ChemRisk, and other chromium company agents in the preparation of the 1997 Zhang paper, as well as the effort to surreptitiously control the Blue Ribbon Panel through its industry controlled or influenced member and the attorney sponsored evidence presented to it.136

At an unknown point during the discovery process, Erin Brockovich—either alone or with the agreement of Praglin—began lobbying California State Senator Deborah Ortiz to hold new public hearings about the information uncovered by Praglin’s depositions and document requests. Although Praglin denied (and Ortiz confirmed) his personal lobbying to make a presentation to a California legislative hearing, they must have both known that such publicity might increase pressure on PG&E to settle the now several-year-old Aguayo case without additional proof that chromium ingestion causes cancer. Ortiz has acknowledged that Brockovich provided the main impetus to the new hearing, which focused on “possible interference in the Scientific Review of Chromium VI Toxicity.” For her part, Brockovich has admitted that she coaxed Ortiz and Praglin to investigate the panel. Following Brockovich’s fervent lobbying, Ortiz called Praglin and requested he make a presentation.137

136 Chip Jacobs, “Dropping Science”; and Rebecca Renner, “Allegations of Manipulation Lead to Withdrawal of Cr(VI) Report,” *Environmental Science & Technology* (August 1, 2003): 276-277 (Paustenbach’s comments). When Kerger received the subpoena notice he called Kirk Wilkinson (attorney at Latham & Watkins) “because he was the project leader, the contact that I had worked with previously on that case.” Kerger is not sure who is paying for his attorney at the deposition but knows he is not intending to do so, and Wilkinson recommended he call McLeod, who is representing him. Deposition of Brent Kerger, 143-4 and 147-8. The Ye and Kerger depositions were both taken by Praglin. In addition, most of the emails and documents cited in this paper were first brought to light through Praglin’s depositions and documents subpoenas.

137 Chip Jacobs, “Dropping Science”; Personal communications with Deborah Ortiz, August 27, 2013; and Gary Praglin telephone communication, September 5, 2013.
On February 28, 2003, Ortiz opened the hearing at her blunt, fiery best. She questioned how state entities could have allowed the apparent subversion of the chromium report. Her opening comments unambiguously set the tone of the hearing:

Since I’ve been in elected office, few things have disturbed me as much as the allegations I received concerning the report issued in August of 2001 by the Chromate Toxicity Review Committee. I did not write the law to set safe drinking water standards for chromium VI just to see it undercut by underhanded methods. You can’t clean up tainted water with a tainted report. I would say to industry: You asked for balance, but you have to understand that conflicts of interest and hidden agendas throw any legitimate process out of balance. I would say to UC and Cal/EPA: Californians shouldn’t have to be afraid at the tap because someone was asleep at the switch. And I want to make clear to everyone that with this hearing, the public interest is getting put back into the public process.

What followed only increased the already high temperature of the committee proceedings and its audience’s growing indignation. Praglin and Brockovich took the offensive, with Froines in support. California officials, also indignant due to the accusations directed at them, claimed no California laws had been violated, while not denying many of the allegations. Although Ortiz invited all of the figures central to the controversy to the initial hearing, neither anyone representing PG&E nor any of the panel members, except Froines, appeared.138

As the first witness, Erin Brockovich opened the hearing with a scathing condemnation of a panel that was “corrupt, skewed, and biased for self interest.” She then introduced the star of the show, attorney Gary Praglin.

Praglin presented what many observers considered a shocking and harrowing Power Point presentation at the Senate. Though a long discussion of documents obtained during discovery, Praglin claimed that 1) PG&E sought to stack the Blue Ribbon panel with one current and one former consultant, 2) sought to render Doctor Froines, the one cautious scientist on the panel, ineffective, 3) surreptitiously choreographed presentations by other consultants, 4) copied portions

138 Senate Hearing, February 28, 2003, 1, 4, and 5.
of the report from a biased industry-sponsored paper, and 5) sought to have a paper that ChemRisk employees ghostwrote form a primary base for the decision that ingesting chromium does not cause cancer. He conducted a particularly virulent attack upon two ChemRisk scientists, Dennis Paustenbach and Deborah Proctor. Praglin contended that even while serving as Blue Ribbon panel member, Paustenbach had orchestrated ChemRisk’s actions, with Proctor serving as the go-between for Paustenbach and corporate attorneys and their agents.139

The next witness—Professor John Froines of the UCLA School of Public Health—purposely kept his comments away from the controversy surrounding the Blue Ribbon panel’s composition, focusing instead on what he saw as shortcomings in the report itself. He argued that the panel should have taken more time preparing the report. The short time frame resulted in insufficient evaluation of the toxicological evidence. In addition, the “report was much too definitive; and . . . was much too conclusory.”

Froines then set out how he believed the panel should have approached the issues. In effect Froines contended that the panel had approached the problem from a direction opposite from what California law required. Froines agreed that there is uncertainty in the evidence of stomach carcinogenicity; however, OEHHA had a duty to take a health protect approach, especially to known carcinogens. As a first step the panel should have recognized that Cr(VI) is a known carcinogen, thus beginning their deliberations “with an index of suspicion.” In fact, Cr(VI) is the second most potent carcinogen ever reviewed by the California Scientific Review Panel. Thus, rather than considering whether or not there is proof about chromium’s carcinogenetic potential for cancer upon ingestion, Froines believed it should have considered whether there is compelling evidence

139 Gary Praglin Testimony, Senate Hearing, February 28, 2003, 7-49.
that it isn’t. Since the panel did not “take enough of a public health perspective,” the report should not be used as a basis for regulatory action.\textsuperscript{140}

Following Froines, several other witnesses testified, most of them arguing against the report and lamenting the limited notice to the public. None of the other panel members or PG&E representatives spoke at this, the second hearing.

Although the hearing appears to have resulted in limited press coverage, University of California at Davis officials, as well as panel members Last, Schenker, and Paustenbach felt compelled to respond to the allegations at a third hearing held in April.\textsuperscript{141} At that hearing, Professor Lawrence Coleman, the Vice Provost for Research, told the Senate Committee that the University stood behind the report’s validity. They did, however, believe that they could have better handled the selection of panelists. Future panel protocols should have a written conflict-of-interest disclosure policy. In reply, Ortiz countered that the University already had a policy requiring faculty members not to participate in decisions if they have received $500 over past year from an interested party. The University’s attorney replied that this policy was not relevant, since the panel only made recommendations, not decisions.

Professor Last then defended his actions while serving as chairman of the panel, stating he felt the plaintiff attorney had tried to intimidate him during the deposition and did not present Last’s deposition testimony about the selection process accurately to the Senate Committee. Last did not provide a specific example of the inaccuracies, but averred that prior to selecting each panel member he asked each person orally about his conflict-of-interest, but now agreed it should have been in

\textsuperscript{140} Froines Testimony, Senate Hearing, February 28, 2003, 54-9.
writing. With regard to Paustenbach, Last stated, “I recall asking Dr. Paustenbach whether he felt he could give objective interpretations to the panel that would not be in any way influenced by his prior history, and he said, ‘Yes.’” Although a far cry from true avoidance of conflicts of interest, this was apparently sufficient for Last. From his testimony, it is not clear if Last understood that Paustenbach still served as a confidential consultant to PG&E's attorneys at the time of the panel.

Throughout his testimony, Last refused to concede any weaknesses in the report. He claimed that only the first draft involved cutting and pasting, an activity they did with several articles. Furthermore, the information Paustenbach provided was a “very small part of the report.” He claimed the last draft only had a minute percentage still copied. He also oddly dismissed the importance of the three industry witnesses, stating their input did not have any influence on the panel because the presentations were not peer reviewed. He did not state why the committee held hearings if it had no intention of considering such input. Even though he now knew about the ghostwriting of Zhang’s new article by Paustenbach’s firm, Last did not mention it, still claiming the panel used the “best evidence.” Finally, he saw no problems with not citing Proctor’s article: “we had been given permission by Dr. Paustenbach to use this material in any way we saw fit. I saw no problem with that.” He was not asked if he would have accepted this behavior in one of his students’ papers. In his conclusion, Last summed up his opinions about the report in one succinct, brusque, unyielding statement:

I am proud of this report and stand fully behind it. It is scientifically sound, it draws completely appropriate conclusions, and it provides an important peer review of an inappropriate and poorly reasoned public health guideline prepared by OEHHA. It is important to protect the health of the citizens of California with public health guidelines based on the best available science, which our report provides, rather than on a single outdated and almost universally discredited study.142

142 Jerold Last Testimony, Senate Hearing, April 2, 2003, 15-22, “I recall asking” at 18, “we had been given” at 21, and “I am proud” at 17. An email form Froines during the process of report writing contradicts Last’s characterization of Paustenbach’s influence. It suggests that Paustenbach appeared to be the principal author of three chapters, along with portions of fourth chapter. Gary Praglin Testimony, Senate Hearing, 15-22.
Professor Marc Schenker spoke next, expressing frustration at the suggestion he had not been completely impartial in his activities related to the panel: “Unfortunately, there have been many distortions, innuendoes, falsehoods in this, and I would like to clarify that record in my comments in response to any questions.” Apparently, like Paustenbach he had informed Last of his previous work for PG&E, which Last presumably did not think constituted a conflict of interest. Schenker told the Committee that while he had taken on legal work from PG&E, it had not affected his opinions and payment for his work went to the school, not him. He seemed to suggest that this meant there could be no conflict-of-interest concerning his prior activities. Later in the testimony, this stance became somewhat discredited when he was forced to admit the money went to his department for use as clinical income and he still had PG&E files in his office as claimed by Praglin. Nor did he describe how his department weighted such income when deciding upon advancements, duties, and research funding.

Schenker then defended the panel’s findings as mainstream science, repeatedly mentioning the EPA and the World Health Organization. To him, the science should be the focus of any investigation, not the process. Yet, this conclusion concerning “mainstream science” and “process” ignored California’s requirement of prioritizing the protection of public health, instead focusing on what can be absolutely proven about toxic substances. Thus, he shed no light on how he believed prioritizing public health protection should affect regulations and scientific certainty.

Ortiz did not appreciate Schenker’s quibbling. As she informed him: “But in something as compelling as public health in the State of California, to suggest that it’s not significant to ask the question whether the process is one that we can trust the science is, I think, a little bit naïve, and it ignores the obligation we have as public officials.”

143 Marc Schenker Testimony, Senate Hearing, April 2, 2003, 22-30, “Unfortunately” at 22 and “But in something” at 28.
Following the two panel members’ defense of their action Eric Behrens, counsel for the University of California, attempted to shift the focus of the hearings to complaints about Praglin’s presentation. He gave the Committee members a handout that alleged “how amazingly misleading Mr. Praglin’s presentation was,” but did not discuss any of the misleading statements in his presentation. He castigated Praglin for suggesting that Last attempted to cover up anything. In Behrens opinion Last was obviously not trying to hide how he used the ChemRisk article, simply because he had testified that he was guiltless of the charge. In making this argument, however, Behrens ignored the problem of plagiarism in the final product, instead contending that even though Praglin knew Last had denied using the article improperly, “he [Praglin] presented to the Senate committee information that completely distorted what he knew were the facts, and I find that extremely disturbing.”

Behrens then moved on to Schenker, contending the scientist’s statement and actions were completely innocent. Behrens rightly pointed out that Praglin had left out the last part of a sentence when he quoted Schenker as stating to Last, “‘my work on the panel would be, in my opinion, the same as my work for PG&E,’ The rest of the quote, which he left out, continues: ‘That is, to provide my expert independent scientific opinion to the body.’” Nor had Praglin been completely truthful about Schenker’s work for PG&E when he implied it still continued while Schenker served on the panel.

Mr. Praglin knew by invoices that Dr. Schenker had not billed any time during the period when he was on the panel and, in fact, that he had stopped working for PG&E before he was asked to be on the panel. The evidence was clear; yet, what does he do in his presentation? He tries to play games with the Federal Express and when those materials were returned.

Behrens made some good points about Praglin’s selective use of evidence—as is done by all good attorneys—yet in his display of smoke and mirrors, Behrens failed to defend Schenker’s

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144 Eric Behrens Testimony, Senate Hearing, April 2, 2003, 31.
primary conflict-of-interest problem: he had failed to disclose his recent work for an interested party and still retained documents given to him by that party. Behrens dealt with this potential problem of conflict of interest by informing the Committee that the University’s written conflict-of-interest requirements did not apply to the panel. Since the panel was advisory, the code for conflict-of-interest did not apply and, thus, Schenker had not broken any university policy. Shortly after Behren’s statement, both Last and Schenker similarly downplayed the significance of the university’s conflict-of-interest rule, claiming it did not apply to the panel—again ignoring the appearance of conflict in their actions.145

Winston H. Hickox, Secretary of the California Environmental Protection Agency, was not so sanguine about conflict of interest when he testified to the Committee after the University’s presentation.

First, let me state, I concur with the conclusion that the University of California’s procedural process by which the panel was selected and the panel accomplished its work may be viewed as having been flawed. Therefore, the report produced by this process may not be able to be relied upon with a high degree of confidence. I appreciate, Senator Ortiz, that the committee has brought to our attention these issues, and given the serious allegations, I’m prepared to do all of the following.
I have recommended to Dr. Denton, the director of OEHHA, that the conclusions of the chromate panel not be relied upon for the purpose of arriving at a PHG for chrome VI. I am withdrawing the original task order and will draw up another to ensure that no members of the original panel provide peer review for the new chrome VI PHG being developed by OEHHA.146

Following Hickox’s presentation, OEHHA Director Denton explained the agency’s reasoning for regulating ingestion of chromium. She recognized that the Blue Ribbon panel-castigated Borneff study was not robust, yet it was the only study that examined the potential oral

146 Winston H. Hickox Testimony, Senate Hearing April 2, 2003, 42.
carcinogenicity of a known carcinogen. In her testimony, Director Denton once again demonstrated the opposite approaches to the issue taken by the agency (as required by law) and the panel:

From a public health perspective, we have always taken the approach that if a chemical is a carcinogen by one route, unless we have evidence that it’s not carcinogenic by another route, then we assume that it’s carcinogenic by other routes as well; and especially for chrome VI, which is a very potent inhalation carcinogen. That was a public health protective approach.\textsuperscript{147}

William Vance next explained his activities in the committee, providing an overview of Cal/EPA’s process for recommending individuals names to Last for the panel. All but two of the panel members came from Cal/EPA’s list of 36 people. Vance had nominated Paustenbach based on his publications—almost all written while he was a consultant to industry attorneys. Vance’s list also included other three other ChemRisk scientists—Kerger, Finley and Proctor—recommended to him by industry consultant Newman. Environmental groups—most likely paying little attention to the issue and with little notice of the panel’s formation—did not suggest any names. In his testimony, Vance did not indicate that he ever reached out to achieve a balance to his list. Rather, he freely provided information solely to industry, without consideration of the limited publicity surrounding the panel. After being contacted by Newman, Vance sent him a detailed email concerning the panel, advice on how to approach them, and when the public meeting might take place. Vance later declared he would have given the same information to environmental groups had they approached him.

Thus, Vance apparently neither cared about whether environmental groups might be less well informed about the panel than the chromium industry, nor that he was acting as an advisor to industry on how best to proceed. He further maintained that his disclosure of the public meeting to Newman prior to its official announcement was not a problem, since DHS posted it on their website on the same day. However, he did not indicate whether, at the time of his disclosure, he had known

\textsuperscript{147} Joan Denton (director of OEHHA) Testimony, Senate Hearing April 2, 2003, 47.
they would be publishing it earlier than originally planned. Nor did he say why specific notice to one side was necessary if it was being posted anyway. Interestingly, he notified Last that the Exponent members and occasional plaintiff expert, Professor Max Costa, might have conflicts because they worked on the PG&E trials, but appears not to have mentioned Paustenbach and ChemRisk's conflict. Yet, throughout his testimony, Vance stressed that he was not attempting to slant the panel toward industry. However, Vance’s full contacts with industry or their attorneys may never be known, for as Praglin testified he wrote in an email to Last, “I don’t even leave paper trails on this.”

ChemRisk President, Dennis Paustenbach closed the testimony. His testimony demonstrated why he is such a respected expert witness, providing succinct carefully considered facts and opinions while speaking forcefully in defense of his actions. Paustenbach defended himself on three issues. First, he contended that he left the panel “before the text was written,” adding “There is not a single word that I wrote in those chapters.” He further maintained that only 4% of the report was “borrowed from a published paper by my colleague, Dr. Proctor.”

That same text, or very close to it, appears in several international body reports. So, it is little more. It is not personalized text that is offering opinions from Ms. Proctor or anyone else. It’s generally a summary of that which appears in the published literature. So, I would ask everyone also to quit referring to that as being borrowed text written by Exponent or Paustenbach, because it’s not true.

While technically accurate, this argument ignored the crux of the problem. The article formed the basis of the first draft. As admitted by Last, the panel modified the language to a moderate extent so it did not appear to be simply lifted from the article. Yet, in virtually any college in the nation, the result remains plagiarism.

149 Dennis Paustenbach Testimony, Senate Hearing, April 2, 2003, 55-6.
Paustenbach next covered the issue of conflict-of-interest. Unlike Last and Schenker, he noted the importance of conflict-of-interest. Incredibly, although boasting of being an editor for a journal with the “strictest policy with respect to disclosure and conflict of interest,” he apparently did not have a problem with the appearance of impropriety, claiming he could be impartial even while his employees still worked as confidential consultants for PG&E’s attorneys.

I also believe the conflict-of-interest issue is of critical importance. As Mr. Praglin learned in the depositions that he took of me, I’ve considered this an extremely important issue in the scientific community over the last ten years. As he’s aware, and I think you’re aware of, the journal, of which I’m an editor, may, in fact, have the strictest policy with respect to disclosure and conflict of interest.\(^{150}\)

In a final display of hutzpah, he disputed the contention that PG&E consultants ghost-wrote the Zhang article. In a claim that is further considered below, Paustenbach stated: “That is false,” he declared. “There was and is ample correspondence between the English and the Chinese.” He further declared they had not paid Zhang to write the article. “We paid him a sum of money with which to interact.” Paustenbach then related how that interaction resulted in Zhang acknowledging what had been a growing misunderstanding of his prior misstatement.

Dr. Zhang tells us that in that ten-year period, he had developed five or more reports in Chinese, in China, never released, reevaluating that information. After he saw the questions that we raised about the analysis, he went back and examined and said, “Of course not, it can’t be true. My original conclusions don’t make sense. The further away you get from the plant, the cancer rate’s going up. It makes no sense. It’s against the first principles of toxicology. (Emphasis added)\(^{151}\)

Inconveniently for Paustenbach, none of the words he ascribed to Zhang appear in any of the relevant documents. Nor was this his last assertion contrary to the written evidence, as Paustenbach continued by contending that ChemRisk scientists had known they were ethically required to list themselves as authors, yet acceded to Zhang’s demands:

\(^{150}\) Dennis Paustenbach Testimony, Senate Hearing, 55.
\(^{151}\) Dennis Paustenbach Testimony, Senate Hearing, 56-7.
We asked Dr. Zhang, in fact, to be coauthors on that paper for sake of transparency. Mr. Praglin, I believe, has probably shared with you the four or five drafts wherein we ask—and, in fact, suggested in ordering of authorship—wherein Zhang and my colleagues were listed. Dr. Zhang, on his own decision, chose to keep that as a singular authorship. That we can’t change. We also asked for attribution. We cannot change his decision not to attribute. There’s many reasons that could have happened. That was ten years ago in a Communist country. I can understand why he would not have been . . . why he may have been concerned.¹⁵²

These statements of Paustenbach are difficult to accept. The documents contain not one hint of ChemRisk scientists asking to be included as authors. Indeed, ChemRisk authors did everything possible to hide their participation from the journals. In addition, Paustenbach was well aware that China was at the time very open to collaboration; his company even had an affiliate office in Shanghai. To suggest that the Chinese author was afraid of what his government would do when he retracted his former complaints about its toxic pollution activities, borders on the absurd.

In response to Ortiz asking Paustenbach if it ever occurred to him to disclose ChemRisk’s involvement in the Zhang study while he was on the panel, Paustenbach went even further, either lying or at least misrepresenting ChemRisk’s involvement. As he stated:

We didn’t pay for that study . . . Second, with respect to attribution, I wasn’t involved in writing the final panel report at all. I can find no text in the final panel report—let me repeat that—no text in the final panel report that I wrote, other than that which was borrowed from the published literature. None.¹⁵³

Left unsaid was the intimate involvement of his employees with Zhang, his oversight of the entire process, and the panel’s reliance and plagiarism of Proctor’s as yet unpublished article.

At the conclusion of his prepared remarks, Paustenbach and Ortiz, in fighting parlance, “mixed it up.” Responding to Paustenbach’s testimony, Ortiz again mentioned the cutting and pasting. Paustenbach interrupted her with an outburst of righteous indignation:

¹⁵² Dennis Paustenbach Testimony, Senate Hearing, 58.
¹⁵³ Ibid.
That’s a horrible characterization, Senator. It’s absolutely unfair and unfounded. Totally unfair and unfounded. Two percent, as Dr. Last presented—or 4 percent, as I just testified—of the report is borrowed from a published paper. That’s it. It is not fair to characterize the remainder of the work the way it is.\textsuperscript{154}

Ortiz was having none of it. She let him know that she recognized the misleading nature of his testimony.

I’m sorry you feel it is unfair, but the majority of the conclusion of this committee’s work to conclude that chrome VI is not carcinogenic is comprised of the Proctor study. It may be 4 percent of the total footnotes, etc., but it’s a good part of the conclusion that chrome VI is not a problem.

Ortiz also identified the critical ethical issue at stake in the panel’s process of preparing its report.

But let me also say. . . that even if it is the University’s position that this was not a decision-making body, there are a couple of things that raise the issue that cause concern that it operated as such. By defense counsel, when they walked this report across the country and various courts across the country to say, \textit{See, California has determined this is not carcinogenic, therefore}, they took it to be a decision-making body.\textsuperscript{155}

Thus, after three hearings, the California Senate’s investigation into the UC Davis Blue Ribbon panel ended. Throughout the hearings, the industry and its agents took a severe battering. At its conclusion, the Blue Ribbon panel’s credibility lay in tatters.

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With these hearings, industry’s carefully constructed science absolving Cr(VI) from stomach cancer began to crumble. Even before Ortiz’s confrontation with Paustenbach, thanks to Beaumont’s research, OEHHA began conducting a careful reanalysis of the 1997 Zhang paper. The additional information contained in documents obtained from Praglin and online research, enabled the state scientists to discover found numerous problems with the paper. Ultimately, after reviewing

\textsuperscript{154} Dennis Paustenbach Testimony, Senate Hearing, 60.

\textsuperscript{155} Deborah Ortiz Statement, Senate Hearing, April 2, 2003, “I’m sorry” at 60 and “But let me” at 34-5.
the articles, translations of Zhang’s earlier works, earlier drafts of the 1997 article, court depositions, internal documents obtained in litigation, and Chinese online statistical data, OEHHA concluded that ChemRisk had violated ethical or scientific principles in at least thirteen ways. Falling into categories such as "Misrepresentation of data and study design," "Professional standards," and "Epidemiologic design," OEHHA's March 2003 list of violations includes the following major criticisms:

1) Failing to disclose the manuscript’s authors;
2) Failing to disclose PG&E’s funding of the study;
3) Simultaneous submission to two journals, including signing form(s) stating that the manuscript has not been submitted elsewhere;
4) Falsely stated in the published paper that the site-specific cancer rates weren't available for the province;
5) Failing to disclose key facts about the data presented, including possibly failing to disclose findings of excess stomach of lung cancer risk;
6) Basing their analysis on the level of contamination detected in the wells in 1965, knowing that by the end of that year the picture of contamination in the wells had dramatically changed;
7) Ignoring useful data that was available, e.g. stomach cancer rates for Liaoning province and Tanghezi village;
8) Misrepresenting the study design in several ways to make it seem stronger including misrepresenting the epidemiologic design as a higher quality cohort study by describing "follow-up" of the populations and calling it a "retrospective mortality study;" and
9) Reaching negative conclusions when the latency since first exposure was relatively short for a study of human cancer.\textsuperscript{156}

Overall, the OEHHA review delivered a devastating critique on the 1997 paper, yet it did not include all of the paper’s shortcomings. As disclosed in Brent Kerger’s trial deposition, given in the Aguayo trial as a result of the judge allowing Praglin to conduct discovery, the 1997 paper had at least three other major faults. First, it left out several high hexavalent chromium concentrations included in Zhang’s first study, published in 1987. Second, one of the 1997 graphics reporting hexavalent concentrations wrongly attributed high concentrations to wells in another, less contaminated village.\textsuperscript{157} Finally, some of the contamination levels utilized by ChemRisk were substantially different than those contained in the original article. At his deposition by Praglin, Kerger explained that he understood “they don’t correspond and it’s because it -- they’re based on different data sets.” According to Kerger, the new data was supposed to be a more complete set. Yet, he apparently did not have the new data, although he suggested that someone else might: “It was generated and analyzed specifically to create this paper and I don’t have it.”\textsuperscript{158}

When Praglin questioned Kerger about the missing data during the deposition, he simply maintained that the data did not matter. “Well, these are articles that summarize the findings of an author, and the fact that you don’t have a data set doesn’t really matter. If you have a – a scientific author that’s representing that this is the data, that – you have to take it on face value that there’s a database behind it.” The data “may not include all of the data that was initially referenced in this (the

\textsuperscript{156} Internal OEHHA memo, March 28, 2003, “Scientific issues regarding Zhang and Li,” 1997, cited and quoted in Richard Wiles to Dr. Julie Gerberding, December 23, 2005, page 8-9. (in author’s possession). The violations are listed in David Egilman, “Corporate Corruption of Science,” 173. The short latency period may have trumped all of the other problems. Even if the analysis was correct, the latency period was far too short to make a negative determination about chromium causation of the cancers. Almost all scientists require at least a twenty-year latency period from the time of exposure until the end of the study to even suggest such a negative determination of causation for chromium. Agency for Toxic Substances & Disease Registry, “Chromium Toxicity – What are the Physiologic Effects of Chromium Exposure?” http://www.atsdr.cdc.gov/csem/csem.asp?csem=10&kpo=10 (accessed November 16, 2015).

\textsuperscript{157} Deposition of Brent Kerger, 379 and 394.

\textsuperscript{158} Deposition of Brent Kerger, 153.
original) paper. It may include additional data that Dr. Zhang collected or compiled since he produced this initial version. . .” Kerger even questioned the relevance of this line of questioning: the well data was meaningless, he argued, since they used distance as a surrogate for exposure rather than [the more accurate] hydrologic movement of the water.

In using this defense, Kerger implicitly demonstrated the importance of yet another problem with the ChemRisk paper, distance from the initial water pollution only matters if, as it flows, villagers all drink from the same stream. That was not the case here. During their collaboration, Zhang had told ChemRisk employees that the water in certain wells closest to the plant was so contaminated that it appeared yellow. Local villagers did not drink it; fresh water was trucked in. Yet ChemRisk’s drafts never mentioned the likely reduced consumption of chromium-laced waters by villagers closest to the plant. Nor did they consider that underground water does not flow in straight lines. As OEHHA eventually calculated and the well data documented, at times it bypassed closer villages, while traveling directly to towns situated further away. 159

Over the next three years, several investigative reporters explored the events surrounding this controversy. In early June 2004, Los Angeles author and investigative environmental journalist Chip Jacobs dramatically brought the controversy surrounding the expert panel and senate hearings to the public’s attention in a Pasadena Weekly article with a sensationalized title graphic of a skull and crossbones. The article chronicled Praglin’s efforts at uncovering the facts behind the UC Davis Blue Ribbon panel, the activities of ChemRisk scientists, the Senate Hearings, and lobbying efforts of industry before the legislature. In this article laced with quotes and comments from participant interviews, the accusations flew.

The interviewees all provided predictable responses to questions. Jacobs quoted Praglin, who had declared: “What we have here is the tip of the iceberg. It was definitely undue influence.

159 Deposition of Brent Kerger, 121-131, 153-155, 209, quotes at 153-5.
PG&E’s lawyers knew that the company’s consultants were involved with the blue ribbon panel and thought they could sneak it by the judge.” The central figure in Praglin’s narrative, Dennis Paustenbach, told Jacobs that he was a victim of a “cut-and-paste hatchet job” during a witch-hunt by a plaintiff attorney out to make “a billion dollars” at trial. Paustenbach rightly asserted he had informed Chairman Last of his prior consulting with PG&E. Left unsaid was both the extent of the consulting—$8 million for ChemRisk—or of his employees continuing confidential relationship with the attorneys. Counsel for the university, Eric Behrens, declared he was embarrassed for Praglin because many of the quotes he used at the hearing were taken out of context and, thus, misleading. As at the hearing, the article does not contain any specific examples of this. Blue Ribbon panel Chairman Jerold Last denied any undue influence on the board and complained that Froines was “leaking everything to OEHHA as fast as Paustenbach was leaking” to others. He argued that Senator Ortiz’s hearings had been pure political process. In her interview Ortiz returned the favor, explaining to Jacobs that according to some of Last’s emails, portions of the report were torn almost verbatim from a Merck-funded ChemRisk article written by Proctor and Paustenbach. She expressed satisfaction that Last now refused to serve on more panels, arguing that he had violated his obligation to be impartial. Froines—at the time chairing the state review board for toxic air contaminants—refused to be interviewed for the article.160

Jacobs’ article was but the first of a series of in-depth examinations into the controversy. Two days before Christmas 2005—and forty years after Speth’s aborted investigation into Hinkley’s chromium-laced water—two additional articles documented events surrounding the preparation and publication of the 1997 Zhang article. One was from the Environmental Working Group, which entitled its online expose “Chrome-Plated Fraud.” Providing even more details about the Zhang

160 Chip Jacobs, “Dropping Science.”
paper than Jacobs’ article, it critically followed ChemRisk’s involvement in the 1997 rewrite through documents and depositions obtained from the Aguayo trial, providing links to many of them.161

In an even more remarkable piece given its publisher, the business-oriented *Wall Street Journal* publicized a similar scathing attack upon the 1997 paper, covering many of the same facts as the EWG report. Utilizing many of the Aguayo trial exhibits but with several additional interviews, staff reporter Peter Waldman told a story of deception and ethical malfeasance by ChemRisk in the writing and publication of the “clarification.” In response, Kerger and Paustenbach wrote letters to the *Wall Street Journal*’s editor defending their actions. Kerger claimed Zhang’s 1987 article omitted important findings necessitating a clarification, while their “just days of effort” palled in comparison to Zhang’s research for two decades. Given the disparity in efforts and the “political pressures” that the now deceased Zhang had allegedly communicated to them, “his decisions on co-authorship and the lack of acknowledgement of funding from Americans seemed appropriate.” Paustenbach did not directly respond to the specific issues raised in the paper. He claimed to have had very little involvement in the collaboration, but was proud of the very “well-supported” paper.162

Neither article indicated how it obtained the documents, which—while public—would have required considerable time to obtain and review. Either Praglin (the plaintiff attorney in the Aguayo case then being prepared for trial), or Brockovich may have provided the documents to both EWG and the *Wall Street Journal*. Based upon my experience, defense counsel likely then requested a hearing before the judge, accusing Praglin of trying to taint the potential jury pool. If so, they may well have been correct. The timing of these two articles lends credence to any claim that these

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161 Environmental Working Group, “Chrome-Plated Fraud.”
162 Peter Waldman, “Study Tied Pollutant”; Dr. Brent Kerger letter to the editor, *Wall Street Journal*, January 21, 2006; Dennis, Ph.D., DABT, letter to the editor, *Wall Street Journal*, January 21, 2006. Kerger’s claims of “political pressure” lack credibility. The new article downplayed the pollution problems of China, a position the government would seemingly welcome and encourage. Furthermore, as described in the Benzene chapter, during this period the Chinese was allowing ChemRisk employees and other westerners to conduct research in China similarly downplaying the problems of benzene in the country.
stories resulted from Praglin or Brockovich trying to sway public opinion by releasing selective information. Yet, other than denials of culpability and accusations of taking words out of context—for which no critical examples have been given—no documents have been produced that demonstrate errors in either of the investigatory articles.163

The Environmental Working Group was not satisfied with a simple publication of the events surrounding the 1997 article. On the same day its article appeared on their website, they contacted the Center for Disease Control (CDC), an organization that had a $5 million contract with ChemRisk. EWG informed the CDC of the events surrounding the Zhang 1997 article, requesting a formal review the matter. They also contacted JOEM, the journal that had published the 1997 article. A further March 13, 2006 letter written by senior analyst Renee Sharp and Vice-President Richard Wiles provided Dr. Paul W. Brandt-Rauf, editor of JOEM, with information about ChemRisk’s involvement in other “unethical activities” involving the Chrome Coalition, including ChemRisk’s 1996 proposal to undertake scientific analysis challenging OSHA’s pending PEL. The letter further informed the journal editor that this appeared to be standard practice for the consulting firm: describing how at the meeting to discuss the proposal, Paustenbach explained that he had offered to undertake unacknowledged work for Johns Hopkins similar to that resulting in the new Zhang article. They ended the letter by accusing ChemRisk of engaging “for many years in a practice of designing scientific studies to help its industry clients to undermine public health protections” rather than seeking scientific facts.164

That spring and summer, journal and media attention to the controversy intensified. Yet, although both the EWG and the Wall Street Journal had described scientific problems with the article,

163 Peter Waldman, “Study Tied Chromium-6.” This would not have been the first time Brockovich sought to turn the court of public opinion in her favor. As described previously, the California Senate Hearings occurred largely due to Brockovich’s efforts.
Brandt-Rauf did not conduct an investigation into the scientific basis for the article. Still, by the end of May, Brandt-Rauf, after intense discussions with the journal’s attorneys, decided to retract the 1997 Zhang article because it did not disclose all of its authors. The retraction appeared in JOEM’s July issue. In accordance with the concerns of his legal counsel, Brandt-Rauf carefully worded the retraction and kept it to the “barest minimum of the facts.” Although the EWG’s letter and the expose articles detailed several instances of fraud, he specifically asserted—perhaps at the insistence of his attorneys—that “there is no evidence to suggest the existence of scientific fraud in this work and that the factual content of the article has not been re-evaluated.” In an Archives of Environmental and Occupational Health editorial, Tee L. Guidotti, the President of the American College of Occupational and Environmental Medicine—the parent organization of JOEM—hit somewhat closer to the mark when he declared “[f]ailure to disclose, although seemingly a lesser offense, also undermines the integrity of the system. It is not only an act of omission but a clue that more serious distortions may lie hidden” (Emphasis added). Guidotti further recognized the article’s significance to both litigation and public policy, citing the “epic legal battle” of the Hinkley case and the article’s subsequent use by OSHA in the revised chromate standard.165

Following the retraction, EWG officials attempted to increase the pressure on ChemRisk, with senior vice president Richard Wiles writing to James Popp, president of the Society of Toxicology, that July, alerting him to Paustenbach’s serious violation of the Society’s Code of Ethics, and urging the society to censure Paustenbach. He urged this action “So that its [sic] clear that there’s some price to be paid for deliberate fraudulent activity and then using fake science to weaken

public health protections.” The Society, closely tied to industry, apparently took no action on the letter.

However, the popular scientific magazine *The Scientist* reported on the letter, as well as Paustenbach’s spirited defense of ChemRisk and the article. Paustenbach told the journal’s reporter: “If the journal was using those rules over the past ten years [full disclosure of authors and funding] I think they’d find dozens of papers to have inadequacies in disclosure.” He also again alleged that Zhang was afraid of getting in trouble if authorities learned he was working with American scientists—an allegation ridiculous on its face (as noted above) since ChemRisk was working at the time in China on other matters with full knowledge of the Chinese government and the article did not blame the Chinese government for pollution like Zhang’s earlier article.

ChemRisk officials also offered other defenses to the article. Kurt Fehling, director of operations for ChemRisk, claimed “there is no valid challenge to the underlying science” of the study.” He argued that EWG was on a witch-hunt, having tried to “demonstrate scientific fraud by Dr. Paustenbach” for several years. Paustenbach further claimed that ChemRisk actually had very little to do with the paper: “[we] only assisted the authors with the translation and assisted them in preparing the tables in a way that was required by a journal in the United States . . .” It is hard understand how Paustenbach could honestly believe this, as it ignores the repeated drafts and editing of the paper in English, a language in which Zhang was not fluent, as well as the charts and statistics all prepared by ChemRisk employees.

The following month Wiles wrote to Paustenbach, informing him that they were stunned by his defense of the 1997 article in *The Scientist*. They informed him that *JOEM* had begun requiring author disclosures well before accepting the 1997 Zhang article. He asked Paustenbach to provide
the names of others who had violated the requirements. Wiles also mocked the “heroic” act of ChemRisk scientists’ in attempting to protect Chinese scientists.166

At the same time that EWG and Wall Street Journal claims of fraud surrounding publication of the new Zhang article garnered most of the media attention the fraud, OEHHA and its scientists quietly continued questioning its science. That year, five scientists from the OEHHA and one from the California Department of Conservation published a review of the carcinogenicity of ingested hexavalent chromium that stood in stark contrast to the review published by ChemRisk employees, including Paustenbach, three years previously, reaching a diametrically opposed conclusion.

ChemRisk’s original review had served primarily as a means to further document the importance—importance for litigation—of the series of articles the ChemRisk group wrote describing experiments undertaken while serving as confidential consultants to Latham & Watkins attorneys: experiments designed to demonstrate the low levels of Cr(VI) entering the body and/or the reduction of Cr(VI) to CR(III) in the stomach. This review did not examine epidemiological reports, including either of the Zhang articles. Thus, without considering the epidemiological evidence, Paustenbach, et.al were able to conclude that “the overwhelming weight of evidence in both the historical studies conducted by various researchers and those conducted over the past 5 years by our group [while working as confidential consultants for attorneys] clearly indicates that ingestion of Cr(VI) in drinking water at concentrations at least as high as 100ug/L, and possibly higher, do not pose an increased acute or chronic health risk.” They concluded by suggesting that a

classic rodent bioassay study of ingested Cr(VI) then currently being conducted by the National Toxicology Program (NTP) would provide further information about long-term health effects.

The new OEHHA paper took an opposing tack. While acknowledging that reduction of hexavalent chromium in the stomach may reduce the risk of cancer, OEHHA’s scientists contended that mechanistic studies demonstrated that ingested hexavalent chromium is absorbed in the stomach. The only lifetime study found this absorption linked to an increased risk of stomach cancer. In addition, a meta-analysis of occupational studies revealed a significantly increased in stomach cancers from ingesting of Cr(VI). While remaining cautious, the scientists believed the evidence suggested hexavalent chromium “appears to pose a carcinogenic risk.” In thus concluding, they considered both DeFlora’s opinions and ChemRisk’s research. They also considered the Zhang 1997 article, as well as four other articles prepared by Zhang and other relevant Chinese documents, concluding that the 1997 article did “not appear to be credible.”

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That same year marked the rare publication of a publication partially funded by a plaintiff law firm. Following the same path as the ChemRisk and OEHHA articles, Max Costa and Catherine B. Klein, both members of the Department of Environmental Medicine at the New York University School of Medicine, discussed the current state of chromium as a human carcinogen. The two authors acknowledged support from grants as well as partial support for the literature review from Baron and Budd, a prominent plaintiff law firm engaged in chromium litigation. However, as often occurred for defense experts, Costa did not reveal that he had been used more than once as a plaintiff expert for chromium.

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168 As indicated previously, I still retain an interest in certain asbestos lawsuits managed by Baron and Budd.
At the behest of a Baron and Budd attorney, the authors had conducted a literature review, largely designed to test ChemRisk’s toxicology claims. They initially noted that recent ChemRisk articles “claimed that humans could tolerate considerably higher levels of hexavalent Cr than the drinking water standard of 50 ppb.” They then analyzed this claim by considering several reports of chromium cancers at sites other than the lungs and animal studies. The animal study examination appeared to be designed in part toward disputing papers by De Flora, an Italian scientist at times used by Chromium defense attorneys. In their conclusion Costa and Klein claimed mounting evidence points to distant systemic effects of Cr(VI) whether exposed by inhalation or ingestion. As occurred with defense-sponsored articles, this conclusion likely provided Costa’s client—a plaintiff counsel—with the desired results. The article did not limit Costa to specific effects when called as an expert witness, but provided him with flexibility to testify about whichever organs had been allegedly effected by Cr(VI) exposure.169

The year 2006 also brought another surprise: the appearance of the 1997 “Zhang” article’s “coauthor” into the fray. In a series of three letters from herself and her attorney to Paul Brandt-Rauf, Dr. Li Shunkun—Zhang’s girlfriend and “coauthor” of the 1997 article—claimed that Brandt-Rauf had lied to both the Wall Street Journal and the Environmental Working Group about her acquiescence to the article’s retraction. The first two letters, written in June and November, asserted that she had always opposed the retraction; the data and conclusion were correct. In addition, in language similar to that used by ChemRisk in defense of their actions, she vouched that the work was Zhang’s and hers, “not that of an American company that provided minor consulting and editorial input.” The third letter, written on December 6, 2006 by her attorney Danning Jiang of San Jose, California, demanded a series of actions on the part of Brandt-Rauf, including republication of the article, admission of the lies, and a full written and published apology, stating that the scientist’s

reputation had been damaged. The letter’s tone—demanding a response within ten days—along with its announcement of damages to her reputation, implied further legal action would be forthcoming absent a satisfactory response. However, it does not appear that she filed a lawsuit.

The exact motives underlying these late objections are not clear. ChemRisk officials claimed she had not known about the retraction. Nor is the extent of her involvement in writing the 1997 article known. ChemRisk correspondence concerning the paper did not go to her. Neither Tony Ye nor other ChemRisk scientists appear to have consulted with her while the paper was being written. Zhang’s son said she had little to do with the article, being put on it as coauthor because she was his father’s girlfriend.

Others suggested that ChemRisk was actually the motivating force behind the letters. In an interview, one analyst with the Environmental Working Group stated that ChemRisk scientists likely “twisted her arm into doing something.” He continued by admitting he had no evidence of that, but “would be shocked if that wasn’t the case.” Whatever the motives, no further action appears to have been taken even though Li Shunkun’s claim, if proven, likely would have warranted damages being awarded in a lawsuit.\(^\text{170}\)

Numerous additional developments to the continuing saga came in 2008. Early that spring, PG&E settled another lawsuit involving claimants from the Hinkley area. In an interview with a local paper, the plaintiff counsel described the settlement as a result of the judge’s recommendation to PG&E after his review of the corporation’s internal documents concerning the leakage.\(^\text{171}\)

Two other recently published scientific publications may have influenced the utility company’s decision: the NTP rodent drinking water study and a new analysis of the original Zhang

\(^{170}\) Dr. Shunkin Li to Paul Brandt-Rauf, November 26, 2006; Danning Jiang to Paul Brandt-Rauf, December 6, 2006 (Letters in author’s possession); and Melissa Lee Phillips, “Chromium Paper Retracted Unfairly” (analyst quote).

1987 China study. James J. Beaumont and other California state scientists prepared the new analysis. These authors initially discussed the recently completed NTP studies of cancer in rats and mice provided with Cr(VI) laced drinking water, noting that they provided clear evidence of cancer. They then explained that there had been three studies in humans, but that two with either very low Cr(VI) or no concentration reported found no overall cancer increase. In contrast, the China study found relatively high levels of Cr(VI) in the water and provided data on stomach cancer. They recognized that the Chinese data was difficult to interpret but believed the apparent contradictions were explained by assuming a relatively fast groundwater velocity and self-limitation of human exposure at higher-level sites.

Beaumont’s paper then proceeded to bring more specificity and rigor to the original ecological study of the Jinzhou Health and anti-Epidemic Station that had been published in 1987. They estimated exposures at the various villages by considering the initial 1965 well sampling and estimating the groundwater percolations, using the hydrogeologic characteristics described by the original investigators as the basis for groundwater movements. They estimated population and age-adjusted cancers, based upon census data from the National Bureau of Statistics of China. The stomach cancer rates in the four exposed regions were all elevated above the rates in unexposed regions, and in comparison to the entire province. In contrast, mortality from other cancers was not elevated. They then spent almost an entire page discussing the admitted limitations of the study, but concluded that their analysis helped “to quantify and reinforce the association [of hexavalent chromium to stomach cancer] first reported by Zhang and Li in 1987.”

In a most unusual course of action, the journal published a commentary about hexavalent chromium ingestion, cancer, and epidemiology immediately following the article. The author, Allan H. Smith, a professor of epidemiology in the University of California, Berkeley School of Public

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Health, wrote the commentary with support from a National Institute of Environmental Health Services (NIEHS) grant. Smith commended Beaumont and his colleagues for taking on the rigorous task of conducting an epidemiologic analysis with poor data. While admitting that because of its limitations the Beaumont paper could not prove ingestion of chromium causes cancer, he suggested that it provided evidence of an increased risk. He cautioned that its greatest weakness—the relatively short latency period of 10 to 18 years—might well have caused an underestimation of the risk.173

ChemRisk scientists responded to both articles, with Paustenbach accusing Smith of implying “inappropriate behavior “regarding the Zhang and Li 1997 article and Kerger, Butler, and Ye addressing “2 errors” in the Beaumont paper. Neither criticism mentioned the cancer findings of the new NTP rodent study—formerly suggested by Paustenbach as the study that might decide the issue. Smith responded by noting that his paper “was confined to the facts and does not contain innuendos as he [Paustenbach] claims.” Smith added—apparently somewhat mirthfully—that while he did not agree with Paustenbach’s statement that Smith suggested there was a reversal of scientific conclusions between 1987 and 1997, readers could judge for themselves. Beaumont and colleagues responded to the attacks by reminding readers that the 1997 paper was retracted and then buttressed their positions on all issues raised by the ChemRisk scientists, along the way noting several mistakes in the ChemRisk letters.174

174 J. Beaumont, Letters to the editor, Epidemiology 20, no. 4 (July 2009): 625-628. The NTP rodent study for Cr(VI), formally published in July 2008—one year prior to ChemRisk’s comments—fully supported Beaumont’s claims of evidence implicating Cr(VI) as causing cancer when ingested. The NTP publication concluded that the 2-year drinking water studies provided “clear evidence of carcinogenic activity” (Italics in original). NTP Technical Report, Toxicology and Carcinogenesis Studies of Sodium Dichromate Dihydrate (CAS NO. 7789-12-0), Washington, D.C.: (National Institutes of Health, Public Health Service, U.S. Department of Health and Human Services, 2007); and NTP Technical Report, Toxicology and Carcinogenesis Studies of Sodium Dichromate Dihydrate (CAS NO. 7789-12-0), Washington, D.C.: (National Institutes of Health, Public Health Service, U.S. Department of Health and Human Services, July 2008): 70. As several commentators reported, “taken together, these NTP studies demonstrate that SDD [a hexavalent chromium compound] was clearly carcinogenic in male and female rats and mice following exposure in the drinking water.” The authors went on to explain that at least a portion of the hexavalent chromium escapes gastric reduction, thus entering systemic circulation. Furthermore the concentrations of SDD did not appear to overwhelm the gastric reduction capacity.
Despite the new NTP findings, ChemRisk employees did not limit their responses to letters to the editor, but also sought to regain the initiative. In 2009 Kerger and colleagues (this time including Paustenbach) published a new version of the Zhang 1997 article. The authors claimed money for the original 1997 study came from PG&E, which, while technically correct, ignored the fact that the contract for the study provided for confidential litigation consultation with the law firm Watkins & Latham. Thus, monitoring of the study was conducted by those attorneys—not PG&E.

The manuscript compared stomach and other cancer rates in five exposed villages to three agricultural villages without hexavalent chromium contamination. They found the cancer rates were not statistically different between the various villages. The authors stated that they picked the three villages because of their demographical and epidemiologic similarities to the five contaminated villages. They were also all up-gradient from the groundwater flow. The paper did not indicate whether any effort had been made to determine if any of the inhabitants of the three villages worked at the chromium plant or in the contaminated areas. Nor did they indicate what “lifestyle activities” might have caused the increase rate of cancer in all of the villages from the rest of the province. They concluded that despite the short latency period, “the current study showed that the JinZhou groundwater pollution incident was not associated with significant excess risks of mortality from . . . unadjusted stomach or lung cancers . . .” In their concluding sentence they suggested that now the “scientific community can evaluate these findings [ChemRisk’s and OEHHA’s] with greater depth and transparency.”

The scientific community virtually ignored the new ChemRisk article. One draft EPA report, for example, found that Kerger group did not have a sound rationale for excluding the urban population since there was little difference between stomach cancer rates in urban and rural areas.

during this period. Similarly, the California 2011 Public Health Drinking Goals for Chemicals in Drinking Water – Hexavalent Chromium (Cr(VI)) found that the conclusions reached in both the 1997 and 2009 ChemRisk papers “does not appear to be credible.” Several governmental agency documents, including the EPA 2010 draft IRIS document, the ATSDR 2012 Toxicological Profile for Chromium, and the 2011 OEHHA PHG document selected the Beaumont 2008 articles over the Kerger 2009 article as the most the most useful reanalysis of the Zhang 1987 original data and conclusions.

In 2010, more bad news descended on the chemical industry. First, in September, scientists at EPA decided that small amounts of Cr(VI) in tap water could cause cancer. Then, in December—almost 45 years to the day following Speth’s original investigation into chromium-tainted tap water in Hinkley—the Environmental Working Group published its latest bombshell findings: at least 31 American cities had water containing levels of chromium above the proposed California PHG of 0.06 ppb. This number constituted 89% of the 35 cities in which the EWG had commissioned laboratory tests. The news was no better on the local scene in California. In November the Los

175 EPA Draft Toxicological Review of Hexavalent Chromium, EPA/635/R-10/004c, April, 2010; and Pesticide and Environmental Toxicology Branch, Office of Environmental Health Hazard Assessment, California Environmental Protection Agency, Public Health Drinking Goals for Chemicals in Drinking Water – Hexavalent Chromium (Cr(VI)), Sacramento, California: July 2011, 73.

"Angeles Times" reported the Hinkley plume was again moving toward residences, having moved 1800 feet beyond a containment barrier set by PG&E.177

That same month, industry agents publically altered their tactics from epidemiology to toxicology. In a letter to the Board of Scientific Counselors, Executive Committee Meeting, Kerger first applauded EPA for its efforts, then argued it was not focusing on the most relevant studies concerning chromium. He complained that EPA tended to focus on studies that supported its conclusions of a mutagenic mode of action (MOA), providing “an incomplete and unbalanced interpretation of the most relevant scientific literature.”178 Kerger’s, Butler’s and other industry comments succeeded in having the EPA await the results of the ongoing American Chemical council (ACC) studies on MOA—studies the ACC had specifically designed to cast doubt on EPA’s linear dose model in favor of a threshold model that would not require regulation. The EPA initially rejected the request in an April 2011 letter to the ACC. Ten months later it quietly reversed itself, pushing back the release for at least four years. Although little known at the time, industry had help from secret attorney sponsored experts. Three scientists of the nine-member chromium review panel—a group supposedly vetted by a third party for conflict-of-interest issues—had either worked as expert witnesses for industry or assisted in industry studies.

The first, Steven Patierno—Executive Director of the George Washington University Cancer Institute, and one of PG&E’s key litigation expert witnesses—had also consulted on the ACC MOA research, still ongoing at the time of the review panel meeting. Ironically, as a former

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178 Brent Kerger to Board of Scientific Counselors, Executive Committee Meeting, “General and Specific Comments Responding to Docket ID No. EPA-HQ-ORD-2010-0540,” December 29, 2010. (in author’s possession). Kerger specifically deplored the use of the Beaumont 2008 article since it “incorrectly included an industrial town with demonstrably different demographics and site-specific cancer trends.” (16). Interestingly, he did not indicate who paid him or Butler to submit their comments. Although there is no proof that attorneys were involved in its preparation, the letter reads remarkably similar to many expert reports I have seen prepared with assistance of counsel.
reviewer for EPA’s chromium findings, Patierno had criticized the EPA for citing Max Costa’s papers in its report because of his expert witness involvement with plaintiffs.

Another member, Joshua Hamilton, the chief scientific officer at the Marine Biological Laboratory in Woods Hole, Massachusetts, had an even more egregious conflict of interest. He had acted as an expert witness for PG&E twice, first in a lawsuit during the early 2000s, then again starting in 2009. As the panel met in May 2011, PG&E again hired him, this time to consult on the Hinkley cleanup, ultimately paying him over $100,000 for his latest work. One month following the review panel meeting, while acting as a consultant for PG&E, Hamilton signed a declaration concerning the risks of Chromium that is particularly one-sided. In it, he touted his membership on the review panel as an example of his expertise. The declaration reads very similar to many affidavits I have personally prepared for experts and/or seen provided by experts of other attorneys in support of the position desired by the attorney. In the legal profession this is known as puffing: reaching absolutely as far as you can with an extreme characterization of the evidence or opinion, while still having a colorable defense to perjury. His discussion of the NTP mouse study and the California draft PHG are especially illuminating as to Hamilton’s bias. He first castigated OEHHA for proposing a PHG that is “one million times lower” (italics in original) than the NTP study dosage that found cancer, then contended that the “calculations embodied in the draft PHG do not represent established science.” This type of over-the-top claim is not typical for scientists who recognize that regulatory limits invariably will be very significantly lower than doses used in studies. However, it is a typical ploy for attorneys in setting forth the best possible position in litigation or regulatory practice. As a scientist, Hamilton was well aware that conducting animal studies at exposure levels intended for 1 in a million public health standards are not possible given monetary, space, logistical, and time constraints. Thus all animal studies for cancer are conducted at high dosage levels then extrapolated down. Such extrapolations based on the NTP study were not
unusual, but, in fact, standard science. Contrary to Hamilton’s claims, the California calculations were not outside of established science. Rather, they were virtually identical to those used by both EPA and other states. The only difference was that under California law, CAL/EPA must set PHG’s low enough to keep risks extremely low.

In comparison to the first two individuals, John Wise’s conflict was relatively minor. He had worked in Patierno’s laboratory as a graduate student. Then in 1997 he had worked for a consulting firm and was assigned research for an industry client in the Hinkley lawsuit. Since he claimed to not be aware of the client’s ultimate identity, he likely worked through attorneys. While it might stretch credulity to suggest he did not know which side he was working, in the world of litigation science, he played only a minor and perhaps unknowing role.  

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This saga is far from over. Media and public interest group attention continues. In the late winter of 2013, the Center for Public Integrity published an up-date of its story about the Zhang 1997 article, including its retraction by JOEM, as well as more details concerning ChemRisk scientists’ involvement in the chromium controversy. Public health advocates and industry/attorney-sponsored experts also continue to prepare contrasting reviews and studies.

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179 United States Environmental Protection Agency National Center for Environmental Assessment, “Peer Review Workshop for EPA’s Draft Toxicological Review of Hexavalent Chromium,” May 12, 2011; David Heath, “EPA Unaware of Industry Ties on Cancer Review Panel,” Center for Public Integrity, November 4, 2013, http://www.publicintegrity.org/2013/02/13/12184/epa-unaware-industry-ties-cancer-review-panel (accessed April 14, 2014). The article explains that 11 of the 68 members appointed to EPA peer review panels since President Obama took office have been primary authors of research articles funded by the American Chemistry Council. The review panels are not the only EPA advisory groups partially manned by industry attorney experts and confidential consultants. Dennis Paustenbach is one of only nine members of EPA’s Board of Scientific Counselor Executive Committee. United States Environmental Protection Agency, “BOSC Executive Committee,” http://epa.gov/osp/bosc/ec.htm (accessed April 14, 2014). Joshua Hamilton’s conflict is particularly egregious. One month following the review panel meeting, as a consultant for PG&E he signed an affidavit concerning the risks of Chromium that is particularly one sided. It reads very similar to many affidavits I have prepared for experts and/or seen provided by experts of other attorneys. The opinions expressed are not those of a “fair and balanced” scientist. Joshua Hamilton, “Declaration of Joshua Hamilton,” July 9, 2011 (in author’s possession). In contrast, industry has not stinted from objecting to scientists being appointed to EPA panels who had the slightest purported bias. See, for example, Herbert L. Needleman, “The Case of Deborah Rice: Who is the Environmental Protection Agency Protecting?” *PLOS Biology* 6, no. 5 (2008): 940-942.

180 David Heath, “How Industry Scientists Stalled.”
Government funded research also continues. In an article funded by NIEHS and published on July 18, 2011, Anatoly Zhitovich reviewed the state of research for chromium in drinking water. In it, he rejected industry’s latest efforts at changing the focus of the decision-making, concluding:

Multispecies and multisite carcinogenicity of Cr(VI) along with its broad genotoxicity provide a strong basis for a classification of Cr(VI) exposures through drinking water as likely to be carcinogenic to humans. Diverse lines of evidence demonstrate the importance of a DNA-reactive mutagenic mechanism in Cr(VI) carcinogenicity, lending mechanistic support for a linear low-dose extrapolation of cancer risks in humans. The bioavailability results and kinetic considerations indicate incomplete gastric detoxification of Cr(VI) at environmental levels of exposure, predicting its uptake and genotoxic metabolism in the small intestine.181

Similarly, in a study examining small intestine tumors in mice following ingestion of hexavalent chromium, Alan H. Stern, of the New Jersey Department of Environmental Protection, concluded that at doses even lower than those used in the NTP study hexavalent chromium escapes reduction in the stomach. “The cancer potency derived from the NTP data is, therefore, deemed to be relevant and applicable to human exposure. Cr (6+) is, therefore, identified as ‘likely to be carcinogenic to humans’ in accordance with the USEPA’s cancer characterization rubric.”182

In contrast, industry and their attorney-sponsored experts continue in their attempts to change the terms of the discussion in an effort to find an argument for reducing regulation. Proctor’s efforts are now directed toward suggesting that there is a threshold level for hexavalent chromium’s carcinogenicity. She and other scientists from the new scientific consulting firm, TOXstrategies (founded in 2008)—which she joined as a principal scientist in 2009—suggest there is not yet enough information to determine if there is a threshold for Cr(VI).

Although available data generally support the plausibility of these key events, several unresolved questions and data gaps were identified, highlighting the

need for obtaining critical toxicokinetic and toxicodynamic data in the target
tissue and in the low-dose range.\(^{183}\)

Thus, as first occurred in the “Tobacco Wars” then increasingly during asbestos litigation,
ChemRisk researchers and other scientists with close ties to industry attorneys have taken
increasingly major roles in shaping the underlying medical literature of yet another toxic substance.
The ongoing regulatory and litigation battles surrounding Cr(VI) displays their increasing
sophistication and loyalty to their clients. As Allan H. Smith suggested earlier in response to
Paustenbach’s criticism of his commentary about chromium, the readers can judge for themselves
whether any fraud has occurred in industry’s defense of chromium. We will now turn to another
toxic substance, Benzene, with fewer clear connections to attorneys, but very similar research
methods.

\(^{183}\text{Chad M. Thompson, et al., “Application of the U.S. EPA Mode of Action Framework for Purposes of Guiding}
\text{Future Research: A Case Study Involving the Oral Carcinogenicity of Hexavalent Chromium,” *Toxicological Sciences* 119,}
\text{no. 1 (2011): 20–40 (Funding for the article was provided by the Cr(VI) Panel of the American Chemistry Counsel). Also}
\text{see Deborah M. Proctor, Chad M. Thompson, Mina Suh, Mark A. Harris, “A Response to ‘A Quantitative Assessment}
\text{of the Carcinogenicity of Hexavalent Chromium by the Oral Route and its Relevance to Human Exposure,’”}
\text{*Environmental Research* 111 (2011): 468-470. (Funding for the comment was provided by the Cr(VI) Panel of the American}
\text{Chemistry Counsel.)}
CHAPTER 8

BENZENE: THE TREE OF LIFE, --- AND DEATH

Introduction

Benzene is a colorless, slightly water-soluble, aromatic compound liquid with an atomic carbon ring structure of 6 carbon atoms and 6 attached hydrogen atoms that was first identified in the second half of the nineteenth century. The discovery of benzene’s ring-like nature helped unlock the field of organic chemistry. By 1890 August Wilhelm von Hoffman, the chemist who isolated the compound, could wax poetic regarding the benzene tree, full of life:

In the benzene nucleus we have been given a soil out of which we can see with surprise the already-known realm of organic chemistry multiply, not once or twice but three, four, live or six times just like an equivalent number of trees. What an amount of work had suddenly become necessary, and how quickly were busy hands found to carry it out! First the eye moves up the six stems opening out from the tremendous benzene trunk. But already the branches of the neighbouring stems have become intertwined, and a canopy of leaves has developed which becomes more spacious as the giant soars upwards into the air. The top of the tree rises into the clouds where the eye cannot yet follow it. And to what an extent is this wonderful benzene tree thronged with blossoms! Everywhere in the sea of leaves one can spy the slender hydroxyl bud: hardly rarer is the forked blossom [Gabelblüte] which we call the amine group, the most frequent is the beautiful cross-shaped blossom we call the methyl group. And inside this embellishment of blossoms, what a richness of fruit, some of them shining in a wonderful blaze of color, others giving off an overwhelming fragrance.¹

At the time, Benzene seemed the miracle product, offering a solvent with a myriad of uses. Still, even as Hoffman lauded benzene, medical professionals began compiling reports of its deadly effects on the blood system. As the years passed, the health effects of benzene became increasingly clear. Yet, even though it is now known to cause numerous short term and chronic diseases at relatively low exposure levels, benzene remains an important raw material for the chemical industry.

Few chemicals are produced in the United States in greater volume. U.S. production of benzene in 1985 totaled 9.73 billion tons. A 1980s estimate by NIOSH concluded that close to 2,000,000 U.S. workers have the potential for benzene exposure.²

This important, yet deadly, organic compound was first discovered and utilized in the early to mid nineteenth century. Michael Faraday, better known for his laws of electricity, isolated benzene in 1825 from a thick mixture of oil and gas. He called the extracted substance “bicarburet of hydrogen.” A few years later German chemist Eilard Mitscherlich provided the substance’s modern name when he distilled what he called “benzin.” Industrial-scale production did not occur until twenty years later, four years following Charles Mansfield and August Wilhelm von Hoffman’s isolation of the chemical from coal tar. Once it became available in sufficient quantities, its commercial uses as a solvent proliferated. Benzene seemed miraculous in its ability to dissolve tars and other hard-to-clean industrial deposits. Paint manufacturers also discovered that paints containing a benzene base dried much faster.

Commercialization did not bring about a swift understanding of benzene’s molecular form. Chemists struggled to understand how it could exist as a molecule. Finally in 1861 Baron Kekulé von Stradonitz awoke with a vision of benzene’s ring-like nature. An apocryphal story credits the Bohemian noble’s awakening epiphany to a snake wrapping itself in a ring around his head while he slept.³

Historically heavily used by industry, its commercial uses have included the printing industry and the manufacture of dyes, rubber, plastic, linoleum, adhesives, leather products (especially shoes), paints, enamels, lacquers, drugs, and important industrial chemicals, as well as maintenance and

³ Devra Davis, The Secret History, 379.
cleaning. By the 1930’s, despite its well-known toxic nature, American industry used benzene almost universally. During that decade the booming rotogravure—a method of transferring pictures to a drum for printing on paper—industry became but the latest heavy users of the solvent in the United States and Europe.

Legendary industrial health doctor Alice Hamilton became one of the first and loudest advocates of more care in the increasing usage of benzene. As early as 1922 she drew attention to “the growing menace of benzene (Benzol) poisoning in American industry.” Her 1931 general review of benzene poisoning listed dozens of examples of its toxic effects. In a 1936 speech, she acknowledged the value and usage of benzene, while also noting its dangerous ability to quickly poison users. She noted how benzene owed its

value to two properties, the ability to dissolve fats and volatility, for a solvent to be useful must evaporate fairly quickly. The solvents are used to dissolve natural gums, such as shellacs, resins, rubber, gutta percha; for the newer cellulose compounds, nitrate and acetate, used in making lacquers, dopes for fabrikoid, waterproofing, artificial leather, patent leather; for celluloid, nonshatterable glass; for fats, oils, greases, in degreasing machine parts and in dry cleaning; also as thinners for coatings and as removers of coatings.

Although some industries had by then begun restricting its usage, Hamilton cautioned that “it is still used in sealing compounds for cans and bottles, in dope for patent and artificial leather, in floor stains and finishes, in rubber cements, in making drugs and chemicals and in mixtures for dry cleaning.” Besides being distilled for specific usage in these processes, in the 1930s benzene increasingly entered the atmosphere as a waste product following combustion of many materials, including gasoline and tobacco.4

Because of its high toxicity, benzene is now heavily regulated. Yet, its environmental presence through usage and airborne contamination is still ubiquitous in most urban centers and industrial areas. Although its combined levels in indoor and outdoor air measurements is very low, approximately 5 parts per billion (ppb), in the United States, it is currently regulated to a level not that significantly higher, 1 part per million (ppm).\(^5\)

As industry standards and regulations gradually became more stringent, benzene’s commercial uses declined, with industry increasingly turning to less hazardous compounds. Yet, even with this reduction, the solvent is still widely used as an intermediate during the production of numerous chemical substances and is a minor component of gasoline. Thus, most exposures to benzene today occur principally in the petroleum and chemical industries or as the result of gasoline engine emissions and combustion products, although it is still used as a solvent in some less developed countries.\(^6\)

Although the toxicological properties of benzene are among the most thoroughly studied of organic compounds, the exact process of benzene toxicity and the diseases it causes are still controversial today. While its disease causing mechanisms, to a degree, remain shrouded in mystery, medical scientists agree bone marrow acts as a trap for benzene and its lipophilic (affinity for lipids, one of the chief structural compounds of living cells) metabolites. Researchers have linked it in varying degrees to a wide variety of hematopoietic (blood formation) type diseases. In addition, based on toxicological principles, most medical practitioners expect there is variability in human

susceptibility to benzene. The literature contains numerous suggestions of just such sensitivity depending upon age, sex, stature, and other factors.\(^7\)

As knowledge about benzene’s chronic effects increased during the past forty years, the issues relating to its effects and ability to induce disease at low levels became increasingly complex and contested. Today, virtually all scientists agree that at historically high doses benzene caused several acute and chronic diseases, including, leukopenia, agranulocytosis, anemia, pancytopenia, and aplastic anemia. They also agree it causes myelodysplastic syndrome (MDS) and acute myelogenous leukemia (AML) at lower doses, although the necessary dose is hotly contested between public health advocates (some of whom act as expert witnesses for plaintiff counsel in benzene cases) and industry affiliated scientists (a number of whom act as expert witnesses for defense counsel in benzene cases). Other chronic diseases, such as reproductive organ damage, acute lymphocytic leukemia (ALL), chronic lymphocytic leukemia (CLL), chronic myelogenous leukemia (CML), multiple myeloma (MM), non-Hodgkin lymphoma (NHL), childhood leukemia, and lung cancer, have also been associated with benzene, but their causal connection to benzene is even more hotly contested by public health and industry associated scientists.\(^8\)

Unlike the other toxic substances discussed in this Dissertation, specifics concerning direct attorney involvement in benzene research are limited. Unlike asbestos, chromium, and tobacco, there have been no bankruptcy or court-remanded major releases of attorney documents normally protected from disclosure. Still, circumstantial evidence of intensive attorney involvement abounds. Since the nineteen eighties, benzene has become increasingly subject to litigation and attempts at

\(^7\) D. Pyatt, “Benzene and Hematopoietic,” 533; and Bernard Goldstein, “Benzene Toxicity,” 548.
regulation. As a result, the same types of expert, consultant and author activities as seen in tobacco, asbestos, and chromium are present in benzene research. Thus, this chapter will focus on the experts, both defense and plaintiff, as a surrogate for attorneys, comparing and contrasting their activities to each other and to those found in other substances with substantial attorney involvement.

The research into the effects of benzene tends to be segmented into two camps: government funded research (which has catalogued increasingly smaller doses necessary for benzene damage to the body), and the industry/attorney funded research (downplaying the danger of virtually all exposures below the current governmental limits, as well as limiting the specific types of diseases at issue). On the defense side, Dennis Paustenbach and his company once again have played a major role, providing the same varieties of services as seen in numerous other toxic substance areas.

Accordingly, the majority of benzene medical research and review articles—especially those published since 2000—can be grouped into two virtually diametrically opposed collections. The largest group, often written by expert witnesses for the defense, is funded by the oil and chemical industries or through its attorneys. These articles almost invariably minimize the diseases caused by benzene and the risk associated with recent exposures, often claiming the research suggests a threshold exposure level necessary for benzene-associated leukemia, a position at odds with all government health related regulation, which assumes cancer causing substances affecting DNA and cellular processes has continuing, if decreasing effects down to negligible exposure. At times company oriented authors also express opinions that other benzene caused cancers are unlikely, maintaining there is “no evidence” for benzene caused malignancies except for one or two forms of AML, and then, only at doses higher than found in the workplace today. Their published articles also tend to minimize the exposures of individuals who currently work around benzene, while maximizing the exposures commonly found through the early 1950s.
The other collection of more cautionary articles is written by a diverse group of independent, foreign, and government medical scientists conducting research and an overlapping, but fairly small, group of medical scientists who serve as expert witnesses for plaintiff counsel in benzene litigation with varying degrees of frequency. This research is largely funded through government grants, with plaintiff attorneys rarely supporting research or preparation of review articles, likely because—as with other toxic substances—plaintiff counsel have no desire to fund research out of their own pockets.

Medical scientists in this group generally look at threshold from the other side of the issue. In the opinion of most of these scientists, there is no reliable evidence for a threshold for benzene-induced leukemia. One dose may be sufficient to cause not only AML, but also several other leukemias, MM, NHL, and perhaps other cancers.

Meeting the Defense Experts

The positions of the two competing sets of experts can be characterized through the positions of a relatively few individuals on each side. On the defense side, litigation experts have been responsible for much of the research into benzene. One defense litigation expert group is already familiar from their appearance in the tobacco, asbestos and chromium chapters, Dennis Paustenbach and his colleagues at ChemRisk. They have been providing assistance benzene research assistance to clients for almost three decades.

Dennis Paustenbach and ChemRisk

Benzene is no exception to ChemRisk scientists’ practice of attending attorney gatherings. With benzene, as with other toxic substances, ChemRisk’s business model is unabashedly slanted toward seeking litigation and regulatory challenged clients and providing them with the evidence they need to defend their position. As noted previously, Paustenbach has attended several DRI (corporate defense attorney) conferences. They also attend other legal conferences. For example,
one Chemrisk scientist attended the 2013 Harris Litigation Conference for Benzene, a meeting attended almost exclusively by defense and plaintiff attorneys to learn about the newest trends in litigation.

One Chemrisk partner and medical doctor has used a different means to put his name before benzene defense counsel, writing an article in the Defense Research Institute’s (DRI) journal—*For the Defense*—to which numerous defense counsels subscribe. In a very subtle and sophisticated promotion for his firm, David Galbraith decried the increasing filing of “trace benzene” lawsuits. He began his comments with a brief and cursory examination of when knowledge about benzene hazards became known, claiming that prior to the seventies there were only “occasional case reports” of benzene’s “linkage to leukemia.” Alice Hamilton likely would not have agreed with Galbraith. In 1936 she wrote, “we have in the literature instances of various forms of leukemia . . . following exposure to benzol,” citing specifically to a 1934 article. The first reported case of benzene-induced leukemia had been reported six years earlier, in 1928. In his 1952 article discussing occupational cancers, Hueper also linked benzol to leukemia. By 1960 most doctors viewed benzene as strongly suspected to cause leukemia. That year, even a conservative occupational disease textbook listed benzol as a suspected cancer-producing agent. Four years later a survey of leukemias in *California Medicine* listed benzol as an agent factor for leukemia. At least one federal court also disagrees with Galbraith’s characterization of benzene knowledge before the 1970s. In a 1990 opinion a Kansas federal judge wrote, “By the 1960s, many widely-disseminated scientific publications, including standard American medical journals and textbooks, were reporting the carcinogenic danger of benzene exposure as established through hundreds of individual cases.” [emphasis added]

Following his assurance to the attending attorneys that their clients could not have been aware of workers’ problems before the mid 1970s, Galbraith then told them how ChemRisk could
provide a defense to more recent exposures, providing several reasons why most epidemiological studies with positive findings have faults and biases. His list of important studies linking benzene with leukemia included the Pliofilm cohort and the industry managed study in China. He ignored the similarly ongoing—and more advanced—NCI/China study, which had positive findings from lower exposures than found in the original Pliofilm cohort, apparently believing it was not worth mentioning. His other references chiefly consisted of industry-funded studies. In concluding his survey of the literature, Galbraith delivered a subtle solicitation for business, arguing that the reason new cases are being filed is because industries have not conducted the proper examination of the benzene exposures at their facilities.9

Notwithstanding direct appeals to defense counsel such as Galbreath’s, the best advertising for Paustenbach and ChemRisk has remained their ability to generate well-written peer-reviewed negative studies on virtually any toxic substance topic requested by a client. ChemRisk, with Paustenbach in almost all cases listed as a coauthor, has published by far the greatest number of peer-reviewed literature on benzene of any group involved in benzene research: well in excess of twenty articles. The range of issues covered in their benzene articles is even more impressive. The breadth of their research into benzene exposure levels is notable by itself. They have analyzed benzene exposures in refineries, petrochemical plants, chemical manufacturing sites, printing facilities, marine transport barges and terminals, gasoline terminals, and railroad roundhouses.10 In


10 Unlike many other studies of similar locales, these reports certainly provided the “proper examination of benzene exposures” at Exxon facilities. In conclusions consistent with their other toxic substance publications prepared for industry, these articles determined that, for many years, workers at Exxon plants have not been exposed to sufficient benzene to cause leukemia. Marisa I. Kreider, et al., “Benzene Exposure in Refinery Workers: ExxonMobil Joliet, Illinois, USA (1977-2006),” Toxicology and Industrial Health 26, no. 10 (2010): 671–690 (refinery); Shannon H. Gaffney, et al., “Occupational Exposure to Benzene at the ExxonMobil Refinery in Beaumont, TX (1976–2007),” International Journal
addition, other articles discuss the benzene airborne concentrations related to gasoline, certain drinks, and a number of solvents. Yet, as with many other substances, they have not limited themselves to one area of research. They can and have provided virtually every type of data and opinion desired by a defense attorney for a toxic substance case. The research methods are just as impressive, including epidemiological case-control studies, modeling of benzene toxicity, exposure analysis, general methodological, risk assessment, urine sampling, and reviews of literature.


Until approximately 2000 ChemRisk scientists did not include funding data in their publications. Since then most, but not all, articles have included a funding statement and a Conflict of Interest disclosure. In general, these statements are more complete than for other industry expert witnesses. The vast majority of their benzene-related articles fall into this category—almost all of the potential conflicts involving either ongoing or potential litigation. Most of the articles affirm that funding for the research came from the company whose premises or products are being examined.

Although none of the articles clearly assert that the research commenced at the direction or request of attorneys, several factors point in this direction for virtually all of the articles. Paustenbach and his firm were involved with attorneys in their benzene research as early as 1992. Paustenbach’s first attempt to reanalyze the benzene exposure for the Pliofilm cohort listed H. D. Peterson, as attorney at Bryan, Cave, McPheeters & McRoberts, as the last coauthor. Although an attorney has never again been listed as coauthor, almost all of the issues discussed in Paustenbach’s subsequent articles were being litigated during the period of publication.\(^\text{13}\)

While the nature of the work and the ensuing articles has not changed over the past fifteen years, ChemRisk’s funding and Conflict of Interest statements have evolved. For the first ten years of the new millennium, the funding statements in virtually all cases indicate that one or more corporations paid for the research. Each of the named corporations is (or was) involved in benzene

litigation. The Conflict of Interest statements generally, but not always, disclose that at least some of the authors have, or may, serve as expert witnesses and/or that the client is in litigation.14

For the past few years, ChemRisk articles frequently have indicated they are self-funded. Yet, the article often admits that some authors have served as benzene expert witnesses. Thus, being self-funded does not mean that the papers are prepared independent of litigation. It may simply reflect the fact that Chemrisk scientists have been closely questioned about funding at depositions and trials and now desire to short-circuit this line of questioning. When a consulting firm receives extensive funding from the client for research and a detailed report, preparing an article for publication entails minimal additional expense. In ChemRisk’s case, the articles are often prepared in conjunction with litigation consultation and testimony. In at least a few cases, the “self funded” article was based upon a funded report provided to the client.

In addition, these types of papers are also profit center generators. Virtually all of the self-funded articles were likely useful for Daubert considerations and for regulatory comments. Publication is extremely important when a company seeks work from attorneys. Whether the individual being considered has published and can rely upon articles covering the areas at issue in

14 Amy K. Madl, et al., “Airborne Concentrations,” 1945 (Diesel fumes) (Funded by Union Pacific Railroad Company and Norfolk Southern Railroad. The research was “conducted in preparation for serving as expert witnesses in litigation . . .”); Amy K. Madl, et al., “Airborne Concentrations of Benzene and Mineral Spirits,” 1965 (Stoddard solvent) (Funded by Union Pacific Railroad Company and Norfolk Southern Railroad. The research was “conducted in preparation for serving as expert witnesses in litigation”); Pamela R. D. Williams, et al., “Reconstruction of Benzene,” 742 (Partially funded by the API. There is no mention of experts.); Pamela R. D. Williams, et al., “Benzene Exposures,” 586 (Funded by ExxonMobil who is involved in litigation. No mention of expert witnesses.); Pamela R. D. Williams, et al., “Characterizing Historical,” 349-50 (Some data and analysis was “funded on behalf of client involved in litigation.” The writing was self-funded. There is no mention of expert witnesses.); Pamela R. D. Williams, et al., “Airborne Concentrations,” 561 (Funded by U.S. Steel Corporation. At least two authors are expert witnesses); Pamela R. D. Williams, et al., “Occupational Exposures Associated,” 572 (Funded by ExxonMobil who is in litigation. At least two authors are expert witnesses); Julie M. Panko, et al., “Occupational Exposure to Benzene,” 529 (Funded by ExxonMobil and at least two expert witnesses); Shannon H. Gaffney, et al., “Occupational Exposure to Benzene,” 300 (funded by ExxonMobil. At least two authors are expert witnesses); Marisa L. Kreider, et al., “Benzene Exposure,” 689 (Funded by ExxonMobil as “part of a larger effort to understand benzene exposures in several domestic ExxonMobil refineries.” There is no mention of litigation or experts.); T. E. Widner, et al., “Airborne Concentrations of Benzene,” 157 (Funded by ExxonMobil, “a firm that has been involved in the study of benzene for several decades.” “At least two of the authors have served, or are likely to serve, as expert witnesses”); and Shannon H. Gaffney, et al., “Occupational Exposure,” 185 (Same funding and COI comments as Widner article, supra.).
Daubert hearings and regulatory actions is a paramount factor for attorneys when selecting an expert.\textsuperscript{15}

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A 2013 article—Rachel M. Novick, et al., “An Analysis of Historical Exposures of Pressmen to Airborne Benzene (1938–2006)”—demonstrates how this is accomplished. The article discusses potential benzene exposure in pressmen. Although described as “self funded” the authors clearly anticipate that both they and it will be used at trial; it could even be considered a solicitation for business.

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Conflict of Interest
All of the authors are employed by ChemRisk, a consulting firm that provides scientific advice to the government, corporations, law firms, and various scientific/professional organizations. ChemRisk has been engaged by entities involved in the printing industry. This paper was prepared and written exclusively by the authors without review or comment by employees or counsel for those entities. It is likely that this work will be relied upon in industrial hygiene research, risk assessment research, and litigation. Some of the authors may be called upon to serve as expert witnesses.\textsuperscript{16}

The funding section carefully informs the reader that no one outside of ChemRisk reviewed the article’s contents or suggested or asked that it be written. However, it also notes that they have “been engaged by entities involved in the printing industry.” Thus, when considering whether this

\textsuperscript{15} Jennifer Sahmel, et al., “The Role of Exposure,” 835 (Self funded. There is no mention of expert witnesses, even though Paustenbach is a coauthor); Dennis J. Paustenbach et al., “Comparison of Modeled,” 298 (Self funded. An author is an expert witness); J. J. Keenan, et al., “Gasoline: a Complex,” 294 (Self funded. Some “authors have served as expert witnesses”); David Galbraith et al., “Benzene and Human Health,” 39-40 (Self funded. Some authors are expert witnesses); Jeffrey S. Knutsen, et al., “A Calibrated Human,” 12 (Self funded. Two authors have been consultants and/or testifying experts” for benzene.); Dana M. Hollins, et al., “Airborne Benzene Exposures,” 332 (Self funded. Some authors have or may serve as benzene expert witnesses); and Rachel Novick et al., “An Analysis of Historical Exposures,” 11 (Self funded. Some authors “may be called upon to serve as expert witnesses).

paper might be biased notwithstanding the “self funding,” three factors must be considered. First, the article may be based upon a report previously provided to and edited by the attorneys. Second, if the facts and opinions expressed in published article had not been beneficial to their industry client, its attorneys would likely not continue to retain ChemRisk, since their usefulness at trial would be compromised. Novick’s paper implicitly recognizes the importance of this by noting it may be relied upon in litigation. Finally, ChemRisk scientists have been writing papers for attorneys for over twenty years, with a perhaps unparalleled record of excellent service to their clients. Among these services is a consummate ability to anticipate needs requiring their work. They are well versed in what is generally required by attorneys, especially for a general issue such as this. In a corollary to this, ChemRisk attorneys are also well versed in the machinations of Daubert hearings and how to provide the evidence required by their client to both gain acceptance of its experts and find fault with opposition experts. They fully understand that the benzene papers they produce are extremely beneficial at both hearings and trial. Thus, there is little need for attorney supervision to ensure the articles written by ChemRisk scientists meet the needs of litigation requirements.

Since 2000, Paustenbach and his ChemRisk colleagues have provided benzene industry defense counsel with excellent expert witnesses backed by a cornucopia of well-written, peer-reviewed articles. The articles have provided critical components to the defense strategy of limiting the ability of plaintiffs to show sufficient exposure, limiting the diseases at issue, and making the methodology necessary to pass Daubert muster both complex and costly. In using the articles, ChemRisk experts have consistently limited any admissions of possible hazard, while expressing certainty in their industry-oriented positions. Selective analyses, critiques of adverse studies, and proposed rigid methodology protocols have enabled ChemRisk experts to contest almost any benzene claimant’s disease and exposure. These traits are particularly visible in two areas of ChemRisk publications—examinations of exposures at refineries/petrochemical plants and studies
of exposures to benzene as a solvent—both of which will be discussed in the next section of this chapter.\textsuperscript{17}

\textit{Otto Wong and Richard D. Irons}

Nevertheless, Paustenbach and ChemRisk have had minimal input in the benzene industry’s newest and largest research effort in the early twenty first century, industry’s Shanghai Health study. As we shall shortly see, two other entrepreneurs/scientists have led the effort in this study. Their efforts have been as critical—some might say even more essential—then ChemRisk’s in conducting research aimed at developing litigation-ready evidence. The first, Dr. Otto Wong, has been assisting the benzene industry for approximately the same period as Paustenbach. The second, Dr. Richard D. Irons, is a Professor of Toxicology in the Department of Pharmaceutical Sciences at the University of Colorado at Boulder. He also owns the consulting firm Cinpathogen.

Wong first began work related to benzene during the 1980s, when he reported that chemical workers exposed to benzene could contract leukemia. Yet, a year later, when the Louisiana Chemical Association contracted with him to evaluate industry data related to excess cancers along the Mississippi, Wong found no linkage between any cancer in the neighborhoods and industry. He instead called for “more quality research.”

Three years after the work for the Louisiana group commenced, Wong learned of the inherent problems in writing an article critical of a client. At the time of his hiring by the Louisiana Chemical Association, Wong was also chief investigator for an industry-sponsored study of vinyl chloride. In 1991 he published an article describing excess brain and liver cancers in the industry. The sponsoring group believed that his failure to provide the draft to them for review before

\textsuperscript{17} The same limitations on conducting exposure evaluations do not apply to defendants, they can and will spend whatever it takes to demonstrate insufficient exposure. For example, in one case ChemRisk billed Ashland, Inc. for 400 hours of work by 12 scientists estimating a plaintiff’s benzene exposure. “New York Benzene Lawyers Request Sanctions in AML Lawsuit,” PRWeb, \url{http://www.prweb.com/releases/2007/08/prweb544199.htm}, (accessed June 14, 2014).
publication betrayed their trust and called on him to provide a clarifying letter to the editor. In a subsequent letter that surprised the journal’s editor, who believed the original paper was very reasonable, Wong retracted some of his most damaging findings and concluded that his own interpretation of the data rejected any causal linkage between vinyl chloride and brain cancer. Interestingly, during the same period, even industry toxicologists admitted privately there was at least some linkage between vinyl chloride and brain cancer.\textsuperscript{18}

At times in his articles, Wong treads a close line on ethical issues. In his conflict of interest statements Wong has occasionally admitted working for companies as a consultant/expert. However, unlike Paustenbach he has not revealed that the work was as an expert witness at trial.\textsuperscript{19}

Irons has a long history of working for industry. From 1977 to 1988 he was a senior scientist with the Chemical Industry Institute of Toxicology in Triangle Park, North Carolina, where his interests included benzene. Since then he has been on the faculty of the University of Colorado, at Boulder, while continuing to consult with industry. Interestingly, the 2004 curriculum vitae on his university faculty page, makes no mention of his outside consultation interests. In January 2007, Irons formalized his consultant practice, founding his consulting firm, Cinpathogen. That year he spoke at a conference directed toward defense counsel for industry toxic substance lawsuits. At the conference, he participated in a session entitled “Introducing Sound Science to the Courtroom.”

Like Wong, Irons’ Conflict of Interest statements are at times vague and do not include his work as an expert witness at trial for the benzene industry. On at least one occasion, Irons,


\textsuperscript{19} See, for example, Otto Wong, Fran Harris, Thomas W. Armstrong, and Fu Hua, “A Hospital-based Case-control Study of Acute Myeloid Leukemia in Shanghai: Analysis of Environmental and Occupational Risk Factors by Subtypes of the WHO Classification,” \textit{Chemico-Biological Interactions} 184 (2010): 112-128, 127.
announced that the article—like most of his articles, useful for litigation purposes—“was supported by the authors themselves with no third-party funds.”

As already noted, these researchers/experts have fairly homogenous opinions about the toxicity of benzene. Although there are a substantial number of hematopoietic diseases that arise from abnormalities in the blood and marrow, these individuals assert benzene only causes AML—“the weight of evidence from the epidemiology literature is that AML is the only leukemia clearly shown to be elevated.” As one ChemRisk scientist argued, “despite considerable effort and scholarly review, there is no reliable evidence to causally link benzene exposure with any other hematopoietic malignancy, except for specific subtypes of AML.” Thus at trials and in their numerous industry-sponsored publications, industry experts consistently and forcefully maintain that a number of diseases known to be potentially related to benzene have not been proven to have sufficient association for a finding of causation by benzene. These include MM, NHL, CMML, and CLL. Otto Wong and his coauthors have even declared “there is no evidence” [emphasis added] supporting benzene causation of multiple myeloma. When necessary for trial purposes, industry experts have even cast doubt on the benzene connection to more generally recognized benzene related diseases, such as MDS and aplastic anemia (AA).


When AML is the disease at issue in litigation, industry experts—with their peer-reviewed papers in hand—provide a robust defense for all leukemias, even though, as noted above, all medical researchers and scientists now agree that benzene can cause AML at historic high levels. The controversy stems around whether benzene causes cause AML at exposure levels experienced in the workplace during the past thirty years and, increasingly, does it cause the specific type of AML suffered by the plaintiff. At trial and in peer reviewed articles, industry-funded scientists have consistently answered this question in the negative, frequently claiming epidemiological studies have demonstrated—or at least suggested—a threshold level, below which benzene does not induce AML and a limited ability of benzene to cause many subtypes of leukemia.

In using the threshold defense, industry lawyers can almost always find an expert to state that the plaintiff did not have sufficient exposure to reach the threshold level, no matter how high the exposure. As an expert witness, Irons provides a fairly high threshold, testifying that scientists generally agree that 100-ppm-years of exposure can cause leukemogenesis, with reasonable disagreement between 25 and 50-ppm-years. However, if the exposure level reaches even higher than that, defense counsels have other options. For example, David Pyatt, who worked for ChemRisk, has stated that based on Pliofilm cohort he believes the threshold is between 50 and 500-ppm-years, with the best estimate at 200-ppm-years. He reached this conclusion even while a chart he prepared for an article demonstrated that the Pliofilm researchers found an elevated SMR of 2.29 for 5-50-ppm-years and frequent defense expert witness Kenneth Crump reported a 3.25 SMR for 5-50-ppm-years, both of which suggest the threshold—if there is one—is at least as low as 5.0-ppm-

years. Even Paustenbach found a 1.79 SMR for 5-50 years. During the mid 1990s Wong apparently agreed with the higher estimate, although approximately a decade earlier he had reported a two-fold increase in relative risk for individuals with 15-ppm-year exposures.23

Defense experts have also increased the odds of a defense verdict in benzene trials by arguing that even with AML, only certain subtypes are benzene-related, thus dramatically reducing the number of individuals suffering from benzene related AML. Leukemias are discrete diseases distinguishable from each other and even AML has seven or eight subtypes. Defense attorneys can readily find an expert witness—with supporting peer-reviewed literature—to discount virtually all of the subtypes.24

In the end, the industry-sponsored experts agreed with Carl Mackerer of the Mobil Oil Corporation environmental sciences laboratory, who “believes that regulating benzene at levels below 1.0-ppm is unreasonable: ‘It would cost billions of dollars, be extremely difficult for industry to meet, and be of questionable positive health impact.”25 Left unsaid: the fact that litigation costs dramatically impact industry positions and the “questionable” nature of the public health impact being largely due to industry-sponsored studies.

Plaintiff Experts

In general the experts who testify for plaintiff counsel are both more heterogeneous and less certain about the risks of exposure to benzene than the defense experts (even though at least two have also testified for defense counsel in benzene-related cases). Virtually all of them do not believe

24 D. Pyatt, “Benzene and Hematopoietic,” 540. As one trial judge wrote while citing approvingly to Wong’s pretrial hearing testimony, “leukemia consists of different types of diseases that have different patterns and different etiological agents. Simply because the authors group their findings under broad headings like "lymphatic and hematopoietic cancers" does not mean that the authors believe all leukemias are derived from the same source. Tara Austin v. Kerr-McGee Refining Corp., 25 S.W.3d 280, 291 (Tex. Civ. App. June 29, 2000).
benzene has a threshold level below which it does not affect health. In addition, they are much more willing than defense experts to accept government financed studies at face value. Finally, and critically important, unlike the primary defense litigation experts who are conducting defense related research, they are not the primary investigators for the most innovative recent public health oriented Benzene research.

Although numerous experts consult with plaintiff attorneys, four individuals have the best credentials, and—judging from one defense counsel PowerPoint lecture—appear to present the biggest challenge to defendants at trial. In a presentation at Mealey’s November 2007 Benzene Litigation Conference, attorneys from the well-respected defense firm, Shirrmeister Diaz-Arrastia Brem discussed the future of benzene litigation. The talk emphasized the potential of an avalanche of cases if courts accepted the concept of no threshold for benzene disease, with one molecule capable of instigating the disease process. They identified these primary plaintiff experts as advocating this theory: Peter Infante, Bernie Goldstein, Martyn Smith, and Marvin Mehlman.26

The four come from varied backgrounds that are largely distinct from those of defense experts. Peter Infante is a retired federal government epidemiologist who had conducted benzene investigations at NIOSH and OSHA. In the 1970s he co-led perhaps the most important benzene epidemiological research, that of the Goodyear Pliofilm plant. This study confirmed the long-suspected carcinogenicity of benzene. Bernie Goldstein and Martyn Smith serve as professors at the University of Pittsburgh and the University of California at Berkeley, respectively. Goldstein, now professor emeritus, was Dean of Pittsburgh’s School of Public Health from 2001-05. At Berkeley, Martyn Smith has ongoing research projects in Benzene genotoxicity, biomarkers of carcinogenesis, and non-Hodgkin Lymphoma. This research includes collaboration with the NCI in a China

26 Andrew Schirrmeister and Bob Flora, “The Future of Benzene Litigation: Emerging Products, New Types of Exposure, and Where the Cases are Being Filed,” PowerPoint presentation at Mealey’s Benzene Litigation Conference (November 6, 2007), (quoting from Goldstein’s litigation testimony) (in author’s possession).
benzene study. While having acted primarily as plaintiff witnesses in benzene trials, in certain specific fact situations, both Goldstein and Smith have served as expert witnesses for defendants.

Mehlman is possibly the most unique figure of the four. He came from an industrial background, having been chief toxicologist at Mobil, until fired for his opinions about benzene hazards in the 1980s. He did not accept the firing meekly. He sued, claiming Mobil fired him because he informed its Japanese refinery they had dangerously high levels of benzene in their oil. At trial, Mobil argued it fired Mehlman because he used company personnel, time, and equipment over a course of years in connection with a publishing firm owned by Mehlman’s wife. They also argued that Mehlman’s allegations should not be believed because the Japanese refinery gasoline levels of benzene were not as high as Mehlman claimed he had been told.

During the trial, Mehlman—who Mobil had nominated for membership in the National Academy of Sciences a few months before his dismissal—testified he had traveled to Japan in September 1989 for a conference. While there, he visited the refinery owned by a Mobil subsidiary, and was told their benzene levels in gasoline exceeded 5%. Mehlman then informed local company officials that those levels could result in disease and must be reduced. Within a day of his return to the United States, Mobil started the pro forma investigation into activities that Mehlman insisted company officials had been aware of for years. The jury obviously believed Mehlman rather than Mobil. In March 1994 it awarded Mehlman $7 million in lost wages and damages for his discharge from Mobil Oil five years previously.\(^27\)

\[\textit{Peter Infante}\]

Peter Infante’s views on benzene solidified prior to his agreement to testify for any plaintiff counsel. Most of his general opinions concerning benzene toxicity formed well before his retirement

\[\text{\cite{27, Constance Holden, “Fired Mobil Scientist Awarded $7 Million,” Science 264 (29 April 1994): 656.}}\]
from NIOSH and OSHA in 2002. As a government official he had forcefully argued that repeated calls for more research should be characterized as “Take no protective action until definitive evidence becomes available.” (emphasis in original). Both his writings and his expert testimony reflect this belief. He understood definitive proof for many benzene diseases was not available, but argued that, given the rarity of the various blood related cancers and the ability of benzene to both cause complex changes in the marrow and affect different people in different ways, the weight of the evidence required stronger regulatory action and the acceptance of causation in the courts. As early as 1993, when he was with OSHA’s Health Standards Program, Infante believed that benzene in gasoline caused lymphopoietic cancers (LPC), particularly leukemia and multiple myeloma (MM). Today, he also argues that benzen’s effects extended to most other hematopoietic cancers. Infante has been passionate about his beliefs, even castigating IARC for its 2009 findings of limited evidence—rather than sufficient evidence—for benzene’s causation of acute lymphocytic leukemia (ALL), non-Hodgkin’s lymphoma (NHL), chronic lymphocytic leukemia (CLL), and multiple myeloma (MM), when the members admitted that in the relevant meeting they did not have time to consider all of the data.28

Throughout his career, Infante fully accepted the concept of no threshold level for injury by cancer causing substances. In 1987, Infante decried the attempts to claim a “no effect level” for benzene. More recently, he has contended that existing risk assessments may set the dose response curve too high. Infante based this opinion upon his research while in government service and records that were not available to his original landmark benzene study at the Goodyear Pliofilm facility. These records contained information about several Pliofilm cohort leukemia cases not

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considered in Infante’s original study. In a recent paper discussing his Pliofilm research, Infante maintained that the additional cases “suggest that low-level and particularly low cumulative benzene exposure and risk of leukemia may well be underestimated when relying upon dose-response analysis derived from the high level exposed Pliofilm cohort.” His conviction was further bolstered by his discovery that same year of a case of take-home (disease caused by benzene taken home on clothes) leukemia in the wife of a Pliofilm cohort member.29

Since his retirement from government service Infante has not conducted original research, restricting his writing to reviews and analysis. These have included examining cancer risks in a British cohort, industry suppression of knowledge, the incompleteness of IARC’s review of benzene, the identification of leukemia cases missed in the first Pliofilm study, and benzene as a cause of multiple myeloma. Similar to most defense witnesses, in his articles Infante often did not disclose funding or indicate that he had acted—and continues to act—as an expert witness for plaintiff counsel. Since most of the articles are extremely helpful in areas of plaintiff cases, such as corporate knowledge, or in the case of the MM study, proving causation, the appearance of bias must be considered. Neutral observers who read Infante’s articles thus have no means to recognize these potential biases.30


Bernard Goldstein’s writings have argued the probability of a benzene association with a number of blood related cancers. These opinions evolved over time. Even today, he recognizes that while several other cancers are probable, they may not be absolutely proven. For example, he has agreed that “the causal relation between benzene and MM remains unproven, there are sufficient data to make this association highly probable.” In a 2010 article he further maintained that there is “increasing evidence of a close relationship between lymphoid tumors and the types of myeloid tumors known to be caused by benzene.” This belief in the wide cancer-causing effects of benzene results from “the relatively non-specific mechanism capable of producing multiple chromosomal changes, and there is evidence that the early hematopoietic stem cell, which is believed to be targeted by benzene in causing myeloid cancers, is also the progenitor of lymphocytic cell types.”

Goldstein argues that a major reason for the lack of non-industry studies on other benzene related cancers is due to benzene already being listed as a known human carcinogen. “Because benzene is a known cause of human acute myelogenous leukemia there has been little reason for organizations such as the International Agency for Research on Cancer (IARC) or the US National Toxicology Program (NTP) to perform standard hazard identification reviews of benzene as a possible cause of other cancers such as lymphomas.”

PubMed does not contain any articles in which Goldstein specifically discusses the possibility of a threshold for benzene when the keywords Goldstein, benzene and threshold are entered. In his appearances at trial, he has maintained that knowledge of exact dose is not as important as taking the approach of a diagnostic doctor, who will often link a disease to a substance.

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33 Bernard D. Goldstein, “Benzene as a Cause,” Abstract 147.
by a work history without full knowledge of the specific dose of exposure. In 2011 he suggested the possibility that the dose-response curve for benzene and AML may be supralinear, meaning that the potential of benzene to cause disease is higher at low doses than would normally be expected under current no-threshold linear dose-response models. In this paper Goldstein and colleagues discussed how the Clean Air Act has helped reduce the risk of AML. After providing environmental and smoking exposure rates for benzene, they concluded the Clean Air Act had a tenfold greater impact on reducing AML rates then does smoking cessation. This suggestion is consistent with testimony Goldstein has given during litigation: “[I]f it’s a question of is there any risk at 1 molecule . . . you could argue that theoretically there’s a risk of 1 molecule.”

Yet, Goldstein presents an interesting and complex case. Although Goldstein believes that low doses of benzene can cause cancer, he has still critically examined epidemiological studies that find such an occurrence. He wrote at least one letter to the editor suggesting a potential major flaw in its calculation of its determination of the dose response. Although used by plaintiff counsel, in his articles he is often circumspect. In 1990 Goldstein examined the Pliofilm follow-up and concluded, “Overall, these findings are not sufficient to make an unequivocal statement that benzene is a cause of multiple myeloma. G30). 34

In addition, contrary to defense witnesses, Goldstein’s opinions have changed over time as more information about the mechanistic and genotoxic actions of benzene have been discovered. One of his most recent papers concludes, “Benzene should be considered a cause of human lymphoproliferative disorders.” He reached this conclusion based upon not only the epidemiological

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evidence, which he admitted is inconsistent, but also on the increasing mechanistic recognition of
how the diseases arise and how benzene affects the relevant systems. In reaching this conclusion he
did not ignore industry studies, rather using them and explaining how they actually provide some
support for his position. He also recognized that the strength of evidence regarding lymphocytic
neoplasms has been a long-standing issue. As his strongest argument for benzene’s connection to
numerous neoplasms, this article was open to criticism as simply providing plaintiff attorneys what
they wanted.

Yet, this article was not made out of whole cloth, but rather represented a gradual
evolvement of his opinion over several years as knowledge increases. As further evidence of his
neutrality in litigation, in his Conflict of Interest statement, Goldstein noted that he has served as
expert witness in benzene cases roughly equally for plaintiffs and defendants.

*Martyn Smith*

Since 1985 Smith has authored or coauthored ninety-seven more benzene related articles,
being listed as lead author in seventeen of them. Many of the articles he coauthored are part of the
joint Chinese government NCI benzene studies that commenced in the 1990s. Most of Smith’s
articles concern specific experiments on benzene actions in the body, although a few involve
epidemiological type investigations. A few also more generally discuss benzene causation of
diseases.\(^{35}\)

Smith’s convictions have not been as certain or rigid as those of the defense experts. In his
2010 review of advances in knowledge about benzene health effects, he wrote cautiously that
benzene causes not only AML but “probably other hematological cancers.” He argued that “the

\(^{35}\) Pubmed search on July 18, 2014 of terms (Smith MT[Author] and benzene). (Hard copy in possession of author.)
consensus clearly shows that benzene causes AML/ANLL [acute non-lymphocytic leukemia] and MDS, even at relatively low doses, and that AML often arises secondary to MDS.”

Smith has also argued that study biases could hide some association to cancers such as non-Hodgkin lymphoma, while “evidence for an association with childhood leukemia is growing.” The ultimate basis for his belief of the multiple disease nature of benzene came from the multiple alterations in the blood and marrow that researchers have found with benzene exposure, likely a result of a multimodal mechanism of action for benzene toxicity.

However, Smith has acknowledged that there is not sufficient proof to demonstrate a benzene connection to certain leukemias. He has even testified for defense counsel that there are no published epidemiological studies linking APL to benzene. Furthermore, he has found no scientific basis to equate APL with AML. 36

This opinion might be subject to change. More recently, Smith has argued that evidence for malignancies other than AML has grown steadily. He based this opinion not only upon epidemiological studies, which he characterized for some of the diseases as mixed, but also on animal, mechanistic, and other studies. He also noted that German researchers, as well as an international committee of experts, “have concluded that benzene could cause any malignant hematologic disease because these diseases all arise from damaged omnipotent stem cells.” Even so, Smith has been tentative in this latest opinions; only stating that benzene “causes acute leukemia and probably other hematological cancers.” This is not the opinion of an individual longing to testify at trial. 37


37 Martyn T. Smith, “Advances in Understanding,” 133, 137.
Unlike most defense-oriented authors, Smith has also acknowledged the “striking variation in benzene toxicity among workers with comparable levels of occupational exposure.” In the review discussed various factors, including confounders, which might be involved in this variation. Smith believed that it is this variation that makes the epidemiology so difficult, but also holds the promise that exposure biomarker development might further “elucidate” the etiology of diseases likely caused by benzene.\textsuperscript{38}

In his writings Smith further argued that benzene affects the blood system even at low occupational exposure, with no evidence of a threshold. Therefore “[t]here is probably no safe level of exposure to benzene, and all exposures constitute some risk in a linear, if not supralinear, and additive fashion.” Still, he admitted there are numerous questions about benzene, including whether there is a threshold.\textsuperscript{39}

Smith’s 2010 review of benzene’s health effects provides an excellent example of how his writings contrast with those from ChemRisk authors. The first distinction is the use of quantitative and qualitative adjectives. In contrast to ChemRisk articles that described reported benzene associated cases between 1930 and 1960 as “occasional,” Smith characterized them as “many.” Unlike Chemrisk scientists, after examining the epidemiological data, Smith found “no evidence of a threshold cumulative exposure” level for benzene malignancies. While noting that various consultants (including Paustenbach of ChemRisk) have reanalyzed the Pliofilm reanalysis intending to influence regulatory or legal proceedings, Smith submitted that subsequent studies in China and


\textsuperscript{39} Martyn T. Smith, “Advances in Understanding,” 136, 142.
Australia “confirmed and expanded” its findings, although they too, have been subject to consultant criticisms. ⁴⁰

In several of his articles, Smith disclosed that he has received consulting fees from attorneys on both sides of the issue, as well as fees from the governments of Australia, New Zealand, and the United States. ⁴¹

Myron A. Mehlman

As evidenced by his stand on benzene hazards while in Japan, while at Mobil. Mehlman recognized the seriousness of benzene exposure. Yet, during his employment at Mobil, Mehlman’s articles displayed some of the traits normally found in industry generated articles, such as a call for more research.

For example, while at Mobil Mehlman held out the hope that there might be a threshold for benzene’s effects. In 1985—much to Infante’s annoyance—he suggested the need for studies to determine if bone marrow lesions occurred only down to certain levels, below which there might be no effect.” ⁴² In a 1987 response to Infante’s criticism, Mehlman wrote that he agreed with Infante that there is a “qualitative relationship between benzene exposure and leukemia. He believed, however, that “many scientists believe that a level of 10-ppm is adequate” to reduce the occupational level of benzene-caused leukemia to an acceptable level. Citing a Goldstein article, he argued that more research should be conducted prior to further regulatory action. ⁴³ The following year he argued that current rodent studies on gasoline did not use the properly characterized gasoline

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⁴¹ Martyn T. Smith, “Advances in Understanding,” 143.
vapor composition, thus may not be relevant to human exposures. “[A]dditional data” he concluded “are needed to resolve the question of the effect of gasoline vapor on human health.”

A 1989 article, coauthored by Mehlman, revealed some of the equivocality of his views concerning benzene hazards during his tenure at Mobil. The article reviewed benzene hazards and exposures in gasoline. The paper referenced an OSHA allowable limit of 10-ppm, suggesting that the article was prepared prior to the limits reduction, and Mehlman was constrained in his public writings by company policy. Even so, the article eschewed the certainty found in many industry writings. Mehlman recognized that benzene caused more than one type of leukemia, and remained uncertain about the potential of a threshold. “Of major concern is the well documented increase in various types of leukemia, especially acute myelogenous leukemia. . . . Uncertainty exists regarding the dose-response relationship and potential for a threshold for carcinogenicity with benzene.”

The views taken in a 1991 commentary leave open the possibility that Mehlman’s prior public proposals for more research may have been attempts to keep higher powers in Mobil happy. In this essay, Mehlman encouraged regulatory agencies to take a tougher stand, taking almost the exact opposite position from his 1985 proposition. Instead of urging further study before action, he now urged reducing allowable exposures specifically because data was not available at lower doses, but "we do not know of any safe level above zero." In fact, at 0.004 to 0.1-ppm, his recommended regulatory levels were orders of magnitude below even the current standard.

Since leaving Mobil, Mehlman became increasingly supportive of broadening the diseases medical practitioners recognize as caused by benzene. Shortly after being fired by Mobil in

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November 1989, Mehlman wrote a series of articles discussing the “dangerous and cancer-causing properties of products and chemicals in the oil refining and petrochemical industry.” It is likely these articles resulted from two intertwining motivations: a desire to hit back at Mobil for his firing; and a desire to finally express his true beliefs about petroleum, unrestrained by his official position. Given the tentative nature of his previous writings and his strong actions in Japan, I believe the later provided more of an impetus, especially since he was still in litigation with Mobil and the articles could potentially be used against him at trial.47

In 2006 Mehlman wrote research reviews in the *Annals of the New York Academy of Science* about hematopoietic cancers. In them he contended that, without question, benzene exposure was significantly related to types of leukemia and lymphoma other than AML. In one review he maintained there was sufficient evidence to reasonably conclude that products containing benzene were causally linked to non-Hodgkin lymphoma. A second review concluded that benzene products were significantly related to morbidity and mortality from CML.48


Mehlman began testifying for plaintiff counsel in benzene related personal injury cases no later than 1997. He continued to serve into the 21st century—at least occasionally—as an expert plaintiff witness for benzene causation of diseases, including CML.\(^49\)

In the new century, Mehlman published another series of articles about the perils of benzene. Although the articles provide neither funding nor conflict of interest information the topics, their strong conclusions suggest they may have been written for litigation purposes. His failure to note a past, and possible ongoing availability as plaintiff expert witness is especially incongruous in light of his comments concerning industry paid consultants.

The differences in interpretation of findings between industry and industry sponsored research and independent, usually university-based researchers are becoming well known, and the bias in favor of industry by paid consultants has been noted by many individual investigators in the field. These differences in conclusions have prompted policy statements from the NTP as described above, and such findings are leading to more complete disclosure in scientific journals and more careful scrutiny of negative results in industry studies. Biases in science based on profit motivation of industry does not only harm workers in the field but also the population in general by not warning about exposure to toxic and cancer-causing substances with attendant increases in cancer risk for all.\(^50\)

Before turning specifically to examine how attorneys and their surrogates have influenced consideration of the issues, I will first provide background information about the growth of early medical knowledge concerning benzene’s chronic toxic effects, as well as the role government and industry played in that growth.


Historical Medical Knowledge

To the manufacturer, the introduction of this cheap and powerful solvent may seem an advantage; to the physician, interested in the producer more than in the product, it can only seem a disastrous innovation in industry.
– Alice Hamilton

As with the case of chromium, medical practitioners began reporting benzene-related health issues well before the twentieth century. In 1862, less than two decades after its commercialization, the British medical journal *Lancet* described benzene as a “new domestic poison.” The first reported case came from Sweden, where nine young women working in a bicycle tire factory in Uppsala, Sweden died after using benzene rubber cement for varying periods of three weeks to four months. The reporting doctor specifically noted the decreased number of red blood cells and the almost complete leukopenia (loss of white blood cells) in the victims.

Beginning in 1916, occupational medicine pioneer Alice Hamilton wrote a series of articles providing the first thorough report on the health effects of benzene. By 1922 she could provide accounts of a plethora of benzene poisoning cases from the United States and Europe. The first reported case in the United States came in 1910 from Maryland, where several young woman using a benzene-based sealing mixture died in circumstances similar to the Swedish women. Over the next few years, doctors reported several more cases from the same can factory. Other cases from across the United States quickly followed. During the century’s second decade, additional European cases appeared first in German publications, then in official British publications. These cases included accounts of illnesses from industrial exposures to benzene fumes in empty tanks and barrels, some of which had been thoroughly cleaned, in at least one case having been washed twice with steam and

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thrice with water. Significantly, even at this early date, a few of the articles specifically noted the differences in susceptibility between individuals.52

As transpired with asbestos, the National Safety Council (NSC), a health and safety organization established by industry, provided early warnings to its members about benzene’s toxic nature. In 1926 the NSC issued a report on benzol—the German term for benzene. The manuscript listed the doses known to cause narcosis and weight loss in animals. As it further noted, “the most characteristic pathological effect of benzene is perhaps its destructive influence upon the cells of the blood and the blood forming organs.”

Along with the warning, the Council also provided a few specific examples of the danger, referencing many of the case reports of deaths and injuries from industrial exposure and remarking on the blood problems associated even if lesser exposures. In one case of eighty-one workers studied, twenty-six “had a blood picture characteristic of benzol poisoning; and this ratio of about one man in three affected was maintained even in those workrooms with efficient local ventilation . . .” Even at this early date, the Council recognized the toxicity of even low levels of benzene: “We were therefore forced to conclude that . . . the use of benzol (except in enclosed mechanical systems) even when the workers are protected by the most complete and effective systems of exhaust ventilation . . . involves a substantial hazard.”53

Although early twentieth century medical practitioners described repeated cases of benzene toxicity, they did not understand that the acute diseases they kept finding hid the longer chronic


effects of benzene, such as leukemia. A few even reported using benzene in the treatment of leukemia. In the words of one correspondent, “One is impressed with the drug as a powerful agent—a two edged sword, which is apparently a remedy of great promise in leukemia, but which used carelessly may defeat the purpose of its use and produce an equally serious condition, namely, aplastic anemia, hypoplastic bone-marrow and a fatal termination.”54 Nor was he alone in his opinion. As another doctor opined: “A year ago I expressed the opinion that ‘benzol is a remedy of remarkable potency in myeloid leukemia.’ In spite of all that follows which may seem to argue to the contrary, I am convinced that it is the most helpful of all known agents in the treatment of leukemia of this type.”55

Yet, even while some doctors touted the benefits of leukemia treatment with benzene, other clinicians and researchers began the process of unraveling the mysteries surrounding the causal connection between benzene and cancer type diseases. In 1917 Doctors Maurice Packard and Reuben Ottenberg noted that the unknown agent that causes leukemia probably acts similarly to the mode by which benzene causes aplastic anemia.56 Eleven years later, the first reported case of benzene induced leukemia appeared.57 Medical literature in the 1930s and 1940s repeatedly warned about the linkage between benzene and diseases, including leukemia.58

Nevertheless, over two decades elapsed following Doctor Packard and Ottenberg’s observations before *The British Medical Journal* could announce, “it is pointed out that the evidence, rapidly accumulating, that chronic exposure to benzene produces leukemia commands serious consideration.”\(^{59}\) Three years later, the American Petroleum Institute toxicological review of benzene noted “reasonably well documented instances of the development of leukemia as a result of chronic benzene exposure.”\(^{60}\) Even then American medical officials remained cautious. As late as 1955 Harold L. Stewart, Chief of the Pathologic Anatomy Branch at the National Institutes of Health would only go so far as to write, “[b]enzene deposits in bone marrow may initiate leukemia.”\(^{61}\)

Thus, the exact year in which benzene action as a causal agent for leukemia became generally accepted cannot be identified. Throughout the 1960s journals published additional case studies of benzene-induced leukemia from the United States and Europe. By 1964 one medical reviewer, citing to a 1958 book and a 1961 article, could report that “the leukomogenic action in man of . . . benzol has long been accepted . . .” By the late 1960s numerous other American medical journals and textbooks were reporting of benzene’s carcinogenic “as established through hundreds of cases.” However, the first epidemiologic studies to discover excesses of leukemia related to benzene did not occur until the 1970s, when one case control study and two cohorts demonstrated increased incidences of leukemia after benzene exposure.\(^{62}\)

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Japanese researchers published the first epidemiological suggestion of a causal connection between benzene and leukemia. In 1971 Ishimaru and colleagues published a case control study of adult survivors form the atomic bomb detonations in Nagasaki and Hiroshima. They matched 303 leukemia cases with 303 controls. They found that individuals employed in occupations involving medical x-rays or utilizing various solvents, including benzene, had twice the risk of contracting two types of leukemia, as did those in other occupations. Chronic leukemia had a 1.8 times higher rate among the studied occupations, while acute leukemia had an even higher rate, approximately 2.9 times higher in the studied occupations.63

The first published cohort study of the decade came about directly due to the establishment of NIOSH and OSHA’s in the early 1970s. These agencies provided the federal government with the money and means to use state-of-the-art epidemiological tools in the effort to better understand benzene’s health consequences. Goodyear Rubber Pliofilm manufacturing plants, located in Akron, Ohio and St Marys, Ohio, a small town 200 miles west southwest of Akron provided an ideal setting for the study. The Akron plant began commercial production in 1936, with the St Marys plant coming on line in the 1940s. In 1948 a second Akron plant replaced the first. The initial Akron plant operated on the second and third floors of a multistory building, sharing the second floor with a balloon fabric production operation that also used benzene. In addition to the longevity of the operations and the size of the cohort, the sites had a stable work force, few confounding factors and good records, all of which improve the ability to conduct the statistical analysis required in epidemiological studies.64


The product itself, Pliofilm, was a thin, flexible, yet strong material similar to the products used today in kitchen plastic wraps. The production process required dissolving rubber in benzene, then converting it to rubber hydrochloride, which was subsequently neutralized and turned into a “cake”. After further processing a casting unit turned it into a film. The entire process reduced the benzene in the product form 90% to 10%. After further drying, the film moved to a finishing operation where it was rerolled, cut, stretched and packed. Benzene levels in the final product were believed to be negligible.65

Two NIOSH epidemiologists, Peter Infante and Robert Rinsky, headed the investigatory team. As designed, the study included all 748 white males who worked in production at the Goodyear Pliofilm facilities between 1940 and 1949, and who were alive on January 1, 1950. Even before the study began, Goodyear officials knew of leukemia cases among the workers, of which they provided at least partial information to the NIOSH team. At the time, Infante believed the company was cooperating fully. As described later in this chapter, he would later learn otherwise. The limited available data exposure data provided by Goodyear indicated workers had been exposed to benzene atmospheric averages of between 10-ppm to approximately 100-ppm benzene during the relevant time period.

The researchers determined each individual’s health status through June 30, 1975 from medical records, with causes of death being identified through death certificates. The study protocol assumed that the cohort members with unknown health status (25%) were alive, thus biasing the relative risk of developing leukemia toward an underestimate. They then compared the observed deaths to the expected deaths from the general US white male population death rates, as modified

for age and calendar period. They also evaluated the death rates against a group of fibrous-glass workers employed in Ohio during the same period of time.66

The study marked the first quantification of benzene’s leukemia risk, detailing extremely troubling risks associated with benzene. When compared to the general population, the cohort had an SMR for leukemia death of 507, (7 observed deaths versus 1.38 expected) with an SMR only slightly better in comparison to the fibrous-glass workers, 473 (7 observed versus 1.48 expected). Four years employment at the plant doubled the risk of leukemia, with the risk increasing in a steep upward line to a thirty-three fold risk increase for those employed at least ten years. The investigators ended their study with a fervent hope for the future. “We hope that our findings, which demonstrate overwhelmingly an increased risk of leukemia in workers exposed to benzene, will stimulate efforts to control occupational and consumer exposure to benzene, an agent known for almost a century to be a powerful bone-marrow poison.” As befitted its status as the first, and many would say the preeminent epidemiological study of benzene, it became a major factor in OSHA’s attempt to lower the eight hour average exposure standard for workers from 10-ppm to 1.0-ppm.67

Although the most thorough study, the Pliofilm cohort was not the sole benzene epidemiological investigation published that year. In another 1977 article, Dow Chemical scientists examined a benzene-exposed cohort from three chemical production areas of a Dow Michigan plant. Superficially, the studies are in many ways remarkably similar. The Dow study included 594 workmen in three benzene production areas of the Michigan division of Dow Chemical, who

worked at the sites for some period between January 1, 1940 and the end of 1970. They obtained medical records and death certificates for an even greater percentage of the cohort than the Pliofilm study. The exposure data was both somewhat better and, on average, lower than the NIOSH study. In a data collection similar to the Pliofilm cohort, the Dow study’s expected deaths were obtained from the U.S. white male mortality for the relevant years. Finally, like the government study, they found a statistically significant \( p = <0.047 \) increase of leukemia among the cohort (3 observed versus .8 expected from the National Cancer Survey) as compared to 7 observed versus 1.38 expected in the Pliofilm cohort. Calculations of the SMRs from the two studies seemingly provided substantial support for the dose response relationship of benzene and leukemia. Thus, the Dow cohort, with lower exposures and diluted with more recent, less exposed, and short latency period employees, had an SMR of just over 40% that of the Pliofilm cohort.

Here, however, the similarities ceased—for whereas the Pliofilm investigators announced their findings as confirmation of the linkage between benzene and leukemia—the Dow investigators prominently proclaimed: “No mortality directly attributable to benzene exposures were observed.” The distinction came not from the data, but in how the studies were designed and analyzed. Initially, the Dow study design intentionally looked at lower but still significant exposures. However, the group was diluted with more recent employees, some with very short latency periods. Over half the cohort commenced work at the sites only after 1950, while a number began work between 1960 and 1970. By including these later workers, the investigators diluted the group in two ways: first, the more recent employees likely had far lower exposure peaks; and two, at least some of the workers had latency periods likely far too short for benzene induced leukemia to have arisen (the article was submitted to the journal on December 30, 1975).

However, the real problem was not with the study design; if it had been followed, the article would have announced confirmation of the Pliofilm study. Rather, the analysis appears to have
departed from the study protocol. This occurred in several ways. First, the researchers only calculated their SMRs for diseases with five or more deaths, even if—as was the case with leukemia—the SMR was statistically significant. Next, rather than beginning the article by calculating SMRs and providing a discussion of the statistically significant findings, they skipped this step and went immediately to examining each individual case for reasons that appear designed to exclude workers with leukemia. Finally, the investigators excluded from the analysis all individuals who had been exposed to vinyl chloride, thus eliminating one of the leukemia cases from consideration, even though as opposed to benzene, vinyl chloride’s linkage to leukemia then and now is at best possibly suggestive. Since the investigators collected medical data on the entire cohort, including those who had been exposed to vinyl chloride, it appears likely the determination to eliminate individuals exposed to vinyl chloride was a later decision, for otherwise they need not have gone through the time and expense of collecting data for the excluded individuals. In the end, instead of announcing a finding of possible increased risk, the Dow employees substantially exonerated their employer. At the conclusion of the paper, rather than suggesting that there might be at least a suggestion of a relationship, they wrote: “varied work histories and the lack of medical history made a retrospective assessment of the possible relationship to benzene exposure very judgmental” (emphasis added), a position very understandable for Dow employees who valued their jobs.68

Although there is no evidence that attorneys were involved in the study or the decision on how to present the data, the paper’s structure and language are such that attorney involvement cannot be ruled out. Certainly, defense trial attorneys would not want to see the publication of a statistically significant SMR for benzene and cancer. In addition, the analysis of the three benzene

cases was very similar to the method by which an good defense trial attorneys attack unwelcome health data, including high SMRs, under cross examination. Although the involvement of attorneys is not known in this case, as we shall shortly see, this type of deconstruction of statistically significant findings by Dow and other company agents has become common, often with considerable attorney involvement.

In an unusual distinction between benzene and most other toxic substances, although numerous case reports of blood-related cancers in benzene-exposed workers appeared throughout the twentieth century animal studies did not confirm its carcinogenic potential, providing but a hint of a relationship. To some degree, this is due to the limited number of studies—all of which were by means of injection—conducted prior to the 1970s. In a 1932 study 8 out of 33 mice developed either leukemia or a lymphosarcoma within 4 to 11 months of having subcutaneous injections. However, the study did not use control animals, making conclusions uncertain. Several subsequent attempts to induce cancer in benzene-exposed animals proved unsuccessful. Thus, at the time of OSHA’s first full attempt to regulate benzene in 1977, it lacked confirmatory animal evidence.

Confirmation finely arrived in the late 1970s with Cesare Maltoni’s carefully conducted experiments, funded by the Ramazzini Foundation. In November 1977 he reported early results indicating the effects. They became definitive in 1979 with rat zymbal gland cancers demonstrated at two dose levels with a dose response relationship. One year later, another researcher reported a statistically significant increase of hematopoietic malignancies in mice. Extensive further testing throughout the 1980s demonstrated cancers in mice and rats in numerous locations.

Beginning in the mid 1980s, the NTP expanded on these studies with broader dose ranges, reporting studies at lower doses. In 2002 the primary investigator for the NTP studies, James Huff of the National Institute of Environmental Health Sciences, wrote a tribute to Cesare Maltoni and David Rall, head of the NTP in which he briefly discussed the studies. In it he noted “carcinogenesis
results of 21 mutually tested chemicals including benzene were compared between the Ramazzini Foundation and the NTP, finding remarkable concordance of overall results . . .” Between 1977 and 1997 the two programs identified 13 target sites of benzene-induced animal malignancies. Thanks to these efforts the seeming benzene incongruity in the mammalian paradigm (human cancer agents also cause cancer in animals) was finally put to rest. Although most scientists welcomed this result, as Huff has explained, the benzene industry was chagrined. According to Huff, industry employees contacted him “persistently”—calls coming at least weekly—in an effort to obtain early results of the benzene bioassays. He recalls finding this amusing since everyone already knew benzene causes cancer (leukemia) in humans and wondering why industry had such high interest in the results. One caller finally provided the answer. In a turnabout to their normal antagonism to animal studies, industry hoped to use the results to question the epidemiological findings and suggest other chemical exposures and workplace confounders were more likely culprits. Instead, the results confirmed the human studies, resulting in a momentary crisis in industry strategy. “When our pancarcinogenesis findings confirmed and complemented Maltoni’s, industry was seemingly taken aback, and momentarily puzzled regarding their next strategy.”

Work Place Regulatory Efforts

“Chemical companies have failed to gather or have withheld from OSHA evidence that would have strengthened the case for a lower work place standard. Yet when the standard is finalized, industry usually has sued OSHA to block its implementation, arguing that there is inadequate evidence of risk.”

- Sidney M. Wolfe

Efforts to control benzene exposures commenced about a decade following medical knowledge of benzene’s hazards. In 1926 the National Safety Council (NSC), not only warned its

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members about the dangers of benzene, it also issued the first national recommendation for protective measures directed at benzene. The NSC urged the use of proper ventilation, enclosed apparatus, gloves, and respirators as necessary systematic medical exams and worker education for all businesses utilizing benzene. One year later, a NSC publication made the first proposal for a benzene exposure standard. The publication’s author, noted industrial medicine authority C. E. Winslow, advocated 100-ppm as an initial limit.

The first formal regulation on the state level of benzene began seven years later, when Massachusetts established the Division of Occupational Hygiene within Massachusetts’s Department of Labor and Industries, with a mission of investigating the toxicity of industrial toxic substances, including benzene. Since the NSC report, it had become obvious that 100-ppm did not protect workers. Therefore, the Massachusetts division initially set a maximum acceptable concentration (MAC) of 75-ppm. They soon reduced the MAC to 35-ppm, one third of the prior recommendation.\(^\text{71}\)

National nongovernmental organizations began their standard setting involvement in 1946 when the American Conference of Governmental Industrial Hygienists’ (ACGIH) recommended 100-ppm as a threshold limit value (TLV). Two years later they lowered the TLV to 35-ppm, subsequently reducing it to 25-ppm in 1963. That same year the American Standards Association, a group of primarily industry related experts, listed 100-ppm of benzene as a safe average limit for an eight-hour workday. It is not clear where they obtained their safety data, since well before 1963 even

industry organizations such as the American Petroleum Institute recognized that 100-ppm was not safe.\textsuperscript{72}

The enactment of legislation establishing NIOSH and OSHA resulted in increased attention to the health consequences of benzene, with resulting reductions in exposure level standards. The first national mandatory standard came about in 1971 pursuant to the establishment of OSHA. The law that established OSHA also allowed for the initial utilization of voluntary standards as the original regulatory levels for substances. Thus, as part of its acceptance of “national consensus standards,” OSHA set the limit at the American National Standards Institute’s voluntary level of 10-ppm for an 8-hour time-weighted-average (TWA) workday, with an acceptable short term ceiling concentration of 25-ppm.\textsuperscript{73}

During this time period, international standards lagged behind those in the United States. In 1974, IARC announced with regard to benzene: “The data reported do not permit the conclusion that carcinogenic activity has been demonstrated.” The agency based this conclusion primarily on the paucity of animal data. With regard to human data, IARC noted, “It is established that exposure to commercial benzene or benzene-containing mixtures may result in damage to the haematopoietic system. A relationship between such exposure and the development of leukaemia is suggested by many case reports, and this suggestion is strengthened by a case–control study from Japan.” Without IARC’s confirmation of benzene’s carcinogenicity, the International Labor Office (ILO) refused to go as far as OSHA when it set it 1978 multi-country benzene exposure level standard. It instead adopted the ACGIH’s recommended level of 25-ppm as an international standard.

\textsuperscript{72} Ilise L. Feitshans, “Law and Regulation, 300; and Peter Drinker, \textit{API Toxicology Review: Benzene} (“In as much as the body develops no tolerance to benzene, and as there is a wide variation in individual susceptibility, it is generally considered that the only absolutely safe concentration for benzene is zero.”)

\textsuperscript{73} Ilise L. Feitshans, “Law and Regulation,” 300; and Mary C. White, et al., “Occupational Exposure to Benzene,” 233.
International recognition of benzene’s proven carcinogenic properties began one year later with IARC’s 1979 identification of benzene as “carcinogenic for humans.” Still, as late as the 1982 meeting of IARC, the association gauged the existing animal benzene data as only “limited evidence of carcinogenicity in experimental animals.” Its subsequent 1987 evaluation the organization extended the finding to include animals.\textsuperscript{74}

In the United States, events during the mid 1970s brought increased pressure on OSHA to lower the benzene limit. In 1974 NIOSH issued a criteria document warning of a likely link between benzene and leukemia. That same year the ACGIH finally lowered its TWA for benzene to 10-ppm. On April 23, 1976 The United Rubber, Cork, Linoleum, and Plastic Workers petitioned the Secretary of Labor to issue an emergency temporary standard for benzene, reducing the limit to 1.0-ppm. Secretary William J. Usery denied the request on May eighteenth, citing a lack of scientific evidence. A few weeks later the National Academy of Sciences issued a report, declaring; “Based on available literature, it can be concluded that benzene may be associated with leukemia; therefore, benzene must be considered as a suspect leukemogen.” Finally, four months after the union’s petition, NIOSH issued a recommendation agreeing with the petition; occupational exposures should be limited to no more than 1.0-ppm as determined by a 2-hour air sample.\textsuperscript{75}

These requests, reports, and recommendations—when combined with the election of Jimmy Carter in November 1976 and his selection of academician Eula Bingham as OSHA Administrator—led the agency to rapidly move forward on benzene regulation. Upon her


confirmation Bingham embarked on a campaign to reduce worker health hazards. Benzene figured prominently among her first initiatives; work beginning on the emergency temporary standard even prior to confirmation. She overcame staff and internal attorney objections to the proposed ETS—they cited to the limited scientific proof of a “grave danger”—by urging Infante to publish his Pliofilm findings at the earliest opportunity. Bingham signed an emergency temporary standard for benzene on April 28, 1977, setting the standard at 1.0-ppm, effective May 21, 1977. That same morning, in what may have been a coordinated action, AFL-CIO attorneys filed a request for judicial review with the Circuit Court of Appeals of the District of Columbia (D.C. Circuit).

The AFL-CIO was not alone in its desire for a judicial review, as the petroleum industry in particular vowed to reverse the decision. In May the American Petroleum Institute challenged the action. Disapproving of the liberal-leaning D.C. Circuit, the group filed in the Fifth Circuit, known for its more “industry friendly” rulings. One day prior to the emergency temporary standard’s operative date, a Fifth Circuit judge issued an interim restraining order, preventing OSHA from implementing the standard. The API’s lawyers next turned to the problem of venue. Although the OSHA statute requires that judicial review occur in the location of first challenge, the petroleum industry’s attorneys urged the D.C. Circuit Court to release jurisdiction because the union’s lawsuit was premature. In a two-to-one decision the court agreed.\(^76\)

Faced with the ETS languishing in the Fifth Circuit, OSHA quickly formally proposed 1.0-ppm as a permanent PEL. The extensive summer hearings generated substantial pushback from industry representatives, as well as their attorneys and consultants. Among the numerous objections, five predominated: 1) there was no demonstrated risk at the current level of 10-ppm—the oil industry designated ten expert witnesses on this issue; 2) a threshold is likely; 3) OSHA made no attempt to quantify prevented cancers; 4) An industry risk assessment demonstrated that the

proposed standard resulted in only 2 fewer cancers in the exposed group; and 5) the analysis did not contain a risk assessment. In addition, the objectors also questioned whether the Infante reports provided sufficiently “conclusive” evidence that benzene is generally leukemogenic in exposed workers.” Five experts were prepared to testify that even after the Pliofilm study, there remained doubt in the medical community about the leukemogenic properties of benzene.\(^77\)

Despite these criticisms, OSHA issued its first final rule for benzene in February 1978, having concluded that the hearing record as a whole removed any doubt about benzene’s ability to cause leukemia in humans and the cost to industry would not be crippling. Other federal and international agencies, including NIOSH, NTP, and IARC, agreed. Thus, with the leukemia-benzene being proven and costs feasible, OSHA officials believed lowering the benzene standard to an 8-hour TWA of 1.0-ppm, with a ceiling limit of 5.0-ppm was both justified and in conformation with the law. OSHA estimated that most industrial benzene exposures had decreased to below that level, but more than 35,000 workers remained at risk. Even with an estimated cost to industry of $500 million, OSHA characterized the health benefits as “likely to be appreciable.”

In issuing the standard, OSHA had not believed it necessary to conduct a risk assessment or cost benefit analysis during the decision-making process. The rationale for this refusal to consider the specific risk was its mandate to protect the health of workers by ensuring that businesses keep exposures at “safe” levels. Since safe levels of exposure for cancer-causing substances cannot be determined, once it becomes clear that workers are at risk, no further calculations or characterization of risk are necessary. Thus, in establishing a lower benzene standard, OSHA simply determined that

\(^{77}\) Notice of Intent to Appear in the hearings for OSHA Docket No. H-059 In re Proposed Revised Permanent Standard for Occupational Exposure to Benzene, on behalf of American Petroleum, et al., 14, 24-5. In addition to the American Petroleum Institute, numerous oil companies joined in the notice, including Exxon Corporation, Shell Oil Corporation, and Texaco Inc. The knowledge concerning benzene hazards that these three companies had at the time of the hearings will be further discussed infra.
the best available scientific evidence proved workers exposed to the chemical had an elevated risk of leukemia, with 1.0-ppm being the lowest feasible setting for the standard.78

For the most part, participants in the rulemaking did not challenge benzene’s capacity to cause leukemia. However, both labor unions and industry challenged the rule in the Fifth Circuit: labor because the limit was too high, and industry primarily due to the high cost to businesses and the lack of necessity for lowering the standard. Nor did the Fifth Circuit voice concern about the agency’s finding of benzene’s leukemogenic properties. Rather, it unanimously agreed with industry’s position that OSHA’s statute required a “reasonable relationship” between the health benefits provided by a standard and the economic costs.79

Litigation thus moved on to the Supreme Court. Finally, in 1980, after an almost three year legal battle, the Supreme Court’s decision in Industrial Union Department v. American Petroleum Institute (Benzene Case) permanently vacated the standard. The ruling did not come easily. If the number of opinions from the nine-member court—five—and the opinion with the largest number of unqualified signatures—Thurgood Marshall’s searing dissent—mean anything, the discussions in chambers must have been impressively raucous. None of the justices could assemble four colleagues who agreed on the controlling factors and principles in the case.

In the plurality opinion to which only two other justices joined—one other, Justice Powell, wrote a concurring opinion—Justice Paul Stevens ruled that the administrative record did not support the rule. He found that OSHA based its regulation on the belief that there was “no safe level”—similar to the API’s 1948 belief—and set the standard at the “lowest feasible level,” without

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regard of the specific benefits. OSHA had neither determined that a “significant risk” mandated further protections or even included evidence in the record that demonstrated such a risk. Alarmingly, at the possibility of businesses being forced to spend millions of dollars for an indeterminate benefit, Stevens held that any new rule lowering an exposure limit must be based upon “substantial evidence” of “significant risk” above that level of exposure. The Justice further explained that “the requirement that a ‘significant’ risk be identified is not a mathematical straitjacket . . . So long as they are supported by a body of reputable scientific thought, the Agency is free to use conservative assumptions in interpreting the data . . . risking error on the side of over-protection.”

Justice Stevens’ opinion would not have been controlling but for Justice Powell’s concurrence and Justice Rehnquist’s distaste for the entire statute establishing OSHA. Justice Lewis Powell’s opinion agreeing in the result echoed the Fifth Circuit’s ruling, focusing on the agency’s failure to conduct a cost benefit analysis. Justice William Rehnquist agreed with the plurality in so far as the result, but none of the reasoning. He considered the entire law unconstitutional for being impossibly vague. The statute contained no objective standards by which the Administrator’s actions could be judged.80

A day following the opinion’s publication The Washington Post reported “industrial elation and regulatory panic.” The potential breadth of the opinion certainly justified at least a modest form of these reactions. The regulatory standards issues considered in the Benzene case echoed throughout regulatory practice. Yet, the very agency most affected, OSHA, exhibited little perturbation. The reason is basic to politics. The new conservative administration had cleaned house at the agency and

labor department upper echelons, installing an industry friendly Secretary and Director. Further
action on benzene thus stalled in the Reagan administration until repeatedly kick started.

This was not an unusual occurrence for the republican administration. Reagan had won the
White House partly on a platform of reducing regulations, particularly those that affected large and
small business. Throughout his presidency he appointed numerous officials who dismissed
environmental issues as unimportant. Although Reagan was known as the Teflon president, during
his terms in office at least one environmental regulator resigned in disgrace due to industry

A consortium of labor unions and public health groups provided the first kick. On April 14,
1983 they petitioned new OSHA administrator Thorne Auchter to issue an ETS. Although this
attempt ultimately failed, it caused a limited turnover of the administration’s regulatory engine.
When Secretary Auchter denied the petition on July 1, 1983, he also declared that new data,
including animal studies, justified expedited reexamination of a lower benzene PEL.

Unfortunately, the resulting ignition ended in a misfire. Rather than put itself in the middle
of the dispute—and possible anger the administration’s industry allies—in October OSHA urged
negotiation by the stakeholders. Labor sent officials from the AFL-CIO, the United Steelworkers of
America, the Oil, Chemical and Atomic Workers International, and the United Rubber Workers.
Industry representation came via industrial trade organizations; the Chemical Manufacturers
Association, the Rubber Manufacturers Association, the American Iron and Steel Institute, and the
API. The man chosen to mediate the negotiation, Philip J. Harter, was very familiar with the
proposal. In his previous year’s law review article, he had recommended just such negotiation as a
means to alleviate rancor in rulemaking. In the field, however, the proposal did not work as well as on paper. The meetings ended in March with the parties unable to reach agreement on a comprehensive plan, the agreed requisite for providing a proposal to OSHA. Commentators have proposed various explanations for the failure—among them apathy by OSHA, the resignation of OSHA’s enthusiastic Administrator and subsequent appointment of a man deeply distrusted by labor, both sides believing they could do better through litigation, and the intrusion of politics caused by the nearness of the next presidential election.  

After fruitlessly waiting through the presidential campaign, in late 1984 labor again used the courts in an effort to kick the regulatory engine to life. On December 10 the United Steelworkers of America asked the D.C. Circuit Court of Appeals to require issuance of a new benzene standard within eight months. In a replay of former court cases, the API and other industry groups joined the lawsuit as respondents.

This time, however, two additional media stimuli added to the kick’s thrust. In February documents surfaced through United press International that a Shell Oil company consultant had not only found a high rate of leukemia at two plants in 1983, but had also informed the company that he believed benzene exposure was the most likely cause. Numerous papers across the nation carried the story. Later that month the Wall Street Journal printed an interview with Pliofilm cohort researcher Robert Rinsky. The discussion included new information about low exposure leukemias. As reported by the Wall Street Journal, Rinsky found an appreciable risk for leukemia, even at 1.0-ppm exposures. Approximately two weeks later the API urged new OSHA Administrator Robert Rowland to take action on the standard. They proposed a range of possible PEL’s “because of the widely divergent interpretations of the available risk and feasibility evidence on benzene.”

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considering this for a couple weeks, Rowland rejected the API’s suggestion’s, opting instead for a return to a draft standard of 1.0-ppm.

While these actions proved that life still remained in the engine, it continued to sputter. As now required by President Reagan’s Executive Order 12291, OSHA submitted the proposed standard to OMB. There the draft languished well past the authorized sixty-day period allowed for review. Misfires continued well into fall as first the Secretary of Labor and then the OSHA Administrator resigned. The engine finally caught in December when new Labor Secretary William Brock nominated career industrial hygienist John Pendergrass as OSHA’s administrator.

Although the ensuing process could not be called a smooth running engine, regulatory activity did advance. Following a second round of rulemaking during 1986, OSHA issued a final rule for a modified airborne benzene limit in September 1987. OSHA grounded its final standard of 1.0-ppm on two risk assessments conducted by EPA, one based on human data and one on animal data. They provided good agreement on benzene cancer risks: $1.0 \mu g/m^3 (1.0 \text{ ppb})$ lifetime exposure to benzene equated to an approximate 2.3 in a hundred thousand risk of death from leukemia. By extrapolating from this number and using a working lifetime of exposure, OSHA determined that the 1.0-ppm standard for benzene exposure equates to 10 excess leukemia deaths per 1,000 employees so exposed. Although the rule provided similar protections to the 1977 rule, including a limit of 1.0-ppm for airborne benzene, neither industry nor labor objected, thus allowing the rule to become law without further court review.

Industry likely felt relieved that the standard was not further lowered given the results of the risk assessments. Their fear appears justified since ten years later the ACGIH, after a new review of the evidence, lowered its recommended TWA for benzene to 0.5-ppm. However, the governmental
engine has again stalled. As with almost all health standards since its 1987 final rule for benzene, OSHA has remained mute.  

### Table 1: History of benzene regulatory standards

<table>
<thead>
<tr>
<th>Time Period</th>
<th>ACGIH TLV (ppm)</th>
<th>OSHA PEL (ppm)</th>
</tr>
</thead>
<tbody>
<tr>
<td>&lt;1946</td>
<td>100</td>
<td>N/A</td>
</tr>
<tr>
<td>1947</td>
<td>100</td>
<td>N/A</td>
</tr>
<tr>
<td>1948 – 1948</td>
<td>50</td>
<td>N/A</td>
</tr>
<tr>
<td>1949 – 1957</td>
<td>35</td>
<td>N/A</td>
</tr>
<tr>
<td>1964 – 1969</td>
<td>25 (ceiling)</td>
<td>N/A</td>
</tr>
<tr>
<td>1957 – 1975</td>
<td>25</td>
<td>N/A</td>
</tr>
<tr>
<td>1972 – 1986</td>
<td>----</td>
<td>10</td>
</tr>
<tr>
<td>1987 – current</td>
<td>----</td>
<td>1</td>
</tr>
<tr>
<td>1977 – 1996</td>
<td>10</td>
<td>---</td>
</tr>
<tr>
<td>1997 – current</td>
<td>0.5</td>
<td>---</td>
</tr>
</tbody>
</table>

TLV = threshold limit value, a recommended standard from the American Conference of Governmental Industrial Hygienists (ACGIH); PPM = parts per million; PEL = permissible exposure limit, an enforceable standard created by Occupational Safety and Health Administration (OSHA). All concentrations represent time-weighted averages (TWA) unless otherwise noted.

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**Company Knowledge**

My 27 years of experience through employment with a federal research agency (NIOSH) and regulatory agency (OSHA) leads me to conclude that

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84 Chart contained in David Galbraith, et al., “Benzene and Human Health,” 2.
petrochemical industry representatives and their contractors often withhold information from the Federal Government and misinterpret positive study findings by others. - Peter F. Infante

Throughout the twentieth century, corporate knowledge about the hazards of benzene never lagged behind other sources of information, companies often being better informed than the government, general medical practitioners, or, at times, even experts in the field. As described earlier, one industry safety organization had alerted its members to the extremely hazardous nature of benzene and its connection to blood diseases as early as 1926. Even at this early period, well before any voluntary standards, the National Safety Council informed its members that benzene constituted an extremely difficult product to utilize safely. As the Council documented in its report on benzol:

We are forced to conclude that the control of the benzol hazard (except where the substance is used in completely closed systems) is exceedingly difficult; that in practice, systems of exhaust ventilation capable of keeping the concentration of benzol below 100 parts per million are extremely rare; and that, even when this is accomplished, there remains a decreased, but substantial hazard of benzol poisoning.

Twenty years later benzene remained a very difficult, if not impossible, substance to use without extreme precautions. Indeed, industry recognized the continuing elusiveness of handling benzene safely. In its 1948 toxicological review of benzene—a product ubiquitous in the petroleum industry—the American Petroleum Institute emphasized the near impossibility of using benzene without risking significant health consequences: “A limit of 50-ppm or less is strongly recommended, particularly where exposures are recurrent.” They further understood that even this was likely insufficient since “[i]t is generally believed that various individuals differ in their bone marrow response to benzene...” The report further acknowledged that the variation is so wide that

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86 C. E. A. Winslow, Final Report of the Committee, 118.
“there is little correlation between the degree and duration of exposure and the severity and nature of the findings in the blood on microscopic examination.”

The author of this review expressed particular concern about the widespread effects on the blood-forming system. “Chronic benzene poisoning is refractory to treatment. Practically all therapeutic measures attempted have failed.” In addition, “delayed toxic effects appears likely,” with “reasonably well documented instances of the development of leukemia [having] been cited.” The conclusion cautioned that there is no safe level of exposure to benzene. “In as much as the body develops no tolerance to benzene, and as there is a wide variation in individual susceptibility, it is generally considered that the only absolutely safe concentration for benzene is zero.”

At the time, petroleum companies took notice of the warning. That same year, an industrial hygienist at Mobil, one of ExxonMobil’s predecessors, recorded his understanding of a definite connection between cancer and benzene. Five years later, company officials received notification that benzene containers should contain a label indicating “poison,” since “no concentration of benzene is considered to be safe.” Safety manuals also began echoing the API’s concern. Conoco’s 1953 safety manual declared in language almost identical to the API’s, “as there is a wide variation in individual susceptibility it is generally considered the only absolutely safe concentration for benzene is zero.” Exxon’s other major predecessor, ESSO, issued a “Toxigram” for benzene in 1958 that both reiterated and strengthened the warning: “Most authorities agree that in the light of present knowledge, the only level which can be considered absolutely safe for prolonged exposure is zero.”

88 Mobil Oil Corporation, v. Anna Mae Ellender, 449.
89 Mobil Oil Corporation, v. Anna Mae Ellender, 450.
90 Mobil Oil Corporation, v. Anna Mae Ellender, 449.
91 Esso Research and Engineering Company – Medical Research Division- Linden, New Jersey, “Toxigram” Benzene, April 14, 1958 (in author’s possession. The Toxigram further noted that although the maximum allowable concentration had been reduced to 25-ppm, “in actual fact, this figure may still be too high.”
Yet, even as the nation moved to a greater environmental and public health conscience, the petroleum industry travelled down the opposite tack, becoming ever less willing to accept adverse information about benzene hazards. The first indication of this new position came in the early seventies, as Congress began discussing the possibility of enacting legislation for federal environmental and occupational health agencies. In 1970 the API asked Bernard Goldstein, a young investigator at the Institute of Environmental Medicine at New York University, for an update on benzene literature worldwide, including experimental and workplace studies, as well as case reports. Goldstein’s review reached a frank and unsurprising conclusion: benzene causes leukemia. “[T]here is reasonably good evidence that inhalation of benzene is associated with an increased incidence of acute myelogenous leukemia, and possibly other hematological neoplasms.”

Given the API’s recognition of this probability in 1948, this could not have come as much of a surprise. However, in the new regulatory and litigation environment in which individual companies could be subject to costly regulations and worker lawsuits—asbestos companies were already being increasingly sued after the 1964 Sinai report on asbestos cancers—the API did not welcome the news. Goldstein—now the former dean of the University of Pittsburgh School of Public Health—has described what happened next: “API refused to fund us [the Institute]. They had all sorts of reasons, but basically, that was the end of their funding. At no time did anyone ever say that because you found this, we can’t fund you. I got no pressure to change my publication. Of course, I did get lots of questions to justify it. And I got no more funding from them to do this work.”

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92 Devra Davis, The Secret History, 383 (API refused); B. D. Goldstein, Benzene Toxicity: A Critical Evaluation. Report for the American Petroleum Institute, (Washington, DC: Hemisphere Publishing, 1977). Since this monograph appeared, virtually all API funded publications have dismissed the suggestion that there is a current problem. In the numerous API funded studies I have reviewed, if the study finds a potential problem, it is minimized or the effects are blamed on sufficiently historic exposures that few alive today—that is, potential litigants—had long term exposures to benzene above 1-5.0-ppm. See, for example, Neill K. Weaver, “Gasoline Toxicology, Implications for Human Health,” Annals of the New York Academy of Sciences 534 (June 1988): 441-451; and Mary Burr Paxton, “Leukemia Risk Associated with Benzene Exposure in the Pliofilm Cohort,” Environmental Health Perspectives 104, suppl. 6 (December 1996): 1431-1436.
Of course, the API and the petroleum industry understood that Goldstein was correct. A 1973 article Dr. Robert Eckardt, director of the Medical Research Division for Esso Research and Engineering, acknowledged the benzene leukemia connection: “[The] accumulation in the literature of cases of leukemia following benzene exposure leads to the inevitable conclusion that benzene is a leukemogenic agent.” Goldstein’s sin was not in providing the information to industry but, rather, his desire to publish it in the open scientific literature. As we shall see shortly, other researchers, more willing to modify their initial internally provided conclusions before publication of the results, had long lucrative careers conducting research for the oil and chemical industry.93

Nor was Goldstein alone in encountering difficulties after providing inconvenient and potentially costly health hazard news to the oil industry. Although not specifically about benzene, Colin Soskolne’s experience provides another example of the oil industry’s extreme reluctance to publically acknowledge news about chemical health hazards. While he was working toward his graduate degree at the University of Pennsylvania, Colin Soskolne received a request from Exxon to create a database. They hoped to create one similar to Soskolne’s revolutionary health database for South Africa. In compiling the information for inclusion in the Exxon project, Soskolne amased records on approximately 10,000 people, tracing their work and medical related history until they retired or died. Their average location and type of job could be linked to the most frequent chemical

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93 Infante PF, et al., “Leukaemia in Benzene Workers,” 76–78. As with asbestos, even world-renowned researchers at times placed industry money above strict professional ethics. As Devra Davis writes in her expose of cancer, “contrary to policies in their respective institutions at the time, Doll, Adami and Trichopoulos did not acknowledge much of their work for industry in publications they wrote for hire.” For example, Monsanto Dow Chemical and at least one other company secretly paid Doll for advise on matters that included vinyl chloride. While being paid, Doll published his research of vinyl chloride in the *Scandinavian Journal of Work, Environment and Health*. He argued that angiosarcoma was associated with vinyl chloride, but more common cancers of the brain and liver were not. He based this on four studies, while ignoring others. Although his research found an excess of brain cancers (29 versus 20 expected) he dismissed it as “not statistically significant,” concluding nothing suggested any brain cancers are occupational in origin. In the article he not only failed to mention his consultancy income, but also did not mention that the Chemical Manufacturers Association paid for this work on the article. Devra Davis, *The Secret History*, Quote at 312, 378-9; “Renowned Cancer Scientist was Paid by Chemical Company for 20 Years,” *The Guardian*, December 8, 2006, http://www.theguardian.com/science/2006/dec/08/smoking.frontpagewews (accessed November 17, 2015); and Richard Doll, “Effects of Exposure to vinyl chloride. An Assessment of the Evidence,” *Scandinavian Journal of Work, Environment and Health* 14, no. 2 (April 14 1988): 61-78.
exposures. The data also accounted for cigarette usage, physical characteristics, and genetic history.

With the database completed, Soskolne linked each person with cancer of the upper respiratory tract to three comparable workers (a nested case-control study). He then calculated the odds of workers exposed to inorganic chemical sprays developing upper respiratory cancer. To Exxon’s dismay, he found those exposed to the mist had approximately four times greater chance of developing upper respiratory cancer then did individuals without such exposure (almost as high as the nonsmokers risk of cancer from asbestos). Other factors, including smoking, did not alter the relationship.

Exxon did not lightly accept the findings. Soskolne described Exxon’s next move:

[S]cientists from the company began to ask questions of my work that seemed a bit odd. But I took them seriously. Each time one of them raised an issue, I would redo, revise, and recalculate the work . . . All these queries had the intent of trying to make the big risks we found go away. Under this pressure, of course, I basically redid everything.

Ironically, with many of his adjustments, the risks grew. 94

These two examples are not atypical. When workers began dying at its Elizabeth, New Jersey Bayway Labs from lead poisoning, Standard Oil officials in 1924 suggested, “nothing ought to be said about this matter in the public interest.” When the New York Times reported the next day on individuals dying and literally going insane at the labs, the reporters quoted one supervisor as stating “these men probably went insane because they worked too hard.” 95

These and other activities within Exxon have been remarkably similar to those advocated and managed by tobacco attorneys. Take yet another example: in 2008 Exxon organized an

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94 Devra Davis, The Secret History, 252-253; Soskolne believes Exxon’s actions amounted to suppression: “This paper (referring to the cited paper below) uses ethical analysis for exposing how the Exxon Corporation had attempted to suppress the work of Dr. Soskolne in the early 1980s regarding the discovery of an association between occupational exposure to strong-inorganic-acid mists in the development of laryngeal cancer.” Colin Soskolne, “Ethical Decision-making in Epidemiology: the Case Study Approach,” Journal of Clinical Epidemiology 44, suppl. I (1991): 1255-1305. Although Exxon attempted to modify and suppress the findings, Soskolne’s paper was published. C. L. Soskolne, “Laryngeal Cancer and Occupational Exposure to Sulfuric Acid,” American Journal of Epidemiology 120, no. 3 (September 1984): 358-69.

95 Gerald Markowitz, et al., Deceit and Denial, 20-1 (“Nothing ought” and “these men”).
international symposium on benzene. The “volunteer” organizing committee was composed primarily of corporate employees and experts known to be friendly to industry’s position. It did not include anyone associated with the then ongoing joint Chinese/NCI study, which had found increasing linkages between benzene and various diseases. Bernard Goldstein, likely invited as a token contrary representative, provided the sole contrary voice at the symposium. Notwithstanding this almost monolithic nature of the scientists involved, the group secured funding from several federal and foreign agencies, along with significant industry support. This is not a solitary example. As of 2015 ExxonMobil has continued to withhold and slant information about benzene, as well as strive to ensure its positions are well publicized. Later in this chapter we shall examine how one ExxonMobil litigation support firm has miraculously eliminated all hazards from benzene at Exxon/Mobil’s facilities.96

The other half of the new Exxon/Mobil, Mobil Oil, had comparable historical knowledge to ESSO and reacted to adverse information in a similar unreceptive manner. Mobil’s 1960 to 1983 western region medical director learned during medical school in the 1940s that benzene caused blood diseases. At one 1990s benzene trial he testified that Mobil knew of benzene’s hazards at least by the 1950s. Although by the 1980s Mobil employees were no longer spoke of “low dose” benzene hazards, in 1983 Mobil’s director of epidemiology and medical information services admitted that benzene’s high dose relationship to leukemia had been first reported about 70 years previously and was well known to Mobil. Yet Mobil did not inform many of its own employees about this relationship. In one almost incredulous example, when a Mobil safety officer was called to testify at

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96 Michael G. Bird, Helmut Greim, Debra A. Kaden, Jerry M. Rice, and Robert Snyder, “Preface,” *Chemico-Biological Interactions* 184 (2010): 1-2. This chapter will also examine how one litigation support firm employed by ExxonMobil has miraculously eliminated all hazards from benzene at Exxon/Mobil’s facilities.
a 1993 deposition for a benzene trial, a Mobil attorney had to inform him that benzene caused leukemia.97

Similar stories can be told of other petroleum companies. Like Mobil, Texaco limited public dissemination of information, keeping benzene and leukemia knowledge close to the vest. During the 1970s, Texaco’s Material Safety Data Sheet (MSDS)—product safety information required by law—for benzene did not mention leukemia or cancer, merely the possibility of “damage to blood forming organs.” Ron Richards, the epidemiologist employed by Texaco to draft its benzene MSDS in use during the mid 1970s, testified at a trial in which a worker’s widow alleged Texaco did not provide an adequate warning about leukemia to her late husband. Richards “candidly admitted that [prior to preparing the MSDS] he was aware of many of these [reports of the leukemia connection to benzene] studies and reports,” yet still sought to justify his MSDS wording. First, Richards stated his belief that the studies did not demonstrate benzene’s carcinogenic danger “to a reasonable degree of certainty.” Incredibly, he also testified Texaco that chose the damage warning “because this more comprehensive term was superior to the language “cancer hazard.”

In ruling on a motion, the judge found that Texaco and Richards offered two “factually irreconcilable positions” for why cancer was not mentioned in the MSDS. In light of these positions, the judge continued, “the jury could reasonable infer that Texaco’s warning was designed to conceal, rather than inform of the carcinogenic danger posed by its product.” He further opined “Texaco ignored scientific and medical evidence linking benzene exposure to leukemia, finding this evidence “reasonably certain” only when OSHA issued its emergency standard in 1977.” 98

Shell Oil Company had similarly vague warnings in its benzene MSDSs. Shell’s 1985 MSDS for JP-4 jet fuel stated in part: “Repeated high level benzene exposure may produce injury of the

97 Mobil Oil Corporation, v. Anna Mae Ellender, 449-450.
98 Diana L. Mason v. Texaco, Inc., 1515-16.
blood-forming tissues causing blood abnormalities and possibly leukemia.” Three years later—ten years after Infante’s authoritative Pliofilm study—Shell’s 1988 MSDS raised the operative term possibly to suspected: “Prolonged and repeated benzene exposure may cause serious injury to blood forming organs: benzene is suspected of carcinogenic (leukemia) potential in man.”

Shell’s inability to provide honest information in its MSDS is, however, of vanishingly small significance, when compared to its other transgressions. Subterfuge reached exceedingly greater heights in this energy company. Shell had long known about the dangerous aspect of long-term low exposures. In 1943 Dr. M. H. Soley informed Shell Development Company, “prolonged exposure to concentrations [of benzene] may be most dangerous.” By the 1970s Shell Oil Company had specific experience with this danger. As the Pliofilm cohort received media attention throughout the nation, Shell knew that benzene risks associated with leukemia were not confined to high exposure level manufacturing facilities such as those of Goodyear’s Pliofilm cohort.

In 1978 Shell either conducted or paid for a private report on benzene. Entitled “Human and Animal Toxicology of Benzene,” it provided an uncommonly frank examination of benzene diseases. On one page it indicated “the association of the condition—myelofibrosis and Myeloid Metaplasia—with benzene over-exposure suggest a causal relationship.” Interestingly, the author did not believe it necessary to use epidemiological studies to confirm the relationship, a necessary prerequisite according to industry trial attorneys and experts. Nor does it appear that Shell made or allowed public dissemination of the report.

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100 M. H. Soley, “Report to Shell Development Company on Enzenes, Nitrobenzenes, Anilines and Xylidines.” (1943), quoted in Mobil Oil Corporation v. Anna Mae Ellender et al., 449. In its affirmation of the case, the Texas Supreme Court described the 1943 report to Shell as warning “that prolonged exposure to low concentrations of benzene may be very dangerous.” Mobil Oil Corporation V. Anna Mae Ellender, 968 S.W.2d 917, 923 (Tex. 1998).

Shell’s specific knowledge of a corporate problem with leukemia began at least by 1979 with a lawsuit in which a contract worker alleged benzene exposure from Shell’s facilities had caused his leukemia. In response, Shell employees compiled a list of past employees who had developed leukemia. The largest number came from Shell’s Wood River, Illinois, Manufacturing Complex (Wood River). Between 1973 and 1982 at least twenty Shell Oil company employees at the two refineries died of leukemia; fourteen died at Woods River, and six at Deer Park. In 1980 Shell scientists calculated a statistically significant proportionate mortality ratio (PMR) of 3.49 for leukemia at the plant. Results from its Deer Park, Texas, Manufacturing Complex (Deer Park), did not follow until three years later, when they revealed a similar mortality ratio of 3.29 PMR. In March 1982 Shell officials invited Thorne Auchter, at the time Reagan’s appointed head of OSHA, to dinner in Houston. There they suggested that OSHA use Shell-proposed scientists to study the matter.102

Subsequently, Shell hired Dr. Philip Cole, an eminent professor of epidemiology at the University of Alabama at Birmingham as a consultant in May 1983. That summer Cole and finalized a second review of the leukemia excess at Wood River. Among other results, Cole found a statistically highly significant four-fold mortality excess of acute myelogenous leukemia among Wood River employees. His review concluded “it is likely that there was a real problem with leukemia, especially myelocytic leukemia at Wood River in the years prior to 1980.” He further informed them “if Shell has an overall problem with leukemia, it is not restricted to Wood River.” Cole provided a brief, to-the-point summation to corporate officials: “[B]enzene is an established cause of AML . . . [T]here is no reasonable possibility that the data are the result of any error, nor is

102 “20 Cancer Deaths,” Los Angeles Times, (This news article reported on a number of Shell documents and letters that had been obtained by United Press International. One of the letters referred to the dinner event. In the same letter Shell officials referred in a disparaging manner to “some of the scientific work coming out of your organization.”); and David Egilman, et al., “Manipulated Data in Shell’s,” 223.
it likely that these findings can be attributed to confounding by a non-occupational cause of AML. . . . [B]enzene is the most likely cause of the excess.”

Shell quickly learned that the news was not destined to remain private. As part of their effort to quantify the risk of benzene, on July 8, 1983 OSHA requested manufacturers provide data on occupational exposures to benzene. On July 28 Shell provided OSHA with what they called the preliminary results from the mortality study. That same month Shell’s president received “back up materials” from Paul F. Deisler, Jr, Shell’s vice president for health, concerning Shell’s history of leukemia. These materials included new data from Wood River and Deer Park, as well as epidemiological studies in progress and a summary of OSHA’s regulatory activity. The materials disclosed that at 4.54, Wood River’s SMR for leukemia was even higher than the previously calculated PMR. The materials also listed Deer Park’s SMR as 3.7, although it was “not quite” statistically significant. However, a footnote indicated that one employee with leukemia and several months of service, as well as many years with another company, had not been included. Including this case resulted in a higher (and significant) SMR.

Shell did not publish the internal calculations or either of Doctor Cole’s reviews. Instead, a few days after circulating the “back up materials” to key personnel, the company issued an employee communication and press release explicitly contradicting the findings. This action brought about the desired publicity. On August 31, 1983 an Associated Press story appeared headlined “Shell Oil Co. says it has found no link between benzene and a relatively high incidence of deaths from a certain type of leukemia in Texas and Illinois plants that handle the chemical.” Corporate Medical Director

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104 48 Fed. Reg. 31412-4, July 8, 1983; H. L. Kusnetz (Manager, Safety and Industrial Hygiene, Shell Oil Company) to T. Auchter (Director, OSHA), July 28, 1983 (Letter transmitting initial mortality study results of Wood River and Deer Park facilities); and Peter F. Infante, “The Past Suppression of Industry Knowledge,” 269.
Dr. Joyner is quoted as saying “We have no reason to conclude that a leukemia risk currently exists at any of our refinery locations.” Peter Infante disagreed. He wrote to Shell:

Regardless of the specific etiological factor(s) a highly significant increase in leukemia over the past ten years is clearly apparent at the Wood River facility. Some may consider this an epidemic. Yet your ‘employee communication’ resulting from this study . . . clearly contradicts the study results and the interpretation of those results by your consultant . . . I would like to suggest you consider issuing a revised ‘employee communication’ in light of the above comments.

Shell never acted on Dr. Infante’s suggestion.105

Faced with bad data from Cole’s first two studies, that fall Shell initiated yet another study, the Benzene Historic Exposure Study at Woods River and Deer Park, to “assess the historical workplace benzene exposure for the diagnosed cases of leukemia.” The study’s architects planned to do this by estimating “the historic benzene exposure for all jobs at [the two sites] where benzene may have been encountered.”106 Since Shell had taken few air samples during the relevant period, the researchers based most of the exposure estimates upon interviews, at least several of which were not blinded (the interviewers knew the status of the individuals they interviewed). They asked the interviewees to subjectively estimate the level of benzene present at their work stations with a seven-step odor scale, ranging from no detectable odor to so strong they needed to get away.

Even while conducting the study with the odor standard, Shell knew this method was ineffective at accurately determining exposures. This information came from Industrial Health Engineering Associates, Inc. (IHEA), which provided consultation during the study. IHEA

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consultant Knowlton Caplan advised Shell of dangers inherent in using a subjective odor scale. These included the potential that “the operable odor threshold for a refinery worker may well be significantly higher than the official threshold of 01.5-ppm, due to olfactory fatigue or olfactory adaptation. Further although benzene has a distinct odor, it is likely that other petroleum hydrocarbons also present would mask or confuse the odor.” He continued his criticism by informing Shell and offering a suggestion to improve its potential for accuracy: “If we estimate mere odor perception as a 01.5-ppm exposure, I fear we will be seriously underestimating the exposure . . . I urge that a serious effort be made to determine what the ‘adaptation’ factor may be for benzene. If a thorough literature search does no yield this, perhaps you may want to consult a real odor expert on this subject.” The resulting published papers provide no indication Shell acted on this advice; nor do Shell officials discuss the difficulties with subjective odor determinations of exposures. In its submission to OSHA, they discussed the problem but indicated a literature search did not find any relevant information. 107 By not consulting with an odor expert and using this method of calculation—which included underestimations caused by random miscalculation—Shell researchers considerably biased the findings toward the null.

Shell’s method of determining work locations further biased the study toward the negative. Doctor Cole’s original review had previously highlighted problems with Shell’s methodology for determining exposure levels. Cole expressed particular concern about the “follow-back study of worksites,” one of the nine prior research activities he reviewed. He warned that the “study’s negative result will not be perceived as highly persuasive” in part because “benzene, being highly

107 K. Caplan, “IHEA Wood River and Deer Park Visits to J. Randsdell, Shell Oil,” cited in David Egilman, et al., “Manipulated Data in Shell’s,” 224-5, fn. 27. Caplan even offered the company a more accurate methodology for determining benzene levels, which involved the use of an air-dispersion model tied to known leak rates and emission data. In response to Shell experts’ displaying a “notable lack of enthusiasm” toward this proposal, Caplan opined, “it is pure assumption on my part that reluctance to embark on this project is based on the departure from the usual criteria of precision.” David Egilman, et al., “Manipulated Data in Shell’s,” 225.
volatile, may permeate nearly the entire facility and affect just ‘susceptible’ individuals . . .” wherever they might work.\(^\text{108}\)

Notwithstanding this cautionary advice, within six months of his report, Cole and Shell initiated the BHES study with very similar methodology. The study not only limited exposure estimates to specific sites, in addition, when an individual had refinery-wide responsibilities and little information about specific worksites, the researchers assigned them to the ‘nil exposure’ category.

The interviews were similarly tainted. Despite numerous interviewees recalling using benzene regularly as a solvent, Shell dismissed the evidence, incorrectly stating that “benzene was not available in the refinery” at the time of employment—this despite the fact that Shell admitted benzene was the product of choice for a solvent and the BHES study admitted it was available from 1942 on, well within the time period for almost all of the relevant employees.

It is difficult to not draw the conclusion that the researchers consciously used these decisions in methodology and evidence control to push the study toward a negative bias. These decisions resulted in the researchers determining that at least eight of the men with leukemia had minimal exposures even though they worked throughout the facility for nine years or more, most dating back to the 1940s or earlier. This included at least three of the nine men—there may be others—the study categorized as having “nil” exposure. For example, one of the men with leukemia—although determined to have virtually no exposure had worked throughout the plant from 1932 until 1951.

More than one interviewee reported that this individual and others like him had used benzene as a cleaner.\(^\text{109}\)

Shell submitted the BHES report to OSHA in February 1984. Predictably, it concluded, “at this time, the excess leukemia remains unexplained.” Shortly afterward, in an exchange of letters


with an increasingly suspicious OSHA epidemiologist, Peter Infante, Shell admitted the uncertainty of their exposure determinations and the difficulty of obtaining accurate measurements. “At best, we would have to take each employee with known post-1975 exposure and try to estimate pre-1975 exposure with the methods we used in the historical exposure study we recently submitted to OSHA. This would require an extremely heavy commitment of resources on our part, and the resulting data would still be clouded with uncertainty.”

Infante’s suspicions about the report had begun one month earlier, when, after reviewing Shell’s July submission, he sent a letter to Howard Kusnetz, Shell’s manager for Safety and Industrial Hygiene, requesting specific data on individuals with blood abnormalities along with their benzene exposure levels. He knew the information should be available from reading a previously published article describing Shell’s “health surveillance system.” R. E. Joyner, Director of Shell’s Medical Department, responded partially to the first letter, indicating that no employee meeting benzene exposure criteria had a blood abnormality. Infante repeated his request on February 22, this time directing it to Joyner. In his March 5 response, Joyner again denied any blood abnormalities among those exposed to benzene and requested that OSHA redefine its own criteria in the data it requested. That same day Kusnetz wrote to Infante’s boss, requesting assistance in halting Infante’s overbearing requests and suggesting they were putting a strain on Shell’s working relationship with OSHA. He further accused Infante of being “unscientific and harassing” in his requests that, to them, appeared to be in support of “a conclusion he has already drawn . . .” After an unexplained delay, perhaps due to either office politics or consultation with attorneys or individuals in higher authority, Infante wrote a third letter to both Kusnetz and Joyner, again requesting the information. In this letter he also informed the Shell employees that personal attacks on him served only to divert

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attention from the true issues, the toxic effects of benzene on workers. He also informed them that by using these tactics, Kusnetz and Joyner raised suspicions about whether they might be withholding information. One month later Shell provided the “nonexistent” blood and exposure data for workers with blood abnormalities who were assigned benzene-exposure jobs. Given the methodology Shell used in the BHES study to determine which employees had benzene exposure, doubts may remain about whether they provided all of the relevant employee data.\(^{111}\)

Shortly thereafter, in an extraordinary display of hutzpah, Kunstler wrote an editorial in his capacity as President of the American Industrial Hygiene Association (AIHA). It accused NIOSH of acting in a “blatant scientifically unethical manner.” The unethical act involved NIOSH’s release of a pre-publication update on the Pliofilm study—Infante was one of the lead investigators—prior to it undergoing “the agency’s review process.” According to Infante, the claim is false. He personally questioned the lead author, Rinsky, who verified the study had undergone internal peer review prior to any discussion with the press.\(^{112}\) Some might question Kusnetz’ motives in this accusation, since the update included a risk assessment needed by OSHA prior to the conclusion of the benzene rulemaking evidence gathering period.

\(^{111}\) Infante PF [Director, Office of Standards Review, OSHA] to Howard L. Kusnetz [Manager, Safety and Industrial Hygiene, Shell Oil Company], January 27, 1984 (letter requesting data for blood abnormalities among benzene exposed workers); P. F. Infante to R. E. Joyner [Director, Corporate Medical Department, Shell Oil Company], February 22, 1984 (letter requesting data for blood abnormalities among benzene-exposed workers); R. E. Joyner to P. F. Infante, February 13, 1984 (letter stating no employee meeting benzene exposure criteria had a blood abnormality); R. E. Joyner to P. F. Infante, March 5, 1984 (letter asking OSHA to redefine its criteria in requesting data related to blood abnormalities); H. L. Kusnetz to T. Auchter [Director, OSHA], March 5, 1984 (letter asking OSHA Director to alleviate Shell Oil Company from benzene data requests by Dr. Infante); P. F. Infante to R. E. Joyner and H. L. Kusnetz, April 17, 1984 (letter requesting data for blood abnormalities among benzene-exposed workers; and R. E. Joyner and H. L. Kusnetz to P. F. Infante, May 17, 1984 (letter providing data for Shell benzene-exposed workers with blood abnormalities). All of the above letters are from citations in Peter F. Infante, “The Past Suppression of Industry Knowledge,” 269-70. Another letter from Shell that Infante cited revealed that the company knew about the problem. As previously noted, they treated Infante’s boss, Thorne Auchter, to dinner in March 1982. There they suggested Auchter use a group of Shell proposed scientists to study the problem rather than Infante.

Over the next few years Shell employees and contractors published six papers based at least partially upon the BHES. Although there might be questions about the actual effort made, none of the studies found a relationship between benzene and the leukemia cases at Wood River and Deer Park. In every case, the articles failed to discuss, or even consider, the extensively reported “wide variation in individual susceptibility,” as described in the 1948 API review and Cole’s earlier internal report. They also ignored internal objections to the BHES exposure analysis, including the potentiality of benzene fumes being widely spread throughout the facilities. Since the authors did not combine the Wood River and Deer Park cohorts in any of the papers—four were for Wood River and two for Deer Park—even though NIOSH had suggested this approach, the studies remained underpowered and thus less likely to show a positive result. The first two—a “brief communication” published in 1985 and a 1986 case-control study—both limited their data to Wood River, ignoring the Deer Park cases. In late 1984, Shell submitted a draft of these papers to NIOSH and OSHA providing an updated and more complete treatment of the BHES data, entitled “A Case-Control Study of Leukemia at an Oil Refinery.” Upon receipt, the agencies criticized not only the draft but also the underlying BHES data.

We are concerned that the statement ‘The study produced no distinctly positive result, not even for benzene, and the reason for the excess leukemia among refinery workers remains unexplained,’ could serve to imply that benzene has adequately been ruled out as a cause for the observed excess. We do not agree with that implication. The small size of the study population and the lack of good information on which to estimate exposures for the subjects, do not support this implication. We believe exposure to benzene is the most likely explanation for the excess incidence of leukemia.\footnote{David Egilman, et al., “Manipulated Data in Shell’s,” 229 (quoting from J. D. Millar letter about submission of case control study, June 25, 1985).}

Shell ignored the comments. Although both papers reported a significant excess of leukemia, they also ignored certain leukemia cases and used the faulty BHES exposure data to determine that
most of the leukemia cases did not work in the highest exposure areas. Thus, both papers reiterated the conclusion in Shell’s report to OSHA that “the excess of leukemia remains unexplained.” Although there is no direct evidence that Shell’s attorneys took part in vetting the studies, the resulting papers provided the best possible spin on the data for trial applications. Given that the papers provided the exact type of conclusion desired by defense trial attorneys, such participation cannot be ruled out.114

The 1989 study still excluded Deer Park, but included most other workers and extended the follow-up to 1984. It found forty-four leukemias versus the 29.6 expected. Although the authors admitted that benzene might be the cause, they minimized that possibility, citing to the 1985 and 1986 studies. They also theorized that any health hazard was rapidly decreasing.115

The final study in 1995 continued the concept of diminishing danger, reporting, “any leukemogenic exposures at the plant have been reduced to a point at which they are insufficient to cause leukemia.” In reaching this conclusion the authors again used faulty exposure methodology, treating sick individuals with documented exposures as non or low exposed. In addition, the final article differed in its data presentation and analysis—intimating less of a problem—from the internal report to the company that provided the basis for the publication.

Once again the disparity between the internal report and the published article leaves open the possibility of attorney involvement in the publication process. The published report eliminated virtually all of the legally troublesome elements of the internal report. The study’s design demonstrates close attention to the needs of industry trial attorneys in limiting trial usage of potential bad results. In at least one trial, defense attorneys kept both the report and the article out

of evidence because the authors stated the cause of death was unknown and they studied the mortality of the diseases, not the causation. In each instance of variance, the article minimized any possible benzene connection that had been included in the internal report. In perhaps the most blatant example, the final article did not contain eight leukemias included in the internal report because they did not meet the arbitrary minimum work period used in the published article. The authors admitted other leukemias might have been underreported in the article, since some individuals diagnosed with myelodysplasia/myelofibrosis would have been designated in earlier periods as having leukemia.\textsuperscript{116}

As occurred with earlier Shell communications to its employees about benzene, the information Shell provided to its employees during this period did not tell the whole story. In the employee communiqué about the Wood River Mortality Study of this period, Shell informed its employees that “WRMC hourly employees had a higher than expected rate of myelofibrosis and myelodysplasia in the 1980s (6 cases observed/0 expected). Myelofibrosis and myelodysplasia are pre-leukemic disorders of the bone marrow.” It further stated that the individuals all worked began working before 1946, but the reason for the increase is unknown. In the questions and answer section of the report it indicates that benzene is one of the causes of myelofibrosis and “certain high chemical exposures” can cause myelodysplasia.\textsuperscript{117}

The two Deer Park studies also presented “bad data” in the best light possible. As with the Wood River studies, these papers ignored other similar Shell locations, thus reducing the power of

\textsuperscript{116} Y. Honda, E. Dezell, and P. Cole. “An Updated Study of Mortality Among Workers at a Petroleum Manufacturing Plant.” \textit{Journal of Occupational and Environmental Medicine} \textbf{37} (1995): 194-200; and David Egilman, et al., “Manipulated Data in Shell’s Benzene,” 224, 228-30. Cole and his colleagues provided more than one internal unpublished report to management. Another internal report by Cole’s colleague Elizabeth Delzell, submitted prior to the Honda article report, reaffirmed that “in the 1980’s hourly workers had statistically significantly more than expected deaths from myelofibrosis and myelodysplasia,” stating that the cause was unknown. Interestingly, this report identified cases of myelofibrosis that were not included in the Honda article. The unpublished Delzell Report is cited and quoted in LeBlanc (Malcolm Louis) v. Chevron USA Inc., 2009 U.S. Dist. Lexis 64632 (E.D. La. (2009).

\textsuperscript{117} “Shell Employee Communication Re: Wood River Mortality Study,” 3, 7-8, quoted in LeBlanc v. Chevron USA Inc.
the study. Even so the data demonstrates a significant health risk at the facilities. Like many industrial studies, the University of Pittsburgh and Shell researchers found an overall “healthy worker effect” in the population, with overall mortality significantly reduced. This effect did not extend to blood-related cancers. Like the comparably underpowered Wood River cohort, Deer Park workers experienced excess of cancers in the lymphopoietic tissue. Although the SMR’s did not reach 200, the overall lymphopoietic tissue diseases averaged an SMR of 127. The SMR may have been depressed to some degree because the researchers used the surrounding Harris County—an area with other major industrial sites that likely used benzene—as the comparison population. Still, the authors admitted the “study supports other epidemiologic evidence [other refinery population studies] of an association between exposure to petroleum and its by-products and risks of cancer of lymphopoietic tissue, particularly lympho-reticulosarcoma and a residual category containing multiple myeloma, myelofibrosis, polycythemia vera, and certain non-Hodgkin’s lymphomas.”

The researchers observed a dose response relationship between length of employment and certain cancers. Both refinery workers and chemical workers had pronounced employment duration—duration was used as a surrogate for dosage—relationships for cancers of the lymphopoietic tissue, with the strongest response of 434 SMR (Chemical) and 273 (refinery) for individuals working more than 30 years. Workers likewise demonstrated a strong relationship between these cancers and the time since first employment, with the age-specific death rates supporting the relationship. Unsurprisingly, the authors stated they did not find any specific relationship between specific work areas or job assignments, thereby probably precluding any legal conclusion that the diseases were caused by benzene.118

Although Shell internal documents had previously acknowledged a likely connection between these types of neoplasms and benzene, the only time the word benzene is used in Marsh’s article is in the “Brief History” section, which noted that benzene “has been handled in the manufacturing complex.” The authors made no further effort to delineate where or how it was used, even though Shell internal documents established the fumes could have permeated through major portions of the site. The authors did not find a specific work area relationship to the various cancers. Given the relatively few number of individuals involved and the limited power of the study, and the permeation of benzene fumes throughout the facility, this should not have been unexpected. The authors made no attempt to determine which products might have been responsible, or were even capable of causing the blood related cancers. As with some of the Wood River studies they used, they speculated, “most of the workers studied were not exposed to any single chemical agent but rather to a great number of chemicals.” Thus, once again a study with seemingly bad data for Shell was of little use to plaintiff counsel in benzene litigation because it failed to draw any causation conclusions.  

The following year, Shell Corporate Medical Department employees published an article on Deer Park in which, according to the article, the excess deaths vanished. It examined 1981 to 1988 morbidity patterns at Deer Park, rather than the leukemia incidence that was the subject of the 1983 internal report. Its authors studied a range of health issues over this period. They spent as much time on potential haemorroidal problems as they did for lymphatic and hematopoietic tissue neoplasms, using only a paragraph to explain the lymphatic findings. Although these types of health issues were statistically abnormally high, with an SMR of 124—similar to Marsh’s—the authors explained the results were subject to doubt because of so few neoplasms and the lack thereby of statistical significance. With no explanation, and no reference to Shell’s prior internal report that

found an SMR of 3.7, the article simply stated that they found “lymphatic and haematopoietic tissue neoplasms were raised (SMR 124), but were based on only four cases,” and were not statistically significant.

To an even greater extent than Marsh’s article, this effort has the appearance of being specifically designed to reach a negative result for leukemia. First, the comparison population was not the general local populace, but other Shell workers, apparently including those working at Woods River and other sites that likely used benzene. Second, the study only covered seven years and included all of the workers at Deer River during that period, even new employees and those who had worked as little as one day. In their words, the “population was dynamic, with individual workers entering the observation period . . . at various times and remaining for varying periods.” Thus, an unknown number had an insufficient latency period, likely depressing the overall percentage of neoplasms. In addition they did not continue following anyone who retired during the period.¹²⁰

Clearly, most, if not all, petroleum corporations hid their knowledge of leukemia problem even though they were aware of the relationship well before the general public and even many in the medical community. Yet they were not alone in either the knowledge or their attempts to limit dissemination of that information. Other major corporations likewise attempted to hide their chronic benzene health problems. As previously mentioned, Peter Infante initially believed Goodyear fully cooperated with the government investigation. He later learned the “cooperation” was not as full as it appeared. In actuality, Goodyear knew much more than they told the investigators. At least by the early 1960s, Goodyear Rubber knew that workers at its Pliofilm plant were being diagnosed with AML.

The first publically known diagnosis at the Goodyear plant occurred in 1954. That year, ten days after entering private practice for hematology in Akron, Doctor Marvin Sakol saw his first patient from the Pliofilm facility. This patient became the first of several that Dr. Sakol diagnosed with a form of AML. The second came nine months later in April 1955. The third came in November 1958; the fourth and fifth in 1959 and 1960. The year 1961 brought two more. By 1963 Sakol had diagnosed nine men from the Pliofilm department with leukemia, most with a form of AML.121

In 1963, after learning that Goodyear routinely performed blood counts of the workers, Sakol informed Goodyear’s company physician of his suspicions that the leukemias were work-related. Its response: “all chemicals had been tested and are non-toxic.” Further inquiries into which chemicals the company used brought a curt response of “this is none of your business.” Interviewing the patients, Sakol learned the primary chemical they used was called “urbine.” When he again approached the company doctor, he was informed the “urbine” had been checked out and “found to be entirely innocuous.” Sakol subsequently learned that “urbine” was Goodyear’s code name for benzene.

The company doctor not only mislead Doctor Sakol, he also misdiagnosed several of the workers who had leukemia. He informed at least three that they had anemia. He initially informed a fourth that he had heart trouble before again providing a misdiagnosis of anemia. Whether or not these misdiagnoses were intentional is unknown, yet it is difficult to understand why the doctor did not seek help in his diagnosis if he could not properly read the blood counts, particularly since he provided company testimony at leukemia compensation proceedings. In fact, without his testimony, company attorneys would not have been able to defend the cases. In one letter the company offered

121 M. J. Sakol to A. B. Smith, (Medical Officer, Industry-wide Studies Branch, NIOSH) (transmitting draft paper submitted to Cleveland Blood Club concerning cases of leukemia and benzene exposure among Pliofilm workers at an Akron facility), cited in Peter F. Infante, “Benzene and Leukemia, Pliofilm Revisited: I,” 216.
in a case involving a PLIOFILM worker with AML, the company doctor stated: “I am unable to find anything in his medical record which would indicate that his working environment was in any way related to the disease which resulted in his death.”

Thus, although the reasons for the company doctor’s misstatements and misdiagnoses are not known, they likely related to the workmen’s compensation claims being defended before the Ohio Industrial Commission. These claims increased as more long-term workers became sick. By 1960 Doctor Sakol began assisting some widows of Pliofilm workers with their workmen’s compensation claims. With the exception of one worker, the company fought each of the claims, until finally in 1969 the Commission ordered Goodyear to pay similar claims “as industrially related.”

An August 6, 1990 memorandum Dennis Paustenbach wrote to his client—the American Petroleum Institute—raises further suspicions about the intentional nature of Goodyear officials withholding and distorting information vital to workmen’s compensation claims. The memo described a meeting he had with Goodyear executives while gathering information to write an article that reinvestigated the Pliofilm study. In it Paustenbach remarked how he “definitely got the impression that much more information was available than what was provided to NIOSH.” It also appears that the API might already have known about of the withheld information, for as Paustenbach continued, “[a]s we are aware, the wording of the discovery request was such that industrial hygiene and other process related data were not submitted.”

Other chemical companies were no more forthright. If it had been published or made available to NIOSH, one Dow Chemical study might have helped them develop the risk analysis that

the Supreme Court desired. Unfortunately, similar to several other corporations that learned internally about the hazards of even low exposures to benzene, Dow Chemical delayed publication. In March 1977 corporate officials received a study from Dow Chemical geneticist Dr. Dante Picciano establishing that Dow Chemical workers exposed to benzene concentrations under 10-ppm demonstrated a significantly elevated risk of chromosomal breakage. Doctor Picciano finished his report of the findings in June 1977, sending it to three outside consultants, who generally agreed with the conclusion. By July, the researcher’s supervisor, Doctor D. J. Killian, had sent the study’s report to Dow’s medical research director, Benjamin Holder, located in Midland, Michigan. The accompanying memo informed the director that the study “identifies an unsuspected occupational health hazard.” It further recommended forwarding the discoveries to the government.

Yet, rather than notifying the government about the results, corporate officials preferred to prevaricate. One month later Dow officials, including Texas medical director Dr. John Venable, testified at OSHA’s benzene standard setting hearings. None of the Dow witnesses mentioned the findings. Dr. Venable, who had reviewed the study, instead testified that the current 10-ppm standard “will continue to protect our employees against any ill effects from benzene exposure.”

Although NIOSH repeatedly asked for the information, Dr. Holder did not provide the study until March 1, 1978, four months after the record closed for the OSHA benzene regulatory proceedings. Only then did NIOSH learn of the study’s worrisome findings. The study had examined workers with exposures between 2 and 10-ppm exposures to benzene, comparing them to workers with no exposures. Twenty-three percent of the exposed workers had chromosomal abnormalities as compared to 2.3 percent of the unexposed workers. In short, workers exposed to what Dow and the oil companies considered low doses of benzene had ten times the abnormalities compared to unexposed workers. Since the regulatory record did not include the study, Department

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124 Sidney M. Wolfe, “What the Court Didn’t Know.”
of Labor lawyers were unable to use it before the 5th Circuit Court of Appeals, then considering the arbitrariness of the new proposed lower standard. OSHA’s Director of Health Standards at the time has opined that “it would have been useful evidence” to present to the court.

Dow later claimed the delay resulted from Dow’s uncertainty of the workers’ exposure levels. However, the exposures ultimately provided to OSHA in March 1978 matched those used by Dr. Picciano in June 1977. Dr. Picciano subsequently resigned over the difficulties he had in releasing the data. The problems included the nine-month peer review process as compared to a negative study that took six weeks, even though the two studies used the same exposure data.¹²⁵

Although Dow has disputed Picciano’s allegations, they have received considerable support from his boss, Marvin Legator, the well-respected former Director of the Division of Environmental Toxicology in the Department of Preventive Medicine and Community Health at the University of Texas Medical Branch. After receiving his PhD in Microbial Genetics and Biochemistry in 1951, Legator had begun his career for Shell Oil in Modesto, California. He left that position to pursue what he thought would be more exciting work in the Food and Drug Administration. Several years later, and disappointed with the lack of research funds, Legator reacted positively when approached by Jack Killian, medical director at Dow Chemical. Killian described how Dow needed someone to help with a new worker-monitoring program. Legator later remembered thinking at the time, “Boy, we can really find out whether benzene is getting to the bones of these guys!” When then offered a well-funded consultanship to Dow at the University of Texas Medical Branch, he leapt at it. For a few years, Dow provided ample funds to Legator’s team, who carried out studies of chromosomes in exposed workers.

¹²⁵ Peter F. Infante, “The Past Suppression of Industry Knowledge,” 268-9; and Sidney M. Wolfe, “What the Court Didn’t Know.”
These studies commenced in 1964, with blood being taken from workers when first employed. By 1974 the program included 1,302 employees whose blood had been drawn at first employment. In 1977 two studies led by team member Picciano showed substantial increased numbers of chromosomal breaks in exposed individuals as compared to non-exposed individuals and a control group. Marvin Legator acted as one of the peer reviewers for the study. In 1980 Legator observed, “because of the serious implications for worker health, there is no question the study should have been sent to the government.” In a 2003 interview, Legator explained what instead happened next:

Now the interesting thing about this is we were probably one of the most well advertised groups in Dow Chemical as far as TV and radio, you know, the latest state of the art application of genetic toxicology in the work place, but when we found—we found Benzene caused chromosomal aberrations in workers then we found the going was a little tough. In fact, I think I can say with—in terms of the timing, that shortly after our findings there no longer existed a toxicology group at Dow. 126

Thus, as with tobacco, when researchers’ results did not fit within Dow’s litigation and regulatory position, funding was terminated.

126 Sidney M. Wolfe, “What the Court Didn’t Know”; and David Todd Interview of Dr. Marvin Legator, The Texas Legacy Project October 23, 2003, http://www.texaslegacy.org/bb/transcripts/legatormarvintxt/html (accessed May 31, 2014). Additional information on Marvin Legator comes from Marvin S. Legator, “The Successful Experiment that Failed,” in H. Tristram Engelhardt, Jr., et al., Scientific Controversies: Case Studies, 465-486, 481-486; and Devra Davis, The Secret History, 382-4 (containing quotes from Davis’ interview of Legator in 2004). This type of tobacco attorney like action was not confined to benzene. In the 1958 researchers for Shell and Dow informed those companies that the product they made in the pesticide, DBCP, shrank gonads, and caused weight loss, hair loss, and gross lesions on the lungs, kidneys, and testes. If released, it likely would have resulted in numerous lawsuits. Dow kept the internal report—provided by Mark A. Wolf—within its internal files, even though in 1960, Dow’s chief industrial hygienist, V. K. Rowe, recommended that DBCP be treated as highly toxic. Shortly thereafter the Dow toxicologist published an article in Toxicology and Applied Pharmacology noting such damage to rats, guinea pigs, rabbits, and monkeys at very low exposure levels. He also wrote guidelines for including the information in the manufacturers safety data sheets (MSDS). None of this information appeared in the MSDS until 1977, when similar damage in California factory workers surfaced. Even then, the warning did not mention the testes being a potential target. Devra Davis, When Smoke Ran Like Water – Tales of Environmental Deception and the Battle Against Pollution (New York: Basic Books, 2002), 196-97.
The Post-Pliofilm Peer Reviewed Literature

One feature of benzol (benzene) poisoning which is very irritating to insurance attorneys is the way the damage progresses even after all exposure has ceased.
- Alice Hamilton, 1936

As with other toxic substances, attorneys have long been involved in benzene medicine. In language similar to that for silica, lead, and other toxic substances public health pioneer Alice Hamilton took issue with the compensation system set up for benzene poisoning sufferers by attorneys and politicians. Hamilton’s specific complaint centered on the rigid standard established for benzene disease compensation—a standard that mandated strict conformance to specific requirements. Like silica, the compensation system seemed designed to deny, rather than allow, claims. Before the 1980s there was little or no compensation for anyone who contracted leukemia from benzene exposure. At times, physicians even altered their diagnosis from leukemia to aplastic anemia so widows could receive benefits. Hamilton dismissed this standard as wrong. She argued that benzene attacks the bones and marrow, presenting a wide range of diseases and symptoms. She bolstered her arguments with reference to literature showing various forms of leukemia and other blood diseases following benzene exposure. Hamilton was however, hopeful. She believed “the time is not far distant when it will not be necessary to show a completely classical picture, in order to prove damage from benzol.” Unfortunately, Hamilton did not have much success as a seer. Forty years passed before epidemiological confirmation of the benzene leukemia linkage. Even this did not result in easier compensation for benzene disease sufferers. Rather, it marked the full-scale entry of litigation-oriented scientific experts into the research field, with industry fighting each new revelation of benzene damage to the body.127

In the aftermath of the Pliofilm cohort report in 1977, industry attorney-controlled experts generated peer-reviewed articles directed toward responding to increasing litigation and government regulatory efforts. As already discussed, they placed a substantial focus on minimizing the bad publicity arising from public disclosure of internal reports of benzene-related leukemias. A number of the new articles and other submissions to regulatory proceedings went so far as to contend the standards already overprotected the work force.

Industry must have been aware that these efforts were unlikely to force OSHA to raise the standard. Rather, they served other legal purposes. First, if ultra-low levels of benzene caused leukemia, the EPA might require oil companies to reduce gasoline emissions through expensive new control systems during production. Equally important to the bottom line was the danger of increased lawsuits from individuals exposed to low levels of benzene—which were already rising. To counter this trend, benzene companies needed contrary evidence. Juries are often impressed with large peer-reviewed medical research articles, especially when they are numerous and they debunk government studies as “junk science—”a position that a significant minority in our country is readily willing to accept. They are certainly more impressed than if the defense team has no studies to offer in rebuttal to studies linking leukemia and other cancers to increasingly smaller exposures to benzene. Thus, similar to tobacco, asbestos, and chromium, industry litigation expert studies showing negative results often followed in the footsteps of positive government, independent, and foreign studies that fueled regulatory efforts and opened the door to lawsuits for injuries suffered due to benzene exposure.\[128\]

The first federal research initiative that industry sought to marginalize came from NIOSH’s decision to study Goodyear’s Pliofilm plant. Following the initial 1977 report, NIOSH scientists, led

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\[128\] On declining trust in scientific authority, see Harry Collins, *Are We All Scientific Experts Now?* (New York: Polity, 2014).
by Robert Rinsky, continued to follow the cohort and conduct additional analysis, publishing the first substantial Pliofilm cohort report in the *American Journal of Industrial Medicine* during 1981. With the Supreme Court ruling that OSHA must have some quantification of risk for its regulations, researchers returned to site records for exposure data, publishing a risk assessment analysis in 1984 by Kenny Crump, a statistician frequently used by defendants in toxic substance cases. Crump’s analysis, based upon not only the Pliofilm cohort but also Ott’s study and Wong’s first 1983 study, confirmed the dose response nature of benzene, with substantial excess risk well below 100-ppm-years. He also wrote that his analysis suggested higher exposures might be “less effective per ppm-year in producing leukemia than lower exposures.”

During this period Rinsky continued to update the Pliofilm cohort. Along with colleagues, he also published two other papers. The first considered the hematologic effects of benzene, while the second examined how the risk for cancers linked to benzene changed over time. They observed an overall SMR of 337 for leukemia and 409 for multiple myeloma (MM). The cohort also demonstrated a strong dose response relationship.

In the interim between their risk assessment updates, NIOSH scientists prepared an article (1996) describing the affect benzene exposure had on the Pliofilm cohort members’ white blood counts, even at relatively low exposures. This study had significant ramifications for regulation and litigation of lower dose exposures, finding a strong dose response relationship between white blood cell counts and benzene exposure. The authors wrote that their findings were consistent with animal studies that demonstrated such effects in the 10-30-ppm range. An industry study of rubber plants cosponsored by four rubber companies and the United Rubber, Cork, Linoleum, and Plastic

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129 Kenny S. Crump and Bruce C. Allen, “Quantitative Estimates of Risk of Leukemia from Occupational Exposure to Benzene,” prepared for Occupational Safety and Health Administration, 1984, OSHA Docket H-059b, exhibit 152. (In author’s possession)
Workers of America provided some limited support to NIOSH, finding associations in varying degrees between solvents and leukemias at the various plants.\textsuperscript{130}

Industry consultants, particularly those that were, or would shortly become, litigation experts, quickly attempted to discredit the findings. In an effort to counter the criticism, NIOSH scientists wrote at least one letter to the editor (1989) and an article (1995) concerning risk assessment estimates by industry-funded scientists.\textsuperscript{131} The criticism ranged widely. Even with the paucity of confounding agents—other variables that can also cause the disease—one industry consultant managed to suggest there must be confounders.\textsuperscript{132} Two other consultants published an analysis of the Pliofilm cohort in an industry-favored journal, \textit{Regulatory Toxicology and Pharmacology}. They attacked the NIOSH study by eliminating leukemia cases that they considered unrelated to benzene. However, this is not legitimate methodology. Scientists do not distinguish between diseases that might have been caused by the agent and those that are found in the general public. They count all instances of disease, since the work force is still part of the public: the effort is directed toward


\textsuperscript{132} S. H. Lamm, “Heterogeneity of the Akron and St. Mary’s Plants,” September 1, 1977, Post-hearing comments submitted to OSHA Docket H059, Exhibit no. 217–21-E.
determining how many cancers occur in excess of the public level, not how many occur that are not associated with the public.\textsuperscript{133}

Recognizing that these tactics stood on very shaky scientific grounds, most of the responses by industry, while minimizing the current risk, recognized the possibility of lymphopoietic cancers being associated with the historic production and usage of benzene at refineries and chemical plants. Instead, industry litigation experts attempted to lessen the impact of the report by contending it was no longer relevant, given modern industrial processes.

To accomplish this feat, industry turned to Dennis Paustenbach, who proposed alternative exposure estimates of the cohort—using different methods of extrapolating air sampling data to those periods where none existed, making alternative assumptions about the accuracy of the air sampling, and using modified reconstructions of the process and plant histories. The resulting numbers—Paustenbach’s—dramatically reduced the risk at lower exposure levels. Paustenbach’s reanalysis of the Pliofilm study is an archetypal example of how to mangle a good epidemiological study and neutralize its effect. Since there were few benzene level measurements in the 1940s and 1950s, NIOSH had estimated the levels. Paustenbach reduced the risk at lower levels by elevating the exposure estimates with worst-case measurements. Some workers assigned exposures were more than an order of magnitude higher than in the original study. These changes resulted in overall levels so high that NIOSH scientists remarked, “one would assume that such extreme exposures would produce an epidemic of serious benzene poisonings,” an event that had not occurred. This did not seem to bother API, which conducted a risk assessment based upon the new exposure estimates, finding that “occupational exposure only to very high concentrations” could cause leukemia.

Paustenbach’s 1992 Pliofilm study provides perhaps the best example of his ability to provide attorneys what they desire, no matter the improbability of the conclusion. The ChemRisk president reached his conclusions in this paper by estimating exposure rates in the Pliofilm cohort so high that the cohort should have been experiencing much higher rates of acute illness than actually occurred. Almost uniquely for defense expert articles, this paper included a member of the law firm Bryan Cave LLP—an also a defense tobacco and asbestos firm—as a coauthor.134

Less than ten months after the publication of Paustenbach’s new exposure estimates for the Pliofilm cohort, Kenneth Crump, prepared a new risk assessment for benzene-induced leukemia based upon Paustenbach’s numbers. “We believe that the Paustenbach et al. matrix is likely to provide a better representation of exposures in the cohort.” He noted that the “80- to 90-fold differences in risk obtained in this study between linear and nonlinear models are extremely important to the regulation of benzene.” The Western States Petroleum Association had funded the article. In a rare admission for defense experts, Crump noted that industry had reviewed the article before its publication, thanking the Benzene Task Force of the American Petroleum Institute for their review and suggestions.135

Other industry-supported authors also developed risk assessments based on Paustenbach’s reanalysis. Industry presented these new assessments, all with lower risk estimates than Rinsky’s or Crump’s previous efforts, at a 1994 the American Conference of Governmental Industrial


Hygienists (ACGIH) Threshold Limit Value (TLV) Committee meeting considering a reduction in the benzene TLV.\textsuperscript{136} In a similar 1996 effort, Mary Burr Paxton, then working for API, published a reanalysis of the Pliofilm cohort. The API and the Western States Petroleum Association funded the analysis during a prior period when Paxton worked for the defense litigation firm, Environ. Unsurprisingly, she concluded that the reanalyzed data supported a threshold model for benzene. She approvingly described Paustenbach’s 1992 effort to modify the exposure levels as “more rigorously defined exposure estimates.”\textsuperscript{137}

Although these papers all significantly reduced the projected risk, the dose response curve still showed a small risk to the lowest category of exposure. Evidently disappointed by this “half victory,” the oil industry again hired Paustenbach. This time Paustenbach reduced his exposure estimates for the highest groups but not for the lower. Predictably, the new curve fit more readily into the desired result.\textsuperscript{138}

Industry also used a second tactic to demonstrate the obsolescence of the Pliofilm cohort report and respond to Infante’s continuing articles and reports concerning the hazards of gasoline and solvents.\textsuperscript{139} A remarkable number of oil companies, including Shell Oil,\textsuperscript{140} Texaco, Chevron,\textsuperscript{141} and ExxonMobil,\textsuperscript{142} produced epidemiological studies of their own, less heavily exposed workers.


\textsuperscript{137} Mary Burr Paxton, “Leukemia Risk Associated with Benzene,” 1435.

\textsuperscript{138} P. R. D. Williams, and D. J. Paustenbach, “Reconstruction of Benzene Exposure,” 677–781.


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Each of these studies mitigated the hazards associated with benzene. Although the full extent of attorney involvement is not known, almost all of the articles display similar characteristics to those employed by attorneys for tobacco, chromium and asbestos.

Epidemiologist Otto Wong produced some of the earliest studies. He began his career as an Assistant Professor at Georgetown University Medical School after receiving his doctorate in epidemiology for the University of Pittsburgh. He quickly became involved in industry research at an early stage. By 1986 he worked for an environmental consulting firm. Following the disclosures of excess leukemias at Shell Oil’s refineries Chevron decided to have Wong conduct a study at its Richmond and El Segundo, California refineries. In his 1986 paper of the study’s results, Wong noted the very significant healthy worker effect at the facilities—overall mortality was only 72.4% of that expected—with only lymphopoietic cancer showing “a pattern of increased risk suggestive of possible relation to an occupational exposure.” Even here, the higher risk was confined to those hired prior to 1948. Since the cohort included all employees who had worked at least one year from 1950 to 1980, with at least one day of their work at the two plants, the cohort was considerably diluted, both by individuals not exposed to benzene and individuals without sufficient latency. Wong noted that the study provided “considerable reassurance” to Chevron’s workers, concerning their overall health.143

Somewhat surprisingly, Wong’s examination of benzene epidemiology in refineries did not turn out as well as the Chemical Manufacturers Association or the sixteen members of its Benzene Panel might have hoped. In the study of plants belonging to six companies Wong found a slightly raised SMR albeit, not statistically significant, for all lymphopoietic cancers. However, compared to those not exposed to benzene at the plants, the group had a relative risk of 3.2. The risk also had

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sufficient data to back it up for “the relationship between continuous exposure to benzene and leukemia mortality [to be] significant.” Furthermore, when he combined non-Hodgkin’s lymphoma to leukemia, the relative ratio (RR) rose to a statistically significant 9.60.  

In contrast to Chevron, Texaco conducted epidemiological evaluations internally through its Health Department. Dr. Barbara Divine joined the Texaco Health Department in 1978 to run their epidemiology program. During her tenure at Texaco she served on numerous industry health panels, including serving as chair of the API’s epidemiology task force for over ten years. She later worked for litigation consulting specialist, Exponent. Seven years after arriving at Texaco she published a company mortality study, similar to other refinery companies. In a series of papers, she found small elevations of benzene-associated cancers, with subgroups of pipefitters and boilermakers having more significant elevations. She and her colleagues could find no associations by years worked. They also found it impossible to classify jobs by level of exposure to specific chemicals.  

In what appears to have been a massively funded exercise, ExxonMobil also financed seven ChemRisk benzene exposure studies of four refineries: one each at Joliet, Illinois and Baton Rouge, Louisiana, along with others at Beaumont and Baytown, Texas. Exxon also financed benzene exposure studies related to gasoline, marine terminal operations and marine transportation. As with ChemRisk’s other studies for toxic substance exposure levels, its detailed examination of the client’s facilities found employees to have very little risk of exposure to excess hazards. Descriptions of the benzene air levels found by Chemrisk ranged from “very low” in Joliet, to “generally low” in Baton


Rouge and Baytown, with 0.3-ppm or lower generally in Beaumont. They found similarly low concentrations at the Baton Rouge docks. Notably, the benzene levels were also very low in comparison to benzene levels found at similar locations in studies not conducted by ChemRisk. Apparently, only marine transport benzene concentrations remained stubbornly high during their investigations. In a possible admission that marine transport exposures were not as low as hoped for, the article did not characterize them as low, indicating only that the benzene intensity “probably did not exceed the overall TWA.”

The authors of each article sought to assure their readers of the article’s lack of bias, stressing their desire to capture the highest possible exposure averages, frequently using the terms “upper tail” or “upper end” distributions to describe the exposures being reported. Yet, as noted in at least two articles, the benzene levels determined by ChemRisk differed significantly from other similar refinery studies. In contrast to other reports—even those prepared by industry—ChemRisk researchers found most exposures below 1.0-ppm, even for the period prior to 1980. Benzene concentration at the Joliet refinery was “equal to or less than those reported in the literature, despite the fact that the Joliet dataset was targeted to over-represent benzene handling activities.”

Dockworker exposures at Baton Rouge averaged below the mean of 82% of similar studies, both before and after 1990. Yet, this disparity between their findings and other studies did not raise any red flags of concern in the article. Rather, they identified better data quality and accuracy as the reason for the disparities, with theirs being the most robust analysis of dockworkers since OSHA’s formation.

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ChemRisk’s “better quality and accuracy” accrued partially from ignoring a significant portion of the samples available at the facilities. In the Baton Rouge analysis, “better data quality” included discarding 75 area samples because they “were not associated with any specific job titles or tasks, and were judged not to be representative of typical exposures of dock facility employees” who did not work in one location. The Juliet study eliminated 258 samples (over 13% of the samples) as non-representative since they were area samples rather than specific jobs. They also excluded a further 94 emergency response samples and 125 instantaneous samples. The Beaumont study eliminated 869 area, emergency, and unknown samples—over 12% of the total. Since the articles did not disclose the benzene exposure levels contained in these samples, unless a plaintiff attorney obtains them in discovery, we may never know if these samples would have made the results more in line with other, non-litigation oriented studies. 147

At least two courts might take issue with ChemRisk’s characterization of ExxonMobil’s—particularly Mobil’s—benzene airborne concentrations as “low” in the 1970s and 1980s. In Mobil Oil Company v. Anna Mae Ellender, the Texas Court of appeals upheld a finding of gross negligence for its actions in causing Mr. Ellender’s leukemia, which the court attributed to his significant benzene exposures. The judge specifically noted that workers had benzene exposures from ditch and sewer emissions, fugitive emissions from leaking stacks, valves, and pumps, and while working on pumps. Moreover, the court observed that in the 1970s and at least the early 1980s Mobil allowed and even encouraged individuals to wash their hands, clothes and tools in benzene. Certain workers also frequently steam-cleaned equipment that had contained benzene. Mobil’s own industrial hygienist admitted that during his 1973-1976 tenure “it was a normal occurrence . . . benzene was leaked to the ground or spilled to the ground on the disconnection and connection operations. So there was

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always benzene being spilt on the ground.” In addition, Mobil failed to enact a benzene leak
detection/prevention program until 1985. As late as 1988 Mobil had not even labeled all of its
benzene process lines as required by OSHA. Given the facts contained in this and other similar
cases, it is little wonder that ExxonMobil attorneys sought evidence downplaying benzene exposure
risks at refineries.148

Ellender also provides context to ChemRisk’s statement in the Beaumont article that the
petroleum industry has implemented exposure control programs for “decades.” While possibly
technically correct, if the phrase were changed to “reasonable control programs,” for Mobil the
“decades” do not extend back much before 1990, the date which marked the end of ChemRisk’s
investigation. Thus, as the court’s opinion makes clear, for almost the entire period covered by
ChemRisk’s investigation, Mobil did not have “reasonable control programs.”

In the discussion of whether evidence supported a finding of gross negligence, the Ellender
courts looked at Mobil’s programs to control benzene and the information provided to its
employees in the 1970s to 1980s. Monitoring for benzene at the Beaumont plant was almost non-
existent prior to 1976. Warning posters about benzene only appeared at the refinery in
approximately 1987. Even then they contained inadequate information. In 1996 a safety officer of
Mobil did not learn that benzene could cause leukemia until just prior to his deposition, when
Mobil’s attorney informed him of the connection. If its own safety officers were not aware of the
risks, then Mobil’s air monitoring, even when it occurred, was likely not representative of the plant’s
conditions. How can the proper conditions be monitored if the personnel in charge of safety have
no idea they should be monitored? In addition, while Mobil conducted air monitoring for its own
employees, contract workers who performed much of the maintenance work were purposefully

148 Mobil Oil Corporation, v. Anna Mae Ellender, 450-452, 451 (“it was normal”); Shannon H. Gaffney, et al.,
“Occupational Exposure,” 285 (Beaumont); and Mobil Oil Corporation v. Anna Mae Ellender, 923.
excluded. Nor did company officials provide protective equipment such as respirators to contract employees, even though they directed where the individuals worked.\textsuperscript{149}

Other circumstantial evidence also points to the likelihood that the exposure levels used by ChemRisk are not representative of typical refinery conditions. It is extremely coincidental that on the one hand, each of the ExxonMobil studies found low exposure levels, even in comparison to other industry studies, while conversely Paustenbach’s 1992 review of the Pliofilm cohort found higher levels, in some cases extraordinarily higher levels, than other independent researchers. One possible reason for this is the differing results desired by clients for litigation. In the Pliofilm case, the API desired high exposure levels in the cohort to enable risk assessors to estimate only minimal risks at benzene exposure levels below 10-ppm. In contrast, in the ExxonMobil studies the funder—and its attorneys—desired low levels in order to establish at trial insufficient exposure of individual plaintiffs to cause benzene-related diseases.

The benefits provided to attorneys from these studies included more than just low exposure levels. During the process of conducting their exposure research, the group also focused on two other issues of great importance to trial attorneys during Daubert hearings and at trial: establishment that their study is more appropriate than other studies and a complex methodology for determining the exposure of individuals exposed at refineries and other locations. In each article, the authors stressed the thoroughness of their investigation and why it was superior to prior research. This argument can be critical in litigation, since, as noted above, many of the other studies found higher benzene concentrations.

The papers’ depictions of the appropriate methodology in determining exposures as being multifaceted have the potential for even greater value to attorneys. Several of the studies offer up a complex and time consuming litany of necessary steps required for accurate exposure analysis. The

\textsuperscript{149} Mobil Oil Corporation, v. Anna Mae Ellender, 450-51; and Mobil Oil Corporation v. Anna Mae Ellender, 923.
Joliet study offers a one apt example. Since the investigation determined that air concentration is driven by task, not job or location, each task must be analyzed in an individual’s working lifetime to determine the total exposure history. For nearly every worker, this undertaking is extremely time-consuming and, for those who have died, might be impossible. In addition, if this analysis is necessary for an accurate analysis, then virtually every previous—and higher exposure—studies are obsolete, and plaintiff experts relying upon them are subject to challenge under Daubert analysis.

The Baton Rouge study made this very point, noting that no other study had considered all the necessary factors in estimating exposures.150

ChemRisk also has been at the forefront in producing papers on other types of benzene industrial and commercial usage. In one of the most recent ChemRisk articles, lead author Jennifer Sahmel, along with coauthors including Paustenbach, examined workplace exposures to benzene at a petrochemical plant. This paper provides an interesting comparison with tobacco papers. As frequently occurred in papers written for tobacco companies, the Sahmel paper utilized “internal studies” that had not been peer-reviewed, conducted an extremely cursory “literature review,” distorted the literature it did review, and took the exact stance its sponsor likely desired for litigation.

150 Marisa L. Kreider, et al., “Benzene Exposure,” 671-690 (Joliet); Juliet Panko, et al., “Occupational Exposure,” 528-9 (Baton Rouge). In the Exxon Valdez litigation defense counsel served subpoenas on scientists assessing the environmental damage resulting from the oil spill seeking all records, data, and ongoing research. One researcher complained the requests “permanently disrupted” his research “due to the constant need to respond to motions and affidavits.” Thomas D. McGarity, et al., Bending Science: How Special Interests, 175. See Steven Picou, “Compelled Disclosure of Scholarly Research: Some Comments on ‘High Stakes Litigation,’” Law & Contemporary Problems 59 (1996): 149, 155. The British Royal Society wrote a letter to EXXON demanding withdrawal of its support for dozens of groups that have “misrepresented the science of climate change by outright denial of the evidence.” Thomas D. McGarity, Bending Science: How Special Interests, 274. Exxon’s willingness to distort science was also evident in its position on climate change. In 2006 the Royal society, Britain’s independent Academy of Science, sent a letter to Exxon’s UK subsidiary, deploring its continued “inaccurate and misleading view of the science of climate change.” See Bob Ward, British Royal Society, to Nick Thomas, Exxon, September 4, 2006, image.guardian.co.uk/sys-files/Guardian/documents/2006/09/19/LettertoNick.pdf). Recently, documents have come to light disclosing that ExxonMobil scientists informed the company as early as 1981 that fossil fuels were affecting the environment. The company not only failed to disclose their findings, it spent more than $30 million on think tanks and researchers to promote climate denial. Suzanne Goldenberg, “Exxon Knew of Climate Change in 1981, Email Says – but it Funded Deniers for 27 More Years,” The Guardian, July 8, 2015, http://www.theguardian.com/environment/2015/jul/08/exxon-climate-change-1981-climate-denier-funding, accessed January 24, 2016.
The authors stated that they conducted the “literature review” to compare the results from an “unpublished proprietary study” conducted by their client with other published studies. The review found only two relevant studies: a study of a Dow facility, along with a follow-up report of the same facility. After reporting that the Dow plant had exposure ranges of 0.1-ppm to 35.5-ppm, they informed readers that the Dow studies “demonstrated a statistically lower incidence of total mortality rates” and wrote that the original study author “indicated that there were no significant increases in total mortality rate relative to estimated career exposure to benzene.” The ChemRisk authors went on to declare that the Dow research was “consistent with the results from the\[ir\] facility’s own internal epidemiological analysis,” with no association between benzene at these levels and disease.\(^1\)

While the ChemRisk article is technically accurate, its authors ignored several interesting facts about the Dow articles. First, although the original investigators opined that they did not find increases relative to “estimated career exposure” they did discover an excess of leukemia: three cases versus the 0.8 expected from the Third National Cancer Survey data. Even after the authors ruled out one case due to an exceedingly weak potential confounding factor, two cases remained, still a significant excess. After a comprehensive review of work histories and moderate medical records and discussion of all possible confounding factors, they refused to rule out a connection to benzene. Rather, the authors simply stated, “varied work histories and the lack of medical history made a retrospective assessment of the possible relationship to benzene exposure very judgmental.” This statement is a far cry from what Sahmel, et al. implied about the article.\(^2\)

\(^2\) M. Gerald Ott, et al., “Mortality among Individuals,” 3-10, 9 (Dow Chemical Company employed the authors.) (The article’s discussion further states that “three case histories of employees with leukemia were reviewed . . . In our population the expected incidence of leukemia, excluding lymphocytic or monocytic cell types was 0.8 cases . . . compared with three observed cases \(P < .047\)”).
The text of the follow-up study also seems at odds with ChemRisk’s analysis. Like Sahmel, the follow-up authors only used the portion of the original study most advantageous to Dow, stating, “after excluding cohort members with other hazardous exposure [a very tenuous connection], they observed one death from leukaemia against 0.9 expected.” To the authors’ credit, they mentioned that another individual’s death certificate indicated that he had “myeloblastic leukaemia,” although it was not listed as the cause of death. Thus, they actually still included two leukemias from the original study.

Even with this apparent de-emphasis of the original findings, the article does not fit within Sahmel’s conclusion that it failed to find an association of benzene with leukemia “at petrochemical plant exposure levels.” Simply reading the abstract of the updated study immediately raises doubts about the accuracy of Sahmel’s assertion: “four were myelogenous leukaemias and this represented a significant excess in that subcategory.” In their discussion Dow authors never stated that the diseases were not in excess, they simply stated that they could not find a dose response relationship, since “analyses by work area, duration of exposure, and cumulative dose index did not show patterns suggestive of causal relationship between exposure to benzene and any particular cause of death.” However, with a small cohort and only four actual cases, finding such a dose response relationship would be extraordinarily difficult, particularly with a substance like benzene, which is well known to affect different individuals in very different ways and can permeate an entire facility.

In the end, and contrary to Sahmel’s assertion, the authors admitted that the “study provides support for the association between exposure to benzene and acute myelogenous leukaemia,” while also stating the small numbers and competing exposures made its use for risk assessment problematical.153

ChemRisk’s solvent studies raise similar questions about their accuracy and purpose. In one study, the authors admitted that the motive for the studies was the “filing of thousands of so-called “trace benzene” lawsuits in the United States over the last few years.”154 As with the refinery data, in almost all cases the benzene concentrations found by ChemRisk were portrayed in the best possible light for usage by benzene defense counsel. Like the oil industry analysis, ChemRisk scientists described their solvent studies as worst-case upper bound scenarios that might overstate concentrations. For example, they noted their use of garages, whereas—according to them—most applications occurred in large cool rooms, with better ventilation.

Nearly all of ChemRisk’s studies examined products with benzene concentrations below 0.1%. In one group of experiments they spiked solvents with 0.003, 0.008, and 0.07% benzene. Even then the short time period sampling of the highest concentration produced an exposure of 0.27-ppm. In another study—although opining that benzene constituted a trace concentration even when it approached 2% of a formulation—they only investigated formulation levels at less than 10% of that level. Again they found exposures to relatively small samples of this formulation for short periods could approach 0.3-ppm.155 Neither study reported whether other formulas were either considered or analyzed but not reported. Given their claim that “trace concentrations” can approach 2%, such a possibility cannot be dismissed.

During the course of conducting the tests, the investigators developed methodological techniques that make determining whether an individual has a moderate to high exposure very problematical. For example, in accordance with ChemRisk’s methodology, when conducting a dose recreation, the investigator must account for solvent composition, application techniques, and space characteristics: factors which are almost always uncertain and time-consuming to recreate. This

information is often, at least to some degree not available—especially when the plaintiff is deceased:
once more introducing stiff Daubert hurdles in plaintiff cases.\textsuperscript{156}

ChemRisk’s sole group of studies that investigated higher benzene concentrations also
appears to have been designed to obtain low exposures. For a Liquid Wrench formula containing
30\% benzene, they conducted a purported hour-long study. In actuality, the test considered two
fifteen-minute applications spread out over an hour. Each application consisted of two teaspoons of
Liquid Wrench—enough, they averred to loosen 6-8 bolts. (I am admittedly not very mechanically
minded, but I have loosened many bolts and nuts with Liquid Wrench. I cannot remember ever
using less than \( \frac{1}{2} \) teaspoon per bolt or nut—often much more—in my efforts.) In another test
procedure that reduced the study’s exposure, two circulating fans, one at a window, ventilated the
400 square foot garage test site. Even with these methods of ensuring a low exposure result, the air
sample taken from the tester during the “hour” test had a concentration of almost 3.0-ppm, even
though during the fifteen-minute breaks the individual stood more than ten feet from the solvent.

Incredibly, ChemRisk scientists stressed that this scenario “may be more representative of
upper bound or worse case conditions” since occupational settings would have “exhaust ventilation
and/or cooling fans or air conditioning, larger work rooms, or removal of rusted bolts would be
conducted outdoors.”\textsuperscript{157} In contrast to ChemnRisk’s claims, while reading the solvent experiment
articles, I could not help thinking of my childhood friend, whose father was a mechanic. His dad
worked in a garage that, in the winter, was closed with little ventilation. My friend’s home also
contained a workshop in the basement where his father repaired and maintained equipment. He
frequently used Liquid Wrench in his workshop, often bringing it home from the garage. When my

\textsuperscript{156} Dana M. Hollins, et al., “Airborne Benzene,” 324-332.
\textsuperscript{157} Pamela R. D. Williams, et al., “Airborne Concentrations,” 556, 560.
friend and I went down into the basement we would often have to walk around parts sitting in one or more buckets of solvent for cleaning.

These activities were not atypical for mechanics. During the sixties and seventies everyone used solvents freely, without thought of the health consequences. Admittedly, I am not a scientific expert on benzene or the specifics of how solvents were occupationally used into the 80s. Yet, the evidence presented at the Ellender trial appears much more consistent with my recollections of how industrial and mechanic operations worked into the 1980s than with ChemRisk’s experiments.

The findings of one experiment conducted in the seventies also raise further questions about the methodologies and applicability of ChemRisk’s research. In 1978 Peter Infante attended a conference in which he discussed the leukemia his team was finding among the Pliofilm cohort. While there he mentioned a small experiment he had conducted: an investigation that fosters serious questions about the findings of the ChemRisk studies, since the results are orders of magnitude higher than ChemRisk’s. If accurate, they suggest that the past level of exposure in the assumed background general population is higher than thought, raising additional questions about whether the national populace harbors unnoticed benzene-induced leukemias.

Since this meeting is a report to the public, I felt it important to show data for environmental concentrations of benzene in a home garage while stripping paint from furniture with a commonly sold home product. The product itself, which anyone could go out and buy over the counter, contained 52% benzene by our analysis. In the first sample, the atmospheric concentration was 78 ppm, the third was 79 ppm, the fourth was 225 ppm, and the fifth was 195 ppm, with a mean concentration of 130 ppm. This increase in benzene concentration over time is a reflection of the build up of benzene vapor during the 30 minute sampling period.¹⁵⁸

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A more recent substantial international initiative has caused an even greater flurry of defense litigation expert activity. This large two-nation study has called into question the relevance of

industry’s entire low-level benzene exposure research. The project’s origin began with a Chinese Ministry of Public Health-organized investigation of benzene. Initially, the Chinese Academy of Preventive Medicine conducted a nationwide survey in 1979-1981, identifying over 500,000 benzene-exposed workers. The following year Chinese researchers initiated a twelve-city retrospective cohort study, comparing 28,460 benzene exposed workers from 233 factories to 28,257 control workers from 83 factories. The exposure data came from factory records, with the control group being selected from factories with no evidence of benzene.

A 1987 account of the study reported the initial findings, with a subsequent 1989 narrative providing more detailed information about exposures and disease. As reported in 1987, the benzene group had an SMR of 574—almost six times the standard mortality of the general population—with an even higher relative risk between the exposed and unexposed groups of 6.97. The workers’ average exposures ranged primarily between 16 to 160-ppm, with three cases of leukemia in individuals exposed to a low level average exposure of approximately 0.7-ppm.

The two papers provided substantial evidence of the widespread effects of benzene in China, while noting that further investigations are ongoing. They related that, in contrast with a U. S. industry-sponsored refinery study of 454 workers with exposures below 1.0-ppm that did not find any deaths from leukemia, the overall findings in this study were “in line with the findings by Rinsky . . .” that suggested even low dose benzene exposures can cause leukemia. Moreover, the 1989 article reported a significant excess of lung cancer among nonsmokers as well as suggestive possible increases in stomach, liver, esophagus, and other sites. This paper also announced the study’s expansion through collaboration with the United States National Cancer Institute.\(^\text{159}\)

Initial NCI participation commenced in 1987, with the agency providing additional funding to expand the cohort, while also extending the study’s time frame and expanding investigative techniques of the original analysis. The additional resources enabled an increase in the cohort to 74,828 benzene-exposed workers and 35,805 unexposed workers. Cohort members were still young, with fairly short latency periods, having worked between 1972 and 1987 in the 12 original cities in a variety of occupations. In two 1994 papers, the group described the general methods and resources upon which they had based the expanded investigation.\textsuperscript{160}

Substantive results began appearing two years later. In 1996 and 1997, the group published several articles concerning their malignancy findings. The first reported a narrower cancer effect than the original Chinese study, although the suggested associations still expanded prior acknowledged links between benzene and cancer. The links included significantly increased risks for all lymphohematopoietic malignancies, malignant lymphoma, leukemia—particularly acute myelogenous leukemia—aplastic anemia, and myelodysplastic syndrome, with marginally significant excess of lung cancer. While noting that investigations continued, the authors concluded, “Employment in benzene-associated occupations in China is associated with a wide spectrum of myelogenous and lymphocytic malignant diseases and related disorders.”\textsuperscript{161}

Two later articles utilized the study’s information on benzene exposure levels to report on dose-response relationships. The first, in 1996, preliminarily found a risk of lung cancer (SMR = 230) paralleling increases in cumulative exposure. The analysts also discovered additional suggestive associations with nasopharyngeal and esophageal cancer, with statistical evaluations of other associations continuing. The second article’s results included findings of a relative ratio (RR) of 2.2 between the two cohorts for all hematologic neoplasm when exposures did not exceed 10-ppm, with

\textsuperscript{161} S. N. Yin, et al., “A Cohort Study.”
an even higher RR of 3.2 for AML, and MDS. In addition, workers with more than ten years of benzene exposure had an RR of 4.2 for development of non-Hodgkins lymphoma, with the strongest link being to exposures more than ten years prior to diagnosis. Risks for the disease also had a slight tendency to increase with increasing exposures. In the report’s final sentence, the authors prophetically alluded to the coming battle of contrasting research papers when they predicted “the elevated risk for ANLL/MDS [Acute nonlymphocytic leukemia] with recent exposure, and the possible links with NHL are all provocative new observations that should enhance our efforts to understand benzene carcinogenesis in human populations.”

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The battle of contrasting literature exploded in the new millennium. For the early years of the new century, PubMed lists more than 1,700 publications under the search terms “benzene and toxicity”. The combined search words of “benzene and leukemia” results in a list of 464 articles. Yet it is the search word combination of "benzene and China” that results in the most articles: 4,123.

A large proportion of the publications came from the joint Chinese/NCI study and industry’s reactive articles.

As the new century dawned, Chinese/NCI collaborators expanded their investigative techniques, searching specifically into the manner in which benzene affects the human body. The inquiry resulted in publication of a dramatic series of articles. The first paper confirmed earlier findings of increased risk for ANLL and aplastic anemia among workers exposed to benzene. The relative risk for non-Hodgkin lymphoma and chronic myeloid leukemia also exceeded 2.5. In

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addition to the overall increased risk, it even increased for those individuals with less than 10-ppm-year exposures. Two years later Chinese members of the group published an article in Chinese—with an English abstract—reemphasizing the importance of considering individual variation in genetic susceptibility to benzene, an issue that has been studiously ignored by industry authors. That same year collaboration members first reported seeing benzene dependent reductions in white and red blood cells. Significantly different readings even appeared between the control group and members with exposures as low as 0.25-ppm.

At the same time, OSHA researchers continued work on the Pliofilm cohort. In a 2002 article, Rimsky demonstrated a clear dose response relationship between benzene and leukemia; this time providing evidence of excess risk below 31-ppm-years.\[164\]

This series of articles generated considerable pushback from industry experts, primarily focusing on the exposure calculations and the potential of a threshold below which exposures did not pose a calculable risk. Even before the combined results from the Chinese/NCI study became available, litigation consultants attacked the overall study. By the late 1990s, a series of API and industry-funded “attack” papers began appearing in journals. Each article minimized the hazards of benzene. Without disclosing his relationship to the API, Wong launched a series of attacks on the study. He argued that the small number of cases for specific disease categories made the results extremely unstable. Moreover, the study contained several potential biases and confounding factors. To him, however, the most important failing came from the improper calculation of the exposure estimates, which he claimed were too low. Taken together, the study’s numerous problems made it

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unreliable. In a similar fashion, another industry consultant, Robert A. Budinsky, strongly criticized the NCI study for exposure misclassifications and dose estimates.\textsuperscript{165}

These criticisms did not stop the growing concern about low dose benzene risks. With the 2004 China Academy of Preventive Medicine/NCI report, new events seemed to engulf industry’s low exposure/threshold proposals. As the headline of the December 2004 issue of \textit{Science} announced about benzene, “A little is still too much.” With more accurate exposure measurements, the study documented blood disease—altered white blood cell and platelet counts—even in individuals with benzene exposures below 1.0-ppm. In what they described as “particularly robust” findings, the authors described the results of an examination of white blood counts in 250 benzene-exposed workers as compared to 140 controls. The exposed group, even those with exposures below 1.0-ppm, had significantly lower white blood and platelet counts than the control group. In conclusion the authors emphasized that although the data needed to be confirmed it provided “evidence that benzene causes hematologic effects at or below 1.0-ppm, particularly among susceptible subpopulations.”\textsuperscript{166}


Yet, major actors in the petroleum industry were already conducting a counter offensive. When the continuing criticisms and industry epidemiologic studies of the 1980s and 1990s seemed to gain little traction with the scientific community and governmental agencies, the petroleum industry decided to double-down instead of spending the billions of dollars its public relations personnel insisted it would take to control benzene exposures at 0.1-ppm. The rising tide of personal injury lawsuits likely also played a large role in industry’s decision to follow tobacco’s lead by spending massive sums in defensive health studies.167

As had similarly occurred with disclosures of company and attorney activities in relation to asbestos and chromium, most of the documents describing the incubation and birth of this offensive came to light through litigation. In 2005 Lance Lubel, a Texas plaintiff trial attorney, obtained boxes of documents from Dow Chemical Company through discovery in a lawsuit he had filed on behalf of a worker with leukemia. As often occurs, shortly thereafter an unnamed source provided the media, this time the Associated Press (AP), with a number of documents detailing the efforts of the American Petroleum Institute, its Health and Product Safety Stewardship Committee, and others, to obtain major funding for the “Shanghai Health Study” for benzene.

In a story datelined April 28, 2005, the AP provided the first glimpse at the study’s origins. According to the industry documents, the study—expected to cost in excess of $100 million dollars—had the stated purpose of “respond[ing] to allegations” in the NCI/China study. Fearing the possibility of tighter regulations and increasing numbers of lawsuits, petroleum industry representatives devised the study as a means to provide a regulatory and litigation counterweight to the low dose findings of the NCI/China study. The internal documents establish that after

publication of the first reports from the NCI/China study, several petroleum companies agreed to the API proposal, forming a consortium—initially funded by $27 million—to “establish and fund a research program on the lymphohematopoietic health risks from occupational exposure to benzene.” At least five companies agreed to fund the study: BP PLC, ChevronTexaco Corp., ConocoPhillips Co., ExxonMobil Corp., and Shell Chemical, a unit of Royal Dutch/Shell Group of Cos.

As part of his leukemia case, Lubel took depositions of several individuals conversant with the study. In one deposition, Dr. Gerald Raabe, an ExxonMobil employee who had led the API’s Health and Product Stewardship Committee, acknowledged that he took part in raising funds for the study. For his part, Raabe stated his actions resulted from concerns about the soundness of the NCI/China study and his belief that, upon hearing of the study’s results, plaintiff lawyers would believe benzene had health effects for which there is little evidence. Thus, in expressing the basis for his actions, Raabe, directly admitted that a strong factor in designing the industry study, involved obtaining litigation evidence.  

The preliminary pitch for the research similarly assumed that the results of any investigation funded by industry would support the industry position. The proposal, written by Craig Parker, a Marathon Oil toxicologist, described four strategic concerns resulting from the continuing NCI/China reports, that the study would counter: 1) calls for reformulation of motor gasoline “which would have massive financial impacts on petroleum refiners;” 2) further controls on emissions from refineries and marketing facilities; 3) cleanup of petroleum contaminated soil and water; and 4) “litigation alleging induction of various forms of leukemias and other hematopoietic

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diseases from exposure to petroleum derived benzene result[ing] in millions of dollars in expenses to industry.”

In a memorandum to Marathon’s toxicology and product safety manager, Parker touted the project by describing the dramatic benefits of the industry research discussed in the formal proposal: “strong scientific support for the lack of a risk of leukemia or other hematological disease at current ambient benzene concentrations to the general public”; “adherence to current occupational exposure limits (in the range of 1-5.0-ppm) [because they] do not create a significant risk,” and refutation of “the allegation that Non-Hodgkins lymphoma can be induced by benzene exposure.” He also hoped the study would demonstrate a threshold level for all benzene induced disorders, establish whether or not “sensitive subpopulations” existed, and establish a unique molecular signature for benzene-induced leukemias. In making these predications, Parker provided no evidence or analysis as to why the research would reach conclusions that were diametrically opposed to the results of the ongoing NCI/China research.

Thus, as often occurs in industry research that is designed to support litigation, everyone expected the research to fully back industry’s position, without any further discussion of the underlying science. Such analysis was not needed, since, when appropriately designed, almost any research can reach a desired result. Through these means, corporate officials and lawyers are almost never mistaken in their belief that industry research will find that studies criticizing their workers’ exposures are wrong.

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170 C. M. Parker, “International Leveraged Research Proposal.”
In setting forth the desired end product for the study, Parker listed as desirable the very types of “peer reviewed” evidence most useful in defense of lawsuits. These proposed end products included establishment of a safe threshold level of 5.0-ppm and distinguishing between various types of leukemia. Attorneys can use these types evidence to defend virtually every lawsuit, no matter the disease. Acceptance of 5.0-ppm as a “safe” dose for a 40-year career means that plaintiff counsel would have to demonstrate, usually in substantial detail, how a plaintiff’s exposure reached 200-ppm/years. Given the paucity of sampling data, and the numerous ways defense experts can critique exposure analysis, this is an extraordinarily difficult feat to accomplish. Moreover, even if this level of exposure can be be established, the allegation of “unique signatures” for each type of leukemia—backed by “peer reviewed” articles—means almost every diagnosis can be questioned.

The extent to which Parker consulted with industry attorneys prior to his presentation is not known, however he did propose that Otto Wong and Richard Irons—both benzene defense trial experts—lead the respective epidemiology and hematology/molecular biology aspects of the study, with Thomas Armstrong of Exxon Biomedical Sciences Inc. assisting the industrial hygiene team. The teams would focus on three components. The first involved a case study “investigating the hypothesis that there is a relationship between benzene exposure and NHL and or AML.” The second component examined the relationship between AA and MDS with AML. The final component involved investigated biomarkers and benzene effects on a focused population.

Parker’s PowerPoint presentation—used to solicit funding for the study—noted that the proposed study had the added benefit of being able to claim independence from industry, with additional independent boards for monitoring. The presentation did not mention, although everyone likely knew, that several of the individuals on the “independent” boards consulted for industry. For example, Jerry Rice, chairman of the “external Scientific Advisory Panel” has worked with at least three industry related law firms and other chemical concerns. In addition, the Consortium’s
committee structure ensured that the research could be controlled, providing for a business oversight committee with final approval over any protocol decisions made by the review boards. As finally constituted, the study—to become known as the Shanghai study—closely followed the main elements of Parker’s proposal.\textsuperscript{171}

Both at the time of the proposal and when the documents came to light, several individuals raised questions about the appropriateness of the study. Upon learning of the study, Myron Mehlman, Mobil’s former chief toxicologist, wrote to 45 corporate executives, including the CEO’s of all five petroleum company participants—British Petroleum, Chevron, ConocoPhillips, ExxonMobil, and Shell—requesting that they transfer the study to a truly independent investigative body. In response, the study’s “Oversight Committee Chair” informed Mehlman that the “present study was designed by preeminent scientists independent of the sponsoring companies.” This response seems at odds with the Parker PowerPoint presentation. Yet, the statement palls in comparison to her next claim for she further noted that one of the study’s Principal Investigators was from “Applied Health Sciences, an independent research firm in San Francisco.”

In making this claim of “independence,” the chairwoman failed to mention that many of the “preeminent scientists” and “independent research” firms had long track records of providing industry with the conclusions they desired. Principal Investigator and long time industry consultant Otto Wong, for example, headed the “independent research firm” Applied Health Services. An “independent” professor from the University of Colorado, Sherilyn Gross, began employment at

ChemRisk while involved in the study. In addition, ExxonMobil employed at least two of the investigators.¹⁷²

While some might accuse Mehlman of bias in his expression of concern, he was neither alone, nor the first to object. Other independent scientists and industry insiders also questioned the study’s perception of bias. For example, toxicologist Jonathan Ward, the Director of the Division of Environmental Toxicology in the Department of Preventive Medicine and Community Health, University of Texas Medical Branch, questioned the scientific appropriateness of portions of the study. Even some insiders felt uneasy. One of the companies approached for funding, Dow Chemical, decided not to contribute after Dow epidemiologist James J. Collins warned management that the study could generate substantial risk biases. In a 2004 deposition, James J. Collins, then in charge of epidemiology for the company, stated that while he did not know if his advice was the reason for Dow’s reluctance to contribute, he told Dow management that the study’s design could produce a statistical bias toward a low risk result.¹⁷³

In the end, the net result of the conflicting NCI/China and industry Shanghai studies was not an advancement of generally accepted medical science, but rather publication of an increasing number of competing and, for the most part, diametrically opposed articles, muddying the waters in the medical subfield. One set of articles by NCI and Chinese government-funded scientists, stressing caution, advanced new evidence of low dose damage from benzene. The other group of articles,

¹⁷² At least one of the “independent” scientists on the “scientific review panel,” was apparently well liked by industry, having also participated in the previously discussed Chromium “independent review board.” Peter F. Infante, “The Past Suppression of Industry Knowledge,” 271. See Myron Mehlman to 45 Petroleum Industry Executives, June 29, 2005; and Patsy Clegg (Oversight Committee Chair, Shanghai Health Study and in 2006 an employee of Shell) to Myron Mehlman, July 29, 2005. Portions of these two letters are found at “Benzene Industry-Conducted or Industry-Sponsored Studies,” http://www.digitalsynergy.com/web_portfolio/metzger/benzene-industry-studies.shtml, (accessed August 1, 2015) (a website sponsored by the plaintiff law firm, Metzger Law Group.) Ms. Clegg’s 2006 business address and email address were found at “Voluntary Children's Chemical Evaluation Program (VCCEP) Peer Consultations on Benzene, June 15-16, 2006, List of Attendees,” http://www.tera.org/Peer/VCCEP/Benzene/VCCEP%20BENZENE%20MEETING%20REPORT%20APPENDICES.pdf, (accessed August 1, 2015).
primarily from long-term industry consultants, trial expert witnesses, and scientists from a Chinese University well-funded by American petroleum companies, largely provided the exact results anticipated in Parker’s original proposal and memorandum to industry.

In one example, Wong and colleagues studied which risk factors are most closely related to AML and each of its subtypes. They found several environmental and occupational factors that were even more closely related than benzene to certain subtypes. This type of information is extremely useful to defense attorneys. Benzene is a well-known cause of AML, yet in cases involving specific subtypes and occupations the defense now has evidence to contend that the plaintiff’s condition was not caused by benzene.174

Wong’s next article, limiting the diseases caused by benzene, appeared but one page later in the same issues of Chemico-Biological Interactions. In this parallel study Wong and colleagues examined the risk factors for non-Hodgkin lymphoid neoplasms. Although they found that benzene could be a risk factor, this applied to only two of seven subtypes of the disease. Thus, the defense could rule out benzene as a causative factor for plaintiffs with the other subtypes. In addition, other substances also caused the two subtypes, thus clouding the picture even here. Another study undertaken and written primarily by ChemRisk and Irons—and touted as probably the “largest series of CMML [chronic myelomonocytic leukemia] cases diagnosed prospectively in a single laboratory”—concluded “benzene exposure does not appear to be a significant predictor of CMML.”175

Another series of articles that came out of the Shanghai study, appeared, at first glance, to provide information that could harm certain defendants’ benzene cases for a particular disease, MDS. However, even here, appearances can deceive. First, the articles caused very little, if any, harm.

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to the industry’s positions at trial. MDS is less serious than leukemias and constitutes a very small proportion of benzene litigation. More importantly, while the articles might have implicated benzene in MDS, they constituted a negative study for the more important and litigation costly malignancies. In one study, the group examined an international pooled analysis of low-level (>1.0-ppm) exposed refinery workers. While the researchers found risks of above a 2.00 standard mortality ratio (SMR) for MDS for almost all occupations, neither AML nor CLL—much more serious and costly diseases—demonstrated increased risks.

The authors suggested two possible reasons for this finding. First, prior studies may have mistaken some MDS for AML—the alternative that they suggest is most likely. Of course, they did not consider the possibility that they might be mistaken in their diagnosis. In support of their preferred conclusion, the scientists referred to three articles from the Shanghai study, ignoring articles from the China/NCI study. Alternatively, they raise the possibility that benzene might cause MDS at low levels, whereas AML—a much more severe disease—requires higher concentrations. The first alternative had the added benefit to industry of casting doubt on old studies, whereas both alternatives raised the level of exposure needed for AML. Refinery companies also received an additional benefit from the two articles: they cast doubt on a prior Australian study that found a strong association of AML with benzene at refineries. The new studies’ authors, which included the author of the prior Australian study—now all being funded by North American and European petroleum industry—suggested the fault lay with the old study, along with the lack of a relationship in updated case subjects.176

Yet, the industry-funded group has not constrained themselves to only publication of their own research. In a move reminiscent of tobacco, they have also publicly critiqued the most challenging NCI findings. The critiques have been both strident and far-ranging, taking issue with assumptions and data gaps typical of all medical studies. As earlier discussed, Wong began these attacks in the late 1990s—without disclosure of his connection to industry—in his article and letters to the editor outlining grave problems throughout the NCI/China program. Since 2000, Wong’s and other consultant attacks have only increased in their fervor.

In 2010, virtually concurrently with the Shanghai Study’s release of its observations of benzene’s limited ability to cause non-Hodgkin lymphoma, a refinery company trade association euphemistically named the CONsevation of Clean Air and Water in Europe (CONCAWE) paid Douglas Weed to write an article published in the *Annals of Epidemiology* criticizing the University of California researchers’ 2008 California and NIH grant-supported meta-analysis, which identified evidence of causal links between non-Hodgkins Lymphoma and benzene.177

Yet industry’s response to dramatic recent NCI findings provides the best example of this type of criticism. NCI researchers’ discovery of very low-level effects from benzene threatened to

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increase union demands for regulation, and more importantly, potentially dramatically augmented the group of individuals with potential lawsuits. This prospect arose from research that has raised the strong possibility that, at low doses, benzene’s effects might be supralinear—meaning the damage caused by benzene decreases at a lower rate than reductions in the dose.

In a series of three articles in 2006-7, Korean researcher Sungkyoon Kim and his coauthors described urine measurements of Chinese workers that displayed hematoxicity at levels below 1.0-ppm, some as low as 0.2-ppm. At low levels the metabolism of benzene appear to favor the toxic metabolites hydroquinone and \(E,E\)-muconic acid. Through a series of measurements and analyses the group determined that metabolism of benzene actually increased at levels below 1.0-ppm, a finding with potentially devastating implications for the concept of a threshold level for benzene. They concluded with the observation that, if confirmed, this activity has important implications for risk assessment. The group’s further analysis reported shortly thereafter indicated that benzene metabolism above 0.03-ppm is highly nonlinear, with a result that “health risks associated with low and very low benzene exposures can be considerably greater than those currently predicted from occupational studies.”

The researchers extended this argument in 2010 with an article stating the case for benzene metabolism through two pathways. The scientists emphasized that their research provided “strong statistical evidence” for an unknown high-affinity pathway for benzene metabolism at doses lower

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178 S. Kim, et al., “Using Urinary Biomarkers to Elucidate Dose-related Patterns of Human Benzene Metabolism,” Carcinogenesis 27, no. 4 (2006): 772-781; S. Kim, et al., “Modeling Human Metabolism of Benzene Following Occupational and Environmental Exposure,” Cancer Epidemiology, Biomarkers & Prevention 15 (2006): 2246-2252. 2252; S. Kim, et al., “Genetic Polymorphisms and Benzene Metabolism in Humans Exposed to a Wide Range of Air Concentrations,” Pharmacogenetic Genomics, 17 (2007): 789–801. NIEHS and NCI provided primary support for the papers. Three of the authors reported potential conflicts of interest. M. T. Smith wrote that he had received consulting and expert testimony fees from both plaintiff and defense counsel. The fact two of the authors could note they had received prior funding from the API was likely particularly galling to industry. G. Li disclosed that he had received funds from the American Petroleum Institute for consulting on benzene related health issues. N. Rothman reported he had received funding from the API and the American Chemical council as well as receiving consulting fees from a plaintiff counsel in one case. The studies and their analysis are succinctly discussed in Stephen M. Rappaport, et al., “Low-dose Metabolism of Benzene in Humans: Science and Obfuscation,” Carcinogenesis 34, no. 1 (2013): 2–9, 2-3.
than 1.0-ppm. This discovery led to their conclusion: “Because regulatory risk assessments have assumed nonsaturating metabolism of benzene in persons exposed to air concentrations well above 10-ppm, our findings suggest that the true leukemia risks could be substantially greater than currently thought at ambient levels of exposure—about 3-fold higher among nonsmoking females in the general population.”

Industry could not let this direct attack on their litigation defense posture stand. In an apparent effort to assist the other large corporations funding the Shanghai study, Dow Chemical employees, with API funding, wrote an article disputing Sungkyoon Kim’s findings. They did not conduct a new study. Instead, they obtained the data through a Freedom of Information Act request, and then focused almost exclusively on the modeling results in one article, rather than the similar empirical analyses in an earlier article. After reproducing the modeled results, they contended that the “findings of increased production are probably smaller” than found by Kim. The Dow authors then concluded that the original modeling analysis appeared “too uncertain to support any conclusions of a change in the efficiency of benzene metabolism in exposure.” In reaching this conclusion, they had to dispute four different and separate standard analysis techniques used by Kim and his coauthors, as well as suggest that Kim’s results were “inconsistent with knowledge about ‘lung clearance.’” Following the article’s conclusion, the authors provided conflict and financial information; stating that API had provided funding and Dow Chemical used benzene in some of its

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processes. However, they did not mention that Dow Chemical had been defending litigation for low dose exposures.  

Dr. Stephen M. Rappaport, Professor of Environmental Health in the School of Public Health, at the University of California, Berkeley, Kim, and other NCI research colleagues responded with an unusually caustic article dissecting the Price criticisms, arguing that “the methods and arguments presented by Price et al. are scientifically unsound and that their results are unreliable.” They concluded their critique by questioning, “whether Price’s reanalysis of Kim’s work was motivated by scientific skepticism or by an effort to obfuscate the low-dose metabolism of benzene. In either case, we regard the above shortcomings as sufficient to justify retraction of Price et al. from Carcinogenesis.” In the conflict of interest statement that accompanied the article, only Rappaport indicated a potential bias; he had worked for both plaintiffs and defendants, stating

“S.M.R. has received consulting and expert testimony fees from law firms representing plaintiffs in cases involving exposure to benzene and has also received research support from the American Petroleum Institute and the American Chemistry Council. Other authors declare no conflicts of interest.”

The Benzene War continues.

Conclusion

Some might call this chapter speculative history, since there is so little direct evidence demonstrating the role of attorneys in the medical science related to benzene. However, like many historical events, circumstantial evidence clearly points toward one overwhelming conclusion: as occurred with other toxic substances, attorneys have played a major role in the formulation of

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180 P. S. Price, T. D. Rey, D. D. Fontaine, and S. M. Arnold, “A Reanalysis of the Evidence for Increased Efficiency in Benzene Metabolism at Airborne Exposure Levels below 3 p.p.m.,” *Carcinogenesis* 33 (2012): 2094–2099. Like most articles written for defense use in litigation, the “reanalysis” is quite unequivocal. “Kim et al. (2006a,b) findings of increased metabolism do not provide a basis for questioning current estimates of the health risks from low level benzene exposures.” at 2099).


scientific data concerning benzene’s effects on the human body. Many of the same tactics used with tobacco can be observed in the actions taken by industry in defending benzene.

The major defense scientists—Paustenbach and ChemRisk, Wong, and Irons—have all been major defense expert witnesses for industry. The ChemRisk benzene studies display the same attention to litigatory detail as is found in their asbestos and chromium studies—almost all funded by attorneys. Although specific evidence of attorney involvement in their benzene investigations has not been made public, the circumstantial evidence of the litigation purpose for virtually all of their research is vast and—as with their other research—the results were exactly those desired by attorneys in litigation.

Similarly, the Shanghai study documents show a fastidiously controlled research program, with numerous safeguards to guarantee the results. The limited documents available disclose that one of the key factors in the study was to counter the growing number of lawsuits, with the study’s conclusion being designed to counter the lawsuits and growing regulatory pressure.

The net result of these activities has been confusion. By the second decade of the twenty-first century, benzene lawsuits have become very difficult to win. The defense has a cornucopia of defenses, from extremely difficult exposure proof requirements, to doubt remaining about causation for most malignant diseases, to the devolution of disease categories into subtypes for which epidemiological proof is virtually impossible. Although published government and independent articles based on these studies over the past fifteen years have increasingly suggested that diseases other than AML are caused by benzene, and low level exposure is proportionally more hazardous, industry responses continue to downplay the hazard, dismissing suggestions of other diseases, publishing select exposure data, and dividing AML into subcategories to limit the potential plaintiffs.

Similarly, regulatory action has been stalled, even though the China/NCI studies presented increasing evidence of the effects of low dose exposure. As a result of the industry studies,
Governmental agencies must also tread a careful line when providing information to workers and consumers about the dangers of benzene. Several federal agencies have published information about benzene and cancer. They all discuss the numerous non-malignant blood related and other disorders caused by benzene. They all agree that benzene causes AML. However, as discussed below, their online sites, in most instances, only generally stress the long-term effects, while cautioning that at high doses, benzene can be immediately lethal.

The Center for Disease control’s online “Facts about Benzene” is perhaps the least cautionary. It notes that the “major effect of benzene from long-term exposure is on the blood,” with harmful effects to the bone marrow. Furthermore, “long-term exposure to high levels of benzene in the air can cause leukemia, cancer of the blood-forming organs.” It notes that reproductive effects are also possible and some women can experience menstrual and ovary problems. However, it cautions that benzene’s effect on the developing fetus and fertility in men are unknown; even though animal studies suggest the possibility.  

OSHA’s online health discussion of benzene stresses both the potential of leukemia from longer-term exposures and the short term, high dose hazards associated with benzene. “With exposures from less than five years to more than 30 years, individuals have developed, and died from, leukemia. Long-term exposure may affect bone marrow and blood production. Short-term exposure to high levels of benzene can cause drowsiness, dizziness, unconsciousness, and death.”

The National Library of Medicine’s online Toxnet provides both a more detailed description of the potential diseases, as well as a more cautionary approach to dosage. It cautions that workers at

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locations where benzene is used, transported, stored or produced are all at risk from excess benzene exposure.

Chronic exposure to benzene results primarily in hematotoxicity, including aplastic anemia, pancytopenia, or any combination of anemia, leukopenia, and thrombocytopenia. Chronic benzene exposure is associated with an increased risk of leukemia. Summary of clinical effects: Acute neurological toxicity from benzene exposure may cause headache, dizziness, drowsiness, confusion, tremors, and loss of consciousness. Exposure to high concentrations may have effects on multiple organ systems. Sudden deaths occurring below anesthetic concentrations of benzene are apparently due to cardiac dysrhythmias. . . . Chronic hematological effects include anemia, thrombocytopenia, leukopenia, pancytopenia, chromosomal aberrations, [sic] and leukemia. . . . Occupational populations: Individuals working in industries involved with benzene production (petrochemical industry, coke manufacturing), rubber tire or cast rubber film manufacturing, transport or storage of benzene or benzene-containing products, and gas station employees all are at risk for excess benzene exposure.185

The EPA informs readers of its online benzene “Toxics Web Site” that benzene an increased incidence of leukemia has been observed in individuals occupationally exposed. It has been listed as a Group A (known human) carcinogen. Furthermore the agency estimates that lifetime exposures at even less than one tenth (4-14ppb) the current regulatory limit could increase chances of developing cancer by one-in-ten thousand. 186

The Agency for Toxic Substances & disease Registry declares that studies and case reports provide clear evidence of a causal relationship between benzene products and Acute non-lymphocytic leukemia (ANLL), particularly AML. It further notes that some studies suggest an association between benzene and other cancers, including non-Hodgkins lymphoma (NHL) and

multiple myeloma (MM). The agency found that the Pliofilm cohort has the best broadband exposure data set for evaluating cancer risks, while the new NCI Chinese study has enabled “detection of significantly elevated risks at unusually low levels of exposure.”

In short, the evidence produced by industry has assisted litigation and defeated certain regulatory efforts. It strains credulity to suggest that this occurred without major attorney input. To date, attorneys have been largely able to shelter behind the attorney work product privilege. Yet, undoubtedly, like tobacco, asbestos, and chromium, eventually the facts will appear and this issue can be fully explored.

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CHAPTER 9

TRADITIONAL HAZARDS MEET THE NEW MILLENIUM

The twenty-first century has already seen pneumoconiosis diseases leap into numerous headlines, as increased litigation in silica and formerly low-profile asbestos defendants, along with scandals among silica plaintiff attorneys and coal defense attorneys have come to public attention. As litigation in these formerly “quiet” areas increased, so did the greed of certain attorneys and their willingness to stretch the boundaries of ethics. As this chapter demonstrates, this elasticity occurred both in the courtroom and in funded medical research.

Silicosis (a form of pneumoconiosis) is in many respects, a newly rediscovered disease. It has received new attention on both the litigation and regulatory fronts, with the ubiquitous substance rejoining the toxic substance legal and regulatory fights.

This development should not have been unexpected. Although silicosis deaths have decreased, they remain high. In any case, statistics concerning the decrease in silicosis deaths can be misleading and should be viewed with caution. Although often disabling, historically silicosis by itself rarely caused death, even in the early twentieth century. Modern medicine may well account for the reduction in mortalities, with tuberculosis and other silicosis associated deadly secondary infections now often curable. The base silicosis rate may also be underestimated. With the reduction in the highest levels of exposure, modern silicosis cases are usually more moderate, with a concomitant extension of the latency period. Furthermore, individuals who do not routinely have x-rays taken may not even know they have a mild form of silicosis. CDC reports likely mostly account for symptomatic individuals, not everyone with early stage disease. In addition, if an individual has
smoked, it can produce many of the same symptoms as silicosis, such as COPD and severe emphysema, thus masking the disease.¹

Analysis of silicosis studies provides support for this hypothesis. A researcher in the McMaster University Occupational Health Program, Dr. Murray Finkelstein, has estimated that 25% of the individuals with an average 30-year work exposure of 0.1 mg/m³ will contract at least a mild form of silicosis. With 2,000,000 individuals potentially exposed to silica, and 30% of the silica samples exceeding 0.1 mg/m³, up to 150,000 individuals exposed today could contract silica, with more numbers accruing as additional individuals enter the workforce. Articles and reports, such as Finkelstein’s and NIOSH, have not gone unnoticed. Industry experts, including a well-known name, Dennis Paustenbach, have also been busy, writing articles suggesting the current regulatory limit is below the threshold level for silicosis, exposure levels were historically higher than previously thought, with silicosis cases come from exposures over the current limits, and the cristobalite form of silica is no more harmful then other types of quartz.²


Three narratives provide at-times shocking descriptions of Silica’s reemergence as an issue of interest to attorneys, and each are discussed in detail below. The first involves a scandal of misdiagnosed silicosis by plaintiff lawyers and their doctors—and a resulting flood of silicosis claims the Mississippi courts—in the first decade of the twenty-first century. The second examines corresponding defense attorney misdeeds in a related pneumoconiosis; coal workers pneumoconiosis. The third returns us to the hotly contested battles over silica regulation, with much of industry refusing to accept any additional limitations.

Misdiagnosed Silicosis: The Mississippi Deluge

In December 2002 the Mississippi law firm of O’Quinn, Laminack & Pirtle filed seventeen cases in Mississippi state court involving over 2000 plaintiffs with alleged silicosis. Each case had 73 defendants, with every plaintiff claiming exposure from each defendant. That year the number of Mississippi silicosis civil court claims exploded to 10,642—more claims per day than had been filed the entire year in 2000. Additional claims followed: 7228 claims in 2003 and 2609 claims in 2004. In 1998, one primary national defendant, US Silica, had only been named in 198 claims across the country. In contrast, at the height in 2003 plaintiff counsel across the country served them with 19,865 lawsuits.3

As allowed by the U.S. Code in certain circumstances, the defendants in O’Quinn’s and other Mississippi lawsuits removed (transferred) the cases to federal court. Following the cases’ removals, the federal Judicial Panel Multidistrict Litigation centralized the now 22 cases (over 10,000

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3 Mark A. Behrens, “Rand Institute for Civil Justice Report on the Abuse of Medical Diagnostic “Practices in Mass Tort Litigation: Lessons Learned from the “Phantom” Silica Epidemic that may Deter Litigation Screening Abuse,” *Albany Law Review* 73, no. 2 (2010): 521-539. Readers might be justified in wondering if Behrens appreciated the irony of a Shook, Hardy & Bacon attorney writing an article deploring the misuse of medical experts.) Likely part of the reason for this increase resulted from several courts in the twenty-first century actively discouraging asbestosis claims. During the later years of the twentieth century plaintiff counsel inserted at least some individuals suffering from silicosis onto the asbestos claims conveyor belt. As a defense counsel in the nineteen eighties and nineties, I viewed x-rays at more than one deposition with clear silicosis markings—rounded opacities rather than linear markings—that plaintiff experts testified were typical readings for asbestosis.
claims) into one Multi District Litigation (MDL). Even then defendants and plaintiffs were not finished, as numerous additional cases from several law firms became added to the MDL. The Panel assigned the cases to a U.S. District judge in the southern District of Texas, Judge Janis Graham Jacks. Unusual for a federal judge, Jacks had considerable experience with medical issues, having been a nurse and being married to a doctor.\(^4\)

Almost immediately, Plaintiff attorneys began seeking remand (transfer back) to the state courts. During the first in-person conference with the Judge Jacks, she noted this possibility, raising the issue of the Federal Court’s jurisdiction. She was correct in her concern. Normally there has to be complete diversity between the defendants and plaintiffs for a Federal court to accept removal. This was not the case here, with Mississippi defendants being included in each lawsuit. The judge did however agree to allow defendants’ discovery into fraudulent joinder of defendants in specific cases prior to issuing a ruling. In January, the parties submitted briefs on the jurisdiction issue.

Later that month the judge, concerned about the massive nature of the claims, denied all motions to remand without prejudice (meaning they could be re-urged at a later date) and issued an order designed to assist the court in gathering facts. In compliance with the order, the parties agreed to submit sworn “Fact Sheets” to the court and opposing counsel. As each side began preparing the “Sheets,” the Court also permitted other discovery to continue. Over the course of the next year, the Judge Jacks held conference hearings every five to eight weeks on problem issues as they arose. As is usual in large cases, defendants and plaintiffs fought over virtually every issue. On October 14, 2004, prior to deciding on the jurisdiction issue, the Court ordered Daubert hearings for the plaintiff

experts, because they might be relevant for jurisdiction and were necessary to rule on defendants’ motions for sanctions against plaintiff counsel.5

Defendant counsel quickly began deposing experts, starting with a newcomer to litigation, Dr. George H. Martindale, a radiologist in private practice in Mobile, Alabama. Martindale had his certification to read x-rays as a means to supplement his income. At his deposition he testified that he thought he was only conducting a second reading to verify prior positive results. In fact, he was misinformed about the prior reading. Of 4,000 x-rays readings he conducted over 48 days, he found 3,617 had silicosis. He further testified that a notation written on each jacket (which he took to be a prior reading) influenced his results. Dr. Martindale correctly read the notation. Dr. Ray Harron, the primary radiologist used by the screening company, had previously read the x-rays as positive for silicosis. The screening company, perhaps at the instigation of the plaintiff attorneys, decided not to use many of Harron’s readings, likely due to his prior use in asbestos litigation for a large number of the same clients.

Although Martindale did not believe he was actually diagnosing silicosis with his x-ray reading—he had not seen any work histories or conducted a physical—he inexplicably allowed the screening company to add diagnosis language to his reports, which he then signed. This was damning testimony of fraudulent diagnosis, and it apparently took the plaintiff counsels by surprise. At the next status conference, Judge Jacks expressed surprise at the doctor’s withdrawal of his diagnoses, but was assured by the plaintiff liaison (coordinating) counsel that the other doctors would be different. They were not.

During the remaining depositions, most of the doctors admitted that their “legal” diagnosis did not follow the same standards as they used in their practice. Yet, in many respects, this was the least of their failures. For example, the next two doctors, Dr. Hilburn and Dr. Cooper, testified that

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they had conducted very abbreviated physical exams and had not diagnosed anyone, simply noting their findings. In fact, neither had ever diagnosed anyone with silicosis. Incredibly, as occurred with Martindale, they unaccountably allowed N&M, Inc. (the screening company hired by some of the plaintiff attorneys) to supply them with forms that contained diagnosis language. Both doctors signed the forms: one stated that he had not read them due to his busy schedule, while the other, without review, simply turned them over to his assistant for a stamped signature. Greed seemed at the bottom of their actions. N&M paid Hilburn $5,000.00 a day for his work, while Cooper admitted he took the job because it seemed like easy money.⁶

Along with N&M, one doctor stood at the epicenter of the controversy, Dr. Ray Harron. He had a nationwide reputation for extremely liberal readings of x-rays for asbestosis. So liberal, in fact, that many plaintiff counsels refused to use him. But even he, the most culpable of the doctors, blamed N&M, testifying that he “capitulated” to signing their reports. During his deposition, defense counsel showed Harron numerous examples of his previously reading an x-ray as asbestosis for a prior claim and now maintaining that a new x-ray for the same individual indicated silicosis and not asbestosis.

This was not an easy feat. Unless massively severe, silicosis’ features on x-rays are characterized by rounded opacities, whereas asbestos is characterized by linear opacities. Even severe silicosis does not resemble asbestosis on an x-ray, but rather displays massive holes in the lungs, to a degree similar to long-term heavy cigarette smoking. Toward the end of the day’s line of questioning Harron requested time to seek counsel. Judge Jacks quickly allowed him to stop answering questions, telling him she did not know whether his actions were criminal or only quasi criminal. She later wrote that Harron’s “sheer volume” of x-ray reading reversals cannot be explained as reader variability: “it can only be explained as a product of bias--that is, of Dr. Harron

⁶ 398 F. Supp. 2d 563, 587-9; and Eddie Curran, “Doctor’s Testimony.”
finding evidence of the disease he was currently being paid to find.” Although defendants expected to question him the following day, Harron, now represented by counsel, did not take the stand.

The remaining doctors were little better. Dr. Ballard had problems with x-ray read reversals that were similar, if on a lesser scale, to Harron’s. Another expert, Dr. Levy, was not certified to read silicosis x-rays, yet diagnosed approximately 1400 plaintiffs in the MDL. Levy testified that in making his diagnosis he was given a work history and an x-ray report from another doctor. However, he not only failed to conduct a physical examination, he did not even speak to the potential clients, their primary doctors, or the doctor who read the x-ray.

Nor did any of the remaining screening companies come out of the depositions unscathed. One company had the same x-ray read twice in one day, one time generating a report for asbestosis, the next for silicosis. Still, like Harron for the doctors, N&M, the primary screening company used to identify clients, appeared to be the most culpable. It generated approximately 6,750 claims in ninety-nine days of screening. At least 4,300 of the newly diagnosed silicosis claimants had prior asbestosis claims filed with bankrupt Johns Manville. During discovery, N&M turned over its massive collection of records, more than 1.5 million for tens of thousands of individuals. In what was at times a tempestuous deposition, Mr. Mason, owner of the N&M, admitted that each report signed by the above doctors contained the same diagnosis language. In his defense, he stridently emphasized that each doctor had agreed to the language. Mr. Mason further candidly admitted he had no medical training, having started the company after working for another asbestos screening company for a fairly short period.  

In the end, Judge Jacks wrote a scathing opinion and then sent most of the cases back to Mississippi. She described the claims as what one would expect from an epidemic, but they did not

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7 398 F. Supp. 2d 563, 602-611 ("sheer volume" and "it can only" at 638); author’s professional experience; Eddie Curran, “Doctor’s Testimony”; and Mark A. Behrens, “Rand Institute,” 527.
display epidemic characteristics, noting that almost all of the claims involved a mild form of chronic or simple silicosis.

In her opinion imposing sanctions upon the certain of the plaintiff attorneys, she stressed that simply taking a case through Daubert hearings and then not being allowed to use any of the experts does not necessarily warrant sanctions. What warranted the sanctions in this case was the plaintiff counsel required the defendants and the court to go through the Daubert hearing “when the plaintiff has no reasonable basis to believe that the expert can pass muster under Daubert. Thus, she found that this set of facts warranted assessing both court costs and defendant’s hearing fees and expenses. Moreover, the judge held that when plaintiff attorneys “recklessly disregarded” the lack of disease evidence and attempted to overwhelm the court system, their behavior became “vexatious as well.” Judge Jacks further deplored the doctors’ use of “shockingly relaxed standards of diagnosing.” As she viewed it, “truth and justice had very little to do with these diagnoses.”

In September 2003, Judge Jacks formally transferred the cases back to the Mississippi courts. By January 2006, more than half of the returned cases had been dismissed and new silicosis filings plummeted. In 2007 only 15 claims were filed against US Silica. On March 15, 2006 defendants requested sanctions in state court against the O’Quinn law firm. They lost. On March 27, 2008, the Mississippi Supreme Court affirmed the lower court’s refusal to sanction plaintiff attorneys. Since the court did not issue an opinion, we shall never know if they agreed that the plaintiff attorneys did not commit fraud, or they affirmed on other grounds.

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8 398 F. Supp. 2d 563, 571-2, 625 (“shockingly”), and 675-6; and Mark A. Behrens, “Rand Institute,” 528. The b-readers before Judge Jacks had previously generated 140,911 asbestosis diagnoses, with Harron supplying 51,048 of those. Mark A. Behrens, “Rand Institute,” 531.
Doctor Harron did not fare as well. He voluntarily surrendered his license in Florida and California and agreed not to practice in three other states. Two other states revoked his license permanently.\footnote{Mark A. Behrens, “Rand Institute,” 532.}


The timely efforts of industry defense counsel in this case brought to light a scandal of truly epic proportions. Yet, true silicosis victims may have suffered even more from the scandal. As we continue to see significant numbers of silicosis cases diagnosed in numerous states, the 1934 advice of a doctor and attorney still rings true today: “the problem [of silicosis] cannot be conveniently ignored or brushed aside as merely another form of “racketeering” by unscrupulous lawyers and conspiring doctors.” Nor, as we shall see next, are pneumoconiosis plaintiff counsel and expert witnesses alone in their misdeeds.\footnote{M. Kummel, “Medicolegal Aspects,” 1.}

**Disappearing West Virginia Coal Miner’s Pneumoconiosis**

Coal workers’ pneumoconiosis, often called Black Lung, is a closely related disease to silicosis. It is caused by coal, which, like silica, instigates a disabling and, at times, fatal lung fibrosis. Often the silica content of coal seams results in a combined silicosis, black lung pneumoconiosis. Black lung comes in two forms: simple, which can range from no clinical symptoms to disablement,
and complicated, which is characterized by progressive, massive fibrosis of the lungs. As with silicosis and asbestosis, smoking vastly aggravates the disease.

Uniquely, among pneumoconiosis diseases, in the late 1960s Congress had adopted a specific national program of compensation for coal workers pneumoconiosis. The legislation, called the Black Lung Benefits Act (BLBA), requires coal companies to compensate fully disabled miners and their families through a federal claims system. The District Director of the U.S. Department of Labor’s Office of Workers’ Compensation Program (Director) awards benefits for total disablement in cases where the coal worker’s pneumoconiosis contributed to the disability. After the initial finding, either party can request an evidentiary hearing before an Administrative Law Judge (ALJ). Reasonable attorneys fees are paid for workers who are successful at the hearing.  

Black lung’s importance for this dissertation lies in its similarity to the recent silicosis scandal, this time from the opposite side of the courtroom. As described in a Pulitzer Prize-winning series of articles, for the past forty years at least one prominent black lung defense firm has zealously and successfully defended its clients in a manner very similar to tobacco attorneys. Its actions have run the gamut from utilizing never fail experts, to hiding and manufacturing evidence. The tribulations of one complicated black lung victim provides an illustrative example of these tactics.  

George Fox spent thirty years working in an underground coal mine. Having grown up poor, he wanted to provide a comfortable life for his family. Fox typically worked at the highest paying, riskiest jobs, with the highest concentrations of dust. In the 1990s he began noticing a shortness of breath. In late 1997 an x-ray revealed a mass in his lung. Early the next year, West Virginia pathologist Dr. Gerald Koh examined surgical samples of the mass, concluding that Fox had an

“inflammatory pseudotumor,” but did not mention pneumoconiosis. However, at the time Koh’s focus was on ruling out cancer, not on whether Fox had black lung.\(^{15}\)

Soon after, his cough resulted in black mucus. After applying for coal worker’s benefits in 1999, a government-certified doctor diagnosed Fox with the most severe form black lung, complicated coal workers’ pneumoconiosis. Following the Director’s approval of monthly benefits in 2000, Fox’s employer, Elk Run Coal Company—a subsidiary of massive Massey Energy Co.—appealed. At trial Fox represented himself, having tried but failed to obtain a lawyer. Under the language of the BLBA, Elk Run was only required to pay Fox’s attorney an hourly rate, and even that was only paid if Fox eventually won all of the appeals.\(^{16}\)

On the other side of the courtroom stood very experienced black lung defense attorneys. Prior to the hearing they went through a considerable amount of Elk Run’s money to ensure victory. Initially the attorneys obtained Koh’s pathology slides, providing them to two pathologists in their stable of experts: Dr. Richard Naeye and Dr. P. Raphael Caffrey. Unfortunately, both diagnosed complicated black lung disease. In the absence of the gold standard for diagnosis—a pathology report—the lawyers obtained opinions from four radiologists, three of them from doctors associated with Johns Hopkins University. They then presented the radiologist and Dr. Koh’s reports—but not the two more recent pathology reports—to four pulmonary specialists, who universally concluded Fox likely did not have coal worker’s pneumoconiosis. At a later date, when asked, one of the pulmonologists, Dr. James Castle, acknowledged that he had originally diagnosed black lung, but changed his mind because of the Koh’s pathologist’s report.\(^{17}\)

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\(^{16}\) Chris Hamby, “Coal Industry’s.”

\(^{17}\) 739 F. 3d 131, 134; and Chris Hamby, “Coal Industry’s.”
As the hearing proceeded, Elk Run’s attorney did not simply rely upon his own experts, but also sought to discredit the doctor who had formed the basis for the Director’s approval. He did this by misleading, if not outright falsified, questioning of Castle, the pulmonologist who had been influenced by Koh’s report. During deposition, he ignored the defense pathology reports, instead inquiring: “Do you think that [the Labor Department-certified doctor] would have been aided by having all of the biopsy medical evidence [Koh’s] at his hand when he reviewed this case?” In an answer that demonstrated the power of pathology reports, Castle replied, “I think that he would have, and I would certainly hope so, because all of the evidence, as I’ve outlined, clearly indicates that this is not complicated disease.”

Against this well-structured and massively financed defense, Fox had only his own testimony to rely upon. Unsurprisingly, he lost. On January 5, 2001 the Administrative Law Judge (ALJ) denied Fox’s claim. Following the ruling, Fox, with no money for further appeals and needing to support his family, returned to work. Besides the income, he required health insurance to pay for his wife’s chronic illness.

Five years later, after finally finding an attorney willing to take his case, Fox filed a new claim, once again approved by the Director and appealed by Elk Run’s attorneys. This time, the lawyer conducted vigorous discovery, seeking the 1998 pathology slides and all additional documents and reports about Fox’s condition. After what an appellate court called some “foot dragging,” the

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19 739 F. 3d 134; I Chris Hamby, “Coal Industry’s”; Chris Hamby, et al., Johns Hopkins Medical” (quotes). An Administrative Law Judge has even deplored this situation: “Employers’ financial capability far exceeds Claimant’s resulting in a lack of qualified attorneys who are willing to aggressively pursue meritorious claims. Unequal results exist between those cases defended by an Employer and those by the Trust Fund. A way should be found to reward Claimant’s attorneys who pursue meritorious claims.” Quoted in William S. Mattingly, “If Due Process is a Big Tent, Why do Some Feel Excluded From the Big Top?” West Virginia Law Review 105 (2002): 791-826, 824. The individual quoting the judge, Bill Mattingly was not a plaintiff oriented attorney or even a neutral observer: he worked for Jackson Kelly.
company’s attorneys admitted liability for the claim and disclosed the reports of Naeye and Caffrey. Since the BLBA prohibits benefits prior to a ruling against the worker, back payments could only be paid from 2001. In an attempt to obtain further compensation, Fox’s attorney moved to set aside the prior judgment on the basis of “fraud upon the court.”

Fox died on April 14, 2009, well before conclusion of this latest episode of his ordeals. At a July 20, 2011 hearing obtained by the attorney for Fox’s widow, the ALJ determined that Naeye and Caffrey had both diagnosed Fox with “complicated pneumoconiosis,” evidence that the company’s attorneys failed to disclose to either Fox or their other experts. To the judge’s mind, the attorney actions tainted the expert conclusions such that, while “perhaps initially not concocted as such . . . [Elk Run’s] actions, taken as a whole, constitute a scheme to defraud.” After listening to the defendant attorneys’ defense, the judge set aside the prior judgment, finding they had not been merely zealously defending their client during Fox’s prior hearing, but rather committing fraud on the court.

Upon appeal, Fox’s widow lost. Initially, the Benefits Review Board (BRB) accepted the factual findings, but held the “conduct did not rise to the level of fraud on the court” because there was no “deliberate scheme to directly subvert the judicial process.” On further appeal, the federal Appeals court agreed in a split decision. They held that ordinary fraud such as this was not sufficient to set aside a judgment more than one year old. Rather, the fraud must strike at the heart of the judicial system itself. As the Supreme Court had written, fraud on the court is “the product of one party’s ‘deliberately planned and carefully executed scheme’ that severely undermined the ‘integrity of the judicial process.’” Since Fox’s case only entailed fraud against one poor and dead miner, it did not qualify.\textsuperscript{21}

\textsuperscript{20} 739 F. 3d 131, 134.  
\textsuperscript{21} 739 F. 3d 131, 134-7.
In its ruling, the court revealed much about the attorney system of ethics and secrecy. While strongly implying that the Elk Run’s attorneys’ conduct may not have been not “exemplary,” the court nevertheless held that they were under “no obligation to advance those reports [Naeye’s and Caffrey’s] as evidence because someone else may believe them superior,” arguing that “it falls to each party to shape and refine its case.” The court continued, “[a]ny duty imposed on a party to furnish its expert witnesses certain documents would improperly impinge on that party’s right to develop its own evidence, handle its experts, and present its own case.” In the end, the court saw no need to address the issues of the attorneys’ behavior, since their conduct did not affect the integrity of the court, but only a solitary litigant who should have found an attorney for the first hearing. They did not disclose how Fox could obtain a lawyer when no lawyer was willing to take his case for the meager sum established under the black lung law.22

Fox’s claim difficulty was not an isolated case. Both lawyers and judges have admitted that the behavior revealed in his (and other known cases) is likely typical. Nor does his example apply to only a few litigants. The current and historic victims of complicated black lung disease potentially total considerably more than the numbers involved in the silica scandal. Approximately 93,000 individuals mine coal. Government surveillance has established that more than 6% (over 5500) of miners in central Appalachia currently have black lung disease, which is increasingly affecting...

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22 739 F. 3d 131, 137-9. In ruling thus, the court wore blinders to the disparity in resources between the litigants and its effect upon the judicial system. Even Jackson Kelly attorneys have acknowledged—and seemingly agreed with—the government’s belief of the defense’s overwhelming power in black lung cases. For example, an article written by a colleague of Smoot’s noted that the Department of Labor hoped new regulations proposed in 2002 would help level the playing field; “Currently, in establishing their eligibility to benefits, claimants must confront the vastly superior economic resources of their adversaries: coal mine operators and their insurance carriers. Often, these parties generate medical evidence in such volume that it overwhelms the evidence supporting entitlement that claimants can procure.” In the article, Mattingly described the main criticisms of the federal black lung program as it taking too long and helping too few, then appeared to once again agree by noting that in 1997 less than 6% (419 of 6791) claims were granted. Nevertheless, the defense attorney author wrote in opposition to the new regulations, incongruously arguing that they would not help, but rather accentuate the feelings of exclusion. William S. Mattingly, “If Due Process,” 791, 794.
younger workers. A new NIOSH study has determined that surface miners are also increasingly suffering from black lung, including the complicated form.

Yet, only 14% of claims lead to an award at the initial level. Even then, coal companies appeal almost every award. The first level of appeal the BRB has a record of vacating awards, often for very technical reasons—possibly resulting from four of its five members in 2013 having been appointed by Republican administrations. In further evidence of the power of money, less than a third of the miners have an attorney at the initial stage.23

Thus, Fox’s case is but one example of why black lung disease is increasing. Yet payments to miners remain sparse. The company lawyer in the Fox case, Douglas A. Smoot, was a senior partner at Jackson, Kelly, PLLC, a firm specializing in service to the mining industry for almost two hundred years. They are considered one of the top mining law firms due to their success in defeating black lung claims. When pushed for discovery, Jackson Kelly attorneys have always fought back aggressively, arguing attorney secrecy privileges prevent them from providing information. When thwarted at the hearing level, they frequently ask higher courts to intervene, at times accusing administrative judges of bias. When necessary they defy court orders, knowing administrative judges have no contempt powers. Apparently, they are especially adept at battling widows, having fought their survivor benefits and even attempted to reopen deceased miners’ claims. Although Jackson Kelly attorneys have been conducting these activities for more than three decades, in each instance of malfeasance, judges have only looked at the facts of the specific case. It was only in 2013, with the investigative reporting of the Center for Public Integrity, that the historic nature of the actions

became evident. In at least 11 cases reviewed by the public service organization, Jackson Kelly lawyers withheld potentially relevant evidence. In 6 cases the firm offered to pay claimant rather than turn over documents as ordered by judge.\(^{24}\)

The Center’s articles abound with examples of Jackson Kelly’s “zealous” defense of their client. In a pair of 2004 cases before Administrative Law Judge Fletcher Campbell, they refused to answer questions about the financial relationships with their experts, informing the judge that he did not have the power to issue sanctions. In addition, Jackson Kelly routinely refused to turn over unfavorable medical reports, yet in their interrogatories asked whether plaintiffs had disclosed all medical evidence and expert reports. Fox was far from alone in losing because of this refusal. In another case, radiologist Dr. Jerome Francis Wiot, a respected but very conservative x-ray reader, provided them with a several reports interpreting x-rays and CT scans as consistent with complicated black lung, yet they did not disclose them to plaintiff counsel. In another case, Jackson Kelly provided plaintiff with Wiot’s negative x-ray reading, but withheld his positive CT scan reading—a more accurate measurement for black lung disease. In yet an additional case, the firm withheld a positive report of a series of x-rays for two years, then showed a doctor only the final film, which, since he had not seen the earlier clearer x-rays, he read as negative. In large measure, these activities are not excessively abnormal. If a client believes a settlement is inappropriate, just as plaintiff counsel will sometimes send an x-ray to more than one doctor, toxic tort defense counsel will send a case file to four, five, or more expert doctors, until one finally provides the desired report. Under the attorney work product rule, only the desired report is normally released.\(^{25}\)

\(^{24}\) Chris Hamby, “Coal Industry’s”; and Chris Hamby, et al., Johns Hopkins Medical.”

\(^{25}\) Chris Hamby, “Coal Industry’s”; Chris Hamby, et al., Johns Hopkins Medical”; and author’s experience. Asbestos defense firms, such as my own, used Wiot extensively in the 1980s and 90s. In his writings he consistently maintained that readers of x-rays for pneumoconiosis should be conservative.
Yet Jackson Kelly attorneys, such as Smoot, did not stop there. In a new millennium case, Dr. George Zaldívar conducted a physical examination of a plaintiff at Jackson Kelly’s request. As usual, the report he provided to the firm included a narrative, an x-ray reading, and the results of lung-function tests. The narrative posed problems to the defense because it diagnosed the plaintiff with complicated black lung, an automatic indicator of complete disablement. The firms’ lawyer in this case was Smoot—the same lawyer as in the Fox case—who recognized he could not hide the report, since the plaintiff’s attorney knew Zaldívar had examined his client. Instead, Smoot removed the narrative and forwarded the remaining materials: complicated forms and graphs.

In 2004, the plaintiff’s attorney uncovered the subterfuge. At the hearing ordered by the judge to discuss the events, another Jackson Kelly lawyer attempted to defend the actions before being silenced by the angry judge. Over the next few months, additional attorneys attempted to defend the Jackson Kelly attorney’s actions, arguing that since the narrative and forms were not attached, he had not taken the report apart. When the firm still refused to reveal the narrative, the judge referred the case to federal district court for sanctions. Although the court determined it had no jurisdiction, it advised all parties that it “in no way approves the conduct of Jackson Kelly lawyers,” and referred the case to the West Virginia office of Disciplinary Counsel. Ultimately, Smoot’s license was revoked for one year and three other attorneys were investigated, but not sanctioned. Although refusing to sanction the attorneys, in its findings the Lawyer Disciplinary Board wrote that it was “deeply troubled by the act of disassembling Dr. Zaldívar’s report,” warning the attorneys that “violations of discovery orders in the future will evidence a pattern of behavior” inconsistent with good faith. Unknown to the Board, another Jackson Kelly attorney had acted in
essentially the same manner almost a decade earlier, removing a narrative of the same doctor before providing the remaining documents to the plaintiff.\textsuperscript{26}

The circumstances surrounding the cases involving Dr. Zaldivar were, to say the least, unusual. Most of the time, Jackson Kelly attorneys did not have to go to such lengths, having a stable of experts who almost invariably found no or minimal disease. One of the most prominent of those doctors, from Johns Hopkins University, became a leading subject of one article in the Center’s Pulitzer Prize-winning series. This piece covered an expose of the Johns Hopkins black lung radiologist unit that for forty years read x-rays for companies—companies paying up to ten times what miners normally pay their physicians. The investigators found that the thousands of reports were “seemingly ubiquitous and unwaveringly negative for black lung.”

The company attorneys particularly relied upon the head of the unit, Dr. Paul Wheeler. As an expert, he provided coal companies with an impressive array of credentials, having undergraduate and medical degrees from Harvard, as well as a leadership position at Johns Hopkins University as the head of the Black Lung unit. In more than 1500 cases since 2000, involving over 3400 x-rays, Wheeler never found the severe form of black lung. Wheeler diagnosed even the simplest form of black lung—usually insufficient to obtain disability payments—in only 2% of the cases he reviewed. In contrast, other doctors reviewing the same x-rays found complicated black lung in 390 of the cases. Since the year 2000, miners have lost over 800 cases after Wheeler reviewed the x-ray as negative, including 160 cases in which other doctors found the disease. In one case, the plaintiff, who had worked 27 years underground, could not walk more than 100 feet. Three different doctors saw complicated black lung, while Johns Hopkins doctors, including Wheeler, saw something else. Because of their credentials, the coal company won. In another case, the client won in spite of

Wheeler’s negative reports, but only because Dr. David Weissman, head of the federal agency that certifies readers, informed the judge in a letter that Wheeler’s views were “not consistent with a considerable body of published literature by NIOSH.”

One of the major reasons for this discrepancy has to do with the criteria Wheeler applied to his readings, criteria which were at odds with the positions taken by government agencies, textbooks, scientific literature, and the opinions of most doctors specializing in diagnosing the disease, including the chair of the American College of Radiology’s task force on black lung. Due to his seemingly excellent qualifications, Wheeler’s testimony was normally accepted as controlling, although on occasion judges questioned his unfailing inability to detect disease. One judge has remarked that due to Wheeler’s credentials, he often felt compelled to deny claims, even though he had serious doubts about the opinion.

This discrepancy became readily apparent when an investigative reporter gave Wheeler a series of x-rays and asked for readings. Wheeler claimed one was more consistent with histoplasmosis, another had masses too high in the lung, while a fungal infection seemed to him likely in the third. Upon being told all of the x-rays were standard black lung x-rays used for the reader certification exam, Wheeler responded the classification systems “has some quality issues,” adding “the readings of the subject x-rays “are not proven.” Wheeler’s response seems exceptionally odd, given that he has consistently passed the certification test where he has to read the x-rays in conformance with their stated diagnosis.

Following Wheeler’s inability to correctly read standard x-rays, the Center for Public Integrity contacted Dr. John E. Parker, former director of NIOSH’s x-ray surveillance certification programs and now Chief of Pulmonary and Critical Care Medicine at the West Virginia School of

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27 Chris Hamby, “Johns Hopkins Medical.”
28 Chris Hamby, “Johns Hopkins Medical.”
Medicine. The Center provided Parker with a series of x-rays Wheeler had read as negative, informing him of the individual’s name (Steven Day), as well as his age, number of years mining, and the disputed nature of the x-rays. They did not inform him that the Day had lost his disability claim based upon Wheeler’s reading. Parker stated there could be no doubt that the individual suffered from black lung; “[I]f other physicians are reaching different conclusions about this case,” Parker had “serious pause and concern about bias and the lack of scientific independence or credibility of those observers.” This critique echoed that of Dr. Weissman, who had previously described the failure of a certified reader to recognize such a severe case as “very concerning.” A pathologist who viewed Day’s recent autopsy slides further reinforced the solitude of Wheeler’s reading, informing the Center that a majority of the lung tissue had been replaced by scar tissue and coal dust.

Shortly following publication of the articles concerning Dr. Wheeler, Johns Hopkins suspended its black lung x-ray reading program and started an internal investigation, informing the Center’s reporter, “Johns Hopkins takes it very seriously.” Yet, since the review was “internal,” its conclusions have never been revealed. In April 2015, U.S. Senator Robert Casey, Jr. requested the results, but was turned down. In the late summer of 2015, Wheeler continued to have a faculty profile page on Johns Hopkins Medicine’s website. In the early fall, with little in the way of apology, Johns Hopkins finally closed down the black lung unit and announced Doctor Wheeler’s retirement.29

The Department of Labor acted more decisively. On June 2, 2014, BLBA Bulletin No. 14-09 instructed directors to take notice of the Center and ABC’s reports and “not credit Dr. Wheeler’s negative readings for pneumoconiosis in the absence of persuasive evidence either challenging the CPI and ABC conclusions or otherwise rehabilitating Dr. Wheeler’s readings.” The Bulletin remains in effect today. On July 22, 2014, Deputy Secretary of Labor Christopher P. Lu, declaring the investigative reports “sufficiently trustworthy,” announced new regulatory initiatives during his testimony before a U.S. Senate subcommittee. These proposals included a medical evidence disclosure rule, designed to insure miners receive all medical evidence generated in their case. He also revealed that miners with Dr. Wheeler x-ray readings could request their case be reopened or re-file as circumstances allow.30

Sandblasting: Silica’s Regulatory Bottleneck

In the 1970s, OSHA had attempted to prohibit sandblasting with silica. The National Industrial Sand Association (NISA) and a new trade organization, the Silica Safety Committee, led the successful fight against the ban. As usual, the effort had prominently featured attorneys. Attorney Mark Ellis, President of the NISA, illustrated their omnipresent nature. He had previously served in the Federal Mine Safety and Health Review Committee’s Office of General Counsel before leaving government service to become Senior Counsel to the American Mining Congress.31

In the twenty-first century, the United States remains one of the very few industrialized countries without a comprehensive rule to protect the nation’s workers from silica dust. Under its new and proactive leader, David Michaels, OSHA sought to rectify this deficient in the Obama administration.

administration. In a response similar to that displayed for tobacco, chromium and benzene, industry mounted a full frontal assault on the agency’s proposals. Following a suggestion that information provided to OSHA should disclose its ultimate funding agency, industry representatives sought assistance from Republican Senators. At industry’s urging, Republican Senator Lamar Alexander, a senior member of the Committee on Health, Education, Labor, and Pensions, led a group of Senators in expressing deep concern “about OSHA’s attempt to have commenters disclose their financial backers.” He suggested that the request fostered questions about OSHA’s prejudgment of submissions. An Alexander spokeswoman further deplored “[t]he chilling effect the financial disclosure could have [which] seems counter to the idea of robust inclusion of a diverse set of ideas and views to inform the rule-making.” The prestigious journal *Nature* reported on the Senate’s activities, noting Michael had simply requested, not demanded, the financial information. The journal further editorialized its support for Michael’s position, noting it had required similar information for several years.\(^\text{32}\)

Industry did not stop there. During spring and summer of 2011, the Administration’s Office of Information and Regulatory Affairs hosted a series of requested meetings with “stake holders” in the regulation. Of the nine meetings, industry representatives requested seven, with American Thoracic Society and Labor representatives each also requesting a meeting. Industry attorneys (such as Mark Ellis) as well as outside counsel (including Henry Chajet) attended some of the meetings. Knowing that Administrator Michaels had a strong public health inclination, these meetings may have been designed to further delay the proposed rule. Following these meetings, the proposal

remained moribund as it remained immobile in OMB for several months subject to further industry lobbying.\(^{33}\)

Finally, in September 2013, OSHA issued a proposed rule aimed at rectifying the regulatory failure. Hearings on the rule occupied two weeks in March 2014, with over 200 individuals requesting time. Numerous industry trade groups requested up to ten hours each to testify. The U.S. Chamber of Commerce, the American Chemical Council, the Brick Industry Association, the National Association of Manufacturers, National Stone, Sand and Gravel Association, American Foundry Society, and the American Petroleum Institute all presented evidence, most leveling shotgun criticisms at the rule. Several unions, as well as members of the public health community, also presented evidence, many calling the rule the minimum necessary.\(^{34}\)

As has occurred with virtually every other OSHA and EPA proposed public health rule during the last quarter century, industry expert comments attempt to pick apart every basis of the OSHA proposed regulation. Attorneys played significant, if not all together clear, roles in these efforts. A senior in-house attorney at the American Chemistry Council prepared the Crystalline Silica Panel’s comments to the proposed regulations, with assistance from leading outside law firm attorneys. The 227 page comments spent approximately 140 pages lambasting the agency for the “fundamental shortcomings and limitations” in its risk assessments, which caused it to be unsupportable. They declared that there is no evidence of a significant risk below the current level, which is likely the threshold for disease. They further argued that “the evidence does not support the hypothesis that silica exposure *per se* increases the risk of lung cancer in the absence of silicosis,”

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citing an American Chemical Council-funded study. Their post-hearing brief covered much of the same ground, declaring OSHA’s silicosis morbidity projections to be “not credible or reliable.”

At the hearing, the U. S. Chamber of Commerce presented a panel of their selected peer reviewers to discuss the proposed rule. During their presentation they pointed to the mass silica litigation in Mississippi, inflating the number of cases before Judge Jacks to 30,000, and suggested that Michigan’s rate of reported silicosis—a key state in the CDC’s estimation of silicosis—might be due to similar activities there. Upon questioning, they admitted their statements were mere speculation.

The ACC’s Crystalline Chemistry Council also sponsored testimony from several experienced industry experts and attorneys. One, Paul Scott, worked for ChemRisk. He suggested the proposed rule would be impossible to enforce since analytical methods are inadequate for testing respirable crystalline silica at the proposed action level. Following Scott, two attorneys provided the industry’s overview of the major issues, substantially the same as those included in the Council’s attorney written comments. They posited that all current silicosis cases resulted from either very historic exposures, or exposures in excess of the current limit.

The attack did not stop with the hearings. For instance, the Chamber of Commerce’s ten page Post-Hearing submission selectively approved of OSHA’s peer reviewer comments when they helped its position, yet largely ignored other, unhelpful comments. In the document, one of the


Chamber’s experts both faulted OSHA for not following peer reviewer Kenneth Crump’s recommendations and then pointed to what he called “inconsistencies” in Crump’s unhelpful opinions. As occurs in virtually all proposed health-related regulations, the industry expert comments almost universally stressed that the exposure limit was already above the threshold level and castigated OSHA for not giving sufficient consideration to evidence of a threshold. They also repeatedly cited to exposure measurement errors in the data, potentially dramatically affecting the assessment of risk.  

The fight over silica regulation continues today. As of September 2015, OSHA had still not issued a regulation, which might be the result of continued intense industry lobbying. Industry representatives now suggest that OSHA needs to re-conduct its small-business review—which took place in 2003—prior to promulgating the rule.  

* * *  

Today many of the actual contacts and agreements between toxic substance defense attorneys and medical researchers and practitioners remain mantled in the attorney work product privilege. Still, it is clear that the inadequate response to the initial silicosis crisis involved one of the first and wholesale attempts by industry counsel to control public and occupational health medical science. Notwithstanding, and perhaps partly because of, the Mississippi scandal, disparagement of the continuing silica and coal workers’ epidemic continues today. Recent medical literature and regulatory events do not suggest any modification to these practices. Of course, as seen in Mississippi, plaintiff counsels have also subverted science to obtain evidence for their lawsuits. Yet,  

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does this rise to the same level of distortion of science? It certainly distorts science within the specific lawsuit. Nevertheless, as Philip Enterline, noted professor of biostatistics and occasional asbestos defense witness, asserted:

the large sums of money involved in toxic tort litigation have undoubtedly caused some scientists to give misleading testimony and assume positions consistent with one side or the other. As far as the scientific community is concerned, this seems harmless enough as long as the scientists are not in a position to influence scientific opinion . . . Certainly the scientific community pays little attention to court decisions as to what causes what.40

In the end, the actions of the silica plaintiff attorneys in Mississippi, although certainly expensive for silica companies and harmful to the legal system, did not affect the scientific literature and may have caused significant harm to true silicosis victims, as the public now discounts any claims of silicosis outbreaks. At present, toxic tort plaintiff lawyers occasionally engage experts such as the historian of science Gerald Markowitz or medical professionals to conduct limited research, which then results in a published article. Yet, as in other toxic substances, the vast majority of attorney initiated research and publication occurs for the defense.

With the rise of asbestos as the predominant occupational lung disease of the mid to late twentieth century, and, more recently, an increasing number of toxic substance areas of litigation, the methods use to minimize silicosis in the public eye have been both extended and evolved into more complex forms—forms which are now being used to forestall increased silica regulation. Unfortunately for many of the asbestos companies, and a few other toxic substance companies, the role of the attorneys have not remain as shrouded as they were for silica throughout the twentieth century.

Asbestos: New Defendants, Old (Tobacco) Tactics

As the twentieth century closed, federal and state courts became increasingly reluctant to try asbestosis, or even many lung cancer cases. Mesothelioma became the dominant disease being litigated, with commensurately larger settlements and trial verdicts. Fortunately for the former asbestos manufacturing industry, the role and scope of occupational disease science in civil litigation fundamentally changed during the last decade of the twentieth century. With this transformation in its civil litigation usage came a revolution in the scientific literature. As first mentioned in chapter 3, in June 1993, the United States Supreme Court handed down a decision in *Daubert v Merrell Dow Pharmaceuticals, Inc.*—a ruling that required Federal judges to be gatekeepers for the admission of scientific evidence in their court rooms. The Supreme Court’s legally based scientific standard both endowed defense attorneys with the authority to seek disqualification of plaintiff experts and opened the door to greater industry (with its greater financial resources) manufactured science.  

Many public health advocates denounced the decision. Harvard Public Policy professor and public health advocate Sheila Jasanoff argued that “in an ironic turn, the ‘science’ that the Court officially embraced remained profoundly a creation of the court’s own biases, needs, and misconceptions concerning scientific inquiry; while urging judges to defer to scientific authority, the Court gave judges new resources for writing their preconceptions regarding science into law.” The last twenty years of litigation has validated Jasanoff’s implicit concerns and opinions about the ruling’s effects on litigation. Through its straitjacketed approach, *Daubert* has allowed industry attorneys to often exclude plaintiff expert opinions because of the inherent uncertainty of medical science; as well as to manufacture science through published studies in (often) industry sponsored journals. On occasion, judges have required doctors who testify as experts to meet standards that exceed those used in diagnosing patients, as well as inculcate a widespread disregard for the

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uncertainty and skepticism inherent in scientific advances, particularly those that involve the human body. 42

Robert Merton, the noted sociologist of science, highlighted this norm of “organized skepticism” in his 1957 book of essays, Social Theory and Social Structure. Merton defined “organized skepticism” as “a latent questioning of certain bases of established routine, authority, vested procedures and the realm of the ‘sacred’ generally … Most institutions demand unqualified faith; but the institution of science makes skepticism a virtue.”43

As noted above, the importance of Daubert is not its scientific failings, but the further openings it gave to industry’s litigation defense attorneys. They not only used Daubert to limit plaintiff expert opinions, but used the vast monetary resources of industry to reshape the “peer reviewed” topography of occupational health science. They did this by hiring specialty consultant firms to conduct studies, reanalyze data, and deconstruct prior studies in industry friendly “peer reviewed” journals, often publishing two to four very similar articles from a single study or review. Other than those industry-funded scientists, there is very little interest today in rehashing the well-established hazards of asbestos. Thus an individual examining the peer-reviewed literature in 2010 for the hazards of asbestos brakes, for example, finds a very different landscape than that of ten years ago.44

42 Paradoxically, courts hearing criminal trials—with their higher standard of proof making it the very type of trial where the right to challenge expert opinions would seem most useful—have rarely allowed these challenges. On the other hand, civil trials—which only require a proof of more probable than not—have frequently excluded plaintiff experts: see Sheila Jasanoff, “Law’s Knowledge: Science,” S50. This article contains an excellent critique of the Daubert opinion. Also see Jean Macchiarioli Eggen, “Toxic Torts, Causation,” 889, 896; and Michael H. Gottesman, “From Barefoot to Daubert,” 761 (discussing plaintiff difficulties in meeting the standard). For a discussion of the complexities of epidemiologic methodology that has not been considered by the Supreme Court see Richard W. Clapp, et al., “Environment and Health,” 189-215, especially 199-212 (the authors discuss both the methods that can be used to pre-determine the outcome of a study and how easy it is to criticize past studies. Both techniques have been consistently used by industry litigation consultants; in particular see the discussion of industry brake articles, anon); and Jerome P. Kassirer, et al., “Inconsistency in Evidentiary Standards,” 1382-1387, especially 1382 for a discussion of legal standards that exceed diagnostic standards.


44 These “industry friendly” journals are often at least partially funded by industry or industry trade groups, have a substantial number of industry representatives on their editorial board, have limited, if any, disclosure of conflict of
Because of the one-sided nature of the ensuing research, defendants were able to react to the 1990s findings of the international health organizations by using their ability to modify the scientific literature. Nowhere has this new opportunity been used more than in asbestos litigation. Since 2000, asbestos defense attorney-funded litigation support firms such as Exponent, Environ and ChemRisk have published myriads of articles in industry-friendly peer reviewed journals, all arguing for the relative innocuousness of chrysotile and the ability to use it safely with appropriate exposure controls. Using regulations put in place during republican administrations, they even petitioned regulatory agencies to note the new “scientific” findings in publications designed to protect the health of workers. We can see the effect of these changes in a number of cases, including those involving automobile brake workers.  

interest requirements, and rarely describe how their “peer review” process works. The two journals with perhaps the closest ties are Risk Analysis and Regulatory Toxicology and Pharmacology. The journal Risk Analysis has published a number of industry-funded asbestos studies, often without an indication of any conflicts of interest. Shortly before June 2006, the editor in chief of Risk Analysis accepted employment with the industry oriented litigation support firm, Exponent. Center for Science in the Public Interest, “Integrity in Science Watch June 12, 2006,” accessed on May 1, 2010 http://www.cspinet.org/integrity/watch/200606122.html. The journal Regulatory Toxicology and Pharmacology is perhaps the archetypal host of such articles. In 2002 the Center for Science in the Public Interest and over 40 scientists (some of whom testify for plaintiff counsel) wrote to the journal’s publisher and owner requesting it to “hold the journal accountable to norms of publication ethics and to require greater independence of the journal from” the International Society of Regulatory Toxicology and Pharmacology, (ISRTP). The group made this request because ISRTP is supported by a large number of industry trade groups and large corporations that are subject to health and environmental regulations. The letter also listed a significant percentage of the journal’s editorial board as having financial ties to products and companies that have been the subject of articles in the journal. These individuals included several lawyers representing industry and numerous consultants, such as Dennis Paustenbach, paid by industry and their lawyers to conduct studies for litigation and regulatory purposes. (Paustenbach has written at least 16 industry friendly articles published by this journal, including one concerning asbestos in automobile clutch discs.) The letter called on the publisher to require disclosure of interest statements with each article, including all financial conflicts, not just who funded the article. Olav Axelson, et al., letter to Ms. Kirsten Chrisman and Mr. Paul Weislogel of November 19, 2002, in Editor-in-chief, “Special Contributions, Correspondence about Publication Ethics and Regulatory Toxicology and Pharmacology,” International Journal of Occupational and Environmental Health 9, no. 4 (October/December 2003): 386-389. The publisher and journal editor replied that he was confident that the board was balanced, without specifying why he was confident. He further stated that the journal was “peer-reviewed” without specifying how that peer-review worked. Finally, he agreed that there should be a disclosure of conflict of interest policy. Two months later the publisher wrote to announce that it had enacted a voluntary disclosure of conflict of interest policy.  

45 David Michaels, et al., “How Litigation Shapes,” 1137-1169. Since the 1990s automobile and automotive equipment manufacturers have funded “litigation support” consultants with tens of millions of dollars to develop tests and conduct scientific literature “reviews” that “demonstrate” their products are safe. Between 2003 and 2006 General Motors alone made payments of at least $8,975,366.00 to “litigation support” consultants. General Motors Corporation’s Supplemental Response to Plaintiff’s Request for Production of Documents Regarding Consulting Relationship with Authors who Published Industry-Favorable Literature, dated 21 November 2006, Janet Unden, Personal Representative of the Estate of Richard Unden, Deceased, v. General Motors Corporation, et al., Case No. 05:6311, In the Circuit Court of the 13th Judicial Circuit in and for Hillsborough County, Florida. Since General Motors objected to providing any attorney work
The Disappearing Asbestos Brake Disease

Brake and automobile manufacturers have been leaders in this area of attorney driven scientific research in the new millennium, producing numerous articles claiming brake workers are not at risk. Yet, brake manufacturers had early knowledge of the dangers of asbestos. Automobile mechanics and brake lining workers have long been identified as being at increased risk for asbestos disease. As early as 1930, Merewether and Price had included manufacture and repair of brake linings in their list of British occupations at risk of asbestos exposure. In the United States, Hueper had included them in his mid-century listings of at risk occupations.

Initially, brake manufacturers, led by Johns-Manville, paid for limited research at the New York Saranac Laboratories, primarily to help in workmen’s compensation hearings. As early as 1946 they were aware that workers needed respirators for asbestos fibers. At that time, at least one brake manufacturer desired Gardner, the head of Saranac, to write a report for workers’ compensation claims purposes that did not inject the “possibilities of cancer,” but did indicate fibers below five microns in length were not injurious.46

Decades later, brake manufacturers attempted to beat back proposed regulations. Yet even then, they did not claim brake work was innocuous. In 1978, the Friction Materials standards Institute, Inc.’s comments to a proposed brake practices workbook demonstrated that the brake industry understood the hazards associated with vehicle and industrial brake replacement. Among their comments they complained about the booklet showing a mechanic using an air hose to blow dust from a brake assembly, commenting that they did not recommend using an air hose even if the mechanic wore a respirator, because “he is contaminating the rest of the work area.” They also

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46 Thomas Gatke (President, Gatke Corporation) to J. F. D. Rohrbach, August 12, 1946, Box 04-01872, Johns Manville Archives.
indicated that a “good high filtration industrial vacuum cleaner” be used, rather than dry sweeping or brushing the work area. They also suggested the worker be shown wearing a mask while handling a set of brake shoes.47

These statements were not just precautionary, but based upon research. Throughout the 1970s Mount Sinai scientists had investigated asbestos disease in workers and the range of exposures that caused the disease. In the late 1970s they turned their attention to exposures from brake repairs. They and colleagues in Europe and Australia initially analyzed dust from brake drums. The percentage of asbestos fiber ranged from 4.5% in New York City to a low of 1.4% in Australia. They then conducted air sampling during actual brake maintenance and repair in numerous locations throughout New York City. As in most studies, grinding and beveling produced the most sustained dust, 1.7 to 7.0 f/ml for grinding and 23.7 to 72.0 f/ml for beveling. This dust had spread throughout the facility, with a level of 0.3 f/ml even 28 feet away from the operation. Blowing dust from brake drums also produced extremely high short-term fiber concentrations, from 6.6 to 29.4 f/ml.48

Although by 1978 Mount Sinai had a very poor reputation in industry circles, brake manufacturers did not have to rely upon its study; it merely provided confirmation of industry’s own reports. As distinguished British physician Muriel Newhouse—widely regarded as the British doyenne of occupational medicine—wrote in a supportive letter to the editor about the Mount Sinai study, “the subject had been fully reviewed in Great Britain in 1970,” with various forms of dust controls being recommended at that time. The review to which Newhouse referred came from papers presented at a conference held at Ford Motor Company headquarters in England. As with almost all industry studies, the reports suggested that exposures were generally below the current standards. The distinguishing factor between these and more recent industry reports was that the

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47 E. W. Drislane (Executive Director) to E. M. Fenner, May 9, 1978, Box 04-1838, Johns Manville Archives.
standard they approached, but did not exceed, was 2.0 f/ml, not the 0.1 f /ml standard required by OSHA in the twenty-first century.

While the tests conducted by the British automobile companies generally did not exceed the standards, the authors had admitted the dust created could be hazardous and various papers discussed control methods and alternative materials for brakes. D. E. Hickish and K. L. Knight, members of Ford’s Medical Services presented two of the papers. Air sampling fiber counts during brake “blow-out” procedures ranged from 1.12 f/ml to 3.62 f/ml, similar to that found by Mount Sinai. Daylong monitoring of standard brake servicing resulted in fiber averages of 0.68 f/ml throughout the day, well under 2.0 f/ml, but almost seven times over the current standard—and well over the daily amount experts recognized as sufficient to cause mesothelioma. They also found the fiber cloud spread as widely as described by the Mount Sinai study, with concentrations throughout the service garage ranging from 0.07 to 0.49 f/ml. Truck brake maintenance resulted in 7.09 f/ml concentrations during the cleaning, falling to 0.08 following cleaning, with a weighted average of 1.75 f/ml. Their studies did not include filing or grinding brakes, but as the authors cautioned, they “would envisage that these would give rise to considerably increased air contamination by chrysotile asbestos, with the attendant need for strict precautions to prevent the inhalation of fibres.”

Both Ford and British Leyland researchers also presented papers of additional experiments designed to determine if controls could reduce the dust. In his paper about the experiments, British

Leyland employee G. L. Lee noted that sampling “blow-outs” resulted in concentrations of 3-5 f/ml, similar to that found by Mount Sinai and Ford, with alternative cleaning methods substantially reducing the dust cloud. Hickish and Knight found that alternative cleaning methods could decrease the fiber count by up to 40%. Unfortunately, as noted by Newhouse in her letter, even seven years later the controls were rarely used.50

The insurance industry also had ensured that brake manufacturers were aware of the financial risks they assumed in selling asbestos brakes. For example, in a March 6, 1976 letter, Travelers Insurance Company Account Executive, John L. Harris, had informed insurance agents Marsh & McLennan that they would not renew insurance for a brake product, despite the data provided that showed relatively low exposure, because they “could not conclude from this data that ‘the brake product is free of an asbestosis exposure.’” The letter also commented on the individual susceptibility variations between individuals as a reason to be cautious about claims of low exposure. This advice should not have come as a surprise. Brake lining manufacturers faced asbestos lung cancer lawsuits as early as the mid 1950s. In 1979 Bendix had faced at least five claims for laryngeal cancer contracted by its asbestos exposed plant workers. Bendix safety personnel then called Johns Manville to ascertain whether it would assist in developing a defense against the compensation claims. Yet, until the Daubert decision, other than the inconclusive Saranac investigations, brake manufacturers had conducted only a smattering of asbestos exposure or disease studies.51

All of this changed with Daubert. Since 2000, papers published on this topic substantially increased, almost exclusively due to industry sponsored consultant publications. During the ten-year

51 David Michaels, et al., “How Litigation Shapes,” 1149-50; Wilhelm C. Hueper Deposition of June 16, 1977, 9-10; E. M. Fenner to P. Kotin, July 11, 1979, Box 04-1840, Johns Manville Archives; and John L. Harris to Robert Leach, March 8, 1976, Box 04-02644, Johns Manville Archives. Although the latter letter did not involve attorneys, let alone attorneys working for Johns Manville, the company labeled its copy of the document “privileged” in 1983. Since the one animal study on brake material conducted by the IHF in the 1950s was never published, it was likely positive.
period between 1997 and 2006, journals published at least 39 papers relating to brakes and asbestos. One public health historian identified 26 such articles as being associated with litigation. Industry-associated experts wrote eighteen of the 26 papers, while experts who work primarily (but not always exclusively) for plaintiffs, wrote eight. In the industry-sponsored papers, most authors were not academicians or research scientists—for the most part they were owners or employees of self-professed litigation support firms such as Environ, Exponent Health Group, and ChemRisk. Two of these firms, Environ and ChemRisk, received well in excess of $20 million dollars for their asbestos brake litigation support work.\textsuperscript{52} As a result of this work, literature reviews conducted today present a vastly different picture of the status of the science than a review conducted prior to 2000. By seeding the scientific literature, industry has attempted to manufacture, if not a new consensus in medical science, at least doubt about the nature and risk of asbestos disease—doubt necessary for the successful defense of civil lawsuits.

A search of the Internet medical article database PubMed for the period 1998 to 2015 reveals similar statistics:

<table>
<thead>
<tr>
<th>Table 2: PubMed articles with “asbestos” and “brake” used as search words, 1998-2015\textsuperscript{53}</th>
<th>Original Research or Review</th>
</tr>
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<tr>
<td></td>
<td>Negative</td>
</tr>
<tr>
<td>Industry Experts</td>
<td>27</td>
</tr>
<tr>
<td>Plaintiff Experts</td>
<td>0</td>
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<tr>
<td>Foreign Experts</td>
<td>1</td>
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\textsuperscript{53} See Appendix A for the articles. The classification of industry versus plaintiff expert is based upon the author’s professional knowledge of asbestos experts. Although PubMed may not capture all of the brake and asbestos articles published during the period, it provides a broad cross section and includes most relevant journals. PubMed Website, http://www.ncbi.nlm.nih.gov/pubmed. Last accessed on November 23, 2010.
Unsurprisingly, as the above chart demonstrates, both plaintiff and defendant experts published reports helpful to their respective side. However, only industry experts published true research articles, almost all funded through attorneys. As I have shown throughout this Dissertation, funding either through or with coordination by litigation attorneys allows for concealment of any unhelpful data or study results through the attorney-client privilege and the attorney work product privilege. Such concealment is easily accomplished, even with the researcher having “the right of publication,” for this right often generally applies only to the final approved report, with interim or draft reports being subject to editing—final report “approval” being subject to acceptance by the funder. Thus, whenever attorneys are involved it is possible that in addition to published results, the consultant undertook additional studies that did not provide the results desired. In several more recent articles, defense consultant authors have begun proclaiming that the articles are self-funded. In many cases, however, this seemingly pro bono (legal term for without payment) effort on the part of for profit companies to produce science is simply a marketing tool, with the authors’ firm announcing the article to prospective clients in emails or “newsletters.”54

Other authors continue to hide their litigation relationship. Several defense-oriented brake research articles do not reveal the authors’ asbestos litigation consultant and expert witness relationship, as do—on occasion—plaintiff-oriented publications. For example, an article by industrial hygienist and toxicologist Francis Weir, who with his colleagues reported the results of an investigation into the amount of asbestos released into the air when lathes were used to grind brakes, simply stated that his work was funded by a “grant” from Hennessy Industries. Anyone curious to learn why Hennessy Industries would provide a grant for such a study must go to the Hennessy

Industries website to learn that they manufacture and sell brake grinding lathes. Furthermore, Weir has appeared and testified on several occasions for asbestos defendants.\(^{55}\)

Authors who testify for plaintiffs argue the opposite side of the issue just as fervently. Thus, any attempt to distinguish the two sides is open to the accusation of bias on this author’s part. Thus, for the purpose of this section, I am going to assume plaintiff oriented articles are just as biased as those for defendants and leave them out of the analysis. This leaves one type of article to compare to defendant consultant papers: articles by foreign authors. Since the foreign authors do not appear to be involved in litigation, the articles in the chart’s foreign category have one less potential for bias than writings of either defendant of plaintiff oriented experts. Do they confirm the defense experts’ opinions of a negligible, if any, risk?

In a somewhat surprising discovery, almost all strongly suggest the potential of asbestos disease, particularly mesothelioma. The surprise comes from the uniformity of their results in light of the apparent controversy and doubt raised by defense authors for so many years over the possibility of any brake mechanic being at risk. Granted, the conditions described in most of the reports do not correlate to recent brake repair exposure levels in the United States. However, as will be seen shortly, they compare much more closely to historic levels through the 1970s then corporate funded articles. The foreign reports come from countries around the world, including Poland, Iran, Columbia, Tunisia, Japan, and Australia. The one negative foreign report notes that its results may not be applicable to historic practices, for, “[t]hese low levels can be attributed to the wet cleaning or aerosol spray methods used in recent years to replace the traditional compressed air jet cleaning.”\(^{56}\)

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If these foreign author articles are accurate, how have the company-funded articles arrived at their conclusions? Most do not contain new research but rather involve reviews and reanalysis of old studies or reconstruction of historic dust levels. Industry friendly or controlled journals published a majority of them.57 As explained by David Egilman, frequent plaintiff litigation expert, public health advocate, and associate professor of Public Health at Brown University, many of these studies incorporated two practices that have been common in industry occupational health related research since the 1930s silica crisis. First, they redefined the criteria for the disease, in this case the cause-effect relationship between the disease and the exposure. Second, they manipulated the data to obtain the desired result. Often, the studies redefined the relevant criteria for inclusion in the analysis, excluding any data that they deemed not sufficiently trustworthy, which almost exclusively included positive studies. They accomplished this through the use of subjective reliability standards that only neutral or negative studies met. They also included non-relevant studies in their analysis. They then used statistical biases in manipulating the remaining data to ensure the final result.58

The experimental industry studies considered the levels of asbestos exposure for brake work under carefully controlled conditions designed to limit exposure. These airborne dust studies used various techniques to ensure that dust levels remained low. A study by Charles Blake, a Certified


Industrial Hygienist who works for Bureau Veritas—an industry oriented health, safety, and environmental consulting group—provides an example of the methodologies used to assist attorneys in denying that a plaintiff’s disease could have been caused by his brake work exposure. Blake’s initial study examined brake changing to determine if mechanics were exposed to asbestos fiber in excess of the 2003 limit of 0.1 fiber/ml. The study demonstrates how procedures and methods can be manipulated to achieve low exposure rates.

In order to establish both the necessity of his experiment and its accuracy, Blake first contended that historic studies couldn’t be relied upon because they were not strictly controlled. In discussing the early studies, he ignored the Mount Sinai study, as he did one of the early British Leyland studies. While he did mention the Hickish studies with a high exposure of 87 f/ml for a short term exposure and 0.68 f/ml for an entire shift of brake cleaning, he minimized its significance by not discussing the specifics and noting its failure to include complete brake repair/replacement operations.59

Blake then turned to the early NIOSH studies, commencing in 1972. He mentioned six of the studies, claiming that most of them showed exposures to be at or below the permissible exposure limit of 0.1 f/ml for an eight-hour shift. As with the Hickish studies, he did not provide any details of the NIOSH studies. Blake had good reason not to provide more information, because his claims about the studies were very misleading. One of the studies, in a municipal garage—which did not have a profit incentive to cut corners on safety—had an average reading of 0.28 f/ml, even though it characterized the asbestos in sampling as “very little.” Another study determined that the TWA of asbestos exposure for eight of the thirteen workers exceeded 0.1 f/ml. A third study of a private concern found general area air samples of 2.0 f/ml for an eight hour TWA. Blake’s article

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was even misleading about studies with air sampling lower than 0.1 f/ml. In one, the asbestos average was 0.04 f/ml, but as the author writes, “very little brake service activity occurred on the day of the survey.” A fifth survey covered operations using a control—compressed air solvent mist for the asbestos blow-out from the wheel—during the operation. As that study’s author emphasized, “asbestos exposures depend on the types of brake servicing operations and procedures utilized.”

Following this attempt to minimize the importance of prior surveys, Blake then described his study’s five short experiments—four brake maintenance experiments and one cleaning experiment—that took three days to complete. Unlike historic studies, Blake’s team strictly controlled the experiment, for example starting with a clean facility, rather then one that had asbestos dust on surfaces from prior work. This team then limited the amount of work to ensure asbestos did not accumulate as the work progressed. Finally they limited the experiments to very short periods, the four brake procedures lasted only 4.1, 9.7, 17.8, and 19.9 minutes. In contrast, the air monitoring covered extensive time periods: 95, 102, 96, and 103 minutes respectively. In total, the procedures took less than an hour, whereas the air sampling lasted almost a full workday, an

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60 J. M. Dement, “Cincinnati Municipal Garage, Automobile Brake Servicing Operation,” National Institute for Occupational Safety and Health. *U.S. Public Health Survey*. Report No. 32.11. Cincinnati, OH (1972) (average reading 0.28 f/ml); Roberts, D. and R. Zumwalde. Industrial Hygiene Summary Report of Asbestos Exposure Assessment for Brake Mechanics. National Institute for Occupational Safety and Health. Cincinnati, OH (1982) (8 of 13 mechanics exceed asbestos limit); Johnson, P.L. Preliminary Industrial Hygiene Survey -- Auto Brake Clinic, Cincinnati, OH. National Institute for Occupational Safety and Health, Cincinnati, OH (1976) (air samples of 2.0 f/ml TWA); D. Roberts, “Industrial Hygiene Report -- Asbestos, Reading Brake and Alignment Service, Reading, OH,” National Institute for Occupational Safety and Health, Cincinnati, OH (1980) (very little brake activity); D. Roberts, “Industrial Hygiene Report -- Asbestos, Allied Brake Shop, Cincinnati, OH,” National Institute for Occupational Safety and Health, Cincinnati, OH (1980) (exposure depends upon procedures utilized). While Blake is used as an example, he was far from being the solitary industry expert to use this type of misleading historical analysis. Another typical example comes from a Chemrisk authored article. The authors state categorically that “numerous studies have shown that garage mechanics were historically exposed to asbestos levels below . . . current occupational standards [0.1 f/ml].” In support they cite to thirteen studies, most of which were conducted by industry experts and occurred after 1990. They ignore the British Ford studies. This omission is not inadvertent, since they obviously knew about the studies, later citing to the Hickish study as reporting that eleven brake jobs may be performed in one day. Madl, A. K., et al., “Exposure to Chrysotile Asbestos Associated with Unpacking and Repacking Boxes of Automobile Brakes Pads and Shoes.” *Annals of Occupational Hygiene* 52, no. 6 (August 2008): 463-79, 464 and 467.
unlikely scenario for a full-time brake mechanic. This methodology ensured that the time-weighted average was diluted by both the limited time working versus the air sampling and the failure to conduct work over a full workday. There was good reason to keep the test short, since airborne asbestos would have increased throughout the day as the mechanic’s work caused further entrainment.61

Five years later Blake conducted a similar experiment examining asbestos dust generated during sealant and drive clutch replacements. It further demonstrated two of the numerous ways in which airborne asbestos dust levels can be manipulated in a study to result in measurements far below historic levels. Each of Blake’s fourteen experiments in this study took only fifteen minutes and covered only a small portion of the work necessary to complete the job being analyzed. As he did in the earlier brake study, Blake did not consider the build-up of exposures over time. The facility was first cleaned with modern equipment designed for asbestos removal immediately before the limited study. Furthermore, he opened the room between each of the fourteen individual experiments, thus allowing any airborne fibers to dissipate, further reducing work area fiber concentrations that would normally be accumulating.62


62 Charles L. Blake, G. Scott Dotson, and Raymond D. Harbison, “Evaluation of Asbestos Exposure within the Automotive Repair Industry: A Study Involving Removal of Asbestos-Containing Body Sealants and Drive Clutch Replacement,” Regulatory Toxicology and Pharmacology, 52 (December 2008): 325-326. This journal’s acceptance of Blake’s article is very understandable, given the journal’s close ties to industry. Its editor, Gio B. Gori worked during the 1980s and 1990s as a consultant for tobacco companies. Source Watch lists his activities for these companies as including attending conferences, writing and publishing books, letters and papers, and lobbying. Source Watch Website, http://www.sourcewatch.org/index.php? title=Gio_Batta_Gori, accessed May 28, 2009. Many of the journal’s editors, such as Paustenbach and Wong, also have very close ties to industry attorneys. The article was partially funded by Ford and was likely paid for by Ford’s attorneys—perhaps the same attorneys that have funded much of Paustenbach’s work. See David Egilman and Samantha Howe, “Corporate Obstruction of Public Health via Manipulation of Epidemiology,” International Journal of Environmental Hygiene 13, no. 1 (January/March 2007): 123. Paustenbach has a similar article examining heavy equipment brake removal—a study that again conducted circumspect air sampling of limited and constrained activities. In the study they even removed the coveralls after each activity, rather than see the deposits accrued on the clothes during a full work day. A. K. Madl, S. H. Gaffney, J. L. Balzer, and D. J. Paustenbach, “Airborne Asbestos Concentrations Associated with Heavy Equipment Brake Removal,” Annals of Occupational Hygiene 53, no. 8 (2009): 839-857.
Unlike Blake’s study, many brake and clutch repair workers historically spent not fifteen minutes, but much of their day, in areas being used for brake work. This additional period greatly increased the potential for longer and more intense exposures. Asbestos fibers can remain airborne for considerable periods of time and are easily re-entrained. Thus, in many locations, exposures increased as the workday progressed. Taking readings over a fifteen-minute period or for an hour-and-a-half for a ten-minute procedure, both failed to consider this potential for longer period of exposure. More importantly, only a fraction of the airborne asbestos concentration comes from direct exposure to new releases of asbestos fiber. Re-entrainment of asbestos from prior similar activities potentially presents a more significant portion of the exposure. For example, during the 1950s and 1960s, workers had often used brooms to conduct daily or weekly cleanup of shop debris. In shops working with asbestos, brooms might remove large clumps of asbestos material from the floor; however, they also broke up the bundles of fibers, causing smaller fibers to re-entrain into the air, only to settle again hours later on workplace surfaces. The asbestos fiber could remain on surfaces for months or even years, allowing it to become airborne each time activities took place in the room. The common practice of using compressed air to clean fibers off materials or brakes also caused fibers from both that material and many nearby surfaces to break up and become airborne. Contrary to the sterile types of experiments conducted by industry experts, air sampling of specific asbestos work practices must take into account the conditions of the 1950s and 1960s—environments where build-up of fiber from past activities could dramatically increase exposure levels.\(^\text{63}\)

\(^{63}\) Some asbestos fibers can take up to eighty hours to settle out of the air: Iowa State University, *Asbestos Awareness Training* (Ames, Iowa: Iowa State University, 1996), http://www.ehs.iastate.edu/publications/manuals/asbestosbook.pdf, accessed May 1, 2010; and author’s professional experience in interviews, depositions and trials. Both Mount Sinai’s and Ford, UK’s sampling in the early 1970s demonstrate the stark reality of true historic working conditions, with both samplings showing over a hundred times more exposure than found in current supposed historic recreations by corporate funded experts. A. N. Rohl, A. M. Langer, P. Klimentidis, M. S. Wolff, and I. J. Selikoff, “Asbestos Content of Dust Encountered in Brake Maintenance and Repair,” *Proceedings of the Royal Society of Medicine* 70 (January 1977): 32-37; and D. E. Hickish and K. L. Knight,
The methods used and results obtained from these industry-sponsored studies contrast sharply with the few foreign studies found in the PubMed search that used actual working conditions. One such recent study from Japan both reviewed the limited available literature and examined air samples from plants reprocessing brakes and clutches during the years 1982 to 1985. Although the study did not indicate who provided the funding, the participants included not only medical doctors and professionals from several Japanese universities but also employees of the Nagoya City Health Research Institute, so it may have been funded through a government entity. The authors sampled asbestos exposures of workers in three small asbestos brake-reprocessing factories. Initially, they found very significant levels of exposure. These concentrations decreased over the months of the sampling as good housekeeping and wet work practices gradually took hold. As would be expected, the study also found that air circulation, ventilation, and the amount of work performed all affected the levels of exposure. Unlike most of the industry studies that do not note their limitations—likely because at trial this would provide an avenue for cross examination—this study noted that it was not a random sampling of reprocessing sites, and sampling was only conducted during the summer when ventilation was better. This second limitation suggests that sampling in winter months might have resulted in dramatically higher levels of asbestos exposure.64

Other, more recent studies have been conducted in Columbia and Iran. In South American, researchers conducted two studies, one for cars and one for heavy equipment. The automobile samples demonstrated higher exposure, with 8 of the 11 full-shift air samples of brake repair exceeded 0.1 f/cm$^{-3}$. Several of the filters could not be analyzed because they were overloaded with material. In contrast to Blake, the researchers found “extremely” high levels during brake grinding.

The group recognized that the Columbian practices were very different than those used in the United States since the 1960s, but also noted several factors that may have reduced the TWA they calculated below that actually experienced. In their heavy equipment study, they found a broader range of exposures above and below the exposure limit. In at least one of the shops with several exposure readings above 0.1 f/ml TWA, a ventilation system remained active throughout the testing. The highest TWAs reported in the study exceeded the current OSHA standard by more than 600%. Of even greater importance, the researchers found calcified plaques and pleural thickening—“strongly suggestive of asbestos exposure” and disease—in the lungs of 3 out of 10 workers examined.

In Iran, members of the Tehran University Medical Services calculated exposures for auto and heavy truck brake mechanics in 30 brake repair and replacement shops, determining that mechanics’ geometric mean exposure exceeded 0.9 f/ml, nine times the permitted exposure level. The methods used by the mechanics appear to have been similar to those practiced in the United States prior to the 1970s.65

This contrast should come as no surprise. Two of the litigation support firms whose employees have authored the most industry-friendly articles in the PubMed search, ChemRisk and Exponent, Inc., have a history of almost always providing the specific results desired by industry attorneys. Their litigation support strategies have been illustrated throughout this dissertation. In asbestos brake litigation these firms followed a similar course of action. In one typical early 2000s example, General Motors’ attorneys and Exponent developed a program of litigation support. Few

specific are known about the programs development, since it is shrouded by the attorney work product privilege. We know that in June 2003 Patrick Sheehan, a Principal Scientist at Exponent, wrote a letter to attorneys representing General Motors that General Motors characterized as “confidential correspondence from expert consultant to legal staff attorneys describing litigation strategy tasks and budgets for proposed and ongoing work related to defending ongoing, pending and potential litigation.” The purpose of the document was described as being “to set forth litigation strategy tasks to assist in ongoing, pending and prospective litigation.” He sent another letter on July 22, 2003 about the same matter. The General Motors Corporation Privilege Log also lists draft memoranda from expert consultant to its staff attorneys on September 7, 2002 about the same subject. Since the actual wording of the documents that are listed on privilege logs is exempt form disclosure, the specific recommendations are not known. However, the various articles written and the positions taken by Exponent and ChemRisk employees certainly give a sense of the program agreed upon by the parties.66

As occurred for chromium and benzene—and now even silica—Exponent, Environ, and ChemRisk have all written numerous articles that assist in the defense of asbestos brake lawsuits. Yet independent scientific articles rarely, if ever, support the positions taken by these litigation support firms. For example, in a 2008 article funded by the big three United States automobile manufacturers, ChemRisk employees claimed that their review of chrysotile-exposed cohorts in the scientific literature pointed to a threshold level for chrysotile that is higher than the exposure levels

of brake mechanics. To arrive at their conclusion, the group evaluated over 350 studies—but eliminated all but fourteen because they did not meet the subjective inclusion criteria. Industry-funded researchers conducted almost all of the studies that remained for consideration. The authors wrote that they eliminated most of the other research because of “lack of cumulative exposure information, lack of information on fiber type, and/or evidence of significant exposures to amphiboles.” Unfortunately, the article does not provide a list of the evaluated articles, thus it is impossible to determine if the fourteen articles used were in fact the only ones that met the design criteria. Chemrisk scientists then manipulated the studies to subjectively determine a “No effect level.”

At the conclusion of the article, the authors acknowledged funding by the automobile manufacturers, before averring that, “these three funding sources (and their counsel) did not provide editorial comments or review the manuscript prior to submission to the journal.” Left unsaid is whether, as occurred in a ChemRisk chromium study, the funders (more likely their attorneys) edited the initial report that provided the basis for the manuscript. If this contract was similar to the one in the chrome study, automobile manufacturer attorneys could have stopped any publication simply by refusing to approve an initial report until it comported with their desires for litigation. Whether or not there was editing of the report, the article makes clear its purpose of assisting litigation by asserting a no-effect level of chrysotile that is higher than the normal level of exposure experienced by brake mechanics. It is safe to assume that if the group had not been able to provide an analysis that confirmed a no-effect level higher than brake mechanic exposures, ChemRisk principals would have thought long and hard about whether they wanted—for the first time—to publish a study in opposition to their primary clients: companies in litigation or with regulatory controversies.

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In addition to eliminating many positive studies, the authors also assumed the historic levels were those determined by industry litigation support experts, rather than those found by third party authors—such as the previously cited article concerning Japanese brake exposure levels, or historic United States and British industry air sampling for brake work. Even other internal industry reports cast doubt upon this reliance. For example, in a 1977 memorandum a Bell Laboratories employee informed his superiors that his company’s brake work exposure level was higher than permissible. In addition, fibers could not be counted on a number of the air sampling filters because the dust concentrations were too heavy, suggesting the levels were even higher than he found. As stated in the memorandum, “As a result of these measurements it is evident that an exposure to asbestos fibers occurs during the operation of inspection and/or changing of brakes.”

Ironically, at nearly the same time as automobile manufacturers were funding a review of prior epidemiological studies to demonstrate that low levels of exposure to chrysotile did not cause cancer, two new research articles from Italy both found excess cancers in chrysotile worker cohorts. The first article considered workers at a tremolite-free chrysotile mine and found a greatly increased risk of mesothelioma—even among office workers who would normally receive only low exposures. The second found a greatly increased risk of both lung cancer and mesothelioma among chrysotile textile workers.68

Other recent articles discussing low dose mesotheliomas also raised questions concerning the validity of ChemRisk’s review. For example, three years prior to ChemRisk’s article, a group from the University of California at Davis published an article—not cited by ChemRisk—about

mesotheliomas from exposures well below those experienced by brake mechanics. This group even
determined that residents simply living near naturally occurring rock outcroppings of chrysotile or
tremolite—chrysotile’s normal contaminant—had higher risks of mesothelioma.69

Along with these scientific literature litigation support efforts, brake manufacturers have also
used yet another technique to buttress their litigation defense. This one involved using manufactured
science to modify governmental scientific guidance to the public. In 2003 the large defense oriented
law firm of Morgan, Lewis & Bockius LLP—well known for it’s numerous corporate clients—
challenged (without revealing its client) an EPA publication distributed to provide guidance to
automobile mechanics. The publication, known as the Gold Book, had been designed to inform
auto mechanics about the methods that should be used to safely work around asbestos. The
challenge was possible only because of a law enacted in 2001 requiring agencies to follow quality
control guidelines that allowed affected individuals the opportunity to correct misinformation. In
this case the law firm claimed that the asbestos brake health pamphlet was “no longer current from a
scientific perspective.” They based this claim specifically upon studies conducted by Exponent and
funded by the automobile industry. The sponsors of the challenge achieved their goal. By 2006 the
agency reduced the guidance booklet from fifteen pages to two, with minimal detail.70

Finally, there is evidence that John L. Henshaw—at the time a “Teaming Partner” associated
with ChemRisk and an expert witness for brake manufacturers in asbestos litigation, as well as the
former Assistant Secretary of Labor for OSHA from 2001 to 2004 during the George W. Bush
Administration—similarly influenced a change in a health bulletin for asbestos brake work that
OSHA posted on its web site in 2006. Three weeks following the posting, Henshaw sent an email to
the head of OSHA’s Directorate of Science, Technology and Medicine, noting that the bulletin

might be subject to a Data Quality Control challenge. The scare tactic worked. As of May 2007, the OSHA bulletin contained a disclaimer that it “is not a standard or regulation, and creates no new legal obligations,” thus reducing the warning’s effectiveness. OSHA also suspended the employee who had prepared the original advice for “issues related to the accuracy” of the bulletin and failure to include current literature, including ChemRisk’s work. Shortly afterward, Henshaw informed a Baltimore Sun reporter that his intervention “was not undertaken on behalf of anyone by [himself].” He did not indicate whether or not the email generated additional work as an expert witness for brake manufacturers.71

Georgia-Pacific’s Attempts to make its Asbestos Problems Disappear

Brake manufacturers are not the only targets of asbestos plaintiff lawyers today. Nor are they alone in their desire to use any means to thwart asbestos litigation. Prior to the bankruptcy of the vast majority of the most highly visible asbestos defendants, Georgia-Pacific had a very low profile in asbestos litigation. Insulators and other first-wave plaintiffs rarely used their products, which were primarily used in dry wall and ceiling applications. Furthermore, while on occasion fiber released from usage of their products could be substantial, it did not normally result in severe asbestosis, as was often the case with asbestos insulation. Even so, as the major asbestos companies declared bankruptcy, Georgia-Pacific must have been aware of the sizeable potential of having to defend itself against an ever-increasing numbers of lawsuits.

Georgia-Pacific had first become involved in manufacturing asbestos products when it

71 David Michaels, et al., “How Litigation Shapes,” 1137-42, 1166-68, 1169 n. 78. This was not the first time Henshaw provided assistance to the asbestos brake industry. While Assistant Secretary of Labor for OSHA he responded to questions from Washington Senator Murray, averring that NIOSH studies “have characteristically been below the current applicable OSHA permissible exposure levels for asbestos” [0.1 f/ml]. Like Blake’s brake experiment article, this answer is extremely misleading. In his thirteen citations, he relied on five of the same studies as Blake—a number of which found levels well above the exposure limit—as well as five other studies testing whether specific controls could reduce the amount of asbestos fiber released into the air. John L. Henshaw to the Honorable Patty Murray, February 10, 2004, https://www.osha.gov/pls/oshaweb/owadisp.show_document?p_table=INTERPRETATIONS&p_id=24758 (accessed December 20, 2015).
purchased Bestwall Gypsum Company in early 1965. During the next decade, ten different plants throughout most of the United States manufactured a line of at least thirteen asbestos products, including joint compounds, patching materials, and ceiling finishing materials. Throughout their manufacturing period, Georgia-Pacific shipped most of the products dry, requiring mixing with water before use, as well as sanding in many cases, following application. The products’ chrysotile asbestos content varied from less than 1% to almost 30%. Popular ReadyMix, one of the few products sold wet, contained between 4 and 12% asbestos and required sanding after it dried on the wall. The company continued selling its asbestos products until late 1977.\footnote{Georgia-Pacific’s Responses to Plaintiff’s Interrogatories, Abate, et al. v. ACandS, et al., in the Circuit Court for Baltimore City, Case No. 9307601, December 13, 1993, 1-2, 4-5, and 8; and Georgia-Pacific’s Answers to Plaintiff’s Standard Interrogatories, Laing v. Georgia-Pacific Corp. et al., Circuit Court for the County of Wayne, Michigan, No. 86-606418-NP, October 2, 1987, 7-13.}

Corporate executives learned of asbestos hazards within two years of purchasing Bestwall. In September 1967, Georgia-Pacific’s Western regional manager for Gypsum products attended a Gypsum Association Safety meeting at which the members discussed a number of lung cancer cases being found in a neighborhood surrounding an asbestos plant. This knowledge quickly became known throughout the company. Twenty-five years later, Oliver E. Burch, the company’s General Sales Manager for the Gypsum Division, admitted that he became aware of asbestos hazards in the late 1960s. In another deposition, James R. Hurd, Georgia-Pacific’s Safety Manager, admitted personal knowledge about the inherent dangers of asbestos since 1967.\footnote{Minutes of the Safety Committee Meeting of the Gypsum Association, September 19, 1967, 2; Deposition of O. E. Burch, Abate, et al. v. ACandS, et al., in the Circuit Court for Baltimore City, Case No. 9307601, December 8, 1993, 56-57; and Deposition of James R. Hurd, Decker v. Armstrong World Industries, Inc., No. 86-2385-D, Circuit Court of Dallas County, Texas, February 27, 1987, 21-22 and 34.}

Further confirmation of the dangers involved in dry mixing and sanding drywall associated products arrived in the 1970s. At a 1973 meeting of the Gypsum Association Technical Committee attended by two Georgia-Pacific representatives, various members expressed concern about the hazards of asbestos in joint compounds. The members recommended conducting studies of dry
mixing and sanding operations. At least one set of subsequent Association tests confirmed that individuals mixing and sanding joint compound products were often exposed to asbestos fiber concentrations exceeding OSHA limits. Although the Association decided that the readings were inaccurate, the results fell in line with other studies undertaken during the same period. In a series of papers published between 1975 and 1979 Mount Sinai Hospital scientists had found sanding, sweeping, and mixing joint compound type products could release fibers up to 12 times those allowed under OSHA regulations.\(^74\)

Even with the knowledge of their product’s potential hazard, Georgia-Pacific officials did not undertake any testing or research designed to determine either the amount of fibers their products released upon usage or the potential for disease from their specific products. Georgia-Pacific not only failed to conduct any testing, but C. William Lehnert, Georgia-Pacific’s manager for research and Development, also delayed fully warning customers, providing warning labels in April 1973 only on the products he understood covered by federal law, specifically excluding products he wrongly believed were not covered under the OSHA regulations. Thus, one of Georgia-Pacific’s most popular products, ReadyMix, did not have a warning label until OSHA cited the company for failure to comply with its regulations. In an apparent effort to limit the fallout from the labeling, Lehnert also recommended having its field sales people inform customers that the company was not

aware of any health problems in the Gypsum industry, even though management knew about the exposure studies and did not conduct any systematic research of health problems.\textsuperscript{75}

Lehnert’s confidence was short-lived. In May 1974, Burch forwarded an article to various officials that described medical examinations conducted in New York City of painters who worked around dry wall-associated products. X-rays of the seventeen building construction painters found asbestos-induced fibrosis in nine of their lungs. The article further noted that OSHA officials believed taping and spackling compounds might expose workers to hazardous amounts of asbestos.

One month later, Georgia-Pacific attorneys, concerned about litigation, forced rewording of the company’s warning labels. On June 7, 1974, Safety Manager Hurd informed plant managers that:

\begin{quote}
The new wording for the caution label is in compliance with OSHA regulations. The fact that the wording "Respirator Protection Required When Sanding" was added will only give us additional protection if any liability threats are made against our products. (emphasis added)
\end{quote}

Thus, apprehensions about liability finally trumped marketing desires. The fear of litigation appeared justified as articles expressing concern continued. In one example, Georgia-Pacific executives received a 1975 New Jersey article entitled “Spackling may Harbor Dangerous Asbestos Level.” Still, in relaying the news about the new labels, Hurd sought to calm any fears of sales decline, assuring the managers that “\textit{Our sales effort should not be affected}, as the implication of the OSHA wording is to wear a respirator while working with these products anyway” (emphasis added).\textsuperscript{76}


Even as the sales of Georgia-Pacific asbestos products appeared about to end, sales and marketing personnel continued assuring customers about the safety of their products. In January 1977 Burch had replied to one letter, “I assure you that you need not concern yourself over the possibility of harm due to the Asbestos fiber content in the Joint Cement Product you recently used. . . To our knowledge, there is no known case of harm from Joint Cement containing Asbestos fiber, even after prolonged exposure, numbered in many years. . .” (emphasis added). Burch wrote these words even though he knew about the New York City findings and that Georgia-Pacific had received its first asbestos-related lawsuit in 1976. Four months later Hurd had written a similar letter to another customer.

To our knowledge, Mr. Udavack, there is no known case where harm has come to someone from their use of our Ready-Mix joint compound containing asbestos fibers, even after prolonged exposure numbered in many years . . . I hope that after receipt of this letter you will rest secure in the knowledge that you need have no concern or anxiety over the exposure you or members of your family may have suffered, or may suffer in the future, due to the use of our product.77

Yet another half year later, The Consumer Product Safety Commission (CPSC) announced it intended to ban asbestos-containing joint compound. At the time, Union Carbide supplied much of the fiber used in certain of Georgia Pacific’s products. As part of their preparation for a presentation to the CPSC in objecting to the proposal, Union Carbide conducted studies of the fiber release during application. As with all company-conducted experiments, Union Carbide had an interest in obtaining results that did not justify banning products that used their fiber. Understandably, the results of their studies demonstrated that fiber release did not justify banning the product. The results, however, were not insignificant, and given current regulations, would today fully warrant

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77 O. E. Burch to Craig Burningham, January 21, 1977; Georgia-Pacific's Answers to Plaintiff's Standard Interrogatories, Laing v. Georgia-Pacific Corp. et al., 94; and J. R. Hurd to Robert J. Udavack, from J.R. Hurd, May 20, 1977. In subsequent depositions, both individuals did not recall conducting any research or contacting medical personnel prior to responding to the letters. Deposition of O.E. Burch, 123-7; and Deposition of James R. Hurd, 50.
wearing respirators while working with the product, as well as conducting extensive asbestos cleanup following any significant project using the products. In a five-day study of relatively limited removal and reapplication of spackling and taping compounds (eleven feet of dry wall was removed and replaced) they determined that workers could encounter short-term exposures of up to 0.5 fibers/cc and exposure of up to 0.3 fibers/cc averaged out over a day. Their report indicated that these results were consistent with other reports of commercial sanding operations reporting 1-4 fibers/cc.\textsuperscript{78}

In contrast to Union Carbide’s efforts to show minimal exposures, Georgia Pacific officials did not even try to delay the ban. Despite repeated claims of safety to customers, G. E. Wilson, a Georgia-Pacific vice president, wrote to the Commission supporting the ban. Georgia-Pacific was out of the asbestos business.\textsuperscript{79}

* * *

The new millennium was not kind to Georgia-Pacific’s efforts directed toward maintaining a low profile in asbestos litigation. By 2005 it faced more than 60,000 legal claims with potential liability of $1 billion. It sought relief that year by taking a page (or perhaps a whole chapter) out of the tobacco litigation playbook, initiating a secret research program controlled by attorneys and designed to exonerate its products. As a first step the company hired John C. Childs as Chief Litigation Counsel to head of a group of 13 in-house litigation lawyers. In his words, Georgia-Pacific hired him “to develop and design an in-house defense to the asbestos litigation.” With twenty-four years product liability and toxic tort law experience in private practice, Childs seemed an excellent fit to combat the massive litigation facing the company.\textsuperscript{80}

\textsuperscript{78} H. B. Rhodes and J. F. Collins (Union Carbide executives) to S. John Byington, Barbara Franklin, and R. David Pittle (Consumer Product Safety Commission), December 14, 1977, Box 04-1847, Johns Manville Archives; and Harrison B. Rhodes to S. John Byington, July 14, 1977, with attached “Airborne fiber Counts in a Consumer Spackling and Drywall Replacement Installation Project,” Box 04-1847, Johns Manville Archives.

\textsuperscript{79} Jim Morris, “Facing Lawsuits”; G. E. Wilson (Georgia-Pacific Vice President) to S. John Byington (Chairman, Consumer Product Safety Commission), July 6, 1977 (In author’s possession).

On August 22, 2005, Childs began the research program around which he planned to base his defense by “specially employing” Mr. Stewart Holm—already Georgia-Pacific’s primary toxicology scientist—“to perform expert consulting services in connection with pending and anticipated litigation concerning alleged exposure to asbestos.” Childs informed Holm by letter that his consulting duties were in addition to and distinct from his normal responsibilities.

The letter sent by Childs and signed by Holm clearly demonstrates the character of the anticipated program. Like all significant tobacco research it would be fully controlled by the attorneys, with only good results being released. As a “litigation consultant” Holm reported directly to in-house counsel while “performing workplace simulation tests and analyzing data from prior tests performed by third-parties.” Childs further explicitly informed Holm that the work carried out in his new duties was attorney work product, with all materials prepared by him carrying the following marking: “PRIVILEGED AND CONFIDENTIAL – PREPARED AT DIRECTION OF COUNSEL IN ANTICIPATION OF LITIGATION” (no emphasis added). In signing the letter, Holm further agreed to obtain the express permission of Georgia-Pacific before disclosing any of his supplemental work. To further help shield Holm’s activities from disclosure in litigation, the letter required Holm to confirm that he had never been involved in asbestos matters at GP and had no knowledge of work performed by other GP employees with regard to asbestos. By including this language, Childs sought to exclude any discovery of Holm’s activities. If he had never been involved with asbestos prior to this special assignment, Childs could argue Holm’s endeavors were not his normal business practices.81

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81 John C. Childs to Stewart E. Holm, August 22, 2005 (in author’s possession).
Upon assuming his new duties, Holm began familiarizing himself with the asbestos scientific and medical literature. Spriggs & Hollingsworth, an outside law firm working for Georgia-Pacific, provided him with many of the articles. After reviewing the materials, he met with attorneys from the law firm to discuss his findings. Holm then prepared proposals for the “litigation-driven research projects” he believed should be undertaken: first presenting them to Georgia-Pacific attorneys and then to senior management, including the company’s CEO COO, and CFO and the General Counsel. It appears that management immediately accepted his proposals.\(^{82}\)

Although the presentation has not been revealed, given subsequent events, Holm probably indicated the names of potential expert companies he should contact concerning the research. It is also likely that, prior to the presentation, he had discussed company names (or given the names) with Georgia-Pacific attorneys, since all of the experts involved in the resulting research are high profile defense oriented consulting firms. Almost immediately following the presentation, Holm flew to Europe to meet with David Bernstein. This was not Holm’s first encounter with Bernstein. He had spoken to him about the project even before making the PowerPoint presentation to Georgia-Pacific executives. Quickly satisfied that they had found their man to manage animal tests for chrysotile, in January Georgia-Pacific signed Bernstein to a contract in January.\(^{83}\)

Holm’s eagerness to hire Bernstein is understandable. Bernstein, a toxicologist based in Switzerland, had a long history of assisting companies with their difficult toxicology work. He began working for tobacco companies (BAT) at least by 1983, also soliciting Lorillard for work later that decade. His tobacco work continued until at least 2004 when he published an article for Philip Morris purporting to demonstrate that even though smokers compensated for better filters by using

\(^{82}\) Deposition of Stewart E. Holm, In re: New York City Asbestos Litigation, June 6, 2011, 63, 71-75 (in author’s possession).

\(^{83}\) Deposition of Stewart E. Holm, June 6, 2011, 79-80.
different smoking techniques that resulted in more smoke entering their lungs, they still did not
deposit as many particles in their lungs as before the new filters were introduced.\textsuperscript{84}

Bernstein may have intrigued Holm, because of his prior short-term asbestos inhalation
experiments on rats for Union Carbide and a Brazilian mining company. He argued that his
extremely limited experiments demonstrated that chrysotile rats quickly cleared chrysotile from their
lungs, making the asbestos fiber virtually innocuous. In contrast, during the nineties he performed
two-year studies of rodents for refractory ceramic fiber companies in which he used chrysotile as a
positive control, finding it caused scarring in the lungs of rodents.\textsuperscript{85}

In addition to hiring Bernstein, Holm also set about compiling a short list of litigation
support firms capable of properly reexamining Georgia-Pacific asbestos product fiber release.
Subsequently, company attorneys and he met with representatives from Exponent and ChemRisk at
a law firm office in Denver, Colorado. Holm was very familiar with ChemRisk’s substantial defense
work. He was even more familiar with Exponent, having previously worked with this litigation
specializing consulting firm on other company projects. After considering the two presentations,
Georgia-Pacific hired Exponent. They also retained another consulting firm known for its litigation
defense work—Environ—to conduct other research estimating worker exposure to joint
compounds.

Holm could be confident in his selection, having the knowledge that all of these firms
invariably provide their clients with reports exonerating the products at issue. Yet, ever cautious,
Georgia-Pacific attorneys did not want to rely solely upon the conclusions of the three companies.
They added extra insurance into the contracts, requiring Georgia-Pacific’s approval prior to release

\textsuperscript{84} David M. Bernstein to Dr. Graham Smith (BAT scientist), November 16, 1983 (in author’s possession); David M.
Bernstein to J. Daniel Heck (Manager, Life Sciences for Lorillard), June 24, 1986, (in author’s possession); and David M.
Bernstein, “A Review of the Influence of Particle size, Puff Volume, and Inhalation Pattern on the Deposition of
\textsuperscript{85} Deposition of Stewart E. Holm, June 6, 2011, 159-60; and Jim Morris, “Facing Lawsuits.”
of any information. Between this language and the expert’s role as agents of Georgia-Pacific attorneys, Chief Litigation Attorney Childs could colorably claim all of their work prior to publication as privileged, not subject to disclosure to opposing parties in litigation.\footnote{Deposition of Stewart E. Holm, June 6, 2011, 76, 91-2, 102-3. Paustenbach and ChemRisk may have lost out on the project because of a paper they wrote while working for a non-joint compound client, a paper that castigated pipe insulation as the cause of almost all asbestos disease problems. In the paper, ChemRisk exonerated most other products, but had the following unkind words for joint compound: “In particular, painters may have experienced elevated exposures to asbestos during various drywall activities like mixing spackling compound or sanding.” “Some studies also indicate that drywall tapers may have at times had similar exposures to that of insulators.” “Not surprisingly, considerably higher fiber concentrations were associated with various drywall activities. Specifically, they found historic average fiber concentrations typically ranging from 0.9 to 2.4 fibers/cc during application and the preparation of premixed joint compound, and 5 to 10 fibers/cc during hand or pole sanding activities based on short-term (4–65 min) or peak samples. . .” Pamela R. D. Williams, Amanda D. Phelka, and Dennis J. Paustenbach, “A Review of Historical Exposures to Asbestos Among skilled Craftsmen (1940-2006),” \textit{Journal of Toxicology and Environmental Health, Part B}, 10, No. 5 (2007): 319–377 (Quotes at 365, 365, and 361, respectively). Also unsurprisingly, ChemRisk was not left out of this funding opportunity for long. In 2012 they published a typical, well-argued and written article for another former joint compound manufacturer, Kaiser Gypsum. Using mortality data, they argued that the historical deaths from joint compounds have historically been so minimal prior studies must have been wrong. Although the reference list is long, they did not cite to their prior article that cautioned about wallboard workers. Amanda D. Phelka and Brent L. Finley, “Potential Health Hazards Associated with Exposures to Asbestos-Containing Drywall Accessory Products: A State-of-the-science Assessment,” \textit{Critical Reviews of Toxicology} 42, No. 1 (2012): 1-27.}

In later court pleadings, Georgia-Pacific characterized the research that followed as “properly commissioned studies to explore scientific issues that repeatedly arise in joint compound litigation, disclosed its role in the studies themselves, and submitted them to the technical rigors of scientific peer review by qualified scientists who were neither affiliated with nor selected by Georgia-Pacific.” The research conducted by eighteen scientists cost the company at least $6 million. $3.3 and $1.5 million went to Exponent and Environ, respectively. Company funded researchers undertook three distinct research projects:

1. Bernstein exposed laboratory rats to the reformulated products, using non-standard carcinogen protocols requiring only very short-term inhalation;

2. Exponent researchers sought to recreate the 1970s asbestos Ready-Mix and a dry joint compound; and
3. Environ conducted to tests to estimate worker exposure to asbestos dust when the products were sanded.87

Bernstein’s work was the most critical to Georgia-Pacific’s defense, and, due to its unusual protocol and short exposure period, also the most suspect. In 2007, Exponent researchers applied a recreated joint compound to a small section of wallboard, allowed it to dry, and then sanded it while collecting the dust. They then shipped the dust to Bernstein’s laboratory near Geneva, where he supervised a series of extremely short-term rat experiments. In the pilot study, Bernstein used three groups of fourteen rats, confined in breathing tubes for six hours per day, over a period of five days. The first group breathed filtered air, the second, chrysotile fibers selected by the researchers, and the third a mixture of chrysotile fibers and aerosolized joint compound particles. Upon killing the rats after this period, Bernstein found that the chrysotile rats’ lungs appeared virtually identical to the filtered air lungs. To Bernstein, this meant there is no risk of lung cancer from chrysotile. The later experiments built upon this finding.88

At his deposition, Holm acknowledged that the preferred method for testing fibers for carcinogenicity in humans is a two-year animal study. Thus, the five-day inhalation test did not conform to international standards for determining if a substance causes cancer. Instead, they relied upon another European biopersistence protocol, which suggested that fibers not lasting ten days in the lungs would not be carcinogenic. Thus, since the chrysotile did not appear to last ten days, they believed the test was predictive of whether chrysotile would cause disease. The group reached this conclusion even though the international body that developed the carcinogenic protocols had previously concluded that all forms of asbestos cause cancer. In addition, rodents had a much more likely chance to clear the relatively low level of fibers to which they were exposed for a short period.

87 Jim Morris, “Facing Lawsuits.”
88 Jim Morris, “Facing Lawsuits.”
than would humans, since they have the ability to clear asbestos fibers from their lungs ten times faster than humans. In the end, Holm admitted to his questioner that the international cancer organization likely would not believe the lack of fibers after only five days exposure would be very meaningful. Furthermore, the Georgia-Pacific’s researchers had not conducted the retest that the international body called the “major criterion for hazard.”

As it turns out, Bernstein had reason to doubt the accuracy of his test even before he conducted it. During the early to mid 1990s Bernstein had coauthored a series of articles describing a study of rats and hamsters exposure to manmade fibers—a study that used chrysotile as a positive control. The group found that chrysotile induced fibrosis in rats in as little as three months, with increasing fibrosis occurring over the two year study period. In general, the group found the chrysotile-induced pneumoconiosis to be at the high end (severe to complete obstruction) of one widely used scale of fibrosis measurement (Wagner).

As noted above, the dust used by Bernstein was obtained through sanding a recreated Georgia-Pacific ReadyMix mixture. In addition to preparing and sending them to Bernstein,

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89 Jim Morris, “Facing Lawsuits”; Jim Morris, “Exporting an Epidemic,” The Center for Public Integrity, http://www.publicintegrity.org/2010/07/21/3401/exporting-epidemic, Accessed October 4, 2015; Deposition of Stewart E. Holm, June 27, 2011, 492-6, 587, (in author’s possession); and Deposition of Stewart E. Holm, June 28, 2011, 640-647, 668-672 (in author’s possession). Interestingly, an international expert panel that included Bernstein as a member endorsed the two-year test protocol in the mid-1990s. Notwithstanding his participation in that panel and his prior work demonstrating chrysotile fibrosis, Bernstein subsequently built upon his growing experience of exonerating chrysotile. Following his project for Georgia-Pacific, the International Chrysotile Association—an asbestos producer trade group—paid Bernstein $200,000.00 to write an article revisiting the issue of chrysotile health risks; an article in which he exonerated most chrysotile exposures, indicating that “low exposures ... do not present a detectable risk to health.” Once again, he ignored his earlier—and more complete—study. Jim Morris, “Facing Lawsuits.”


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Exponent scientists also analyzed airborne samples of the sanded recreated product. Using what they described as more modern techniques than previous analyses, they found extraordinarily fewer fibers than the studies conducted decades ago. One of the problems they noted had to do with overloading of air monitoring filters. Counter-intuitively, they argued that overloaded filters meant that the prior researchers found more, not fewer, fibers than were present. They did not explain what this would mean for pipe insulation air monitoring levels—which very frequently resulted in overloaded fibers—nor what it meant for the resulting regulations which relied in large degree upon the historic air monitoring of pipe insulation for its permitted exposure levels.\footnote{Jim Morris, “Facing Lawsuits.”}

While Exponent and Bernstein concentrated on actual working conditions, Environ took a more theoretical approach. They developed and validated models predicting breathing-zone concentrations of dust. Unsurprisingly, their work also found considerably less dust in the breathing zone. They did not explain why the historic air monitoring, conducted using filters on the workers’ lapels, failed to provide a good prediction of breathing zone concentrations.\footnote{Jim Morris, “Facing Lawsuits.”}

The research resulted in at least thirteen articles appearing in mostly industry friendly journals. Publication of the articles was not without some difficulties. For example, \textit{Toxicological Sciences} twice rejected one of Bernstein’s papers. At least one reviewer complained it contained little new information.\footnote{Deposition of Stewart E. Holm, June 6, 2011, 235-9, 242.}

In another tactic closely reminiscent of tobacco attorney schemes, Georgia-Pacific paid its consultants to spread the word of their findings in conferences around the globe. To ensure the “revealed word” was accurate, Georgia-Pacific attorneys and attorney agents often met or had conference calls with each expert prior to their presentations. Bernstein played a leading role in this public relations and litigation assistance effort. Since 1999 to the time of this writing he has given at
talks in at least 19 countries, often at the behest of Chrysotile Trade Associations. On March 10, 2010 he reported on preliminary results of his Georgia-Pacific study at the Society of toxicology’s annual meeting in Salt Lake City. Industry paid for all of his travel.94

The handholding by company attorneys prior to conferences merely continued Georgia-Pacific’s normal legal practices. During the entire litigation-driven process, Childs and his team of attorneys maintained tight control over the research. Childs approved each project and study. In his deposition, Holm testified that he believed Childs had independent meetings with the experts. One of Child’s attorneys, Mary McLemore, was part of the team designated to review the draft articles prepared for submission to journals. For the two articles that did not include him as an author, Holm and McLemore participated in lengthy “WebEx conferences” discussing the articles and suggesting revisions. Holm also understood that McLemore had other phone calls and meetings with experts while he was not present.95

In keeping with Childs’ desire for secrecy, he did not want the litigation driven nature of the research, nor the significant attorney involvement, to be disclosed. This desire created problems in publishing the articles, since in the new millennium most scientific journals now require disclose of conflicts of interest and bias. In at least three articles written by Bernstein, Holm, and others, they lied about funding, indicating that Georgia-Pacific had simply provided a grant for the article. In fact, at the time Bernstein conducted the research he was being paid an hourly rate as an expert litigation consultant, subject to review by both Holm and Georgia-Pacific attorneys. The disclosures also failed to note that Bernstein worked as a litigation expert for Georgia-Pacific, having testified at least once in 2007. Holm also understood that during Bernstein’s research Georgia-Pacific attorneys

were providing management for the project—Holm never provided instructions—yet, this not acknowledged. Even after admitting that the article’s disclosure statement did not comply with the journal’s requirements, he stated during his deposition that he did not have any plans to inform the journal about the inaccuracies. Upon reflection, Holm—possibly at the behest of the attorneys—decided to inform the journal, which published a correction in its January 2012 issue. Even then, the correction did not fully disclose the involvement of Georgia-Pacific in the research.96

In another activity strikingly similar to those employed by tobacco attorneys, Georgia-Pacific also worked with yet other experts to bolster the credibility of their expert’s research articles. In one example, Ken Donaldson wrote a letter to the editor criticizing an earlier letter that had critiqued Bernstein’s methods in determining chrysotile had very limited action on lung tissue. In the 2010 letter, Donaldson took the high ground, claiming he had no ties to asbestos: "While not allied to any asbestos manufacturing company nor any pro-asbestos pressure group nor being in receipt of funds from any such source, we feel it is beholden on us to clarify the situation regarding the use of biopersistence data generally and the Bernstein data specifically," He took this position even though he had been receiving money from Georgia-Pacific since 2006 for asbestos consultation. Holm may even have spoken to Donaldson before making his initial proposal presentation to Georgia-Pacific management.

It is especially hard to believe that this prevarication was a slip of the pen concerning old ties, since as recently as September 2010 Donaldson attended a meeting with Holm, Childs, and

British asbestos luminary Professor Julian Peto. Donaldson was also one of the author’s of two of the litigation-driven asbestos articles, including one he coauthored with Bernstein in 2011. Nor was Donaldson the only one to participate in the lie. Prior to submitting the letter, Donaldson sent it to Bernstein and Holm for their review. Thus, both Bernstein and Holm saw the incorrect statement and likely made no comment about it, probably because it served Georgia-Pacific’s purposes very well.97

Donaldson apparently had no ethical qualms about his actions or the original disclosure on the articles. In a August 2013 email to a public health advocate, Donaldson argued that the disclosure and Holm’s inclusion as author made it clear in the original publication that the research was not conducted free of the company’s influence: “I think a reasonable person would have known the work was carried out in collaboration with Georgia-Pacific rather than as a grant-funded investigation.” He made similar protestations in response to questions from Hazards Magazine editor Rory O’Neill, a professor in the Occupational and Environmental Health Research Group of the University of Stirling, a Scottish university. Donaldson answered only a few of the written questions, not responding to inquires about the court’s ruling or Georgia-Pacific’s rule in the research. While not answering many of the questions, he indicated that he had nothing to hide and welcomed a review of the articles, believing the science “robust.” In an admission that demonstrated the intentional misleading nature of his letter in support of Bernstein, he stated that he was not paid for his contribution to the articles, but admitted to writing and being paid for prior reports to Georgia-Pacific on the hazards of asbestos.98


Childs’ plan fell apart due to the persistence of a plaintiff lawyer with the New York and New Jersey law firm, Weitz & Luxenberg—Jerry Kristal. Seeing Holm as coauthor on one of the articles, he subpoenaed for a deposition. At the deposition in June 2011, Holm admitted the role Childs and other attorneys in the research. He further disclosed some of the problems with the research and the protocols. Holm admitted that he had discussed with the attorneys the substantial quantities of dust prior studies had demonstrated. In an admission that cast further suspicion upon Georgia-Pacific’s research, Holm also admitted that during the research they had applied and tested some historic Georgia-Pacific asbestos products they found, but did not publish the results. In an answer that implied the tests had demonstrated significant fiber release, he explained that they were concerned about degradation of the old product. However, he admitted that they had not tested the products to determine if they had degraded.99

Throughout the deposition, Georgia-Pacific’s outside counsel repeatedly instructed Holm not to answer questions concerning his communications with attorneys, even when they involved the manner in which the experiments were conducted or considerations they made for alternative methodologies or procedures. Similarly, he instructed Holm not to answer questions with other consulting experts concerning discussions about prior studies on Georgia-Pacific products and the problems they might cause in litigation. He was also not instructed to discuss decisions he made about conducting one type of research versus another.100

At the initial June 6 deposition, Kristal requested that Georgia-Pacific produce all documents relating to the research. In response, company attorneys turned over some documents along with a privilege log asserting attorney work product privilege for numerous documents, including all communications with the experts. In accordance with the general asbestos litigation management

100 Lawyer instructions are contained throughout all three volumes. Typical examples can be found at Deposition of Stewart E. Holm, June 27, 2011, 307, 341, 389-390, 413, 492-3, and 609.
order in place in New York City, the matter went before Laraine Pacheco, the Special Master assigned to handle discovery disputes. On June 15 Pacheco recommended that the trial judge, Sherry Klein Heitler, conduct an *in camera* review of the items listed in Georgia-Pacific’s privilege log. Shortly thereafter, Georgia-Pacific filed a motion to vacate the recommendation. Almost six months later, the judge denied the motion, holding that “Georgia-Pacific cannot use its experts’ conclusions as a sword while at the same time attempting to shield the public from information which affects the veracity of its experts’ conclusions.” Noting that Mary McLemore, an in-house counsel at Georgia-Pacific, had reviewed and provided input on some and possibly all of the articles, the judge further expressed her concern that Georgia-Pacific’s attorneys were involved in discussions concerning the content of these purportedly objective scientific studies by Georgia-Pacific’s consulting experts. Georgia-Pacific refused to go gently into the night. They appealed the ruling to the New York appellate court. In a November 8, 2012 brief before the appeals court they argued that Kristal’s fraud claims were “baseless,” accusing him of voyaging on a “boundless fishing expedition.”

Like Judge Becker in the Chromium saga, the appellate court was not amused. In a unanimous decision they denied Georgia-Pacific’s appeal, holding that the motion court properly exercised its discretion in supervising discovery when it granted *in camera* review of the documents to determine whether a crime-fraud exception to the attorney-client privilege applied. In so holding they reviewed the evidence, providing a travel guide to Georgia-Pacific’s blunder-filled territory. Initially,

> [t]o facilitate the endeavor, GP entered into a special employment relationship with Stewart Holm, its Director of Toxicology and Chemical Management, to perform expert consulting services under the auspices of its in-house counsel, who also was significantly involved in the pre-publication review process.

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101 Jim Morris, “Facing Lawsuits.”

They then continued, setting forth the involvement of Georgia-Pacific attorneys and the utter inadequacy of the article disclosure statements. Although long, the judges’ succinct description of Georgia-Pacific attorneys’ actions warrants quoting.

Holm co-authorized nearly all of the studies, which were intended to cast doubt on the capability of chrysotile asbestos to cause cancer. On the two articles that he did not co-author, he and GP’s counsel participated in lengthy “WebEx conferences” in which they discussed the manuscripts and suggested revisions. Despite this extensive participation, none of the articles disclosed that GP’s in-house counsel had reviewed the manuscripts before they were submitted for publication. Two articles falsely stated that “[GP] did not participate in the design of the study, analysis of the data, or preparation of the manuscript.” For articles lead-authored by David M. Bernstein, Ph.D., and co-authored by Holm, the only disclosure was that the research was “sponsored” or “supported” by a grant from GP. The articles did not disclose that Holm was specially employed by GP for the asbestos litigation or that he reported to GP’s in-house counsel. Furthermore, there were no grant proposals, and Dr. Bernstein was hired by GP on an hourly basis. Nor did the articles reveal that Dr. Bernstein has been disclosed as a GP expert witness in NYCAL since 2009, that he had testified as a defense expert for Union Carbide Corporation in asbestos litigation, or that he had been paid by, and spoken on behalf of, the Chrysotile Institute, the lobbying arm of the Quebec chrysotile mining industry. Although GP belatedly endeavored to address the inadequacies of certain of its disclosures, its corrections failed to acknowledge its in-house counsel’s participation and did not make clear that Dr. Bernstein’s testimony as an expert witness preceded the publication of the first GP reformulated joint compound article in 2008.103

Following this blunt elucidation of the facts, the court found they constituted “a sufficient factual basis for “a finding that the relevant communications could have been in furtherance of a fraud.” The court further admonished Georgia-Pacific, noting “it is of concern that GP’s in-house counsel would be so intimately involved in supposedly objective scientific studies, especially in light of GP’s disclosures denying such participation.” In a not so subtle comparison to tobacco, the court footnoted this comment with citation to a prominent tobacco fraud case. The court then concluded by observing that it was not fair for a party to use the attorney privileges both offensively and defensively. Although such blatant distortion of the peer review and production of scientific

103 In re: New York City Asbestos Litigation, 6-7.
knowledge process is rarely seen, its use by tobacco and Chromium attorneys, as well as Georgia-
Pacific attorneys, suggest it is more commonplace than we realize, with attorney secrecy privileges
hiding the effects.\textsuperscript{104}

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Understandably, the appellate court’s ruling caused a flurry of indignation about Georgia-
Pacific’s actions, even among some defense counsel. Beginning in 2013, noted defense tort litigation
specialist and ethicist Nathan Schachtman wrote two articles about the studies in his blog. He
entitled the first, “A Cautionary Tale on How not to Sponsor a Scientific Study for Litigation.” In
the blog, he implicitly agreed with the court’s ruling, while taking exception to their consideration of
the privilege’s relevance. He argued that in this case the application of attorney privileges were very
suspect, seemingly agreeing with the special master that documents related to communications that
the party intends to release to the public are not privileged. In his conclusions, Schachtman (a well-
respected defense counsel) warned readers that defense counsel are not alone in this ethical failing;
the plaintiff’s bar has similar interest in its research agenda. He then placed most other public health
advocates—“powerful interest proxies, such as support groups, labor unions, so-called public-
interest groups, and captured governmental agencies”—in the same category as plaintiff counsel
Finally, he also rightly noted that the need to access data is not limited to plaintiffs.\textsuperscript{105}

Schachtman’s second blog closely followed the Center for Public Integrity’s piece criticizing
Georgia-Pacific, calling the piece an “editorial.” He again began his comments by voicing his
concern about the study: “GP’s position was a serious mistake, and it has opened itself up to a good
deal of justified criticism for “secretive research.” However, this time, he had a more important

\textsuperscript{104} In re: New York City Asbestos Litigation, 8-11.
\textsuperscript{105} Nathan Schachtman, “A Cautionary Tale on How Not to Sponsor a Scientific Study for Litigation,” June 21, 2013,
concern; the studies cast “a shadow over industry sponsorship generally.” He argued that it unfortunately allowed the Center to implicitly cast doubt on all industry research.

He also urged readers to not simply assume Georgia-Pacific committed fraud. Most of the article took issue with the Center’s and other public advocate’s writings, accusing them of being two-faced and using smear tactics. He disingenuously noted that the court had not hold that Georgia-Pacific engaged in a fraudulent scheme, only that plaintiff’s allegations were serious enough to warrant review. In actuality, the court was a bit more explicit than suggested by Schachtman; holding that the facts provided “a sufficient factual basis for a finding that the relevant communications could have been in furtherance of a fraud.”

Still, Schachtman’s call for release of all data for any study used in litigation at first appears like a good step. However, industry funds several orders of magnitude more litigation-driven studies than plaintiff counsel. Almost all other studies, while they may be used in litigation, are neither designed nor specifically intended for use at trial. Many are by individuals and organizations truly concerned about public health. Yet this did not stop Schachtman from railing against plaintiff counsels’ ability to control the scientific literature.

The CPI quotes plaintiffs’ lawyer Alan Golanski as alleging that GP had tried to “seed” the medical literature with “methodologically skewed, litigation-driven research.” Of course, this is exactly what plaintiffs’ expert witnesses have done over the last half century. It really is time to stop. COI disclosures cannot be a full, satisfactory answer because the most potent conflicts arise out of intellectual and political commitments, not money. Increasing transparency and access to study protocols, data, analyses may perhaps help.\(^\text{106}\)

Since plaintiff counsel fund very few studies, what Schachtman was actually called for was the wholesale collection and release of any study data desired by a defendant in trial, likely causing severe hardship to many academic and government researchers who have no connection to the

litigation. This request for a data dump of all research is a common tactic of defense firms. Whenever a study demonstrates a hazard to a product, companies and their attorneys quickly seek to obtain the data so they can conduct a reanalysis that always demonstrates flaws in the process. Since no study is perfect, when they dig hard enough and are paid enough, experts hired to reanalyze the data can always suggest further research is needed before the scientific community accepts the original study’s conclusions. In the pursuit of this manufactured evidence, defendants are in much better financial positions than plaintiff counsel to pick apart studies counter to their position.

Yet, what of Schachtman’s admonition to withhold judgment on Georgia-Pacific until the documents are reviewed and all the facts come out? Unfortunately, that will not happen in the New York cases. In a confidential agreement, Georgia-Pacific settled all of Wietz & Luxenberg’s cases before the judge could review the documents. They also appear to have an ongoing deal to settle new cases with exposure to Georgia-Pacific asbestos products. Thus, once again—although at a substantial settlement cost—the true nature of industry manufactured science could remain hidden behind attorney privileges. Although the magnitude of attorney created science may have peaked during the heyday of attorney generated tobacco studies, it has not disappeared. It remains a significant part of the scientific literature for toxic substances.
CHAPTER 10

CONCLUSION

By the commencement of the twenty-first century, a growing number of post-modernist books and articles addressed the abundant outside factors that influence science—some even questioning whether science can be trusted. Other recent books and articles have studied the interaction of science, the law, and politics. In addition, a profusion of books now catalogue how industrial concerns often choose profits over safety or health. However, the pervasive influence of attorneys in the manufacture and concealment of scientific knowledge throughout the twentieth century—although considered in a few specific events—remains elusive in histories. In the words of some litigation science specialists use with regard to research involving any toxic substance, it is a “data gap.”

In addition, attorney management and manipulation of research, along with “captured” journals, undermines the practice of scientific peer review. As seen in these case studies—and numerous other written examples—if the right question is asked and the data are properly manipulated, an article can almost always reach an attorney’s desired conclusion. Peer reviewers are not equipped to go behind the article’s narrative and check on the data—or data that are omitted from a research article.¹

This is not a trivial issue. The public and medical profession’s understanding of the causes of cancer—as well as other occupational and environmental diseases—frequently has been delayed by failures to release scientific studies or by distortion of study results. The vital role of attorneys in these events has, to date, received only limited attention from historians. Although little noticed by

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¹ See discussion in the introduction and historiography; and see for example Jock McCulloch, “Saving the Asbestos Industry”; David Michaels and Celeste Monforton, “How Litigation Shapes”; and Stanton A. Glantz, et al., The Cigarette Papers.
professional historians—even as they increasingly recognized varieties of external influence on science—attorneys have exercised a continuing influence on medical and especially occupation disease science throughout the twentieth century.

In this dissertation I have sought to lift some of the legal veils from attorney involvement in medical and biomedical research that occurred throughout the twentieth and early twenty-first centuries. I revealed how attorneys’ techniques were developed and utilized to manufacture science with specific desired results. Litigation and regulatory generated science was never designed to find the truth, but rather to obtain evidence for trial. From the first workmen’s compensation laws of the early twentieth century through the latest medical breakthroughs, attorneys have been involved in the production of scientific knowledge about toxic substances as a means to obtain evidence for litigation and regulatory practice.

At the start of this dissertation, I took a step back to provide an overview of the malleability of medical research. This examination quickly revealed how science can be slanted throughout the research process, from determining the initial question to ask, influencing data collection, to finally shaping how the data are presented. In addition to these dangers, attorney involvement and management of the investigations added yet another layer of potential distortion, by providing an ethical—at least in the legal realm—means of hiding any research which did not support the attorney’s position.

The history of how these activities expanded and became more pervasive reflects a continuing, indeed growing, intersection of science and litigation. Initial efforts to find loopholes in compensation laws have progressed to today’s systematic creation of whatever science is necessary to present seeming credible evidence to the jury. In the twenty-first century, sophisticated litigation-support firms are at the beck and call of attorneys, often bragging that, through “scientific” research, they can find the evidence desired by their client. Investigating this intersection is made exceedingly
difficult by not only the sheer volume of the historical record, but more importantly, by the veil of secrecy that courts have endorsed for granted attorneys and their agents.

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In the introduction, I asked four questions concerning the intersection of the law and science:

1) For what purposes have attorneys historically used science?
2) What methods have attorneys used historically to create new scientific knowledge?
3) What, if any, methods have they used to hide scientific knowledge?
4) How has it affected scientific knowledge?

As I demonstrated in the case studies, attorneys have used science repeatedly for the purpose of furthering their positions in litigation, in workmen’s compensation hearings, and before regulatory bodies throughout the twentieth and early twenty-first centuries. In this respect, given their legitimate professional objectives, they have invariably selected and utilized only scientific evidence that supports their position.

However, sufficient scientific evidence has not always been available to ensure a successful outcome to their endeavors. Thus, attorneys have often resorted to contracting with experts for research or re-analysis of prior studies. In this effort, they have not been interested in the scientific truth, but rather in gathering colorably non-perjured evidence that will buttress their case. Here they have employed all of the means discussed in Chapter 3—managing the agenda, controlling or neutralizing the experts, and manufacturing science as necessary. As shown through the case studies they have used whatever techniques are legally ethical—and in certain cases they likely went well beyond the ethical boundaries. When a study or research project does not provide the necessary evidence, the facts can be massaged or hidden: both relatively easy processes, given attorney-client and work product privileges. The research and arguments used by both sides of the asbestos/lung
cancer debate have used numerous created epistemic factors—for example case reports, epidemiological studies, interpretations of data, and criticisms and responses. As we have seen, however, there are questions about whether industry attorneys—or, in some cases, plaintiff counsels—actually believed some of their own arguments, or simply used them to advance a litigation or regulatory agenda.

Each of the case studies provides a glimpse into the growing sophistication of attorneys in the manipulation of scientific knowledge. During the early to mid twentieth century, industry and its attorneys—often with the assistance of subservient organizations such as the Industrial Hygiene Foundation or the Tobacco Institute—sought to move the issues of silica and asbestos disease out of the public eye into more controlled professional settings. With silica, a substance rooted in a discrete number of work places, they succeeded. Perhaps using the silica defense as a model, asbestos industrial concerns and their attorneys placed great emphasis on profits, public relations, minimal regulations, and viable litigation defenses when considering their actions and public statements concerning health issues. As revealed most pointedly in the early asbestos studies and the recent history of Chromium VI litigation and regulation, when profit is involved, there are few limits to efforts designed to create the appropriate evidence for enhancing a litigation position. In these efforts, lawyers usually play a central role.

Unlike silica, however, asbestos—with its ubiquitous presence in industry, construction, and even white-collar jobs and homes—has proved more troublesome. This disease became a poster child of industry malfeasance. Today, even companies with less friable products—many of whom were originally outside the glare of lawsuits—are subject to a growing number of lawsuits for mesothelioma, a deadly cancer requiring only minimal exposure to asbestos. In response, their attorneys have turned to litigation support firms, who, for the right price, almost guarantee successful research or reanalysis of prior study results.
As the twentieth century progressed—and more diseases came to light—defense attorneys turned to increasingly complex methods of developing their evidence. In the early twentieth century, silica attorneys initially simply used available experts to change definitions and disease diagnoses requirements, with little control over research. In the 1930s and 1940s, Johns Manville’s General Counsel Vandiver Brown may have been the first to initiate the practice of attorneys assuming a more active role in medical research. This new role reached its zenith in tobacco 1950s to 1980s tobacco litigation, with CEOs at times taking orders from attorneys. As described by Glantz and his coauthors in *The Cigarette Papers*, tobacco documents provided the first clear picture of the extent to which attorneys had insinuated themselves into research management during the second half of the twentieth century. Tobacco attorneys became involved in every aspect of tobacco health research, using all the methods by which research can be distorted—from selecting promising projects to managing data output—ensuring bad data never became public. They learned that by manufacturing doubt, lawsuits could be readily defended and regulations forestalled.

Tobacco attorneys created an entire system dedicated to the creation of scientific doubt. In the process, they instructed a new generation of lawyers, presenting training programs and clinics on how to obtain evidence for trial, whether or not it represented good science. Although attorneys and other legal professionals now generally decry these actions, many of the highly skilled attorneys and prominent law firms that trained under tobacco attorney tutelage have now progressed to other toxic substance clients, using many of the same techniques they learned in tobacco litigation and regulatory fights. Chromium, benzene, and recent asbestos research are replete with examples of attorney activities remarkably similar to that displayed during the height of the time when attorneys shaped tobacco research.

Just as the roles and methods evolved over the years, so have the types and methods of researchers aligned with industry. Until the late 1960s to 1980s, research normally involved academic
scientists, who occasionally reported results inconvenient for their clients. Since the 1990s, litigation science firms—employing their own researchers—have become much more prominent. While academic scientists are still sometimes hired, they often either have their own separate consulting firm or are being employed under the auspicious of a consulting firm specializing in litigation. As participating scientists have changed, so have publically available reports, with virtually all coinciding neatly with the clients’ best interests. In the regulatory arena, industry scientists not only conduct independent research, but also dissect any studies warning of dangers almost before they have been published. Practically every study that takes a cautionary stance about an industry product is quickly followed by an industry study that creates doubt about the certainty of the science.

Recently, courts and a few commentators have questioned the reliability of evidence obtained from research conducted in support of litigation. In the most famous case of “junk science,” Ninth Circuit Judge Alex Kozinski strongly questioned the credibility of litigation-generated research in the 1995 remand of the *Daubert, et al. v. Merrell Dow Pharmaceuticals, Inc.* case.²

Plaintiffs used three types of expert opinions in an effort to establish that Bendectin caused the birth defects: the first group reanalyzed previously published studies to establish a link between Bendectin and limb birth defects, the second testified concerning similar defects in animal studies, while the third noted the similarity of Bendectin's chemical structure to other drugs suspected of causing birth defects. As was normal practice at the time in plaintiff cases, these experts reanalyzed prior studies and provided opinions solely for the lawsuit, without attempting to publish their opinions.³

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³ Although this case provides an example of plaintiff attorneys managing expert research—and is often cited by industry oriented attorneys and experts—it does not rise to the same level of medical research concern as the examples documented in this Dissertation. As noted by the federal appellate court, the experts in this case reanalyzed prior research, but did not attempt to publish their results. Thus, their “research” neither entered, nor affected the medical literature. *Daubert, v. Merrell Dow Pharmaceuticals, Inc.*, 951 F. 2d. 1128, 1131 (1991).
The appellate court affirmed the trial court’s finding based upon the finding that the “opinions proffered by plaintiff’s experts run counter to the substantial consensus in the scientific community…” Furthermore, the court held that expert testimony “based directly on legitimate, preexisting research unrelated to the litigation provides the most persuasive basis for concluding that the opinions he expresses were ‘derived by the scientific method.’” The Supreme Court signaled approval of this approach in its 2008 decision on the Exxon Valdez case. In a footnote to that opinion, the court questioned the reliability of litigation-generated science. In that case, the court “declined to rely on” litigation-generated research “funded in part by Exxon.”

Some public health commentators have questioned the effectiveness of such a strict, legalistic approach to litigation generated expert opinions. They contend that the real problem is one of financial incentives. These same incentives apply to a broad range of scenarios. Two such commentators—at least one of whom has previously testified for plaintiff counsel—contend that “there seem no strong reasons to treat this conflict of interest differently from other relevant conflicts of interest.”

Yet this is only part of the issue. Perhaps even more telling, as described repeatedly in the cases of silica and asbestos, is the great danger of research being hidden when attorneys are involved. When attorneys become involved in scientific research, the question is no longer a search for scientific evidence, but rather a search for evidence that supports the attorney’s position. Even if the contracted researchers maintain the highest standards of ethics with regard to their activities, this does not guarantee that the correct facts become public. Through the privilege of attorney work

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product, research that does not buttress the position desired by the attorneys can simply languish in some litigation file, and never become part of the scientific commons. It is secret science: in a sense, very similar to military science conducted behind national security barriers. Yet in another sense it is far worse, for not only is medical science hidden, but also a distortion of that science is then presented to the scientific community.\(^8\)

Initially, these agreements to conduct science in secret seem at odds with modern scientific culture. Although the modern public may not view science as being on quite as high a pedestal in the twenty-first century as it was in the early twentieth century, prominent historian of science Steven Shapin has suggested modern scientists share some important moral traits with scientists in the seventeenth century. In his early twenty-first century book, *The Scientific Life: A Moral History of a Late Modern Vocation*, Shapin traces scientific culture from the early scientific age until today. He suggests that through the nineteenth century the public assumed a moral superiority with scientists, based on science’s association with the divine. By the early twentieth century, however, a more secular complex of morals had replaced the divine. Today it is often difficult for outsiders to take seriously any professed intention of only “doing good.” Yet as industrial science and then entrepreneurial science came to dominate many areas of research, the moral vocabulary still remained, with virtue and trust providing continuity back to Newton and Boyle. In Shapin’s opinion, the same personal trust and credibility of knowledge so important in the seventeenth century still provide a link between science and industry (or investors). Industry and investors want research that will result in quick profits—and strong benefits to the bottom line of their financial statements. To obtain these goals, they must employ individuals they trust to provide credible knowledge. This trust is often gained through personal relationships.\(^9\)

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\(^8\) Michael Aaron Dennis, “Secrecy and Science Revisited.”

In one sense, scientists’ willingness to hide data or provide distorted images of the data to the public seem at odds with Shapin’s conclusion. Yet in a larger sense, this activity slides easily into the paradigm. Industry is interested in the bottom line. This includes not only profits, but also limiting obligations and liabilities. Litigation-focused scientific research companies have proven both loyal and trustworthy, providing the “credible knowledge” necessary to limit liabilities and costly regulations. Through their commitment to provide only industry friendly articles, litigation-focused scientists provide the very virtues to attorneys that Shapin believes still inculcates modern scientific culture as a whole.  

However, while litigation-focused researchers warrant the trust and personal relationships they have earned from industry, the net effect of their research activities has been the distortion or delay in knowledge concerning occupational diseases. While the facts often surface in the end, massive human suffering has, at times, resulted from the delays inherent in “manufactured” science. Examples of this proliferate throughout the history of toxic substance disease research. Even world-renowned epidemiologist—and occasional defense expert—Richard Peto has commented about this problem in the scientific literature.

Even the scientific literature is not immune from distortion by financial interests. The decades-long argument that "threshold" dose levels of carcinogens must exist below which the general population is absolutely safe has not been entirely motivated by the scientific plausibility of the hypothesis. With increasing understanding of the derivation of tumours from single cells acted on by mutagenic carcinogens, industry is slowly abandoning "threshold" arguments in favour of arguments (where the biological fallacies are somewhat better concealed by the mathematics) that thousandfold reductions in dose can conveniently be "statistically guaranteed" to produce a million-fold or some other enormous reduction in risk. Even if the scientists who propound such models are disinterested, industrial endorsement of them is not. Much excellent toxicology may be done by industry, and many industrial scientists and managers may be directly and honestly concerned with the prevention of hazards. But so many examples of financially motivated bias exist that the

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10 Of course, scientists hired by plaintiff personal injury attorneys also provide a similar benefit to the bottom line of the lawyer and the client.
motives and work of industrial scientists and consultants are inevitably distrusted. 11

Interestingly, Peto wrote this article to review a book by a politically active environmentalist, Samuel Epstein, and to chastise the environmentalist movement for inflaming political passions against unproven environmental carcinogens, not to bludgeon industry. In his lengthy editorial-like review he informed readers that scientists on both sides can be biased and have careers at stake. There is truth in Peto’s chastisement. Some environmentalists will condemn any substance, no matter how low the risk or how weak the evidence. However, most government and academic researchers are not rabid environmentalists—contrary to Schachtman’s opinion—and do not have the same incentives as industry paid researchers. Many, such as Beaumont and Goldstein, have published articles that discount certain risks and consider both sides of the issue. In contrast, industry attorneys demand commitment from their experts. Scientific opinions must be rock solid and unwavering to stand against cross-examination. Peto implicitly understood this. Although he criticized environmentalists as at times exaggerating the problems, this respected British scientist—who frequently worked with industry—fully acknowledged the problem of industrial intransigence:

My criticisms of Epstein’s [an environmentalist oriented scientist] science, however, must be viewed in the light of the continued resistance of many industries to reasonable controls. One has only to read some of his descriptions of industrial behaviour to see where his passion comes from, and a suitably skeptical reader could derive much important information from the dozen or so detailed case-histories of particular carcinogens that make up the bulk of this book. 12

This dissertation has likely challenged many readers’ understanding of medical research in the twentieth century. Before commencing research for this dissertation, even I, an attorney who participated in some of the activities described in these papers, did not understand the full extent to

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12 Peto, “Distorting the Epidemiology,” 300.

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which attorneys have influenced toxic substance medical research. Given the privilege of secrecy that attaches to attorneys’ and their agents activities, it is not surprising that few historical accounts have even considered the issue. Yet, historic industrial and public health medicine accounts are tragically incomplete without an understanding of the pervasive attorney influences on both private and governmental medical researchers in the twentieth century. We can only wonder and fear what other knowledge remains hidden in the files of law firms. The integrity of public health science may well continue to erode unless we find some way to take both politics and the profit motive out of the health sciences that are designed to determine the human safety or hazardousness of products.
### APPENDIX A

#### ACRONYMS

<table>
<thead>
<tr>
<th>Acronym</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>AAIP&amp;S</td>
<td>American Association of Industrial Physicians &amp; Surgeons</td>
</tr>
<tr>
<td>ACC</td>
<td>American Chemical Council</td>
</tr>
<tr>
<td>ACGIH</td>
<td>American conference of Governmental Industrial Hygienists</td>
</tr>
<tr>
<td>ACS</td>
<td>American Cancer Society</td>
</tr>
<tr>
<td>AIA</td>
<td>Asbestos Information Association (Asbestos industry NGO)</td>
</tr>
<tr>
<td>AIHA</td>
<td>American Industrial Hygiene Association</td>
</tr>
<tr>
<td>ALA</td>
<td>American Lung Association</td>
</tr>
<tr>
<td>ALJ</td>
<td>Administrative Law Judge</td>
</tr>
<tr>
<td>ALL</td>
<td>Acute lymphocytic leukemia</td>
</tr>
<tr>
<td>AML</td>
<td>Acute myelogenous leukemia</td>
</tr>
<tr>
<td>ANLL</td>
<td>Acute non-Lymphocytic leukemia</td>
</tr>
<tr>
<td>APHA</td>
<td>American Public Health Association</td>
</tr>
<tr>
<td>API</td>
<td>American Petroleum Institute</td>
</tr>
<tr>
<td>AA</td>
<td>Aplastic anemia (insufficient new blood cells)</td>
</tr>
<tr>
<td>ARIA</td>
<td>Association for Research of Indoor Air (Tobacco funded NGO)</td>
</tr>
<tr>
<td>ATI</td>
<td>Asbestos Textile Institute (Asbestos industry NGO)</td>
</tr>
<tr>
<td>ATSDR</td>
<td>Agency for Toxic Substances and Disease Registry (United States)</td>
</tr>
<tr>
<td>B&amp;W</td>
<td>Brown &amp; Williamson Tobacco Company</td>
</tr>
<tr>
<td>BAT</td>
<td>British American Tobacco Company</td>
</tr>
<tr>
<td>BATUS</td>
<td>British American Tobacco Company United States</td>
</tr>
<tr>
<td>BOHS</td>
<td>British Occupational Hygiene Society</td>
</tr>
<tr>
<td>BRB</td>
<td>Black Lung Benefit Review Board</td>
</tr>
</tbody>
</table>
IHD - Public Health Service’s Industrial Hygiene Division (United States)
IHF - Industrial Hygiene Foundation
ILO - International Labor Office
IOEH - Institute of Occupational and Environmental Health (QAMA funded NGO)
IOM - Institute of Medicine (Non-profit division of National academy of Sciences)
JAMA - Journal of the American Medical Association
JOEM - *Journal of Occupational and environmental Medicine*
LPC - Lymphopoietic cancer (lymphatic cancer)
MAC - Maximum acceptable concentration (for toxic substances)
MDL - Multidistrict litigation (consolidated federal cases from across the country)
MDS - Myelodysplastic syndrome (bone marrow disorder)
MIMA - Magnesia Insulation Manufacturers Association (Asbestos trade group)
MM - Multiple myeloma (Cancer of the blood)
MOA - Mode of Action (the manner in which a mutagen operates)
MSDS - Material Safety Data Sheet (product safety information required by law)
NAS - National Academy of Science (United States)
NCHS - National Center for Health Statistics (United States)
NCI - National Cancer Institute (United States)
NGO - Non-governmental Organization
NIDA - National Institute for Drug Abuse (United States)
NIEHS - National Institute of Environmental Health Services (United States)
NIH - National Institute of Health
NIOSH - National Institute of Occupational Safety and Health (United States)
<table>
<thead>
<tr>
<th>Abbreviation</th>
<th>Full Form</th>
</tr>
</thead>
<tbody>
<tr>
<td>NHL</td>
<td>Non-Hodgkin lymphoma</td>
</tr>
<tr>
<td>NSC</td>
<td>National Safety Council (industry NGO)</td>
</tr>
<tr>
<td>NTP</td>
<td>National Toxicology Program (United States)</td>
</tr>
<tr>
<td>OCAW</td>
<td>Oil, Chemical, and Atomic Workers International Union</td>
</tr>
<tr>
<td>OEHHA</td>
<td>Office of Environmental Health Hazard Assessment (California)</td>
</tr>
<tr>
<td>OMB</td>
<td>Office of Management and Budget (United States)</td>
</tr>
<tr>
<td>OSHA</td>
<td>Occupation, Safety and Health Administration (United States)</td>
</tr>
<tr>
<td>PEL</td>
<td>Permissible Exposure Limit for toxic substances (United States)</td>
</tr>
<tr>
<td>PG&amp;E</td>
<td>Pacific Gas &amp; Electric Company</td>
</tr>
<tr>
<td>PHG</td>
<td>Public Health goal (California)</td>
</tr>
<tr>
<td>PhRMA</td>
<td>Pharmaceutical Research and Manufacturers of America</td>
</tr>
<tr>
<td>PPA</td>
<td>Phenylpropanolamine</td>
</tr>
<tr>
<td>PMR</td>
<td>proportionate mortality ratio (statistical disease analysis tool)</td>
</tr>
<tr>
<td>QAMA</td>
<td>Quebec Asbestos Mining Association</td>
</tr>
<tr>
<td>RJR</td>
<td>R. J. Reynolds Tobacco Company</td>
</tr>
<tr>
<td>RR</td>
<td>Relative ratio (statistical disease analysis tool)</td>
</tr>
<tr>
<td>SAB</td>
<td>Scientific Advisory Board (EPA)</td>
</tr>
<tr>
<td>SBA</td>
<td>Safe Buildings Alliance (Asbestos industry NGO)</td>
</tr>
<tr>
<td>SMR</td>
<td>Standard mortality ratio (statistical disease analysis tool)</td>
</tr>
<tr>
<td>TASSC</td>
<td>The Advancement for Sound Science Coalition (Tobacco funded NGO)</td>
</tr>
<tr>
<td>TDI</td>
<td>Tobacco Document Center at the tobacco industry INFOTAB clearinghouse</td>
</tr>
<tr>
<td>TI</td>
<td>Tobacco Institute</td>
</tr>
<tr>
<td>TIRC</td>
<td>Tobacco Industry Research Committee</td>
</tr>
<tr>
<td>Abbreviation</td>
<td>Description</td>
</tr>
<tr>
<td>--------------</td>
<td>-------------</td>
</tr>
<tr>
<td>TLV</td>
<td>Threshold Limit Value (Upper recommended exposure limit for toxins)</td>
</tr>
<tr>
<td>TWA</td>
<td>Time weighted average (exposure level for an 8-hour work day)</td>
</tr>
<tr>
<td>TWG</td>
<td>Tobacco Working Group</td>
</tr>
<tr>
<td>VC</td>
<td>Vinyl chloride</td>
</tr>
<tr>
<td>WRA</td>
<td>Philip Morris’s Worldwide Regulatory Affairs division</td>
</tr>
<tr>
<td>WSA</td>
<td>Philip Morris’s Worldwide Scientific Affairs division</td>
</tr>
</tbody>
</table>
APPENDIX B

IMPORTANT FIGURES

George Alexeeff - OEHHA Deputy Director
Aristarchus - Hellenistic Greek scientist and astronomer
H. Thomnas Austern - Covington & Burling tobacco attorney
Domingo M. Aviado - Tobacco researcher and expert witness
Joel Barnhart - Chromium Coalition Chairman and V.P. of Elementis Chromium, Inc.
Jay Beaumont - OEHHA toxicologist
Edward Becker - Federal 3rd Circuit Court Judge
Eric Behrens - Counsel for the University of California Regents
David Bernstein - Toxicologist and industry consultant
Clara Beyer - Confidential aide and back-room advisor to Labor Secretary Perkins
Eula Bingham - OSHA Administrator
Thomas Birk - Germany scientific manager for consulting firm Environ
Susan L. Biro - Chief EPA Administrative Judge
Charles Blake - Certified Industrial Hygienist and asbestos brake expert
Jack J. Bloomfield - Sanitary engineer in U.S. Public Health Service
Paul W. Brandt-Rauf - Editor-in-chief of JOEM
Daniel C. Braun - Physician employed by, then president of IHF
Erin Brokovich - Famous/notorious paralegal in hexavalent chromium case
Vandiver Brown - General Counsel for Johns Manville
Oliver E. Burch - Georgia-Pacific general sales manager
William J. Butler - Epidemiologist and biostatistician consultant
Earle M. Chapman  - Boston physician and expert on silicosis
John C. Childs  - Georgia-Pacific chief litigation attorney
Kenneth Crump  - Biostatistician and risk assessor
Devra Davis  - Environmental oncologist and Professor at University of Pittsburgh
Catherine DeAngelis  - Physician and former editor of JAMA
Sir Richard Doll  - Epidemiologist who established tobacco and lung cancer linkage
Waldemer Dressen  - U.S. public Health Service physician
Philip Drinker  - Professor of Industrial Hygiene and pioneer industrial hygienist
David Egilman  - Professor of Public Health and toxic substance plaintiff expert
Philip E. Enterline  - Professor of Biostatistics and asbestos expert
Brent Finley  - ChemRisk scientist
Harold Fitzpatrick  - Lead attorney for the CPMA
George Fox  - Black Lung victim
John Froines  - Head of UCLA Environmental Program and member of Blue Ribbon Panel
David A. Galbraith  - Chemrisk partner and medical doctor
Leroy Gardner  - Physician and head of Saranac Laboratories
Ernie Getto  - Partner at Latham & Watkins law firm
Herman J. Gibb  - EPA, then independent toxic substance researcher
Stanton Glantz  - Professor of Medicine at the University of California at San Francisco
Bernard Goldstein  - Former Dean of University of Pittsburgh’s School of Public Health
<table>
<thead>
<tr>
<th>Name</th>
<th>Title</th>
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</thead>
<tbody>
<tr>
<td>Gio Batta Gori</td>
<td>Former researcher at NCI, tobacco consultant and editor in chief of</td>
</tr>
<tr>
<td></td>
<td>industry funded <em>Regulatory Toxicology and Pharmacology</em></td>
</tr>
<tr>
<td>Paul Gross</td>
<td>IHF researcher</td>
</tr>
<tr>
<td>Alice Hamilton</td>
<td>Pioneer public health and industrial medicine physician</td>
</tr>
<tr>
<td>Joshua Hamilton</td>
<td>Chief scientific officer at the Marine Biological Laboratory in</td>
</tr>
<tr>
<td></td>
<td>Woods Hole, Massachusetts and industry consultant</td>
</tr>
<tr>
<td>Lloyd E. Hamlin</td>
<td>Medical Director for American Brake Shoe Company</td>
</tr>
<tr>
<td>David K. Hardy</td>
<td>Philip Morris outside national litigation managing attorney</td>
</tr>
<tr>
<td>Harriet Hardy</td>
<td>Distinguished public health and industrial medicine physician</td>
</tr>
<tr>
<td>Leonidas R. Harless</td>
<td>Physician and diagnosing doctor for Hawks Nest silicosis</td>
</tr>
<tr>
<td>Ray Harron</td>
<td>Radiologist who surrendered his license in silicosis scandal</td>
</tr>
<tr>
<td>Theodore F. Hatch</td>
<td>Professor of Industrial Hygiene and industry consultant</td>
</tr>
<tr>
<td>Elmer Hayhurst</td>
<td>Renowned industrial medicine physician and plaintiff expert at</td>
</tr>
<tr>
<td></td>
<td>Hawk’s Nest trials</td>
</tr>
<tr>
<td>William C. Hemeon</td>
<td>Lead engineer for IHF</td>
</tr>
<tr>
<td>Alfred Hirth</td>
<td>Lawyer for Owens-Illinois who was instrumental in IHF formation</td>
</tr>
<tr>
<td>Steven L. Hoch</td>
<td>Chromium defense attorney with Haight, Brown, &amp; Bonesteel</td>
</tr>
<tr>
<td>Stewart Holm</td>
<td>Georgia-Pacific lead toxicologist and litigation consultant</td>
</tr>
<tr>
<td>Gary Huber</td>
<td>Harvard University professor and tobacco sponsored researcher</td>
</tr>
<tr>
<td>Wilhelm Hueper</td>
<td>Environmental cancer physician and researcher</td>
</tr>
<tr>
<td>James R. Hurd</td>
<td>Georgia-Pacific safety manager</td>
</tr>
<tr>
<td>Peter Infante</td>
<td>Former OSHA epidemiologist and now plaintiff expert</td>
</tr>
<tr>
<td>Richard D. Irons</td>
<td>Professor of toxicology and industry expert</td>
</tr>
<tr>
<td>Janis Jacks</td>
<td>U. S. district court judge</td>
</tr>
</tbody>
</table>
Edwin Jacob - Outside counsel to R. J. Reynolds and Special Projects manager
Chip Jacobs - Investigative environmental journalist
Raymond Jones - Deceased Hawk’s Nest silicosis plaintiff
Curtis H. Judge - Lorillard Tobacco Company CEO
Marianne Kaschak - IHF employee and president
Herbert Kaufman - Manager of Mutual chemical company Chromium plant
Brent Kerger - ChemRisk scientist
David Kessler - Physician, attorney, and former FDA Commissioner
Gladys Kessler - United States District Court Judge presiding of federal tobacco case
Sungkyoon Kim - Epidemiologist and toxicologist researcher for China/NCI study
Abe Krash - Arnold, Fortas & Porter tobacco attorney
Sheldon Krimsky - Professor of Urban and Environmental Policy at Tufts University
Jerry Kristal - Plaintiff personal injury attorney
Anthony J. Lanza - Public health physician for U.S. Public Health Service and Metropolitan Life Insurance Company, and silicosis expert
Mark Lanier - Personal injury plaintiff counsel
Jerrold Last - Univ. of California toxicology professor and chairman of Blue Ribbon Panel
C. William Lehnert - Georgia-Pacific manager for research and development
Lance Lubel - Plaintiff attorney with benzene cases
Cesare Maltoni - Oncologist and leader in research of industrial cancer hazards
Thomas F. Mancuso - Industrial medicine epidemiologist and toxic substance expert
Gerald Markowitz - Historian of science and occasional toxic substances plaintiff expert
Edward Masry - Plaintiff attorney who filed Hinkley chromium lawsuit
J. Corbett McDonald - Epidemiologist and asbestos industry researcher
Mary McLemore - in-house Georgia-Pacific attorney
Kathryn M. McMahon-Lohrer - CSR&S attorney
Marvin Mehlman - Former chief toxicologist for Mobil Oil Company
Edward Mereweth - Pioneer British industrial medicine physician
Robert K. Merton - American sociologist of science
David Michaels - Epidemiologist, public health advocate, and head of OSHA
Ronald Motley - South Carolina asbestos and tobacco plaintiff lawyer
Kenneth Mundt - Owner of Applied Epidemiology Inc., Cr(VI) industry researcher
Eric Newman - Attorney with Public Relations firm Hill/Powell
Robert Northrip - Shook, Hardy & Bacon tobacco attorney
John O’Quinn - Texas silicosis plaintiff lawyer
Deborah Ortiz - California state senator
Craig Parker - Marathon Oil toxicologist
Dennis Paustenbach - Toxicologist, industrial hygienist, president of ChemRisk, and defense toxic substance expert
Ernest Pepples - B&W vice president for law
Eugene Pendergrass - Professor of Radiology and silicosis expert
Frances Perkins - Secretary of Labor under Franklin Delano Roosevelt
Sir Karl Popper - Philosopher of Science
Gary Praglin - Plaintiff attorney in chromium lawsuits
Deborah Proctor - Exponent scientist and industry consultant
Gerald Raabe - ExxonMobil employee and head of API stewardship committee
Bernardino Ramazzini - 18th century Italian occupational medicine physician
Henry H. Ramm - General Counsel of R. J. Reynolds and chairman of CTR

Stephen M. Rappaport – Professor of environmental health and China/NCI study researcher

Robert Rinsky - NIOSH epidemiologist and benzene expert

David Rosner - Historian of science and occasional toxic substances plaintiff expert

John Rupp - Covington & Burling law firm partner

Ivan Sabourin - General Counsel for Johns-Manville (Canada) and QAMA

Oscar A. Sander - Federal Occupational Health physician and consultant to industry

H. Lee Sarokin - United States District Court Judge

Royd R. Sayers - Physician and official at U.S. Public Health Service

Nathan Schachtman - Industry defense attorney and ethicist

Marc Schenker - Professor of Medicine and member of Blue Ribbon Panel

Irving Selikoff - Physician and renowned asbestos disease researcher

William (Bill) Shinn - Shook, Hardy & Bacon tobacco attorney

Li Shunkun - Coauthor on the “Zhang” 1997 article

Stephanie Siegel - CSR&S attorney

Sumner Simpson - President of Raybestos-Manhattan (Asbestos brake manufacturer)

Patrick Sirridge - Shook, Hardy & Bacon tobacco attorney

Allan H. Smith - Professor of epidemiology, University of California, Berkeley

Clayton Smith - Pathologist and silicosis expert

Kenneth J. Smith - Johns-Manville Medical director

Martyn Smith - Toxicology professor at University of California, Berkeley

Douglas A. Smoot - Black Lung mining company lawyer

Wayne Stephenson - Plaintiff attorney in first successful asbestos lawsuit
Arthur Stevens  - General Counsel for Lorillard Tobacco Company
Herbert E. Stokinger - Industrial hygienist and member of ACGIH TLV Committee
Victoria M. Trasko - Public Health Service physician
T. David Truan - Physician employed by IHF
William Vance - California EPA scientific advisor
Arthur Vorwald - Leroy Gardner’s successor at the Saranac Laboratories
Christopher Wagner - Pioneer asbestos researcher and secret defense consultant
Allan Wardell - Outside Johns Manville attorney
Theodore C. Waters - Insurance industry workers’ compensation lawyer and IHF official
Edward R. Weidlein - 1930s President of the Mellon Institute
J. Kendrick Wells - B&W corporate counsel
Paul Wheeler - Johns Hopkins University Professor and radiologist
Jeffrey Wigand - B&W Vice-president of Research and whistle blower to FDA
Merrill Williams - Tobacco whistle blower to plaintiff counsel
Charles-E. A. Winslow - Industrial medicine physician and silicosis expert
John L. Wittenborn - Collier Shannon Scott law firm partner
Otto Wong - Epidemiologist and industry expert
Ernst Wynder - Epidemiologist who first linked tobacco and lung cancer
William P. Yant - U.S. Bureau of Mines and Mine Safety Appliances Company official
Tony Ye - ChemRisk scientist and translator for Zhang
Addison Yeaman - General Counsel of Brown & Williamson Tobacco Company
JianDong Zhang - Chinese biomedical hexavalent chromium researcher
Verne Zimmer- - Senior Labor Department Official under Franklin D. Roosevelt
APPENDIX C1
RECENT BRAKE ARTICLES

Industry Expert Studies


Finley, Brent L., Jennifer S. Pierce, Dennis J. Paustenbach, Laura L. F. Scott, Laura

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1 These articles were obtained by searching in PubMed for articles with “asbestos” and “brakes” in the text during the years 1998 through 2008. From that list I selected the articles that discuss asbestos disease related to brakes or fiber release from brakes. [http://www.ncbi.nlm.nih.gov/pubmed](http://www.ncbi.nlm.nih.gov/pubmed). Last accessed on November 23, 2010.


Experts Who Testify Primarily for Plaintiffs


**Foreign Experts**


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———. Exhibit 7: A. J. Lanza letter to A. Wardwell, November 11, 1930.


———. Exhibit 14: Minutes of meeting between Lanza, McConnell, and several Johns-Manville officials, July 15, 1931.

———. Exhibit 16: S. A. Williams to F. V. Meriwether, February 26, 1932.


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BIOGRAPHICAL SKETCH

Craig Alex Biegel was born in 1949 in Philadelphia, Pennsylvania. He grew up in Bucks County, Pennsylvania, attending William Tennant High School.

In 1967 he matriculated at Virginia Military Institute, where he was inducted into the Economics Honor Fraternity, Omicron Delta Epsilon, and graduated with a B.A. in History. Upon graduation, he received a teaching assistantship to Lehigh University, where he graduated with an M.A. in International Relations.

The author’s professional career began in the U.S. Navy, when he served as a line officer in Greece for two years in the early 1970s. Upon receiving an early release from active duty, he attended the University of Virginia School of Law, from which he received a J.D. in 1978.

Upon graduation, the author returned to active duty as a Navy JAG officer. In Japan he acted initially as senior defense counsel for U.S. Navy Courts Martial, then as the Staff Judge Advocate for the Commander Fleet Activities Yokosuka. While in Japan, the author was promoted to Lieutenant Commander (0-4). He subsequently received a transfer to the Navy-Marine Corps Appellate Review Activity, Appellate Government Division, located in the U.S. Navy Yard, Washington, D. C., where he earned the Navy Achievement Medal for his work.

In 1983 the author returned to private practice with the law firm Montgomery McCracken, Walker & Rhoads, in Philadelphia, Pennsylvania. Six years later he moved to Boston Massachusetts, where he became a named partner in the law firm of Tucker & Biegel. While there, he served as national asbestos property damage counsel for a Fortune 500 company, managing cases and lawyers across the country.
In 1994, the author left his practice to open a consulting business and take additional graduate science courses. For the next ten years he assisted plaintiff counsel in asbestos law suits. The author retired in 2005 and then returned to school, having been accepted in the Oregon State University History Department’s History of Science Masters Program. He obtained his M.A. in 2009, having received an award for historical writing and been inducted into the Honor Society of Phi Kappa Phi.

The author’s lead professor at Oregon State University, Ron Doel, left for Florida State University. The author followed in 2008, having been offered an assistantship in the History Doctorate Program. During this period, the author has also lectured across the country and in Europe on OSU’s oceanography program and the role of attorneys in distorting medical literature. He has also authored an article on oceanography for *Oxford University’s Encyclopedia of American History*. 