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## Being Labeled a Felon and Its Consequences for Recidivism: An Examination of Contingent Effects

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THE FLORIDA STATE UNIVERSITY  
COLLEGE OF CRIMINOLOGY AND CRIMINAL JUSTICE

BEING LABELED A FELON AND ITS CONSEQUENCES FOR RECIDIVISM: AN  
EXAMINATION OF CONTINGENT EFFECTS

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## ABSTRACT

Florida law allows judges to withhold adjudication of guilt for individuals who have been found guilty of a felony and are being sentenced to probation. This is accomplished if the judge chooses to sentence an individual to probation with “adjudication withheld.” Such individuals lose no civil rights and may lawfully assert they have not been convicted of a felony on employment applications and elsewhere. Labeling theory would predict that the receipt of a felony label could increase the likelihood of recidivism, and that this effect may vary across groups of individuals.

This research investigates this possibility by examining the reconviction experiences of the population of men and women found guilty of a violent, property, or drug felony and sentenced to probation between 2000 and 2003 in Florida (N=119,648). Logistic regression is used to assess whether applying the convicted felon label has negative consequences for reconviction within three years of sentencing. To assess the possibility that the effects of a criminal label may vary across groups of individuals, separate regression models are run for subsamples (based on race/ethnicity, employment status, sex, criminal history, and crime type) and slope difference tests are calculated to determine whether any of the differences in the impact of adjudication between groups are statistically significant.

Being adjudicated was significantly associated with reconviction for the full sample and all groups examined except for Hispanics and violent offenders. The findings also demonstrate that, while being labeled is a relatively consistent predictor of re-offense, individuals with certain characteristics may be more likely to suffer negative consequences than others. The effect of adjudication on recidivism was significantly larger for black females than black males (and approached significance for females and males). Additionally, labeling appears to be more detrimental for naïve than for experienced offenders and for property than for either drug or violent offenders.

# CHAPTER 1

## INTRODUCTION

While the popular conception of punishment is that it should deter future deviant or criminal behavior, labeling theory proposes the opposite, that officially sanctioning criminal behavior may actually increase the probability of re-offense. Our criminal justice system is founded on the principles of deterrence, retribution, and rehabilitation. The United States is more punitive than any other industrialized nation, arguably with the intention of lowering crime rates. If punishment not only fails to deter future criminal behavior but actually encourages it, our current policies may actually be contributing to a higher crime rate.

Sanctioning may lead to re-offense for a variety of reasons. Some have argued that official assignment of the meaning of deviant or criminal to an individual can eventually lead that individual to take on an identity consistent with that meaning and continue to engage in such behaviors through a self-fulfilling prophecy (Becker, 1963; Lemert, 1967; Matza, 1969). Others have argued that labeling may create additional obstacles to maintaining a non-criminal life and make crime a more appealing option. This may be due to the fact that even after an offender has completed his sentence, he does not shed the label of “felon” and continues to suffer the consequences of such. The loss of civil rights is one such “collateral consequence” of conviction (Travis, 2002).

On April 5, 2007, Florida's Clemency Board approved Florida Governor Charlie Crist's proposal to allow some felons to regain the right to vote, serve on juries, and obtain certain occupational licenses. Those who have committed lesser, nonviolent felonies will now have their civil rights restored automatically when the sentence is completed and all victim restitution payments have been made (The Florida Board of Executive Clemency). Previously, convicted felons were required to apply to have their civil rights restored, and it could take years for a decision because only the Governor and the Executive Clemency Board were empowered to make this decision (American Civil Liberties Union of Florida). While this is a step forward, restoring one's civil rights does not remove the felon label.

Even after the restoration of civil rights, the convicted felon label can have harmful effects, especially on one's employment prospects. Surveys of employers have long found that they are less willing to hire those who have been convicted of a variety of even minor offenses

(such as drunk driving and theft), regardless of job experience and qualifications (Boshier & Johnson, 1974; Buikhuisen & Dijksterhuis, 1971; Holzer, 1996; Schwartz & Skolnick, 1962). Additionally, more recent longitudinal studies of offenders have found support for the proposition that conviction (and other involvement with the criminal justice system) has negative outcomes for both employment and income (Davies & Tanner, 2003; Waldfogel, 1994). If conviction significantly limits employment options, then felons are prevented from fully participating in mainstream society and criminal options for income may become more appealing.

If, contrary to popular opinion, labeling criminals as felons can lead to a change in personal and social identity as well as produce impediments to employment and an increase in the likelihood of reconviction, then great care should be taken in applying such a label. Fortunately, in Florida judges have the discretion of withholding the felon label from individuals found guilty, yet require them to serve their probation sentence. Specifically, under Section 948.01 of the Florida Statutes, it states that:

If it appears to the court upon a hearing of the matter that the defendant is not likely again to engage in a criminal course of conduct and that the ends of justice and the welfare of society do not require that the defendant presently suffer the penalty imposed by law, the court, in its discretion, may either adjudge the defendant to be guilty or stay and withhold the adjudication of guilt; and, in either case, it shall stay and withhold the imposition of sentence upon such defendant and shall place the defendant upon probation .

Those who have adjudication withheld have been found guilty of a felony and are required to serve a probation sentence. But, they are allowed to legally assert that they have not been convicted of a felony. For those who receive this benefit, no civil rights are lost and they may legitimately say on employment applications and elsewhere that a felony conviction did not occur. If labeling theory has merit, this judicial option could benefit both society and the individual offender by preventing the potential negative consequences of the felon label.

This Florida policy presents a unique opportunity to test labeling theory. Withholding (or applying) adjudication represents a direct translation of labeling hypotheses. If, as the theory predicts, the act of defining one as a deviant itself can lead to an increased likelihood of re-offense through either a change in identity and social interactions or limitations on employment options, then the withholding of said definition, or label, should be associated with a decrease in

recidivism.

Other research has examined the impact of a variety of labeling events, including mere contact with law enforcement, arrest, conviction, incarceration, and length of incarceration, on recidivism. While these studies add to the body of research on the perspective, it is arguable that the felony convict label is the most consequential for adults and also represents the most direct test of the labeling hypothesis. As mentioned above, this label lasts forever, even after the court's sentence has been served in its entirety. All interaction with the criminal justice system prior to a felony conviction, while potentially stigmatizing, does not lead to the loss of civil rights or employment-related obstacles that are directly associated with the actual felon label.

Although labeling theory proposes that a variety of official labels can produce secondary deviance, this labeling effect is not necessarily the same for all offenders. The courts are filled with both men and women that are of different racial and ethnic backgrounds and employment and criminal histories that are arrested for a variety of offenses. It is possible that some individuals will have more resources to soften the impact of being labeled. On the other hand, it is possible that those who are already disadvantaged will not be harmed by additional exclusion. These ideas will be discussed more thoroughly in Chapter 2. Right now, it is just important to note that research examining the potentially contingent nature of labeling effects is in short supply.

While labels have been hypothesized to increase recidivism for a variety of reasons, the first step in examining this process is to determine whether official sanctioning is actually associated with an increase in recidivism. While researchers have subjected labeling theory to empirical examination before, many of the tests have failed to adequately address the possibility that labeling effects may be more harmful for some types of individuals. The present research seeks to do just that.

### **The Present Research**

The primary objective of this research is to examine the relationship between an official label and recidivism. More specifically, I test the labeling theory hypothesis that being formally adjudicated as a felon is associated with an increase in the likelihood of reconviction within three years of the original sentence. Additionally, I examine the strength of the labeling-recidivism relationship across a variety of individual characteristics, including race and Hispanic ethnicity,

employment status, sex, criminal history, and offense type. While labeling theorists have long suggested that some individuals may be more negatively impacted by labeling events than others, empirical tests of these propositions are missing.

The paper will proceed as follows. Chapter 2 further explores labeling theory generally and the arguments relating to the potentially contingent effects of formal labels. Chapter 3 reviews 40 years of research on labeling theory and attempts to synthesize the findings in order to draw conclusions about the current empirical state of the perspective. In Chapter 4, the specific research hypotheses are described, as well as the data and methods that will be used to test these hypotheses. The results of statistical analyses are presented in Chapter 5. Chapter 6 briefly summarizes the results, discusses both the theoretical and practical implications of the findings, and provides direction for future research.

## **CHAPTER 2**

### **LABELING THEORY**

#### **Introduction**

Ascending to popularity in the 1960s, labeling theory shifted the focus of criminological theory away from the offender. Labeling theorists argued that initial delinquency is normal and occurs for a variety of reasons, but official reactions to it may lead to future offending. For the first time in almost 200 years, the legal system itself became an object of inquiry and researchers began examining the societal reaction to deviance. This represented a marked shift from mainstream sociological thought at the time. While popular for over twenty years, some proclaimed the perspective dead by 1985 (Paternoster & Iovanni, 1989). It is now experiencing something of a revival in both theoretical (Sampson & Laub, 1997) and empirical (Bernburg & Krohn, 2003; Bernburg, Krohn, & Rivera, 2006) terms.

Generally, labeling theorists proposed that publicly defining and treating an individual as a deviant may result in exclusions from conventional society, a possible identity change, and an increase in the likelihood of subsequent deviance. The likelihood of deviance escalation may also vary across individuals. Specific deterrence theories are in direct opposition to labeling and argue that official sanctioning may increase the perception of risk associated with deviance and potentially deter future criminal behavior. While not the focus of this paper, labelists also argued that the powerful make and enforce the laws and that enforcement may be selective, so that extralegal characteristics, such as race and gender, influence whether official intervention will be imposed. This chapter will examine the intellectual foundations, the basic premises of labeling theory, and explanations for its decline and recent resurgence in popularity.

#### **Intellectual Foundations and Basic Premises**

##### **Symbolic Interactionist Roots**

Labeling theory evolved out of symbolic interaction, a school of thought which developed from the belief that human behavior is the product of purely social symbols communicated between individuals. The existence of the “self” is at the heart of symbolic interaction and, “provides the human being with a mechanism of self-interaction with which to meet the world—a mechanism that is used in forming and guiding his conduct” (Blumer, 1969,



p. 62). The basic idea was that the mind and the self are not innate but are products of the social environment. Generally, symbolic interactionists proposed that people orient action on the basis of meanings they give to stimuli. As such, meaning is the central mediating factor in behavior. The term “symbolic interaction” was coined by Blumer (1969) to describe the work that had previously been accomplished by those such as Charles Horton Cooley and George Herbert Mead.

Cooley (1902) conceived of the “looking-glass self” whereby individuals create their identity based on how others see them. He argued that in social settings people are provided an image of how they are perceived by others. This image could lead to altered behavior in order to meet the expectations of others, a type of self-fulfilling prophecy. The concept of a self-fulfilling prophecy became central to the work of early labelists.

Similarly, Mead (1934) argued that the self is created in interaction with others through the use of language and gestures. The self is the internalization of the “generalized other,” or the society within the individual. This process occurs as a child develops. Initially, infants do not have a “self.” As they grow they learn to take on the attitudes of one other person at a time, which Mead referred to as “play”. An example of this is children playing house, where each child pretends to be one other person. In “game,” children learn to take on the roles of multiple people at once, such as when playing baseball, where one must understand the special role of each player on the field. This is analogous to internalizing the generalized other, or an organized and consistent set of attitudes of others. As we develop, we learn to see ourselves as an object, which can include seeing ourselves as others see us. This influences how we think about ourselves and how we orient action. The self is flexible and can change over time and in different social contexts as we continually interact with others. Socialization does not stop the development of the self, rather, it can continue to change and develop as do the circumstances and occurrences in one’s life.

These ideas were essential to the development of labeling theory. Specifically, the concept of a self-fulfilling prophecy, the proposition that an individual’s behavior can be altered in order to meet societal expectations, is the foundation of the earliest labeling statements by Frank Tannenbaum and Edwin Lemert.

## **Tannenbaum and Lemert**

The first seeds of labeling theory were planted by Tannenbaum (1938), although his ideas were not popularized until the 1960s. He discussed what he termed the “dramatization of evil.” Tannenbaum believed that most children engage in some deviant behavior and the reaction to this behavior can lead to a self-fulfilling prophecy where kids that are ‘tagged’ as a delinquent will engage in more deviant behavior in the future. Only some of the children who engage in delinquent behavior are caught. Those who are caught are treated differently than they were before being caught and differently than others (including those who are guilty of the same behaviors) who have not been caught. This new treatment may lead them to reconsider their identities and change their behavior to fit this new identity. Although at first delinquent behavior is labeled as “bad” eventually the child is labeled as “bad.” This is best summarized by Tannenbaum (1938, pp. 19-20):

The first dramatization of “evil” which separates the child out of his group for specialized treatment plays a greater role in making the criminal than perhaps any other experience.... He has been tagged. A new and hitherto nonexistent environment has been precipitated out of him. The process of making the criminal, therefore, is a process of tagging, defining, identifying, segregating, describing, emphasizing, [and] making conscious and self-conscious; it becomes a way of stimulating, suggesting, emphasizing, and evoking the very taints that are complained of.... The person becomes the thing he is described as being.

Lemert (1951) further elaborated on these ideas with his concepts of primary and secondary deviance. According to Lemert, most youth engage in primary deviance, which occurs for a variety of reasons and is not seen as problematic but rather as normal for juveniles. Youth engaging in primary deviance do not view themselves as “delinquents” but rather as normal juveniles engaging in normal behavior. Primary deviance becomes a problem when it is recognized and the child is stigmatized for engaging in this behavior. This labeling can eventually lead a child to see himself as a delinquent.

Secondary deviance is deviance that occurs after a child has taken on the identity of a deviant. He sees himself as a deviant and continues to engage in delinquent behavior. Lemert (1967) describes a process by which secondary deviance occurs. It begins with primary deviance, which at some point is recognized and labeled. The child may continue to engage in

these behaviors and may be further stigmatized. Eventually, as the stigmatization continues, the child begins to see himself as others do, as a deviant. When the behavior continues after this point (the child has identified himself as a delinquent or deviant), it is considered “secondary”. While it was argued that primary deviance occurred for a variety of reasons, deviance is secondary when it becomes a “means of defense, attack, or adaptation to the overt and covert problems created by the societal reaction primary deviation” (Lemert, 1967, p. 17). While a variety of other causes are responsible for primary deviance, secondary deviation is thought to be caused by societal reaction. It should also be noted that while early labelists focused on identity change, it was later recognized that secondary deviance need not involve identity change but could be the result of a rational adaptation to one or another exclusion. This will be discussed in more detail later in the section on intervening mechanisms.

Harold Garfinkel (1956, p. 420) added to the early work by describing the conditions necessary to transform an actor’s identity through what he termed successful “degradation ceremonies,” or “[c]ommunicative work directed to transforming an individual’s total identity into an identity lower in the group’s scheme of social types.” While non-official labeling is possible (by the family, for example), Garfinkel (1956) focused on the public and official nature of the reaction. Degradation is most effective when there is an official ceremony that ends with the actor being “ritually separated from a place in the legitimate order” (Garfinkel, 1956, p. 423). This early emphasis on official reactions was carried on in most of the later formulations of labeling theory.

While Tannenbaum and Lemert laid the foundation for labeling theory by the early 1950s, it did not reach mainstream appeal until a decade later. Howard Becker is often given credit for popularizing these ideas in the 1960s. This new generation of labelists broadened the range of the theory by examining how acts originally become labeled as criminal and which groups of individuals are most likely to receive formal sanctions. While the early labelists focused on changes in self-identity as the triggering mechanism for a labeling effect, beginning in the 1960s others considered additional mediating factors, such as changes in peer affiliations and conventional opportunities. Finally, it was recognized that a labeling event may not impact all individuals equally, but rather that its effect may be contingent on individual characteristics. Each of these ideas will be examined below. Because the focus of this paper is on the classical deviance amplification component of labeling theory and also on the possibility that this

relationship may depend on individual traits, more attention will be given to these areas of inquiry.

### **Rule Creation and Enforcement**

Prior to the introduction of labeling theory, most criminologists adopted a “norm-based definition of deviance” (Kitsuse, 1980, p. 384) and did not question the origin of deviant labels. Becker (1963, p. 9) took an important step back in the labeling process and argued that:

*social groups create deviance by making the rules whose infraction constitutes deviance, and by applying those rules to particular people and labeling them as outsiders. From this point of view, deviance is not a quality of the act the person commits, but rather a consequence of the application by others of rules and sanctions to an ‘offender’”*  
(emphasis in original).

Lofland (1969, p. 14) also recognized the socially constructed nature of deviance and defined it as, “the name of the conflict game in which individuals or loosely organized small groups with little power are strongly feared by a well-organized, sizeable minority or majority who have a large amount of power.”

Becker (1963) expanded the proposition that power is an essential element in rule creation by discussing it as a moral enterprise. Moral entrepreneurs were said to be those who initiate rule creation and can become crusading reformers, or those who are so extremely disturbed by some social phenomenon (abortion and gay marriage are examples of current issues that may be so categorized) that they will stop at nothing to correct it. While the moral crusader may be motivated by humanitarian beliefs and a desire to help those below him, he may not be aware of the consequences of the moral crusade. Because the crusader is often of the middle or upper class, it is the values and ideals of this segment of the population that are imposed on others.

Becker (1963) supported the argument that rule creation is not a natural process, but rather an enterprise, by illustrating that the passage of the Marijuana Tax Act was largely due to the zeal of one man, Harry Anslinger (the commissioner of the Federal Bureau of Narcotics), not due to the inherent dangers of the substance or an increase in its use. Similar historical analyses have examined the creation of delinquency through the advent of the juvenile court (Platt, 1969) and the discovery of child abuse (Pfohl, 1977) and wife beating (Tierney, 1982).

When a new set of rules has been created, it logically follows that enforcement is necessary (Becker, 1963). It was recognized early on that rule enforcers will work in a selective manner and not all those who break a rule will be labeled as deviant or criminal (Becker, 1963). Race, class, and demeanor at arrest are among the characteristics that labelists argue may impact this decision (Becker, 1963). The status characteristics hypothesis, the idea that these extra-legal characteristics may have some impact on whether one receives a label, has often been mischaracterized by critics of the theory (see Tittle, 1980) who claim that these are the *most* important factors in labeling outcomes (Paternoster & Iovanni, 1989). The early labelists did not make such strong claims.

Becker (1963, p. 12) stated that, “The degree to which an act will be treated as deviant depends *also* on who commits the act and who feels he has been harmed by it” (emphasis added). Schur (1971, p. 66), when discussing the relationship between the rule break and enforcer, similarly stated that, “the parties’ stocks of relevant resources and their relative capacities to wield or resist power are clearly important in shaping outcomes.” He appears to be arguing that differences in power may influence outcomes but do not determine them. In a thorough review of research on the status characteristics hypothesis, Paternoster and Iovanni (1989, p. 368) provide a clear statement of the hypothesis that would probably be accepted by many of the early labeling theorists:

“Given the occurrence of a delinquent act, the decision of organizational agents to sanction officially (to label) an actor is *in part* determined by the social characteristics (race, sex, social class) of the offender and/or of the offended party.”

In addition to broadening the range of labeling theory by encompassing both rule creation and enforcement, labeling theorists also elaborated on some of the original work by more explicitly hypothesizing how a labeling event actually leads to an increase in future offending. This included both expanding on the established idea of changes in self-identity and also suggesting other potential mediators, such as changes in peer group membership and conventional opportunities.

### **Intervening Mechanisms**

Early labeling theorists proposed that official assignment of the meaning of deviant or criminal to an individual can eventually lead that individual to take on an identity consistent with

that meaning and continue to engage in such behaviors through a self-fulfilling prophecy. Becker (1963) recognized that the most important consequence of being labeled a deviant is the drastic change in one's social identity and added that when an individual is known to have committed a crime, it can become his "master status," one that overrides all others.

As such, a man that has stolen will be considered to be essentially a "thief" even though he is many other things, such as a banker or a father. These other identities become secondary and "the deviant identification becomes the controlling one" (Becker, 1963, p. 34). Matza (1969, p. 156) referred to this same process as "signification" and stated that:

To signify is to *stand for* in the sense of representing or exemplifying.... To be signified a thief is to lose the blissful identity of one who among other things happens to have committed a theft. It is a movement, however gradual, toward being a thief and representing theft.

Among other things, the social recognition of one as a criminal may lead an individual to view himself as such and, through a self-fulfilling prophecy, engage in criminal activities.

In addition to identity changes, labelists recognized that essentialization as an outsider has other consequences, including blocked access to conventional others and opportunities, that may assist in perpetuating a criminal career. One consequence of being labeled is exclusion from mainstream social groups, which may lead to what Becker (1963) proposed as the final step in the development of a criminal career—joining an organized deviant group. When treated as deviants, individuals cannot carry out normal everyday routines, but rather they may be forced to socialize with others who have been similarly labeled. This only further solidifies the individual's deviant self-concept. Matza (1969) similarly argued that once an individual has been apprehended, he may cast himself in a new light. Once this change occurs, he may intentionally exclude himself from situations involving conventional others (Goffman, 1963; Matza, 1969). When society stigmatizes an individual, he may be excluded from conventional others and either choose or be forced to socialize with those similarly labeled.

This exclusion from conventional routines and others may also impact mainstream opportunities, such as employment. Becker (1963, p. 34) noted that:

[T]hough the effects of opiate drugs may not impair one's working ability, to be known as an addict will probably lead to losing one's job.... The drug addict finds himself

forced into other illegitimate kinds of activity, such as robbery and theft, by the refusal of respectable employers to have him around.

More recently, this idea that official intervention can lead to blocked conventional opportunities, including both education and employment, has been integrated into a life course perspective (Sampson & Laub, 1997). Specifically, Sampson and Laub's (1997, p. 147) age-graded theory of informal social control, "suggests a 'snowball' effect—that adolescent delinquency and its negative consequences (e.g., arrest, official labeling, incarceration) increasingly 'mortgage' one's future, especially later life chances molded by schooling and employment." The lack of investment in these arguably beneficial social institutions then leads to continued criminal behavior (see Hirschi, 1969; Sampson & Laub, 1993).

Overall, it was argued that labels could have an effect on future behavior by either (a) changing one's self-concept; (b) limiting access to conventional peer groups and increasing contact with other stigmatized individuals; or (c) blocking conventional opportunities. In addition to having a variety of viewpoints on how labels work, there has also been a debate over the impact individual characteristics may have on the sanction-recidivism relationship.

### **Contingent Effects of Formal Labeling**

Although it did not receive much attention until quite recently, some early labelists also proposed that the effect of a label may be contingent on personal characteristics. While these hypotheses were not as fully developed as others during the peak of labeling theory's popularity, much recent theoretical and empirical attention to the deviance escalating effects of formal labeling has focused on the proposition that the effect of a label may be contingent on personal characteristics (Barrick, Bales, Bontrager, & Chiricos, 2005; Bernburg & Krohn, 2003; Paternoster & Iovanni, 1989; Sampson & Laub, 1997). Although some classical labeling theorists acknowledged that vulnerability to the impact of a label may vary across individuals, this area was not as extensively developed or tested as other labeling hypotheses. An exception can be found in John Lofland's (1969) book *Deviance and Identity*.

Lofland (1969) was one of the few labelists to thoroughly discuss the contingent nature of deviance amplification. He argued that the acceptance of a deviant identity depended on the degree of: (1) disorientation; (2) affective bonds with others; and (3) congruence between actor's and other's cognitive categories (Lofland, 1969, pp. 177-205). Disorientation referred to

a “state of high anxiety, fear and ambiguity over how to cope with or manage his proximate life situation” or the strength of one’s self-concept and was highly related to age and extent of formal education such that disorientation (and acceptance of a deviant identity) was more likely for those who are younger and less educated (Lofland, 1969, p. 178). Affective bonds referred to positive emotional attachments to both conventional and deviant others. It was argued that deviance avowal was more likely when an individual has strong bonds with those who assign the label. Finally, the third category indicated that deviance acceptance was more likely when the labeled and the labeler use similar verbal categories to describe the deviant identity.

Although Lofland (1969) provided a foundation for discussing differential susceptibility, few followed his lead. Those who did acknowledge that labeling outcomes may be contingent on characteristics of the actor did not build off his formulation, but rather briefly mentioned other traits (normally with little theoretical rationale). Additional personal characteristics that were mentioned include race and class, stakes in conformity, sex, and prior record. More recent work in this area has elaborated on the relationship between individual traits and labels, but opposing hypotheses have been developed in some cases.

It was recognized fairly early on that the impact of a formal label may be contingent on the offender’s race and social standing, which, even though they are separate concepts, were often discussed together. Ageton and Elliott (1974), following Jensen (1972), hypothesized that the effects of a formal sanction would vary by race and class in such a way that it would be less consequential for minorities and those of low social standing. Harris (1976, p. 433) similarly argued that those who are excluded from “ascriptive membership” in mainstream society (specifically minorities) are less vulnerable to labeling effects. Indeed, for those already denied full social membership based on race, the loss of such membership is redundant, so “the certification ‘deviant’ should not have as severe an impact on identity (if any at all)” (Harris, 1976, p. 433). In a review of labeling theory and research, Paternoster and Iovanni (1989, p. 382) take the idea of social membership further than Harris (1976) and suggest that those (regardless of race) who “refrain from granting legitimacy to the legal order” may, as a result, be “more impervious to labeling effects than are those who ascribe legitimacy to group rules.”

More recently, scholars have argued the opposing position, that official labeling should have more negative consequences for those who are disadvantaged (primarily referring to minority and poverty status). In a discussion of the impact of sanctions on the disadvantaged



urban poor, Sampson and Laub (1997, p. 153) noted that “among the disadvantaged, things seem to work differently. Deficits and disadvantages pile up faster and this has continuing negative consequences for later development...” Those at higher structural locations have greater social resources to combat the ill effects of stigmatization. While in agreement with Sampson and Laub’s (1997) hypothesis, Bernburg and Krohn (2003, p. 1290) added that, “deviant labeling of disadvantaged youths... is enhanced by the negative stereotypes that are already associated with these youths in the mainstream culture.”

It should be noted that the early labelists proposing that the disadvantaged would be less vulnerable to labeling effects (Ageton & Elliott, 1974; Jensen, 1972) were interested in alterations in self-concept, while those arguing the contrary (Bernburg & Krohn, 2003; Sampson & Laub, 1997) were interested in the structural version of labeling theory. This important distinction may help explain how they developed opposing hypotheses as to the interactive effects of race and class and sanctions. In fact, Bernburg and Krohn (2003, p. 1291) recognized that, “official intervention may, indeed, impact higher status people more than it does lower status people,” but that “the argument of Sampson and Laub (1997) concerning the impact of labeling on conventional opportunities are compelling and more consistent with arguments relating to the structural effects of labeling.” Additionally, Bernburg (2002) noted that conflicting moderating forces may be at work so that in some cases disadvantage increases the effects of labeling and in others a decreasing effect is seen. More research is needed to disentangle these processes.

Related to disadvantage is the issue of informal control and stakes in conformity, which have also been suggested to impact labeling consequences. Sherman and colleagues (1992, p. 682) noted that there is disagreement over the relationship between stakes in conformity and labeling outcomes and have referred to the opposing hypotheses as the greater vulnerability and less vulnerability versions. Those who support the greater vulnerability version expect that those who “care more about the opinions of conventional society” will be more vulnerable to negative consequences of formal labeling (Sherman, Smith, Schmidt, & Rogan, 1992, p. 682). This appears to correspond with Lofland’s (1969) argument that those with strong emotional ties are more likely to accept a deviant identity. According to the less vulnerability version, those who have strong informal social bonds, including marriage and employment, will be more insulated from the effects because they have “other social resources that overcome the impact of labeling”

(Sherman, Smith, Schmidt, & Rogan, 1992, p. 682). This distinction between greater and less vulnerability also applies to some of the work on race and class discussed above. For example, the arguments of Harris (1976), Jensen (1972), and Ageton and Elliott (1974) may be said to fit under the greater vulnerability hypothesis while the propositions of Sampson and Laub (1997) and Bernburg and colleagues (Bernburg, 2002; Bernburg & Krohn, 2003) would be supportive of the less vulnerability version.

While the impact that disadvantage may have on labeling outcomes has received some attention over the years, sex has been substantially overlooked in this regard, even in the most recent research. Unfortunately, even when gender is mentioned, adequate explanations for its effect are generally lacking. Similar to the work on race and class, opposing hypotheses have been proposed about the impact of gender on labeling consequences.

Although Ageton and Elliott (1974) found that, “males are more likely to be affected negatively by a police contact than females,” they neither provided a theoretical rationale for examining sex nor explicitly stated a hypothesis for this relationship. More than a decade later, Ray and Downs (1986) hypothesized the contrary, that females may be impacted more by labels than males. This was based solely on the idea that, “Females are expected to be more attentive to interpersonal relationships than males” (Ray & Downs, 1986, p. 171). The relevance of these interpersonal relationships to labeling outcomes was not discussed. In an examination of both deterrent and labeling effects, Keane, Gillis and Hagan (1989), proposed that those who are risk-averse will be more susceptible to deterrent effects while risk-takers will be more susceptible to labeling effects. And, “because of gender differences in orientations to risk, deterrence theory holds more for females, while the amplification argument is more salient for males” (Keane, Gillis, & Hagan, 1989, p. 337).

Still others have argued that being labeled should have the same effect on both males and females because criminological theories, including labeling, are able to explain deviance regardless of gender (Simons, Miller, & Aigner, 1980). So, it has been proposed that the negative consequences of labeling will be (a) greater for males; (b) greater for females; and (c) the same for males and females. Because this area is as under-researched as it is under-theorized, it is difficult to assess the strength of these varying propositions at this time.

Prior record is another potential contingency that has been relatively undeveloped. The most common argument has been that labeling effects would be more consequential for naïve

offenders rather than those with a prior record (Horwitz & Wasserman, 1979; Paternoster & Iovanni, 1989; D. A. Smith & Gartin, 1989). Both Horwitz and Wasserman (1979) and Smith and Gartin (1989) seem to base this hypothesis primarily on research findings from deterrence theory suggesting that naïve offenders are more likely than those more experienced to be deterred (see Cameron, 1964; Thorsell & Klemke, 1972). Paternoster and Iovanni (1989, p. 386) similarly noted that, “it is doubtful that further increments in labeling will continue to produce further deviance” and suggested that, “a ‘leveling-off’ point may be reached, where the process is at equilibrium.” Building on the expectations regarding ascriptive membership mentioned earlier, they argue that this would also be theoretically expected because, “once an actor is excluded from major conventional life situations it is not unreasonable to assume that further exclusion would have little additional meaning” (Paternoster & Iovanni, 1989, p. 386).

Finally, in addition to individual characteristics, it has recently been suggested that environmental contexts may play a role in labeling effects. Paternoster and Iovanni (1989, p. 373) noted that “the effect of status characteristics on labeling outcomes is not invariant, but varies substantially across different social contexts. Similarly, Sampson and Laub (1997, p. 153) argued that those researching labeling outcomes “cannot ignore the effects of larger social contexts” because “deficits and disadvantages pile up faster” in some social environments. Currently, the consideration of contextual effects on labeling consequences has received little attention but may be a fruitful direction for future empirical and theoretical endeavors.

### **Death and Resurrection**

While experiencing popularity during the 1960s and 1970s, by the mid-1980s many scholars assumed the labeling perspective was dead (Palamara, Cullen, & Gersten, 1986). Criticisms were aimed at both its lack of theoretical specificity and empirical support (Paternoster & Iovanni, 1989). Additionally, ideological differences about preferred methodologies managed to creep into and influence some of these debates (Bernburg, 2002).

Criticisms on the theoretical front came both from those who confused labeling and conflict approaches, thought labeling theory did not take its arguments far enough, and argued that it was imprecise and poorly specified. Among potential sources of disfavor, Wellford and Triplett (1993, p. 7) noted that, “labeling theory has suffered from the inattention to its central element, the symbolic process by its tendency to drift into an association with critical and

conflict criminology, and the tendency to treat labeling as a single explanation of secondary deviance.” While many of the original labeling theorists wrote from a symbolic interactionist perspective, research on the theory tended to focus on the mere application of a label, rather than the meaning it had for the labeled individual and for others. This operationalization could also be used in examinations of conflict propositions, which allowed for theoretical lines to blur, opening up labeling theory to criticisms of other, distinctive perspectives. Those who were critical of conflict theories assumed the flaws were also inherent in the labeling perspective.

In addition to critiques resulting from those confusing the labeling and conflict approaches, some supporters of the conflict position criticized labeling for not taking the argument all the way to what some considered its logical conclusion. In response to Becker’s (1967) presidential address for the Society for the Study of Social Problems, Gouldner (1968, p. 107) stated that his “argument is essentially a critique of the caretaking organizations, and in particular of the *low level* officialdom that manages them. It is not a critique of the social institutions that engender suffering or of the high level officialdom that shapes the character of caretaking establishments.” While labelists recognized the negative consequences of formal processing, their critiques were not aimed high enough in the opinion of some observers.

More generally, some critics argued that labeling theory was not truly a theory because it was not falsifiable—it did not contain arguments in axiomatic form or propose testable propositions (Gibbs, 1966). Some labelists contended that was not necessary. In fact, Schur (1969, p. 316) acknowledged that labeling may not “strictly speaking, constitute a theory” and continued to state that, “Clearly it is correct to point out that the reactions analysts are not much concerned to develop, by determining patterns of association between standard variables, the traditional kinds of predictive statements about the etiology of deviance among specified classes of individuals.” These disagreements over whether theories need to put forth explicitly testable propositions run much deeper than labeling theory, but they became entangled in the debate over its adequacy. This intellectual partisanship likely played a role in labeling theory’s demise. In a critique of the perspective, Tittle (1980, p. 242) brings attention to this issue by focusing on the labelists’ preferred methodology rather than on the theory itself:

In the case of labeling theory, it is not only difficult to derive specific empirical assertions [...], but the advocates themselves unashamedly claim to eschew precise propositional statements... in favor of “sensitizing observations” which “jostle the

imagination”.... Indeed, we are told that the particular strength of labeling theory lies in its resistance to formalization and in its vagueness and ambiguity, since such features alert us to important aspects of social life which are themselves relativistic and elusive. Over a decade after these comments, Paternoster and Iovanni (1989) argue that it was this partisanship that led some scholars to use overly stringent criteria in assessing labeling theory that would not have been used in evaluations of other theories.

Because many labeling theorists were not interested in subjecting the perspective to empirical testing, those who did attempt quantitative evaluation were generally not proponents, but rather critics of the theory. It has since been argued that most empirical tests of labeling, “have been conducted with grossly misrepresented hypotheses that are more caricature than characteristic of the theory” (Paternoster & Iovanni, 1989, p. 360). Lemert (1967) recognized this early on and argued that labeling theory is mainly an invention of its critics.

In fact, these tests may have actually helped bring about its demise by not accurately reflecting the theoretical statements (Paternoster & Iovanni, 1989). For example, most tests examined only the direct effect of sanctions on recidivism, but theorists argued that the effect of a label should be mediated either by a change in self-identity, changes in peer associations, or blocked opportunities. Additionally, some theorists argued that the effect may be conditional on a variety of individual characteristics and was not expected to have an invariant effect, but this was rarely taken into account in empirical tests.

By the mid-1980s, many scholars had already accepted the death of labeling theory, likely based on the empirical evidence. This conclusion was drawn too soon and based on inadequate evidence, as Paternoster and Iovanni (1989, p. 384) note, “the bulk of these studies do not constitute a valid test of labeling.” After a period of decline, some scholars have now recognized that the complete rejection of the perspective was unjustified, which has led to a revival of interest in some of the classical labeling arguments.

This second life of labeling theory can be attributed to both theoretical and empirical contributions. A couple of reviews of labeling theory and research (Paternoster & Iovanni, 1989; Wellford & Triplett, 1993) in the late 1980s and early 1990s brought attention to the need to reconsider the claims about labeling theory’s inadequacy. Wellford and Triplett (1993) proposed returning the theory to its symbolic interactionist roots and stressing the importance of informal labels that occur early in life. Some work has been done in this area. Matsueda (1992) and

Heimer & Matsueda (1994) put forth and tested the proposition that, through the process of role-taking, children begin to see themselves as “bad kids” when their parents and teachers treat them as such. These identities are then associated with an increase in future criminal involvement. For the most part, they have found support for these arguments.

Theorists in the mental health field have also modified labeling theory and derived empirically testable propositions (Link, Cullen, Frank, & Wozniak, 1987; Link, Cullen, Struening, Shrout, & Dohrenwend, 1989). Link and his colleagues (1987) argued that it, in order to completely understand how labels work, it was necessary to examine what labels mean to the public. Because mental patients are members of the public, they learn the stigmas that people generally attribute to mental patients. Once an individual is admitted to psychiatric treatment, he personalizes public beliefs about those in his situation (Link, Cullen, Struening, Shrout, & Dohrenwend, 1989). The stigma the patient feels may lead to changes in self-esteem, reduced contact with peers, and unemployment.

In Paternoster and Iovanni’s (1989, p. 383) thorough review of labeling issues, they “argue for a more general synthesis of the labeling and social control framework. We suggest that the labeling of actor as deviant is one factor which may weaken the social bond to conventional society, thereby freeing actor to deviate.” Following both this suggestion and the work by Link and colleagues (Link, Cullen, Frank, & Wozniak, 1987; Link, Cullen, Struening, Shrout, & Dohrenwend, 1989), Sampson & Laub (1997) have incorporated labeling ideas into their age-graded theory of informal social control (discussed above). They are particularly interested in whether changes in educational and employment opportunities mediate the relationship between sanction and recidivism and also whether those who are disadvantaged are more likely to suffer these consequences. Some of these propositions have been subjected to empirical evaluation and have received some support (Bernburg & Krohn, 2003; Bernburg, Krohn, & Rivera, 2006). The results of these findings will be discussed in more detail in the next chapter.

### **Summary**

Having experienced a peak in popularity and its requisite decline, labeling theory is now experiencing a sort of rebirth. Over the years, labeling theorists have focused on a wide range of issues, all related to the identification of an actor as deviant or criminal. This includes the

origins and enforcement of rules, the process by which labeling may produce more deviance, and the recognition that the likelihood of experiencing negative consequences may be contingent on personal characteristics. The areas that are currently receiving the most attention include examinations of the process of labeling and its contingent nature. Of particular interest here is examining the opposing hypotheses regarding which individuals will be more susceptible to secondary deviance.

The next chapter will explore the empirical status of labeling theory by reviewing over four decades of empirical research on secondary deviance and discussing in greater detail the more theoretically informed tests. Special attention will be paid to the limited body of research on whether a label's impact varies by characteristics of the actor.

## CHAPTER 3

### LITERATURE REVIEW

#### Introduction

While researchers have discursively analyzed the empirical state of labeling theory (see, for example, Bernburg, 2002; Paternoster & Iovanni, 1989; Tittle, 1980), no one has yet systematically examined the existing body of empirical work in this area of inquiry. And while a complete assessment has not been conducted, it was assumed for some time that the labeling propositions have been empirically defeated. This can be further evidenced by the relative lack of theory testing in this area for over fifteen years (for recent exceptions, see Bernburg, 2002; Bernburg & Krohn, 2003; Bernburg, Krohn, & Rivera, 2006)<sup>1</sup>. A more comprehensive review, such as the one presented here, can help us more systematically judge the empirical state of the theory and examine potential contingencies under which the propositions may hold.<sup>2</sup>

On a more practical note, assessing the impact of sanctions on recidivism may have important implications for policy makers. If the hypothesis that official intervention may have crime-increasing potential is supported, then it may warrant a reexamination of our nation's current criminal justice policies. Regardless of the outcome, this is an important consideration that has potential implications not only for criminological theory but for justice policies and practices from federal to state and local levels.

In this chapter, I review the findings from 61 studies over the last four decades that report some measure of the official sanction-recidivism relationship. The findings will be compared over a variety of methodological and substantive contexts, and the most rigorous tests of labeling theory will be described in greater detail. Before this is done, it is necessary to revisit previous empirical reviews and to describe the analytical technique used in this paper.

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<sup>1</sup> It should be noted that, while many of the studies included in this assessment are recent, the vast majority were not intended to test labeling theory, but rather deterrence or alternatives to incarceration, among other topics.

<sup>2</sup> While it can be plausibly argued that official sanctions may be associated with increased recidivism for reasons other than labeling (for example, strain, defiance, or changes in one's control ratio), tests of labeling theory have been traditionally conducted without an examination of the mechanism by which sanctions impact recidivism. Increased recidivism is the primary argument of labeling theory and is not a major component of these other criminological theories. Similarly, it could be argued that official sanctions may be associated with decreased recidivism for reasons other than deterrence, but this association is typically taken as support for deterrence theories.



### **Prior Reviews of the Research**

Charles Tittle (1980) criticized labeling theory on two fronts. He argued that labeling required testable propositions and adequate empirical testing, both of which he claimed had not been done. Tittle (1980) then examined the existing research to determine if there was any support for the notion that labeling affects crime. He reviewed three studies which he thought most closely met methodological demands and found one study (Beattie & Bridges, 1970) with results supportive of the perspective and two (Babst & Mannering, 1965; Federal Bureau of Investigations, 1968) producing contrary evidence. While Tittle (1980) felt these studies were the most rigorous of the time, he found flaws with each one.

He suggested that the use of arrest or parole violation as indicators of criminal behavior is not ideal because the relationship between these events and guilt may be weak (Tittle, 1980). These studies also relied on offender populations and did not include any individuals who had never been convicted. Tittle (1980) argued that this may obscure the effect of labeling if there is little difference in reoffending between those already labeled but a large difference between those who have never been labeled and those who have. This raises the issue of relative and absolute labeling effects. While absolute labeling effects refer to comparisons between those who are and are not labeled, relative effects refer to comparisons between those with different types of labels, such as arrest versus conviction (Paternoster & Iovanni, 1989).

Tittle (1980) also felt that adequate controls had not been used and argued that there was likely a selection bias among those sentenced to prison and those sentenced to probation. Specifically, the courts may sentence individuals to prison rather than probation because they are seen, for a variety of reasons, as more likely to recidivate. If these factors are not controlled in tests of labeling, then it is possible that pre-selection alone accounted for differences in recidivism.

Overall, Tittle (1980) did not find much support for the labeling perspective, but he thought it premature to reject the idea that labels have some effect on criminal behavior. Tittle (1980) felt that there had not been an adequate test of the perspective and that labeling theory's poor formulation almost invited falsification because specific hypotheses are required before tests can be conducted. He suggested the importance of specifying the circumstances under which labeling may increase crime. Tittle's disappointment with both labeling theory and the research presuming to test it, can best be summarized by his concluding comments:

Labelists must get down to serious theoretical business. Evasiveness, lauding of ambiguity, and hiding behind a façade of sensitizing concepts will no longer suffice. Researchers, on the other hand, must apply themselves with more facility and care. The meagerness and sloppiness of research on a question of this importance is embarrassing (1980).

Paternoster and Iovanni's (1989) review of labeling discussed nine secondary deviance studies (Farrington, 1977; Fischer & Erickson, 1973; Gold & Williams, 1969; Klein, 1974; McEachern, 1968; Meade, 1973; Thomas, 1977; Thomas & Bishop, 1984; Thornberry, 1971) as a whole, making general comments about the group of studies rather than discussing each in greater depth. Each of these studies claimed to examine the impact of labels on deviance amplification. Paternoster and Iovanni (1989) argued that these and similar studies have contributed to the downfall of labeling theory by relying on inadequate methodologies and thus did not constitute valid tests of the theory. Most of these studies used sanctions experienced by only a small proportion of deviants (e.g. juvenile court appearance, probation, incarceration) as the independent variable, examined relative rather than absolute labeling effects, and were formulated simplistically, without considering intervening mechanisms.

Paternoster and Iovanni (1989) concluded by saying that while many empirical tests are inconsistent with labeling propositions, more theoretically relevant work is needed in the future. Similar to Tittle (1980), Paternoster and Iovanni (1989) felt that most tests of labeling were not adequate and thought it was necessary to examine the contingencies and intervening mechanisms that may be at work.

In the most recent review, Bernburg (2002) examined three studies using samples containing only offenders and five studies using a general population including both those who have and have not been labeled. The three offender sample studies (Horwitz & Wasserman, 1979; D. A. Smith & Gartin, 1989; D. A. Smith & Paternoster, 1990) produced inconclusive results, and Bernburg (2002) suggested that studies based on nonrandom samples of offenders may not be valid tests of labeling theory. He claimed that the best tests of secondary deviance would be longitudinal, would sample from a population with some labeled and some unlabeled individuals, and would control for prior criminal behavior. Other important controls, including race and socioeconomic status, were also suggested.

Bernburg (2002) proceeded to examine five studies (Farrington, 1977; Hagan & Palloni, 1990; Palamara, Cullen, & Gersten, 1986; Ray & Downs, 1986; Thomas & Bishop, 1984) that he felt were more appropriate for testing labeling theory because they used general population samples. Each of these studies showed some support for the labeling proposition. While these are not methodologically perfect, it was suggested that more confidence should be placed in these than the previous three. In Bernburg's (2002) review, the strongest tests provided the most support for the perspective.

The three previous literature reviews seem to reach the same conclusion. That is, many empirical tests of secondary deviance are inadequate and may have led to its decline in popularity as a theory of criminal behavior. Despite these claims, labeling currently receives little attention and many criminologists would argue this is because of the lack of empirical support for the perspective. Examining past reviews leads us to a different conclusion. While it does not appear that labeling has received overwhelming support, researchers have argued that most tests are not appropriate (Paternoster & Iovanni, 1989; Tittle, 1980) and that the most rigorous tests tend to show more support for the perspective (Bernburg, 2002). At the same time, no one has attempted to synthesize the full body of work. Until this is done, we cannot be sure exactly what the prior research says about the effects of labeling. The present research attempts just such a synthesis.

## **Study Acquisition**

### **Selection Criteria**

Sixty-one empirical studies of the official sanction-recidivism relationship are reviewed here (listed in Appendix A). To qualify for inclusion in this assessment, the articles are not required to have the declared purpose of examining labeling theory, but rather only need to report at least one association between a measure of official sanctioning and recidivism. Because labeling theory has waned in popularity over the past 20 years, most of the studies reported here were not primarily aimed at testing the theory but nevertheless report associations between relevant variables. Indeed, many of these studies could be regarded as tests of special deterrence effects as much as tests of labeling theory. In selecting studies for inclusion in this review, the following criteria are applied:

1. Each study is required to measure at least one of the following official sanctions: arrest or police contact, conviction, contact with the juvenile justice system, or incarceration (either jail or prison). Informal sanctions, such as those originating in the family or school, are excluded. Because the purpose here is not to assess which measure is most effective at reducing recidivism, treatment and less severe sanctions, including a variety of types of community control, are also excluded. Those comparing different sentence combinations are not examined, but if the study presents separate data on an individual sanction type, then it is used.

2. Each study is required to measure actual recidivism. Studies examining the likelihood of future offending are excluded.

3. Each study has to make a clear distinction concerning the specific type of sanction imposed. Studies using a general sanction measure, which, for example, may include anything from school sanctions to incarceration, are excluded.

4. Each study has to have been conducted in the United States. This criterion is established to avoid variations that may arise due to cultural disparities or differences in punishment practices and philosophies.

5. Each study has to be available in English.

### **Study Acquisition Methods**

The sixty-one studies reviewed here were located in a systematic search of interdisciplinary internet databases, including Article First, WorldCat, the Criminal Justice Periodical Index, the National Criminal Justice Reference System, Cambridge Scientific Abstracts—Social Sciences, and the Web of Science and also from the bibliographies of previously retrieved labeling articles.

Appendix A provides an overview of each study that meets these selection criteria. The studies are listed in chronological order, starting with the oldest. For each study, the following information is reported by column: author, sample, population, criminal history control, statistics used, length of follow-up period, original offense type, official sanction, recidivism measure, and findings.

The author column includes the authors and date of publication of each study. The sample column contains information regarding both the size and nature of the individuals involved in the research. An indication of whether juveniles or adults were the participants is

listed in the population column. The criminal history control column indicates whether and how the authors controlled for prior record, such as the number of prior arrests or the existence of a prior incarceration. Next, a column specifies which statistical technique was used to arrive at the estimates. The follow-up period column includes the length of time between the sanction and recidivism measures. That is followed by information on what type of crimes were included in the analysis, because some studies focused exclusively on specific offenses. The official sanction column specifies the label, or how the independent variable was operationalized. The recidivism measure column indicates how recidivism was measured, or the operationalization of the dependent variable. Finally, the findings column indicates the direction and significance of the relationship between official sanctions and recidivism.

## **Literature Review Issues**

### **Methodological Issues**

Different outcomes across studies may be partially attributable to variation in methodological procedures. The studies differed on the following characteristics: follow-up period, controls for criminal history, sample size, recidivism measure and statistical method. Follow-up periods ranged from none, due to cross-sectional data, to ten years. Studies with a short follow-up period should be less likely to find support for the labeling hypothesis because an individual may not have had enough time to reoffend. Because labeling is a dynamic and developmental theory by nature, it could be argued that the use of longer follow-up periods would provide a more accurate test of the perspective.

Prior deviance is generally assumed to be the most important potential confounding variable in studies examining secondary deviance. If those who are labeled also have the most serious prior records, then the association between labels and recidivism may be entirely spurious. Researchers use a variety of techniques to account for this relationship including random assignment to sanction, binary measures where either a prior record existed or not, counts which tabulate the total number of prior arrests or incarcerations, and weighted counts, which take into account the frequency and severity of prior criminal acts. Because the relationship between prior and future behavior is strong, we should consider those studies with the most sophisticated controls (weighted counts) for prior behavior to be superior to those that have weaker or no controls at all.

In addition to variation in controls for prior record, the sample sizes varied greatly from 50 to over 200,000 cases. Studies with a larger sample size should be more likely to produce significant findings than those with only 50 individuals, for example. Small sample sizes lead to unstable and inefficient estimates. Because of this, more weight should be given to studies with the larger samples.

There is also variation in the measurement of recidivism. Some researchers use self-report measures of criminal involvement, and others use official measures, such as arrest, conviction or incarceration. In the studies presented here, arrest is the most commonly used indicator of recidivism. We may expect self-report data to provide a more accurate test of deviance amplification because it will tap into subsequent unreported criminality as well as that which is officially recognized. Labeling theory argues that official intervention may increase the likelihood of reoffense, not necessarily apprehension, and only self-report measures allow us to examine criminal behavior unknown to the authorities. It should be noted, though, that self-report data is not perfect. People may underreport their own deviant acts, and may do so in systematic ways relevant to labeling hypotheses. In particular, those who are actually most deviant may also underreport the most severely. This could artificially reduce support for labeling by concealing increases in deviant behavior following a sanctioning experience.

Finally, while logistic regression was the most common statistical technique used, it was not the only one. Many researchers relied solely on descriptive statistics, such as frequencies and percentages, while others used analysis of variance, Cox proportional hazards regression, ordinary least squares regression and Poisson regression. Although no single method can be declared the best technique for examining the labeling hypothesis under all circumstances, more weight can be placed on the cumulative findings if they are based on research using statistical methods suited to the data. Estimates based on multivariate techniques should be given more weight than those relying solely on descriptive statistics.

### **Substantive Issues**

Several circumstances that varied across the studies may condition the effect of official sanctions on recidivism, including type of punishment, population examined, the type of criminal activity, and type of offender. The type of label imposed, or punishment, ranges from recorded police contact to incarceration to length of incarceration. The more severe sanctions should be

more likely to lead to future offending. For example, it would be expected that those incarcerated would be more likely to engage in secondary deviance than those merely arrested. Similarly, it is arguable that the longer the period of incarceration, the more likely it would impact an offender's identity, structural opportunities, or peer associations. Because these are the three major hypothesized mediators of labeling effects, it is plausible that longer terms of incarceration would be more likely to lead to reoffense. While researchers have argued that even contact with the authorities can induce labeling effects (Bernburg & Krohn, 2003; Gold, 1970; Gold & Williams, 1969; Klemke, 1978; Paternoster & Iovanni, 1989; Thomas & Bishop, 1984), the more serious interventions should be more likely to produce such an effect (Paternoster & Iovanni, 1989; Tittle, 1980).

It could be argued that juveniles are more malleable and thus more likely to internalize a label than adults. If this is the case, then we should expect stronger labeling effects in studies using juvenile samples. While most studies examine either juveniles or adults, a few studies examine the effect of adolescent labels on early adulthood criminality. These will be considered juvenile for the purpose of this paper because they examined the impact of a label received in childhood.

Labeling effects may also vary by crime type. It has been argued for over thirty years that labelists need to start examining what individual characteristics, including crime type, condition the effects of a label (Tittle, 1975). While many studies examine any crime that leads to a label, others focus exclusively on a certain crime, such as domestic violence or drunk driving. While theorists have not specified if those engaging in certain types of crimes would be more affected by being labeled, this is an issue that should be explored.

Additionally, certain types of offenders may be more prone to labeling effects than others. It was noted in the previous chapter that the initial labeling event is probably the most consequential. While not fully developed, there is some consensus that naïve offenders will be more vulnerable to the negative consequences of official sanctions. If this is true, then studies of first-time offenders should produce more findings supportive of labeling theory than those using samples of repeat offenders.

Because the studies vary on many methodological and substantive issues, some would recommend conducting a true meta-analysis. This was not done here for a variety of reasons. The primary problem with conducting a meta-analysis for this particular paper involves the

inability to measure effect sizes for a large proportion of the estimates. In a recent book on conducting meta-analyses, Lipsey (2001, p. 16) states that while some findings can be appropriately converted into effect sizes as needed in a meta-analysis, “the major exceptions are findings generated by multivariate analysis.... Meta-analysts have not yet developed effect size statistics that adequately represent this form of research finding, and indeed, their complexity and diversity across studies with regard to the selection of variables involved may make this impossible.” This would require dropping over 60% of the estimates reported here (they cannot be converted into comparable effect sizes due to variation in controls), leading to a smaller and more biased sample, and conducting a meta-analysis with the remaining bivariate findings. This type of data would not be useful for an examination of labeling theory because it is essential that prior criminal or delinquent behavior be controlled<sup>3</sup>.

### **Findings**

Before discussing the overall findings, the procedures used for deciding which estimates were included from each study will be discussed. Multiple findings from the same study were only counted if they were substantively different pairings of the independent (label) and dependent (recidivism) variables. Additional findings were included if they served to illustrate an important point, such as the differential impact a labeling event may have for certain types of offenses and offenders.

For example, if a study initially reported results of the official-sanction recidivism relationship for all initial crimes and also broken down by type (violent, property and drug, for example), then each estimate was counted so that this potential contingency could be examined. For the purpose of comparability, if the recidivism offense was recorded in this manner, only the

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<sup>3</sup> Even if I chose to ignore Lipsey and attempt to convert estimates from multivariate analysis to effect sizes, the problems would not be resolved due to the relatively small sample size of labeling effect relationships involved. This combined with the number of control variables that would be required to correctly specify the model creates substantial difficulties. At present, there are already over 30 characteristics included in the summary tables. This number would have to increase considerably to control for all the methodological variations that could impact the effect sizes. The number of necessary controls may then actually approach the number of relationship estimates. Additionally, Hunter, Schmidt, and Jackson (1982, pp. 33-34) note that, “slopes and covariances are comparable across studies only if exactly the same instruments are used to measure the independent and dependent variables in each study. It is a rare set of studies in which this is true.” The studies examined here are no exception; a variety of sanctions are used to represent a labeling event and similar sanctions are not always measured the same way. The same is true of the recidivism measures. If the findings are not comparable, then synthesizing them through meta-analysis is meaningless.



estimate which did not differentiate crime type was used (only 2 studies broke down the estimates by recidivism offense). Multiple estimates were also counted if results were presented separately for relatively inexperienced and repeat offenders. These criteria for inclusion were selected because there were several studies that made these distinctions. It would have also been desirable to do the same for gender, race and socioeconomic status, but this was not possible. While many studies control for race and socioeconomic status, most do not assess the labeling effect separately for those belonging to these different groups (for a recent exception, see Bernburg and Krohn 2003, where interaction terms for both race and poverty are assessed).

When different models were reported that varied only by the number of independent or control variables included, then the estimate from the most complete model was used. The more specific criteria listed below were also applied.

1. If findings were established by different statistical techniques in one study, then the method that seemed most appropriate for the hypothesis or the one the author presented as the best test was used. This issue primarily involved logistic regression and survival analysis. If the authors presented both results, then the logistic estimates were used because labeling theory does not explicitly state that the time to reoffense should be impacted by labeling.

2. If papers presented multiple follow-up periods, then the estimate from the longest follow-up period was used.

3. Because controlling for potential mediators, such as employment or self-concept subsequent to the labeling event, could hide a portion of a label's effect, estimates from models including these potentially intervening variables are excluded. In each case a model without the intervening variable was presented, so no entire study was excluded for this purpose.

4. If a labeling variable included more than 2 categories (dismissal, conviction and incarceration, for example), then it was coded with respect to the most severe sanction (incarceration, in this case). If estimates were reported for different lengths of incarceration (rather than a continuous length variable), then the results for the longest and shortest sentences were compared.

5. Similarly, if a study provided multiple results for those with varying degrees of prior records, then the results were counted for the categories of least and most experienced. This is primarily presented as a dichotomy and does not present a problem. A few studies provide results based, for example, on how many arrests the offender previously had. In these cases,

those with lowest number of previous arrests and the maximum number of previous arrests were counted, but not those in between.

6. Finally, two studies (Babst, Moseley, Schmeidler, Neithercutt, & Koval, 1976; Gottfredson, Gottfredson, & Garofalo, 1977) examined offenders separately based on risk ratings. Because these data are not comparable to the bulk of the other studies, only the overall relationship was recorded. Additionally, one study hypothesized a nonlinear relationship between sanctions and recidivism (Orsagh & Chen, 1988) and reported findings based on both a linear and a nonlinear model. Because labeling theory predicts a linear relationship, only those estimates were examined.

The 61 studies yield 138 countable estimates of the official sanction-recidivism relationship. Overall, the labeling hypothesis receives more support than its logical opposite, specific deterrence (positive and significant findings indicate that the net effect of sanctioning is characterized more by deviance amplification than deterrence). Of the 138 total estimates of the official sanction-recidivism relationship, only 13.8% of the findings are negative and significant, 23.9% are negative of unknown significance or not significant while 27.5% are positive and significant and 34.1% are positive of unknown significance or not significant. Although the preliminary evidence here provides more support for secondary deviance than deterrence, there is more support for no effect (either deterrence or amplification) because the majority of findings are not statistically significant. To further assess the labeling evidence, I next examine the direction and significance of these relationships in different methodological (Table 3.1) and substantive (Table 3.2) contexts.

### **Methodological Contexts**

Studies vary in ways of controlling for criminal history and it is generally assumed that studies not taking account of past criminality are weaker tests of labeling theory because past behavior is a strong predictor of future behavior. As Table 3.1 shows, approximately 36% of the relationships reviewed here do not control for criminal history. Of these, 28% are positive and significant. Estimates using a binary variable (a record exists or not) are only 13.3% positive and significant. Of the studies using a count measure, such as number of prior arrests, 31% are positive and significant. The most sophisticated control, a weighted count that includes both frequency and severity of prior record, produces only 16.7% positive and significant findings,

Table 3.1. Summary of Direction and Statistical Significance for Labeling and Recidivism Relationships under Varying Methodological Conditions

	Total # Findings	Percent of Findings					
		- Signif	- Ns	-?	+ Signif	+ Ns	+?
Total	138	13.8	21.0	2.9	27.5	26.1	8.0
Control criminal history							
None	50	8.0	20.0	6.0	28.0	24.0	12.0
Random assignment	2	50.0	0.0	0.0	50.0	0.0	0.0
Binary	15	33.3	26.7	0.0	13.3	26.7	0.0
Count	65	13.8	20.0	1.5	30.8	30.8	3.1
Weighted count	6	0.0	33.3	0.0	16.7	0.0	50.0
Sample size*							
1,000 or less	64	21.9	23.4	1.6	20.3	26.6	6.3
More than 1,000	24	4.2	16.7	0.0	45.8	29.2	4.2
Follow-up period*							
1 year or less	14	14.3	28.6	0.0	14.3	21.4	21.4
More than 1 year	73	16.4	20.5	1.4	30.1	28.8	2.7
Recidivism measure*							
Self-report	10	0.0	0.0	0.0	60.0	10.0	30.0
Arrest/contact/report	52	15.4	30.8	1.9	21.2	28.8	1.9
Conviction	14	21.4	14.3	0.0	21.4	35.7	7.1
Incarceration	2	50.0	0.0	0.0	0.0	50.0	0.0
Charges filed	8	37.5	12.5	0.0	25.0	25.0	0.0
Unfavorable parole	2	0.0	0.0	0.0	100.0	0.0	0.0
Statistical Method*							
Logistic regression	57	26.3	21.1	0.0	19.3	33.3	0.0
Other multivariate	25	0.0	28.0	4.0	36.0	12.0	20.0
Descriptive	6	0.0	0.0	0.0	66.7	33.3	0.0

\*Includes only estimates that control for criminal history

out of a total of six reported relationships (50% were positive and of unknown significance). While the pattern is not entirely clear, these findings appear to provide only mixed support for labeling theory. It is important to note that the category with the best control for prior behavior, weighted count, contains only 6 estimates. The strongest support for labeling theory is found in estimates that used a count measure to control for prior criminal or delinquent history. The next most supportive are those that did not control for prior behavior at all, which is not surprising because past behavior is an important predictor of future behavior. Models that included a binary control for prior criminal history were more supportive of deterrence than labeling. While there is no clear picture here, it appears that the type of control used may impact the outcome. Future researchers should be attentive to this issue and use the most rigorous measure of criminal history possible.

Criminal history is arguably the most important variable to control when examining labeling effects. As shown above, studies that did not include such a control were among the most supportive of the theory. Because of this, all findings discussed below only include studies that controlled for prior behavior in some manner. This reduced the total sample size from 138 to 88 estimates of the relationship.

Apparent support for labeling effects is found when studies are compared by sample size. The stronger estimates, those with samples of more than 1,000, produce 45.8% positive and significant findings and only 4.2% negative and significant. On the other hand, studies that consisted of 1,000 or less participants produce only 20.3% positive and significant findings and 21.9% negative and significant findings.

As expected, studies with longer follow-up periods (more than 1 year) show stronger support for labeling than those which used short follow-ups. While only 14.3% of the short follow-up studies have positive and significant results, 30.1% of those studies that tracked offenders for more than 1 year find a positive relationship between labeling and recidivism. At the same time, studies with longer follow-up periods also report more negative and significant findings (14.3% in short follow-ups and 16.4% in long follow-ups). Unfortunately, 21.4% of the studies with short follow-up periods do not report significance levels.

Self-report measures of reoffending produce findings more supportive of secondary deviance than any of the official measures, with 60% positive and significant. Additionally, none of these estimates were supportive of a deterrent argument. This is one of the only

occasions when the majority of findings are actually in support of labeling theory. This may point to the fact that many individuals reoffend after being officially labeled, but may not get back into the system. By this rationale, we would also think that arrest would have more supportive findings because it is the first step of several in the system, and some individuals may never go past this initial police encounter. The data are less supportive when arrest is used as the recidivism measure, with only 21.2% positive and significant compared with 15.4% negative and significant. The results are similar for conviction.

Finally, some variation across statistical methods is also found. Logistic regression, which is the most commonly used technique (two-thirds of the estimates), yields 19.3% positive and significant results and 26.3% negative and significant. Estimates produced by logistic regression, which is arguably the most appropriate statistical technique to use in assessing labeling theory, are actually more supportive of a deterrent than a labeling effect. Other multivariate techniques, which included ordinary least squares regression, survival analysis, structural equation modeling, among others, produce 36% positive and significant findings. Estimates arising out of descriptive statistics, such as frequencies and percentages, are 66.7% positive and significant.

### **Substantive Contexts**

Variable patterns in labeling effects can also be seen along more substantive grounds. As Table 3.2 shows, among types of official sanctions, overwhelmingly the strongest support is found for juvenile justice intervention (83.3% positive and significant), though the sample size is quite small. It is the only case here in which the majority of the findings are actually in support of labeling hypotheses, but of the small sample size, these findings should be interpreted cautiously. Being arrested yields little support, with only 26.7% of the estimates positive and significant compared with 20% negative and significant. While incarceration was found to be more likely to have a labeling effect (22.6%) than a deterrent effect (6.5%), no support is found for labeling effects in relation to the length of incarceration. In fact, for this sanction 23% of the findings are supportive of labeling, 30% are supportive of a deterrent hypothesis. While labeling theory would predict that more severe sanctions, such as conviction and incarceration, would be more likely to lead to secondary deviance than mere arrest, it would also predict that longer

Table 3.2. Summary of Direction and Statistical Significance for Labeling and Recidivism Relationships under Varying Substantive Conditions

	Total # Findings	Percent of Findings					
		- Signif	- Ns	-?	+ Signif	+ Ns	+?
Total	138	13.8	21.0	2.9	27.5	26.1	8.0
Official sanction*							
Arrest/contact	15	20.0	26.7	0.0	26.7	6.7	20.0
Conviction	6	16.7	0.0	0.0	16.7	66.7	0.0
Juvenile justice	6	0.0	16.7	0.0	83.3	0.0	0.0
Incarceration	31	6.5	16.1	3.2	22.6	45.2	6.5
Incarceration length	30	30.0	30.0	0.0	23.3	16.7	0.0
Population*							
Juvenile	19	5.3	15.8	0.0	47.4	10.5	21.1
Adult	69	20.3	23.2	1.4	21.7	31.9	1.4
Offense type*							
Not specified	39	15.4	17.9	0.0	35.9	20.5	10.3
Drugs	6	0.0	16.7	0.0	16.7	66.7	0.0
Domestic violence	18	38.9	22.2	0.0	5.6	33.3	0.0
Drunk driving	4	0.0	25.0	0.0	25.0	50.0	0.0
Violent	10	20.0	10.0	0.0	40.0	30.0	0.0
Property	11	0.0	45.5	9.1	27.3	9.1	9.1
Offender*							
Naïve	5	0.0	20.0	1.3	28.2	25.6	3.8
Experienced	5	0.0	20.0	0.0	0.0	60.0	20.0

\* Includes only estimates that control for criminal history

periods of incarceration would be more likely to lead to reoffense. The findings here obviously do not support this claim.

As expected, juveniles are more likely to recidivate after a labeling event than adults, with 47.4% of the findings positive and significant for juveniles and only 21.7% positive and significant for adults. Additionally, it appears that while adults have a nearly equal chance of a label leading to deterrence (20.3%) as amplification (21.7%), juveniles only experienced a deterrence effect in about 5% of the cases. There are at least two potential interpretations of this finding.

First, it is possible that juveniles' identities are more malleable than adults' and thus more subject to the negative consequences of stigmatization. If true, this would lend support to the labeling proposition that stigmatization may influence one's self-identity or structural opportunities. Another possibility, which would not support labeling theory, is that juveniles are more likely to reoffend than adults anyway because they are in the upward phase of the age-crime curve, while adults should be on the decreasing end of that continuum. Juvenile delinquency is expected to increase toward adulthood and peak around the late teens or early twenties (Vold, Bernard, & Snipes, 2002) regardless of sanction, and adult criminality is expected to decline with age.

While no offense type yielded a majority of findings in support of labeling theory, some differences can be noted. The strongest support is found for violent offenses (40% positive and significant) and property offenses (27.3% positive and significant). Studies examining drug or drinking and driving offenses yielded more support for labeling than deterrence, but were based on very small samples. In the case of domestic violence, much greater support was found for deterrence hypotheses than for labeling, where 39% of the estimates were negative and significant and only 6% were positive and significant. Domestic violence was the only crime type to produce more estimates in favor of deterrence than labeling. These findings seem to suggest that those involved in violent or property offenses are more likely to be negatively affected by legal intervention while those engaging in spousal abuse are less likely to experience such consequences. Future research could help further disentangle the relationship between crime type and labeling effects.

A difference was also found between studies that differentiated offenders in terms of experience level, whether they were first time or relatively naïve offenders or were experienced,

repeat offenders. Naïve offenders yield 28.2% positive and significant findings, while experienced offenders produce no positive and significant estimates. These results seemingly provide support for labeling theory and the hypothesis that labeling effects may diminish with repeated legal interventions. Due to small sample sizes ( $n=5$  for each group), these findings should be interpreted with caution.

One limitation to the above analyses is that the studies are examined only under one methodological criterion, other than criminal history, at a time. For example, if a study has a large sample but examines only bivariate relationships, it may not be the most well designed test. To examine this, Table 3.3 contains findings that have multiple design strengths. Beginning with controlling for criminal history, each set of findings adds an additional methodological criterion, and the sample size of estimates to be considered decreases. If the percent of findings in support of labeling theory increases as the methodological criteria are made more stringent, it will provide stronger support for the theory.

When only criminal history is controlled, 27.3% of the findings are in support of labeling theory. This sample is then reduced by including only those that also use multivariate statistical techniques and the support decreased slightly to 24.4% in favor of labeling. When the sample is further reduced by requiring large samples ( $n>1,000$ ), the percent of positive and significant findings increases to 45.8%. Finally, when the requirement of a long follow-up period (greater than 1 year) is added, the majority (52.9%) of the findings are in support of labeling hypotheses. This is almost ten times greater than the support for specific deterrence indicated by negative significant results. It appears then that the strongest studies provide the most support for the perspective.

It has been mentioned that most of the studies examined here are not ideal tests of labeling theory, but they do have merit. While most failed to examine either mediating or conditioning factors, for the most part, they controlled for important confounders (criminal history) and used multivariate techniques. These previous studies provide a starting position for new studies to build upon and improve. It is rare that empirical tests of criminological theory are able to use the most ideal measures and research designs. At the same time, some studies come closer to this standard than others. For this reason, the examination of the cumulative evidence should be accompanied by the findings from the studies that examined intervening mechanisms and/or potential contingencies of the relationship, Thomas and Bishop (1984), Bernburg and



Table 3.3. Summary of Direction and Statistical Significance for Labeling and Recidivism Relationships among Studies Meeting Multiple Methodological Criteria

	Total # Findings	Percent of Findings					
		- Signif	- Ns	-?	+ Signif	+ Ns	+?
Control criminal history	88	17.0	21.6	1.1	27.3	27.3	5.7
Control criminal history, multivariate	82	18.3	23.2	1.2	24.4	26.8	6.1
Control criminal history, multivariate, large sample,	24	4.2	16.7	0.0	45.8	29.2	4.2
Control criminal history, multivariate, large sample, follow-up>1 year	17	5.9	11.8	0.0	52.9	29.4	0.0

Krohn (2003), Bernburg, Krohn, and Rivera (2006), and Barrick, Bales, Bontrager, and Chiricos (2005). These studies closely followed the arguments of both labeling theorists and critics. Because of the strong methodologies, these studies should be given more weight than the other studies and will thus be discussed in greater detail.

First, using self-report data from a sample of both labeled and non-labeled juveniles, Thomas and Bishop (1984) found a small positive relationship between formal sanctions and later delinquency, controlling for prior delinquency. They proceeded to examine the impact of sanctioning on delinquent self-conceptions, which has been hypothesized to mediate the label-recidivism relationship. Thomas and Bishop found that while police contact is associated with an increase in delinquent identity, it only added one percent to the explained variance. They concluded that their study provided little support for labeling theory overall.

Nearly twenty years later, Bernburg and Krohn (2003) attempted to address many of the flaws in previous research by examining both intervening mechanisms and potential conditioning factors, such as race and poverty, under which a label may have an effect. They found that both

police and juvenile justice intervention are positively associated with early adulthood criminality measures and are partially mediated by employment and educational attainment. Additionally, they created interaction terms and examined potential conditions under which a label may have a stronger effect on recidivism. They found that in some, but not all situations, that labels have a stronger impact on those who are poor or black.

Using data from the same survey, Bernburg, Krohn and Rivera (2006) examined two different mediators of the sanction-recidivism relationship, gang membership and peer delinquency. They found that involvement with the juvenile justice system is associated with subsequent serious delinquency. This relationship was substantially mediated by gang membership and peer delinquency.

While Bernburg and colleagues (Bernburg & Krohn, 2003; Bernburg, Krohn, & Rivera, 2006) have examined potential mediators of the relationship, as well as some potential contingencies, Barrick et al. (2005) presented preliminary results of an attempt to tease out the situations in which labels may have their greatest impact, at both the individual and county level. Taking advantage of a unique situation in Florida, they were able to examine the impact of felony conviction on recidivism for a sample of male probationers, about half of whom were convicted of a felony while the others had “adjudication withheld”. Overall, they found that adjudication was associated with higher levels of recidivism. The effect of adjudication was found to be stronger for those who are white, older than 30, commit property offenses, and have no prior record. Additionally, the effect of adjudication was found to be relatively stable across counties with varying characteristics (percent Black, crime rates, and concentrated disadvantage).

Since 1975, Tittle (and others) has been requesting research to address both the intervening mechanisms and conditions under which a label has detrimental consequences. The foregoing studies have addressed some of these concerns. If conclusions are to be drawn about the cumulative evidence, more weight should be given to the most methodologically sound studies. While conclusive remarks on the viability of the perspective are not possible, there seem to be reasons to continue work in this area. These most recent findings stand as a request for continued rigorous testing of labeling hypotheses. If these findings can be replicated with different data and populations, then we may see a resurgence of interest in the labeling paradigm.

## Statistical Analysis

Although at first glance it looks like there is some support for deviance amplification, especially when the strongest methodologies are employed, three statistical analyses were performed to determine whether these apparent effects may be due to chance. These tests were run both on the full sample (n=138) and the reduced sample (n=88) that included only those estimates in which a control for prior criminal behavior was employed. Because there were no substantive differences in these findings, only the results from the reduced sample are discussed. First, the sign test<sup>4</sup>, which ignores statistical significance but tallies the number of findings in each direction, was conducted. If the null hypothesis is true, then we would expect the number of positive and negative findings to be equal. Providing support for labeling, the results indicated that the set of estimates contained significantly more positive findings than could be obtained by chance alone if the null hypothesis were true. The one-tailed test was significant at  $p < .03$ .

Next, the Adding Zs test<sup>5</sup> was performed because it takes account not only of the direction of each finding but also its significance level. This test addresses the shortcomings of the sign test by accounting for relationship strength. There is less than a .002 chance that a set of findings this significant could be obtained by chance alone if the null hypothesis were true. This more rigorous test provided additional support for the relationship between sanctions and recidivism. Another version of this test was run that also weights findings by sample size, and the results were nearly identical ( $p < .001$ ).

Finally, the Fail-Safe N test<sup>6</sup> was conducted to determine how well these results would hold up if new findings were introduced. Cooper (1998) noted that significant findings are more likely than nonsignificant findings to be published and thus included in analyses such as this. Because results do not have the same chance of being included, it is important to estimate how many of these it would take to reverse the outcome. The analysis shows that it would take 192

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<sup>4</sup>  $Z_{vc} = (N_p) - (1/2 N) / \frac{1}{2} \sqrt{N}$ ; Where  $Z_{vc}$  = standard normal deviate for the overall series of findings,  $N_p$  = number of positive findings,  $N$  = total number of findings (Cooper, 1998, p. 118).

<sup>5</sup>  $Z_{st} = (\sum Z_i) / \sqrt{N}$ ; Where  $Z_{st}$  = standard normal deviate for the overall series of finding,  $Z_i$  = standard normal deviate for the  $i$ th finding,  $N$  = total number of findings) (Cooper, 1998, p. 120).

<sup>6</sup>  $N_{FS,05} = ((\sum Z_i) / 1.645)^2 - N$ ; Where  $N_{FS,05}$  = number of additional null-summing findings needed to raise the combined probability to above  $p < .05$ ,  $Z_i$  = standard normal deviate for the  $i$ th finding,  $N$  = total number of findings, 1.645 = standard normal deviate associated with  $p < .05$  (one-tailed) (Cooper, 1998, p. 123).

undiscovered null findings (or findings that averaged out to zero effect) to overturn the conclusion that official sanctions impact recidivism. Because an attempt was made to be comprehensive and only 138 findings were uncovered, it is relatively unlikely (but possible) that there are nearly 200 null findings excluded from this review.

### **Conclusions**

Labeling proponents argue that being publicly defined as a deviant will increase the likelihood of future deviant behavior. Critics of the perspective argue that the empirical evidence generally does not support the notion of secondary deviance. A systematic examination of research findings that bear upon the question show that while deviance amplification is not supported by a majority of the estimates, it receives more support than its antithesis, specific deterrence, under most conditions and when tested in the most appropriate manner. Overall, there were significantly more findings supportive of the proposition than against it, and the Adding Zs assessment was significant at  $p < .002$ . Additionally, it would take 192 estimates supporting the null hypothesis of no relationship to overturn this conclusion. The evidence presented here suggests that formal labels may increase subsequent criminality under certain circumstances.

It also appears that the impact of a labeling event may be more detrimental to certain types of people. More support was found for juveniles than adults and those involved in violent and property crimes than perpetrators of domestic violence. Additionally, naïve offenders are more likely to be negatively impacted by social sanctions than those more experienced offenders. While many of these results were anticipated, the crime type question was more exploratory in nature. In addition to the characteristics that were examined here, theorists have suggested that other traits, such as race and ethnicity, socioeconomic status, stakes in conformity, gender, and criminal history, may condition that impact of a label on recidivism. Unfortunately, there were not enough studies examining these issues to warrant inclusion in the tables. At the same time, some of the studies did attempt to examine some of these contingencies. Because of their theoretical importance, the findings will be briefly discussed below.

Both race and social class have been hypothesized to interact with labeling effects, but empirical evaluations of these claims are rare. Bernburg and Krohn (2003) examined the impact of race and poverty on labeling outcomes. It was mentioned above in the discussion of the most

theoretically informed studies that they found that, for juveniles, official intervention has a stronger impact on blacks and on those who are impoverished in some (but not all) situations. Sherman et al., (1992) found that the interaction of race and arrest was not significant in predicting recidivism in Milwaukee. In a study examining data pooled from four sites (including Milwaukee) involved in the domestic violence replication studies found that the labeling effect of arrest on recidivism was 11% higher for blacks than whites (Berk, Campbell, Klap, & Western, 1992).

Stakes in conformity has been given more attention than race and socioeconomic status in terms of labeling effects. In a study on specific deterrence, DeJong (1997, p. 569) found that, “individuals with few bonds to society (job, family, education) are more likely to recidivate following a period of incarceration.” Additionally, three of the domestic violence experiments found that arrest had a greater deterrent effect for employed offenders (Berk, Campbell, Klap, & Western, 1992; Pate & Hamilton, 1992; Sherman, Smith, Schmidt, & Rogan, 1992). Two of the studies also found some evidence that arrest may increase reoffense among those who are unemployed (Pate & Hamilton, 1992; Sherman, Smith, Schmidt, & Rogan, 1992). Finally, Berk et al. (1992) examined three-way interactions of race, employment and arrest and found that the effect of arrest on recidivism was 47% higher for unemployed blacks than for employed whites. The marriage X arrest interactions were not significant in any of the reports, but there are theoretical reasons to examine it further. While the results may not be generalizable to sanctions other than arrest or perpetrators of other offenses, they provide guidance for offender characteristics that may warrant further examination.

Because of these findings and the theoretical expectations described in the previous chapter, studies of labeling theory should examine interactions between these individual traits and official labels or disaggregate the samples along these lines and perform separate analyses. The findings from such research would be important for both furthering theoretical development and informing policy.

In addition to specifying contingencies under which labels may increase crime, we also need to empirically examine the intervening mechanisms that are at least partially responsible for this effect. Some theorists argue that stigmatization can alter one’s identity, others argue that labels may limit one’s structural opportunities, such as education and employment, in society, and others argue that labels impact one’s peer associations. Only a few studies to date have

attempted to test the full process by which a label may have an effect. While recent empirical attention has focused on structural opportunities and peer groups as potential intervening mechanisms, it is also important to return to the original symbolic interactionist roots of labeling theory. Future research should attempt to both replicate the work of Bernburg and colleagues with different populations and also to examine whether changes in personal identity mediate the sanction-recidivism relationship. Empirically disentangling the process by which labels may impact subsequent behavior is an important task for both researchers and labeling theorists.

Finally, it has been suggested before (Bernburg & Krohn, 2003; Paternoster & Iovanni, 1989) and was illustrated in the findings here, that using general populations (including both labeled and non-labeled individuals) with self-report measures of deviant behavior may be the most appropriate in examining labeling hypotheses. When official data is used, it is important to examine potential problems with selection bias because it is likely that there are uncontrolled factors that may predict both sanction and recidivism (D. A. Smith & Paternoster, 1990; Tittle, 1980).

Naïve offenders, who are less likely to be found in offender-only samples, were found to be potentially more vulnerable to labeling events than their more experienced counterparts and findings from self-report recidivism measures were all in the expected direction. Panel studies would meet these requirements and also allow us to control for potential deterrent effects. While collecting data from panel studies is generally expensive, it would make for the most appropriate test of labeling theory. In the absence of such data, examining conditioning factors or intervening mechanisms will provide empirical evidence needed to appropriately test the perspective.

Although some pronounced the perspective dead twenty years ago, labeling theory has received some renewed theoretical and empirical attention in recent years. A quantitative assessment of studies examining the impact of arrest, conviction, juvenile justice intervention, and incarceration on recidivism provides modest support for the hypothesis that official sanctions, in certain situations, may increase subsequent deviance. While the findings are still relatively inconclusive, potential for the perspective has been found. What we need to see now is a revival in empirical tests of labeling theory that are attentive to the primary flaws of the previous work. The policy implications are too great to leave these questions unanswered.

## CHAPTER 4

### RESEARCH METHODOLOGY

#### Hypotheses

As the previous chapters illustrate, there have been relatively few studies that adequately test the labeling proposition that formal sanctions increase the likelihood of recidivism. One of the major flaws of the extant literature is the failure to adequately follow the theoretical propositions. Previous theoretical work (and some recent empirical studies) have emphasized that labeling effects may be contingent on characteristics of offenders. Although some of the original labelists discussed this possibility, it has only rarely been subjected to empirical examination. This study seeks to fill that gap in the literature by examining the consequences of one formal sanction, felony conviction, on reconviction across a variety of individual contexts that have either been suggested by theorists or found in empirical analyses. The hypotheses that address this issue are discussed below. First, it is necessary to briefly introduce the relatively unique primary variable of interest, adjudication.

In Florida, it is possible for a person to be found guilty of a felony and avoid being formally labeled as a convicted felon. This is accomplished if the judge chooses to sentence an individual to probation with “adjudication withheld.” This discretionary step withholds the certification of guilt and allows someone who has actually been found guilty by a judge or jury to legally assert that they have not been convicted of a felony. When the decision is made to withhold adjudication, no civil rights are lost and such individuals may legitimately say on employment applications and elsewhere that a felony conviction did not occur.

The first step here concerns establishing whether in fact the labeling event of felony conviction has any effect on subsequent recidivism. A primary prediction of labeling theory would be that those who receive the felon label will be more likely to recidivate in the future.

*Hypothesis 1: Probationers who are adjudicated guilty will be more likely to be reconvicted than those who have adjudication withheld.*

Labeling theory and research have also suggested that the relationship between sanctions and recidivism may vary across individual contexts. While the existence of these contingencies has been recognized, there are theoretical disagreements as to what effect certain characteristics should have and little empirical evidence to provide satisfactory answers. In an attempt to fill

the gap in research in this area, the issue of whether labeling effects on recidivism are contingent on characteristics of the offender will be further explored.

Although race was one of the earliest contingencies to be recognized by labelists, there are opposing hypotheses as to its impact on labeling effects. While it has been argued that minorities and other disadvantaged groups should be both more (Bernburg & Krohn, 2003; Sampson & Laub, 1997) and less (Ageton & Elliott, 1974; Harris, 1976; Jensen, 1972) vulnerable to labeling effects, the limited research seems to be more supportive of the latter (Berk, Campbell, Klap, & Western, 1992; Bernburg & Krohn, 2003).

*Hypothesis 2: The effect of adjudication on reconviction will be greater for blacks than for whites.*

*Hypothesis 3: The effect of adjudication on reconviction will be greater for Hispanics than for whites.*

There is a similar disagreement over the relationship between stakes in conformity and labeling outcomes. The argument centers around whether those who care more about and are more invested in conventional society (frequently operationalized as marriage, education, and employment) will be more or less vulnerable to the negative consequences of labeling (Sherman, Smith, Schmidt, & Rogan, 1992). Generally, the empirical evidence is more supportive of the less vulnerability hypothesis, that labeling effects will be stronger for those with low stakes in conformity (Berk, Campbell, Klap, & Western, 1992; Dejong, 1997; Pate & Hamilton, 1992; Sherman, Smith, Schmidt, & Rogan, 1992). Due to limitations in the data, only employment status is available for this study.

*Hypothesis 4: The effect of adjudication on reconviction will be greater for those who are unemployed than for those who are employed.*

While sex has also been mentioned as a potential contingency, it has been substantially overlooked in both theoretical and empirical terms. Like race and stakes in conformity, opposing hypotheses have been proposed about the impact of gender on labeling consequences. It has been proposed that the negative consequences of labeling will be (a) greater for males (Ageton & Elliott, 1974; Keane, Gillis, & Hagan, 1989); (b) greater for females (Ray & Downs, 1986); and (c) the same for males and females (Simons, Miller, & Aigner, 1980). Because the theoretical arguments are weak and conflicting and empirical findings are nearly nonexistent, a directional hypothesis cannot be drawn at this point.



*Hypothesis 5: The effect of adjudication on reconviction will vary by sex.*

It has also been argued that an individual's prior record may impact labeling consequences. Unlike the other individual traits, there appears to be consensus that labeling effects should be more consequential for naïve offenders rather than those with a prior record (Horwitz & Wasserman, 1979; Paternoster & Iovanni, 1989; D. A. Smith & Gartin, 1989). This proposition also received some support in the literature review provided in the previous chapter.

*Hypothesis 6: The effect of adjudication on reconviction will be greater for offenders without a prior record than for those with a prior record.*

Finally, it was mentioned early on by a labeling critic that labeling effects may vary by crime type (Tittle, 1975). Although labeling theorists never made predictions about perpetrators of which offenses would be most vulnerable to labeling, the prior literature review found evidence that violent offenders are most negatively impacted, followed by perpetrators of property, drunk driving, drug, and domestic violence offenses.

*Hypothesis 7: The effect of adjudication on reconviction will be greater for violent than for property offenders and greater for property than for drug offenders.*

### **Data and Variables**

This study will examine the reconviction experience of the population of men and women found guilty of a violent, property, or drug felony and sentenced to probation between 2000 and 2003 in Florida (N=119,648). The outcome in each analysis is operationalized by whether the probationer is reconvicted of a felony within 3 years of sentencing (1=yes). This involves a new offense for which an individual is sentenced to prison, probation or jail, and does not include technical violations of the terms of probation. This measure of recidivism only captures those actually found guilty of another offense, and not those who may have been arrested and charged, but had charges dropped or were found not guilty. Overall, 20.3 percent of the sample committed an offense that resulted in a felony conviction within three years of the initial sentence.

The primary independent variable of interest is whether adjudication has been formally applied (1=yes) to the offender; 37.7 percent of the sample were formally adjudicated. The models also control for a variety of variables that may be expected to impact both the

adjudication decision and recidivism, including demographic characteristics of the probationer, his or her crime, and prior record. Individual traits include age at sentencing, sex, race, and employment status at offense. The mean age at sentencing was 31 years and 73 percent of the sample was male (yes=1). Three dummy variables were created to represent the racial and ethnic composition of the sample; 35 percent of the sample was black and 12 percent Hispanic. White is used as the reference category in the analyses. The Florida Sentencing Guidelines data base was used to identify offenders as either non-Hispanic blacks (yes = 1) or Hispanic of whatever race (yes = 1). The surnames of all offenders who were not identified as Hispanic from the Guidelines data were checked against the U.S. Census list of Hispanic surnames (Word & Perkins, 1996) and any individual whose name matched one of those on the list was coded as an Hispanic defendant. Finally, 36 percent of the sample was employed at the time the offense was committed.

Dummy variables for crime type—property, drug, or violent (yes=1), were included. Those found guilty of “other” crimes were excluded from the analysis due to the large variation in the offenses included in this category. Violent crime is used as the reference category in the analyses. The seriousness of the crime is indicated by a score generated by the Florida Sentencing Guidelines. Points ranging from 4 to 116 are assigned to the primary offense, and additional points are assigned for additional offenses and a variety of other factors including victim injury or the use of a firearm. A related variable, which partially reflects crime seriousness, is length of supervision in months that is specified by the probation sentence.

Prior record is indicated by points that are assigned by the Florida Guidelines to offense specific prior convictions for a felony or misdemeanor as an adult or juvenile by a state, federal, military or foreign court. As with other research using Guidelines data, this affords an unusually comprehensive measure of an individual’s prior criminal record. Offenses that occurred more than ten years prior to the current offense are not scored if the individual has been conviction free for that time. Also included among our predictors of recidivism is whether the individual had previously violated the terms of supervision while on probation or community control

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Table 4.1. Description of Study Variables

	Mean	Std. Deviation
Reconviction	0.20	0.40
Adjudicated	0.38	0.49
Male	0.74	0.12
Hispanic	0.12	0.32
Black	0.35	0.48
White	0.53	0.50
Age	30.67	10.54
Violent Offense	0.23	0.42
Property Offense	0.47	0.50
Drug Offense	0.30	0.46
Employed	0.36	0.48
Prior Supervision Violation	0.23	0.42
Prior Record Points (ln)	-2.74	3.96
Crime Seriousness (ln)	2.98	0.07
Supervision Length (ln)	9.78	0.74
Inverse Mills Ratio	1.14	0.60

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(yes=1) which can be seen as another measure of prior record. Table 4.1 presents the means and standard deviations of all study variables.

### **Analytic Strategy**

Because the outcome in each model is dichotomous, whether or not the probationer has been reconvicted, logistic regression will be used in the analyses. The initial model will examine the relationship between adjudication and recidivism for the entire sample of probationers. Because the primary focus of this paper is on whether the impact of a label is contingent on characteristics of the offender, subsequent models will be run to examine if (and how) the relationship varies across the following variables: race, sex, employment status, prior record, and crime type. To assess these differences, the sample will be divided and separate analyses will be run for each group (e.g., males and females, whites, blacks and Hispanics). Running separate models for the sub-samples allows us to see what the actual impact of adjudication on recidivism is for each group. Following Paternoster, Brame, Mazerolle and Piquero (1998), a z-test for the equality of regression coefficients (i.e. slope difference test) will be conducted to determine whether the observed differences are statistically significant.

Because it is possible that being adjudicated or not may be the consequence of uncontrolled factors that also predict recidivism, there is a potential “treatment effect” (Moffitt, 1999). In such a situation the judge’s decision to adjudicate the offender is not independent of the residual terms in the equation for recidivism, which would result in biased estimates. To control for this possibility inverse Mills ratios will be included in each model.

The inverse Mills ratios will be computed using the “heckman” command in Intercooled Stata 8. Both equations in this two-stage process will include variables for sex, age, race, ethnicity, property crime, drug crime, prior supervision violation, prior record points, crime seriousness and supervision length. Additionally, in the selection equation, exclusion restrictions, which are “variables that affect the selection process but not the substantive equation of interest” (Bushway, Johnson, & Slocum, 2007, p. 5) are included. Whether the offender went to trial (1=yes) is used to predict adjudication but not recidivism, because this variable is arguably relevant for adjudication and not recidivism. In addition, since the data are drawn from 67 counties, 66 county dummy variables will be included. The option “mills” is included in the command to create a new variable containing the inverse of the Mills' ratio from the selection

equation. This new variable is included in each model.

The results of the full sample, race, employment status, sex, prior record, and crime type models will address the hypotheses described above. The additional slope difference tests will specify whether the effect of adjudication on recidivism varies significantly across individual traits. Together, these analyses further the extant literature on the existence of labeling effects and their potentially contingent nature.

## CHAPTER 5

### RESULTS

The relationship between adjudication and reconviction is assessed using logistic regression. The Pearson chi-square tests of the model goodness of fit produced a fail to reject decision in 12 of the 13 models, results consistent with the assumption that the specified logit models were correct. The one exception (those with a prior record) will be discussed later. The inverse Mills ratio appears to cause a multicollinearity problem, with tolerances as low as .074 for the inverse Mills ratio and .132 for prior record points. Tolerance levels for the remaining variables are consistently above .37 in each model. When the inverse Mills ratio is excluded from the model, all tolerance levels are above .37. Although the inverse Mills ratio appears to create a slight multicollinearity problem with one other variable, it is retained in the models in order to control for the potential treatment effect described in the previous chapter. Additionally, three variables were found to have skew statistics out of the normal range (crime seriousness, prior record points, and supervision length). To address this violation, the natural log of each variable is included in the models instead of the raw value.

Each table presenting the results of a logistic model will include the maximum likelihood coefficients and standard errors for each variable as well as odds ratios. After the results from all the models for a category (e.g., race, offense) are presented, a table will follow presenting the results from the slope difference tests, which indicate whether the effect of adjudication varies significantly across individual characteristics.

#### Full Sample

The full sample estimates are produced from the entire sample of probationers convicted in Florida between 2000 and 2003. Hypothesis 1 predicted that probationers who are adjudicated guilty will be more likely to be reconvicted than those who have adjudication withheld. Logistic regression was used to estimate the effect of adjudication on reconviction. The results are presented in Table 5.1. All the independent variables, with the exception of Hispanic and crime seriousness are significantly related to recidivism. Males, blacks, those who are employed, and those who have violated the terms of a previous probation are more likely to reoffend than females, whites, those who are unemployed, and those who have not violated a previous

Table 5.1. Full Sample Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.135	.021	1.144***
Male	.324	.026	1.383***
Age at sentencing	-.036	.001	0.964***
Hispanic	.027	.026	1.028
Black	.330	.023	1.390***
Property	.525	.039	1.691***
Drug	.456	.037	1.579***
Employed	.310	.021	1.363***
Prior supervision violation	.293	.035	1.341***
Crime seriousness	.000	.014	1.010
Prior record points	.083	.005	1.087***
Supervision length	-.179	.011	0.836***
Inverse Mills Ratio	-.060	.040	0.942

N= 119,648

\*p<.05, \*\*p<.01, \*\*\*p<.001

probation sentence. Property and drug offenders are both more likely to recidivate than violent offenders and the likelihood of reconviction increases with an increase in prior record points. Most importantly, the coefficient for adjudication is significant and indicates that the odds of reconviction are 14 percent higher for those who are adjudicated than those who had adjudication withheld. This provides support for Hypothesis 1; individuals who are adjudicated are significantly more likely to reoffend than those who avoid the label.

As discussed previously, both theory and prior research have raised the possibility that labeling effects may vary by a persons' race, employment status, sex, prior record and offense type. To assess this possibility, the above model is rerun for subsamples based on the aforementioned individual characteristics. Slope difference tests are then conducted to assess whether differences in the coefficients for adjudication are statistically significant.

### **Contextualization by Race and Ethnicity**

Based on prior theoretical and empirical work, Hypotheses 2 and 3 predicted that the effect of adjudication on reconviction will be greater for both blacks and Hispanics than whites. To assess these possibilities, the sample was split by race and ethnicity into three sub-samples (black, Hispanic, and white) and logistic models were run for each.

Table 5.2 presents the results for the sample of black probationers. Similar to the full sample results, crime seriousness was the only non-significant predictors (Hispanic was excluded from this model because it was limited to black probationers) and the others were in the same direction. The coefficient for adjudicated is again significant and the odds ratio indicates that the odds of reconviction of a black probationer who is adjudicated is over 12 percent higher than it is for those who have adjudication withheld.

Table 5.3 presents the results for the sample of Hispanic probationers. Unlike the models from the full and black only samples, adjudication does not significantly impact the likelihood of reconviction for Hispanic probationers. The only other non-significant predictor in the model is crime seriousness.

Finally, the results for white probationers are presented in Table 5.4. All the independent variables are the same direction and significance as those in both the full and black probationer samples. The odds ratio for adjudicated is positive and significant, indicating that the odds of reconviction for a white probationer who is adjudicated is nearly 17 percent higher than it is for



Table 5.2. Blacks Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.117	.033	1.124***
Male	.607	.054	1.835***
Age at sentencing	-.047	.001	0.955***
Property	.378	.053	1.459***
Drug	.433	.055	1.542***
Employed	.303	.032	1.354***
Prior supervision violation	.257	.054	1.293***
Crime seriousness	-.003	.022	0.997
Prior record points	.083	.008	1.087***
Supervision length	-.158	.018	0.854***
Inverse Mills Ratio	.005	.070	1.005

N= 41,938

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.3. Hispanics Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.106	.063	1.112
Male	.364	.099	1.439***
Age at sentencing	-.033	.003	0.967***
Property	.630	.123	1.878***
Drug	.526	.124	1.693***
Employed	.285	.063	1.330***
Prior supervision violation	.328	.127	1.388***
Crime seriousness	.079	.046	1.082
Prior record points	.046	.017	1.047**
Supervision length	-.305	.031	0.737***
Inverse Mills Ratio	-.418	.101	0.659**

N= 14,269

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.4. Whites Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.156	.031	1.169***
Male	.067	.028	1.069*
Age at sentencing	-.028	.001	0.972***
Property	.619	.063	1.857***
Drug	.406	.054	1.501***
Employed	.318	.031	1.374***
Prior supervision violation	.374	.054	1.454***
Crime seriousness	-.030	.019	0.970
Prior record points	.096	.007	1.100***
Supervision length	-.158	.016	0.854***
Inverse Mills Ratio	.006	.057	1.006

N= 63,441

\*p<.05, \*\*p<.01, \*\*\*p<.001

those who have adjudication withheld.

Tables 5.2-5.4 show the impact of adjudication (and other predictors) on recidivism separately for blacks, Hispanics, and whites. While adjudication was associated with a significant increase in the likelihood of reconviction for both blacks and whites, it was positive but not significant for Hispanics. This does not provide support for Hypothesis 3, that Hispanics are more negatively impacted by adjudication than whites. Rather, adjudication is not related to reconviction for Hispanics while it is positively associated with reconviction for whites. Additionally, the odds ratio for adjudication is larger for whites than blacks. Although the results do not support Hypothesis 2 or 3, slope difference tests (Paternoster, Brame, Mazerolle, & Piquero, 1998) were conducted (Table 5.5) to assess whether the impact of adjudication is significantly stronger for whites than either blacks or Hispanics.

White was used as the reference group for the slope difference comparisons. While the coefficient for adjudication was larger for white than black probationers, the difference was not significant ( $p=.1949$ ). Similarly, although adjudication was not significant for Hispanics and was for whites, the difference in the size of the effect was not found to be significant ( $p=.2389$ ). Again, these results do not support Hypothesis 2 or 3 and the null hypothesis of no difference cannot be rejected.

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Table 5.5. Slope Difference Tests of Coefficients for Adjudication by Race/Ethnicity

	Coefficient	z-score	p value
Black	0.117	-0.86	0.1949
Hispanic	0.106	-0.71	0.2389
White (Ref)	0.156		

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While the hypotheses were not supported, the findings are not completely unexpected. Remember that there were contradictory hypotheses about the relationship between labeling effects and race. While recent research supported the less vulnerability version, that minorities are more vulnerable to labeling effects (Bernburg & Krohn, 2003), the data here more closely resemble the greater vulnerability hypothesis, that minorities are less susceptible to the negative

consequences of labeling. Although not statistically significant, the coefficient for adjudication is largest for whites and smallest for Hispanics. Although the pattern of findings here is consistent with the greater vulnerability hypothesis, conclusive support was not found.

### **Contextualization by Employment Status**

Hypothesis 4 proposed that the effect of adjudication on reconviction will be greater for those who are unemployed than those who are employed. To assess this possibility, the sample was split into two groups, employed at time of offense and unemployed at time of offense and logistic models of reconviction were separately run for both groups. Table 5.6 presents the results for the sample of probationers who were employed when they offended. Similar to the full sample results, Hispanic and crime seriousness were the only non-significant predictors and the others were significant and in the expected direction. The odds ratio for adjudication is significant and indicates that the odds of reconviction are over 16 percent higher for those who are adjudicated than those who have adjudication withheld.

Table 5.7 presents the results for the sample of probationers who were not employed at the time of offense. Contrary to the results from the full and employed samples, crime seriousness was found to be significantly associated with an increase in recidivism, indicating that those who commit more serious offenses are more likely to reoffend. Despite this difference, being adjudicated is significant and the odds of reconviction are 13 percent higher for those who were adjudicated.

Tables 5.6-5.7 presented the logistic models of reconviction for both employed and unemployed offenders and indicate that, contrary to Hypothesis 4, adjudication appears to have a smaller impact on unemployed than employed offenders. Although the results are not in the expected direction, a slope difference test was still conducted to determine whether the difference in adjudication coefficients was statistically significant (see Table 5.8). Although adjudication had a larger impact on the probability of reconviction for employed than unemployed offenders, the difference is not statistically significant ( $p=.2514$ ), indicating that the effect of adjudication on recidivism does not vary by employment status.

Although the data do not support Hypothesis 4, the results are not entirely out of sync with prior theorizing on the relationship between stakes in conformity (i.e. employment) and labeling effects. Similar to the opposing hypotheses about race, the employment status

Table 5.6 Employed Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.154	.034	1.166***
Male	.329	.039	1.389***
Age at sentencing	-.032	.001	0.969***
Hispanic	.020	.042	1.020
Black	.322	.034	1.379***
Property	.551	.062	1.735***
Drug	.470	.059	1.600***
Prior supervision violation	.299	.054	1.348***
Crime seriousness	-.029	.020	0.972
Prior record points	.088	.008	1.092***
Supervision length	-.212	.016	0.809***
Inverse Mills Ratio	.063	.071	1.065

N= 43,227

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.7 Unemployed Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.125	.027	1.133***
Male	.323	.035	1.387***
Age at sentencing	-.039	.001	0.962***
Hispanic	.036	.034	1.036
Black	.341	.031	1.406***
Property	.508	.050	1.662***
Drug	.446	.048	1.563***
Prior supervision violation	.320	.048	1.378***
Crime seriousness	.044	.019	1.044*
Prior record points	.084	.007	1.088***
Supervision length	-.154	.015	0.858***
Inverse Mills Ratio	-.101	.049	0.904

N= 76,421

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.8. Slope Difference Tests of Coefficients for Adjudication by Employment Status

	Coefficient	z-score	p value
Employed	0.154	0.67	0.2514
Unemployed	0.125		

argument centers around whether those who care more about and are more invested in conventional society (i.e. employed) will be more or less vulnerable to the negative consequences of labeling. While the prior empirical evidence has been more supportive of the less vulnerability hypothesis (that labeling effects will be stronger for those who are unemployed), the data examined here (although not significant) more closely resemble the greater vulnerability version, that those with more to lose will be more likely to suffer negative consequences of labeling. Although the pattern of findings here is consistent with the greater vulnerability hypothesis, conclusive support was not found.

### Contextualization by Sex

Hypothesis 5 proposed that the effect of adjudication on reconviction will vary by sex. Due to weak theoretical arguments and conflicting empirical findings, a directional hypothesis was not proposed. To further explore the possibility that the adjudication-reconviction relationship varies by sex, the sample was split into two groups, male and female. Table 5.9 presents the results for the sample of male probationers. Contrary to the full sample results, Hispanic was positive and significant, indicating that compared to white males, Hispanic males are more likely to recidivate. All other variables are in the same direction as the full sample model and are statistically significant. The odds ratio for adjudicated is significant, indicating that for males the odds of reconviction for those who are adjudicated are 12.5 percent higher than for those who have adjudication withheld.

Table 5.10 presents the results for the sample of female probationers. There are sharp contrasts in the impact of race and ethnicity on reconviction in the female sample when compared to the male and full samples. Hispanic was positive in both the full and male samples



Table 5.9. Males Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.118	.024	1.125***
Age at sentencing	-.039	.001	0.961***
Hispanic	.080	.030	1.083**
Black	.475	.030	1.608***
Property	.516	.042	1.675***
Drug	.352	.037	1.422***
Employed	.294	.023	1.342***
Prior supervision violation	.273	.039	1.314***
Crime seriousness	-.021	.015	0.979
Prior record points	.071	.006	1.074***
Supervision length	-.184	.012	0.832***
Inverse Mills Ratio	-.103	.044	0.902*

N= 87,906

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.10. Females Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.193	.048	1.213***
Age at sentencing	-.027	.002	0.974***
Hispanic	-.157	.057	0.854*
Black	-.060	.033	0.942
Property	.548	.100	1.731***
Drug	.695	.113	2.004***
Employed	.366	.047	1.441***
Prior supervision violation	.393	.085	1.482***
Crime seriousness	.021	.032	1.021
Prior record points	.131	.011	1.139***
Supervision length	-.135	.025	0.874***
Inverse Mills Ratio	.159	.102	1.173

N= 31,742

\*p<.05, \*\*p<.01, \*\*\*p<.001

(and significant for males). For females, Hispanic was negative and significant, indicating that, compared to whites, Hispanic women are less likely to be reconvicted. Additionally, black was negative and not significant for females (compared to positive and significant in both the full and male samples). All other variables are in the same direction as the full sample model and significant. These differences in the impact of race across the sex sub-samples indicate the possibility of a three-way interaction, so that the impact of adjudication on reconviction for males and females varies by race. This will be explored after the initial results are presented. The results indicate that the odds of reconviction for females who are adjudicated are 21 percent higher than the odds for those who had adjudication withheld.

While adjudication was positive and significant for both males and females, the odds ratios indicate that the impact may be larger for females. Indeed, the slope difference test (Table 5.11) indicates that the differences in the adjudication coefficients for males and females approaches significance ( $p=.0808$ ). Hypothesis 5, that adjudication does impact males and females differently, received marginal support.

It was mentioned above that there is evidence of a potential three-way interaction between race, sex, and adjudication. To further explore this possibility, the sample was divided into six groups (black males, Hispanic males, white males, black females, Hispanic females, and white females) and logistic models were run separately for each group. Following the technique used above to assess whether the impact of adjudication varies significantly across groups, slope difference tests were run. The coefficients and results from the slope difference tests are presented in Table 5.12.

Based on the maximum likelihood coefficients, adjudication appears to be most consequential for black females, followed by white females, white males, Hispanic males, Hispanic females and black males. Because the coefficient for adjudication was largest for black females, they were used as the reference group in the slope difference tests. The difference in the impact of adjudication on recidivism approaches significance for black females and black males ( $p=.0721$ ), indicating that the impact of adjudication on reconviction may be stronger for black females than black males. Earlier results showed that the impact of adjudication on reconviction was stronger for whites than blacks (although not significantly) and females than males (approached significance). The lack of significance found in the race comparisons may be due to the fact that they are conditioned by sex. This is a unique finding that needs to be

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Table 5.11. Slope Difference Tests of Coefficients for Adjudication by Sex

	Coefficient	z-score	p value
Males	0.118	-1.40	0.0808
Females	0.193		

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Table 5.12. Slope Difference Tests of Coefficients for Adjudication by Race and Sex

	Coefficient	z-score	p value
Black Males	0.089	-1.46	0.0721
Hispanic Males	0.106	-1.050	0.1469
White Males	0.144	-0.819	0.2061
Black Females (Ref)	0.214		
Hispanic Females	0.090	-0.647	0.2578
White Females	0.197	-0.166	0.4325

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explored in greater detail in future research.

### **Contextualization by Criminal History**

Hypothesis 6 predicted that being adjudicated would be more consequential for first time offenders than those with a prior criminal record. To examine this proposition, the sample was split into two groups, those with no prior record points and those with any prior record points. Table 5.13 presents the results of the logistic regression model for those who had a prior record. Unlike the full sample model, prior supervision violations have no impact on reconviction for probationers with a prior record. Additionally, an increase in the seriousness of the offense was significantly associated with an increase in reconviction. All other variables are in the same direction as the full sample model and significant. While adjudicated was significant, it was only associated with a 7.7 percent increase in the odds of reconviction for those with a prior record. It should also be noted that the Pearson chi-square test of the model goodness of fit was significant, a result inconsistent with the assumption that the specified logistic model was correct. As such, the results from this model should be interpreted with caution.

Table 5.14 presents the results of the logistic regression model for probationers without a prior record. The results differed from that of the full sample and prior record sample in that crime seriousness was negative and significant, so that an increase in the seriousness of the offense actually decreases reconviction. Additionally, Hispanic was negative and the inverse Mills ratio was negative and significant. All other variables are in the same direction as the full sample model and significant. Being adjudicated was significant and the odds ratio indicated that the odds of reconviction for first time offenders who are adjudicated are 19.4 percent higher than the odds of those having adjudication withheld.

Tables 5.13-5.14 show the impact of adjudication (and other predictors) on recidivism separately for those with and without a prior criminal record. Adjudication was significantly associated with an increase in reconviction for both groups but appears to be more consequential for those without a prior record. Indeed, the slope difference test indicates that adjudication has a significantly ( $p=.0129$ ) larger effect on reconviction for those without a prior record (Table 5.15). This result provides support for Hypothesis 6; adjudication is more consequential for naïve than experienced probationers.

Table 5.13. Prior Record Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.074	.038	1.077*
Male	.468	.049	1.597***
Age at sentencing	-.041	.002	0.960***
Hispanic	.024	.040	1.024
Black	.420	.043	1.522***
Property	.615	.073	1.849***
Drug	.707	.084	2.027***
Employed	.356	.037	1.427***
Prior supervision violation	.067	.063	1.069
Crime seriousness	.089	.026	1.093***
Supervision length	-.216	.018	0.806***
Inverse Mills Ratio	-.316	.048	0.729***

N= 53,931

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.14. No Prior Record Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.178	.027	1.194***
Male	.212	.029	1.236***
Age at sentencing	-.037	.001	0.964***
Hispanic	-.015	.034	0.986
Black	.270	.027	1.310***
Property	.473	.046	1.605***
Drug	.365	.042	1.441***
Employed	.284	.025	1.328***
Prior supervision violation	.191	.040	1.210***
Crime seriousness	-.101	.015	0.904***
Supervision length	-.263	.012	0.769***
Inverse Mills Ratio	-.493	.032	0.611***

N= 65,717

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.15. Slope Difference Tests of Coefficients for Adjudication by Prior Record

	Coefficient	z-score	p value
Prior Record	0.074	-2.23	0.0129
No Prior Record	0.178		

Unlike the conflicting theoretical rationales and empirical findings for the previously examined individual traits (race, stakes in conformity, and sex), there is a greater level of consensus that labeling effects should be more consequential for naïve than experienced offenders. The data here provide further evidence that the initial labeling event may be more detrimental than subsequent events. But, being adjudicated was a significant predictor of recidivism even for those with a prior record.

### **Contextualization by Offense Type**

Based on the synthesis of findings from prior research, Hypothesis 7 proposed that the effect of adjudication on recidivism would be greater for violent than for property offenders and greater for property than for drug offenders. While labeling theorists have not addressed this potential contingency, the examination of prior research presented in Chapter 3 showed that the most support for labeling theory was found in samples of violent offenders, followed by property and drug offender populations. To test this prediction, the sample was split into three groups (violent offenders, property offenders, and drug offenders) and separate logit models were run for each group. Table 5.16 presents the results for violent offenders. Unlike the full sample model, crime seriousness is negatively associated with reconviction for those who commit violent crimes. Additionally, adjudication is not significant, indicating that being adjudicated does not increase reconviction for violent offenders.



Table 5.16. Violent Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.038	.045	1.038
Male	.482	.080	1.619***
Age at sentencing	-.482	.002	0.952***
Hispanic	.074	.063	1.077
Black	.478	.065	1.613***
Employed	.207	.046	1.231***
Prior supervision violation	.281	.086	1.324***
Crime seriousness	-.240	.027	0.787***
Prior record points	.079	.012	1.083***
Supervision length	-.229	.026	0.796***
Inverse Mills Ratio	-.165	.090	0.848

N= 27, 924

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.17. Property Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.190	.032	1.209***
Male	.396	.038	1.486***
Age at sentencing	-.034	.001	0.967***
Hispanic	-.024	.034	0.977
Black	.192	.028	1.211***
Employed	.327	.030	1.386***
Prior supervision violation	.279	.049	1.322***
Crime seriousness	.021	.018	1.021
Prior record points	.076	.007	1.079***
Supervision length	-.174	.016	0.841***
Inverse Mills Ratio	-.168	.050	0.845**

N= 56,136

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.18. Drug Only Model of Reconviction

Independent Variable	Coefficient	S.E.	Odds Ratio
Adjudicated	.076	.035	1.079*
Male	.021	.035	1.022
Age at sentencing	-.033	.001	0.968***
Hispanic	.139	.059	1.149**
Black	.552	.050	1.736***
Employed	.333	.038	1.395***
Prior supervision violation	.309	.062	1.362***
Crime seriousness	.050	.030	1.052
Prior record points	.090	.009	1.094
Supervision length	-.157	.019	0.855***
Inverse Mills Ratio	.156	.086	1.169*

N= 35,588

\*p<.05, \*\*p<.01, \*\*\*p<.001

Table 5.17 presents the results for the sample of property offenders. The findings are similar to that of the full sample model, with the exception of Hispanic, which is negative. The odds ratio for adjudication is significant and indicates that the odds of reconviction for adjudicated property offenders are 20.9 percent higher than the odds for those who had adjudication withheld.

Table 5.18 presents the results for drug offenders. The findings differ from the full, property, and violent samples. Sex is not a significant predictor of reconviction for drug offenders. While being adjudicated was significant, it increased the odds of reconviction by less than 8 percent. Because sex is consistently a predictor of reconviction in all other models, the lack of significance for sex for drug offenders was a bit surprising and indicates the possibility that the impact of adjudication on reconviction may vary by sex. This will be explored after the initial results are presented.

The above tables show the impact of adjudication (and other predictors) on recidivism separately for violent, property, and drug offenders. While adjudication was associated with a significant increase in the likelihood of reconviction for both property and drug offenders, it was positive but not significant for violent probationers. This does not support Hypothesis 7, that violent offenders should be the most negatively impacted by adjudication. Slope difference tests were calculated (Table 5.19) to examine whether the impact of adjudication varies significantly by crime type. Because the coefficient for adjudication was largest for property offenders, they were used as the reference category. The tests revealed that the impact of adjudication on recidivism is significantly greater for property than for both violent and drug offenders. While these results do not support the proposition that violent offenders are most and drug offenders least impacted by adjudication, they do indicate that the consequences of adjudication vary by crime type.

It was mentioned above that there is evidence that the predictors of reconviction for drug offenders vary by sex. To further explore this possibility, the sample was divided into two groups (male drug offenders and female drug offenders) and logistic models were run separately for both groups. Following the technique used above to assess whether the impact of adjudication varies significantly across groups, slope difference tests were run. The coefficients and results from the slope difference tests are presented in Table 5.20.

The difference in the impact of adjudication on recidivism approaches significance for

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Table 5.19. Slope Difference Tests of Coefficients for Adjudication by Crime Type

	Coefficient	z-score	p value
Violent	0.038	2.75	0.003
Property (Ref)	0.190		
Drug	0.076	2.40	0.0082

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Table 5.20. Slope Difference Tests of Coefficients for Adjudication by Crime Type and Sex

	Coefficient	z-score	p value
Male drug offenders	0.049	-1.44	0.0749
Female drug offenders	0.186		

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male and female drug offenders ( $p=.0749$ ), indicating that the impact of adjudication on reconviction may be stronger for drug offending females than males. For females, the adjudication is associated with a 20 percent increase in the odds of reconviction. For males, adjudication is associated with a less than 5 percent increase in the odds of reconviction.

The crime type hypothesis was more exploratory than the other comparisons in this study and was based on the synthesis of a limited number of research findings presented in Chapter 2. Labeling theorists have yet to address the possibility that labeling effects are contingent on the type of crime committed. What is especially interesting here is the lack of significance for adjudication among violent offenders. If violent crimes are more likely to be of an expressive, as opposed to instrumental, nature than either property or drug offenses, then the likelihood of recidivism would arguably be low, regardless of being labeled. In this data, violent offenders were the least likely to recidivate. This is just one possible explanation for the lack of labeling effect on violent offenders. The difference in impact of adjudication on male and female drug offenders was also not expected. Future research and theorizing should pay closer attention to crime type and the potential interaction between sex and crime type.

## CHAPTER 6

### SUMMARY AND CONCLUSIONS

#### Summary of Findings

The previous chapter examined the impact of adjudication as a felon on recidivism in a sample of Florida probationers and across several sub-samples of this probation population. In Florida, judges have the discretion to withhold adjudication from an individual who has been found guilty of a felony. When adjudication is withheld, the individual does not lose any civil rights and can legitimately say they were not convicted of a felony, even though they were found guilty of such an offense. A felony conviction is arguably the most consequential label available in the criminal court system, and Florida's opportunity for withholding adjudication presents a unique opportunity to examine the consequences of having this important label applied.

Seven hypotheses relating to the relationship between adjudication and reconviction were proposed and subsequently tested. Both supporting and contradictory evidence were found. These hypotheses are restated below and are followed by a brief summary of the findings that were reported throughout the previous chapter.

1. *Hypothesis: Probationers who are adjudicated guilty will be more likely to be reconvicted than those who have adjudication withheld.*

Finding: Adjudication is positively and significantly related to reconviction in the full sample model. The impact of adjudication appears to be robust, as it also significantly increases reconviction in 10 of the 12 sub-samples. Hispanics and violent offenders were the only groups of probationers that were not negatively impacted by being formally adjudicated.

2. *Hypothesis: The effect of adjudication on reconviction will be greater for blacks than for whites.*

Finding: The impact of adjudication on reconviction is not significantly greater for black than for white probationers. Adjudication has a positive, significant impact on reconviction for both blacks and whites.

3. *Hypothesis: The effect of adjudication on reconviction will be greater for Hispanics than for whites.*

Finding: Similar to the findings for race, the impact of adjudication on reconviction is not significantly greater for Hispanic than for white probationers. In fact, although not significant,

the odds ratio for the impact of adjudication on reconviction is actually smaller for Hispanics than whites.

4. *Hypothesis: The effect of adjudication on reconviction will be greater for those who are unemployed than for those who are employed.*

Finding: Adjudication was significantly associated with an increase in reconviction for both those who were employed and unemployed at offense. Contrary to the hypothesis, this effect was not significantly greater for the unemployed. Indeed, the association between adjudication and reconviction is greater (although not significantly) for the employed sample.

5. *Hypothesis: The effect of adjudication on reconviction will vary by sex.*

Finding: Marginal support was found for the proposition that the impact of adjudication varies by sex. Females may face more negative consequences from adjudication than males. Based on the odds ratios, adjudication appears to be most consequential for black females, followed by white females, white males, Hispanic males, and Hispanic females and black males. Only the difference in the coefficients for black females and black males approached significance.

6. *Hypothesis: The effect of adjudication on reconviction will be greater for offenders without a prior record than for those with a prior record.*

Finding: While adjudication was associated with a significant increase in recidivism for both those with and without a prior record, the effect was significantly stronger for first-time offenders.

7. *Hypothesis: The effect of adjudication on reconviction will be greater for violent than for property offenders and greater for property than for drug offenders.*

Finding: Adjudication was associated with a significant increase in reconviction for property and drug offenders but not violent offenders. Also, significant differences in the impact of adjudication on reconviction were found across crime type, but these differences were not as predicted. The relationship between adjudication and recidivism was significantly stronger for property offenders than for both drug and violent offenders. Additionally, it was found that adjudication may be more consequential for female than male drug offenders.

Collectively, these results provide support for the labeling proposition that being officially labeled as a felon increases the likelihood of reconviction. Being adjudicated was significantly associated with reconviction within three years for all groups examined except for Hispanics and violent offenders. The findings also demonstrate that, while being labeled is a



relatively consistent predictor of re-offense, individuals with certain characteristics may be more likely to suffer negative consequences than others. The effect of adjudication on recidivism was significantly larger for black females than black males (and approached significance for females and males). Additionally, labeling appears to be more detrimental for naïve than for experienced offenders and for property than for either drug or violent offenders.

Prior research on labeling theory has largely ignored the potentially contingent nature of labeling effects. The current study has helped fill this void by examining the impact of adjudication on recidivism across a variety of individual characteristics, some of which had not yet been subjected to empirical test. The theoretical relevance of these findings will be discussed below.

### **Theoretical Relevance: The Contingent Nature of Labeling Effects**

Race was one of the earliest potential contingencies recognized by labeling theorists. These early labelists (Ageton & Elliott, 1974; Harris, 1976; Jensen, 1972) hypothesized that the effects of a formal sanction would be less consequential for minorities, potentially because they are already denied full social membership so that any further removal would be redundant. More recently, scholars have argued the opposing position, that official labeling should have more negative consequences for those who are disadvantaged (i.e. minorities) (Bernburg & Krohn, 2003; Sampson & Laub, 1997). These contrary propositions have been characterized as the greater and less vulnerability hypotheses, respectively (Sherman, Smith, Schmidt, & Rogan, 1992).

While the results here were not statistically significant, they are more in line with the greater vulnerability version. Adjudication was associated with an increase in recidivism for whites and blacks, but it was not significant for Hispanics. Although this seems to indicate that whites may be more vulnerable to labeling effects than Hispanics, the difference was not significant and there was no difference in effect between black and white probationers. The greater and less vulnerability hypotheses also apply to the relationship between stakes in conformity and labeling effects. Again, while the differences were not statistically significant, the pattern is more consistent with the greater vulnerability hypothesis. Stakes in conformity was operationalized here as employment status at offense and the impact of adjudication was stronger (although not significantly) for those who were employed. While the prior empirical

evidence has been more supportive of the less vulnerability hypothesis, the data examined here (although not significant) more closely resemble the greater vulnerability version, that those with more to lose will be more likely to suffer negative consequences of labeling.

There has been scant theorizing and research on the relationship between sex and labeling effects, and there is no consistency in either theoretical expectations or empirical findings. One of the purposes of the present study was to shed some light on whether the negative consequences of labeling will be (a) greater for males (Ageton & Elliott, 1974; Keane, Gillis, & Hagan, 1989); (b) greater for females (Messerschmidt, 1993; Ray & Downs, 1986); and (c) the same for males and females (Simons, Miller, & Aigner, 1980). The results here provide marginal support for the argument that labeling effects are stronger for women, especially for the comparison between black males and females. The race-sex interaction was not expected and should be furthered explored in future research.

Although there has been little discussion of sex and labeling effects in general, there have been two propositions as to why females may be more vulnerable. Ray and Downs (1986, p. 171) argued that females may be more impacted by labels than males because they, “are expected to be more attentive to interpersonal relationships than males,” while Messerschmidt hypothesized that because men exercise greater power in society than women, “men should . . . have greater opportunity to counteract official labeling” (1993, p. 4). While the data here are more consistent with the notion that females are more negatively impacted by adjudication than males, the data do not allow us to examine either of these propositions.

Another theoretical expectation regarding the contingent nature of labeling effects is that they would be more consequential for naïve offenders than those with a prior record (Horwitz & Wasserman, 1979; Paternoster & Iovanni, 1989; D. A. Smith & Gartin, 1989). The results provide support for this proposition, the likelihood of reconviction was significantly higher for probationers without a prior record than those with a prior record in Florida. The most convincing rationale for such an effect is that once an individual has been labeled, “it is doubtful that further increments in labeling will continue to produce further deviance” (Paternoster & Iovanni, 1989, p. 386).

Finally, labeling theorists have not yet addressed the role that crime type may play in amplifying labeling effects. This relationship was explored here because the literature synthesis presented in Chapter 3 revealed differences in labeling effects for perpetrators of different

crimes. Property offenders, who are the most likely to recidivate, were also significantly more impacted by being adjudicated than both drug and violent offenders. In fact, adjudication was not significantly associated with re-offense for violent offenders. The data examined here support the argument that offense type should be given consideration in both future theoretical and empirical labeling papers.

### **Practical and Social Implications**

One of the primary goals of the criminal justice system is to deter crime and protect society. The results described here indicate that the decision to withhold or apply adjudication may directly impact the likelihood that offenders will re-offend. Adjudicating those who have been found guilty was found to be associated with an increase in reconviction. The option of withholding adjudication is left to the judge's discretion. If this decision were made in more cases (in this sample 38 percent of the probationers were adjudicated), the data here suggest that fewer guilty probationers would re-offend. There is an obvious social benefit to reducing the recidivism rate. Yet, to my knowledge, this option exists only in Florida. Other states should consider implementing a similar policy and following offenders for at least one year to examine whether a similar reduction in reconviction exists elsewhere. In addition to the obvious benefits to society of lower recidivism rates, the individual probationer also benefits from not having been labeled a felon.

As mentioned in Chapter 1, the negative consequences of felony conviction can long outlast the prison or probation sentence imposed by the court and can include both personal and legal obstacles to full social membership. Early labeling theorists proposed that official assignment of the meaning of deviant or criminal to an individual can eventually lead that individual to take on an identity consistent with that meaning and continue to engage in such behaviors through a self-fulfilling prophecy (Becker, 1963; Lemert, 1967; Lofland, 1969; Matza, 1969). This change in personal and public identity may also lead to exclusion from mainstream social groups and a change in the social life of the deviant.

In addition to negative changes in one's personal and social identities, there are legal barriers to enjoying the full rights and privileges of citizenship. While restrictions vary by state, in Florida convicted felons lose all of their civil rights, including the right to vote, serve on a jury, and carry a firearm. Convicted felons may also be restricted from access to certain

occupations because they may be deemed ineligible for certain professional certifications and lose any they currently have. Finally, because employment applications often inquire about felony convictions, and employers are free to refuse employment to felons, those who are formally adjudicated may have a more difficult time securing employment than those who had the label withheld. These personal and structural impediments can push one out of conventional society and allow them to be lured by criminal options. Using the option of withholding felony adjudication more frequently will benefit a larger number of individuals and potentially help them more easily reenter mainstream society and refrain from engaging in criminal activities.

### **Limitations and Directions for Future Research**

While the present research substantively adds to the existing literature on labeling and recidivism, it is not without limitations. The primary limitation of the present research is the potential for omitted variable bias as some of the most potentially useful variables (stakes in conformity and socio-economic status) were not available in the data. While stakes in conformity are tapped into with the measure of employment status at offense, it would have been desirable to include educational attainment and marital status as well. Unfortunately, these variables are missing in a majority of the cases. There are no measures of socio-economic status in the data. Another limitation is that the outcome, reconviction, excludes those probationers who have re-offended but were not caught and officially processed in the criminal justice system. While only 20 percent of the sample was reconvicted, it is reasonable to assume that a higher percentage violated the law within three years of the initial sentence. Finally, while this study advances the literature on the contingent nature of labeling effects, it is unable to address the process by which official labeling leads to recidivism. The hypothesized processes by which this may occur (change in identity and structural impediments) were briefly discussed above and in more detail in Chapter 2.

Future research should attempt to further clarify the variables that may mediate the labeling event and recidivism. This is still an understudied area that was beyond the scope of this paper. Additionally, researchers should pay particular attention to whether and how labeling effects vary for different kinds of individuals. This has been one of the first efforts to do so and has found enough evidence to warrant further investigation. Future efforts should also include some of the interactions that were explored here, specifically race/sex and sex/crime type.

Because they had not been theoretically anticipated, it would be interesting to see if these findings can be replicated or if they are just an anomaly. Most importantly, labeling theory deserves adequate and theoretically informed examination before it is again discarded. The policy implications are too great to leave these questions unanswered.

## APPENDIX

Table A.1. Studies of the Effect of Official Sanctions on Recidivism\*

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS	
Glaser (1964)	1,015 released male prisoners	Adult	None	Descriptive	3 years	Any	< 18 months v. > 18 months incarceration	Unfavorable parole	+, p=?	
Babst & Mannering (1965)	5,274 male offenders w/ 0 prior felonies	Adult	None	Descriptive	2 years	Any	Incarceration v. probation	Unfavorable parole/probation	+, p<.05	
	1,948 male offenders w/ 0 prior felonies								Property	+, p<.001
	483 male offenders w/ 0 prior felonies								Violent	+, p<.05
	2,206 male offenders w/ 1+ prior felonies								Any	+, p>.05
	1,494 male offenders w/ 1+ prior felonies								Property	-, p>.05
	164 male offenders w/ 1+ prior felonies	Violent	-, p>.05							
Gold & Williams (1969)	70 teenagers	Juvenile	Matched on # prior offenses	Descriptive	3 years	Any	Police apprehension	Self-report # subsequent offenses	+, p=.10	

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS	
Beattie & Bridges (1970)	2,852 individuals sentenced to probation or jail	Adult	None	Descriptive	1 year	Any	Jail v. probation	Rearrest	+, p<.001	
	2,472 males sentenced to probation or jail								+, p<0.01	
	380 females sentenced to probation or jail								+, p<.001	
	733 individuals sentenced to probation or jail (no prior record)								+, p>.05	
	2,119 individuals sentenced to probation or jail (prior record)								+, p<.001	
	307 individuals sentenced to probation or jail								Violent	+, p<.001
	1,577 individuals sentenced to probation or jail								Property	+, p<.001
355 individuals sentenced to probation or jail	Drug	+, p>.05								
Gold (1970)	522 13-16 year olds	Juvenile	Matched on # prior offenses	Descriptive	3 years	Any	Police apprehension	Self-report # subsequent offenses	+, p=.02	

<b>STUDY</b>	<b>SAMPLE</b>	<b>POP</b>	<b>CRIMINAL HISTORY CONTROL</b>	<b>STATS</b>	<b>FOLLOW-UP PERIOD</b>	<b>ORIGINAL OFFENSE</b>	<b>OFFICIAL SANCTION</b>	<b>RECIDIVISM MEASURE</b>	<b>FINDINGS</b>
Thornberry (1971)	9,945 boys	Juvenile	Examines separately by # dispositions	Descriptive	?	Any	Incarceration v. arrest	Arrest	?, varies by # previous dispositions
Babst, Koval & Neithercutt (1972)	7,245 paroled males	Adult	None	Descriptive	1 year	Burglary	Time served	Unfavorable parole outcome	-, p>.05
Jaman, Dickover & Bennett (1972)	150 male felons released from prison	Adult	Matched on # prior offenses	Descriptive	6 months	Robbery	Above v. below median sentence length	Unfavorable parole outcome	+, p= n.s.
					1 year				+, p<.05
					2 years				+, p<.05
	240 male felons released from prison	Adult	Matched on # prior offenses	Descriptive	6 months	Burglary	Above v. below median sentence length	Unfavorable parole outcome	-, p= n.s.
					1 year				+, p= n.s.
					2 years				+, p<.05



STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Gottfredson, Neithercutt, Nuffield & O'Leary (1973)	2,482 paroled males with no prior record	Adult	None	Descriptive	1 year	Forgery or fraudulent check use	Time served (months)	Unfavorable parole outcome	+, p=?
	8,493 paroled males with a prior record								+, p=?
	15,513 paroled males with no prior record					Property			+, p=?
	44,689 paroled males with a prior record								+, p=?
	10,322 paroled males with no prior record					Violent			+, p=?
	20,586 paroled males with a prior record								+, p=?
	1,051 paroled males with no prior record					Drugs			-, p=?
	3,916 paroled males with a prior record								-, p=?
Meade (1974)	438 first-time delinquent offenders	Juvenile	Included only those with no prior record	Coleman technique	18 months	Any	Juvenile court appearance	Subsequent juvenile court appearance	+, p<.05
Babst, Moseley, Schmeidler, Neithercutt & Koval (1976)	929 paroled males	Adult	None	Descriptive	2 years	Drugs	Time served	Unfavorable parole	-, p<.01

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Bartell & Winfree (1977)	100 convicted burglars	Adult	# prior arrests, convictions, incarcerations	OLS	4 years maximum	Burglary	Incarceration	Rearrest	-, p=?
								Conviction	+, p=?
Gottfredson, Gottfredson & Garofalo (1977)	5,346 paroled males	Adult	None	Descriptive	1 year	Any	Time served	Parole failure	+, p>.05
Waldron & Angelino (1977)	281 released male felons	Adult	None	Descriptive	4 1/2 years	Any	Time served (< 4 months v. > 4 months)	Rearrest	-, p=.271
								Reconviction	-, p=.196
	111 released female felons	Reincarceration	+, p=.349						
		Rearrest	-, p=.338						
								Reconviction	+, p=.246
								Reincarceration	-, p=.371
Klemke (1978)	188 shoplifters in high school	Juvenile	None	Descriptive	up to 10 years	Shoplifting	Police apprehension	Self-reported subsequent involvement	+, p<.20

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Wheeler (1978)	94 institutionalized females 353 institutionalized males	Juvenile	None	Descriptive	?	Any	Time served	Parole violation	+, p=n.s. +, p=.003
Horwitz & Wasserman (1979)	474 arrested 14 and 15 year olds 142 males w/ 1-3 prior arrests 121 males w/ 4+ prior arrests 63 arrested females	Juvenile	# previous arrests  None	SEM  Descriptive	2 years	Any	Institutionalization	# subsequent arrests	+, p=? -, p=.0323 +, p=.1657 -, p=.025
Phillips, McCleary & Dinitz (1983)	210 youths w/ 1+ period of incarceration	Juvenile	None	Descriptive	18 month average	Any	Length of incarceration	Arrest	+, p=?
Sherman & Berk (1984) (experimental)	314 male domestic violence suspects	Adult	Random assignment	OLS  Logit Survival	6 months	Domestic violence	Arrest	Police report	-, p=.013  -, p=.013 -, p=.012

<b>STUDY</b>	<b>SAMPLE</b>	<b>POP</b>	<b>CRIMINAL HISTORY CONTROL</b>	<b>STATS</b>	<b>FOLLOW-UP PERIOD</b>	<b>ORIGINAL OFFENSE</b>	<b>OFFICIAL SANCTION</b>	<b>RECIDIVISM MEASURE</b>	<b>FINDINGS</b>
Thomas & Bishop (1984)	2,147 students	Juvenile	Self-report scale	OLS	9 months	Any	Police contact	Self-report scale	+, p=?
	716 students w/ 0-1 prior offenses								+, p=?
	637 students w/ 2-3 prior offenses								+, p=?
	794 students w/ 4+ prior offenses								+, p=?
Berk & Newton (1985)	783 male domestic violence suspects	Adult	Previous convictions (y/n)	Logit	?	Domestic violence	Arrest	Police report	-, p=.003
				Survival					-, p=.002
Klein (1986) (experimental)	306 referrable juveniles	Juvenile	Random assignment	Descriptive	6 months	Any	Juvenile court petition v. release	Rearrest	+, p<.05
					15 months				+, p<.01
					27 months				+, p<.05
Palamara, Cullen & Gersten (1986)	437 youths aged 10-18	Juvenile	Self-report scale	MANCOVA	5 years	Any	Police record	Self-report scale	+, p<.001

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS		
Petersilia & Turner (1986)	1,000 convicted males	Adult	# convictions, prior prison terms, jail terms, probation terms	Logit	2 years	Any	Prison v. probation	Arrest	+, p=.095		
	319 convicted males							Violent	Charge filed	+, p=.001	
									Conviction	+, p=.006	
									Arrest	+, p=.348	
	429 convicted males							Property	Charge filed	+, p=.322	
									Conviction	+, p=.167	
									Arrest	+, p=.203	
	255 convicted males							Drug	Charge filed	+, p=.001	
									Conviction	+, p=.006	
									Arrest	+, p=.269	
	506 males sentenced to prison							Any	Months served in prison	Charge filed	+, p=.06
										Conviction	+, p=.062
										Arrest	-, p=.006
	160 males sentenced to prison							Violent		Charge filed	-, p=.0002
Conviction		-, p=.0001									
Arrest		-, p=.178									
216 males sentenced to prison	Property		Charge filed	-, p=.043							
			Conviction	-, p=.027							
			Arrest	-, p=.061							
130 males sentenced to prison	Drug		Charge filed	-, p=.152							
			Conviction	-, p=.088							
			Arrest	-, p=.033							
								Charge filed	-, p=.005		
								Conviction	-, p=.006		

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Orsagh & Chen (1988)	1,425 male offenders	Adult	# prior arrests	Logit	2 years	Any	Length of incarceration	Arrest	+, p=.002
	148 male robbery offenders			OLS		Robbery			+, p=.003
	422 male burglary offenders					Burglary			+, p=.001
Wheeler & Hissong (1988)	329 first time drunk driving convicts	Adult	None	Descriptive	3 years	Drunk driving	Jail v. probation or fine	Reconviction	-, p=ns
	68 drunk driving convicts (at least one prior DWI conviction)								+, p=ns
Wooldredge (1988)	333 juvenile offenders	Juvenile	Prior misdemeanor or felony (y/n)	Logit	3-7 years	Any	> 1 month detention v. case dismissed	Rearrest	-, p=.0016
Beck & Shipley (1989)	55,263 released prisoners	Adult	None	Descriptive	3 years	Any	Time served in prison (6 months- 61 months and greater)	Rearrest	-, p=?

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Smith & Gartin (1989)	325 males with 1 police contact	Both	None  # prior arrests; # prior contacts for index offenses	Tobit	10 years	Any	Arrest	# future police contacts	-, p=.0206
	219 males with 2 police contacts								-, p=.0118
	151 males with 3 police contacts								-, p=.1956
	105 males with 4 police contacts								-, p=.05
	85 males with 5 police contacts								-, p=.0098
68 males with 6 police contacts	-, p=.3235								
Dunford, Huizinga & Elliott (1990) (experimental)	330 domestic violence suspects	Adult	None	Descriptive	6 months	Domestic violence	Arrest	Arrest	+, p=.584
Rasmussen, Benson, Kim & Zuelke (1990)	4,398 convicted offenders	Adult	# prior prison commitments, # prior probations	Survival	1-5 years	Drug	Length of incarceration	Subsequent probation or incarceration	+, p=.1736
Schmidt & Garner (1990)	2,268 individuals released from federal prison	Adult	None	Descriptive	5 years	Any	Length of incarceration	Rearrest	+, p<.10
								Reincarceration	+, p<.01

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Smith & Paternoster (1990)	2,716 referred youths	Juvenile	# prior referrals	Probit	20 months (average)	Any	Juvenile court referral	Subsequent referral	- , n.s.
				BCG/Heckman					- , n.s.
				NLTSLs					- , n.s.
				Tobit					- , n.s.
				Negative binomial					- , n.s.
Berk, Campbell, Klap & Western (1992) (experimental)	1,658 domestic violence suspects	Adult	Prior arrest (y/n)	Logit	6 months	Domestic Violence	Arrest	Police report/victim report	- , p>.05
Clarke & Harrison (1992)	4,919 offenders released from prison	Adult	# prior arrests	Logit	24.8-27.3 months	Any	Length of incarceration	Rearrest	+, p<.05
Hirschel, Hutchison & Dean (1992)	650 domestic violence suspects	Adult	None	Descriptive	6 months	Domestic violence	Arrest	Rearrest	- , p>.05
Pate & Hamilton (1992)	907 domestic violence suspects	Adult	Prior assault (y/n)	Logit	6 months	Domestic violence	Arrest	Rearrest	- , p>.05



<b>STUDY</b>	<b>SAMPLE</b>	<b>POP</b>	<b>CRIMINAL HISTORY CONTROL</b>	<b>STATS</b>	<b>FOLLOW-UP PERIOD</b>	<b>ORIGINAL OFFENSE</b>	<b>OFFICIAL SANCTION</b>	<b>RECIDIVISM MEASURE</b>	<b>FINDINGS</b>
Sherman, Smith, Schmidt & Rogan (1992)	1,133 domestic violence suspects	Adult	Prior violence (y/n)	Negative binomial regression	6-22 months	Domestic violence	Arrest	Police report	+, p<.05
Smith & Akers (1993)	228 males in community control, 226 males in prison	Adult	Prior felony within last 5 years (y/n)	Logit	5 years	Any	Prison v. community control	Rearrest	-, p=.3515
								Reimprisonment	-, p=.0062
Belenko, Fagan & Dumanovsky (1994)	2,758 offender in fast-track; 3,225 offender in regular processing	Adult	Prior arrest (y/n, offense specific)	Logit	2 years	Any	Jail/prison (y/n)	Rearrest	+, p<.05
				Survival				Sentence length Incarceration v. nonincarceration sanction	-, p<.05 +, p<.05
Fagan (1994)	3,424 cocaine, 3,403 crack arrestees	Adult	Prior arrest rate	ANOVA	1 year	Drug	Incarceration >1yr, Incarceration <1yr, probation, fine, dismissal	Rearrest	-, p>.05
Fagan (1995)	800 juveniles charged with a felony	Juvenile	# prior charges	Survival	4 years	Robbery or burglary	Length of incarceration	Rearrest	-, p=.076

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Harer (1995)	865 federal prisoners released during 1987	Adult	# prior convictions, # prior incarcerations, criminal history score	Logit	3 years	Any	Prison term (months)	Rearrest or parole revocation	+, p>.05
	490 federal prisoners recidivating at least once			OLS				Rearrest/parole revocation frequency	-, p<.10
Paternoster & Piquero (1995)	1,422 high school students	Juvenile	# times drugs used	SEM	1 year	Drugs	Juvenile court	Self-reported substance use	+, p<.05
Tolman & Weisz (1995)	588 male domestic violence offenders	Adult	Previous police contact or arrest (y/n)	Logit	18 months	Domestic violence	Arrest Conviction	Subsequent police contact	-, p=.0317
								Rearrest	+, p=.5295
									Arrest Conviction
								+, p=.7146	

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS	
Dejong (1997)	4,505 male arrestees	Adult	# prior arrests	Survival	3 years	Any	Jail sentence (y/n) Length of incarceration (days)	Rearrest	+, p=.1446	
									- , p=.0012	
				Logit					+, p=.0074	
									+, p=.0446	
	1,863 male arrestees with at least 1 prior arrest	Adult	# prior arrests	Survival	3 years	Any	Jail sentence (y/n) Length of incarceration (days)	Rearrest	+, p=.0583	
									- , p=.0133	
									Logit	+, p=.0549
										+, p=.4168
2,642 first time male arrestees	Adult	# prior arrests	Survival	3 years	Any	Jail sentence (y/n) Length of incarceration (days)	Rearrest	+, p=.4325		
								- , p=.1711		
								Logit	+, p=.0193	
									+, p=.0736	

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS	
Davis, Smith & Nickles (1998)	1,133 male domestic violence arrestees	Adult	# misdemeanor convictions, # felony convictions, # battery arrests	Logit	6 months	Domestic Violence	Jail	Rearrest	+, p=.506	
Myner, Santman, Cappelletty & Perlmutter (1998)	138 convicted males	Juvenile	None	OLS	2 years	Any	Length of 1st incarceration (days)	# subsequent convictions	+, p<.001	
Taxman & Piquero (1998)	3,671 drunk driving offenders	Adult	# prior traffic convictions; 1st drunk driving conviction (y/n)	Survival	3 years	Drunk driving	Jail	Days to reconviction	+, p>.05	
	2,528 first time drunk driving offenders								Guilty disposition	+, p>.05
									Jail	-, p>.05
Thistlethwaite, Wooldredge & Gibbs (1998)	683 suspects arrested for domestic violence	Adult	# prior convictions-violent crime	Logit	1 year	Domestic violence	Convicted	Rearrest	+, .05<p<.10	
							Months in jail		-, p>.10	

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Conness (2000)	54 college students who have shoplifted	Adult	None	Descriptive	?	Shoplifting	Arrest	Self-report shoplifting later	+, p=.585
								Self-report more serious crime later	+, p=.646
Gainey, Payne & O'Toole (2000)	276 offenders participating in electronic monitoring	Adult	# prior arrests	Logit	5 years	Any	Time in Jail	Arrest	-, p>.05
				Survival			Time in Jail		-, p>.05
Gross, Cramer, Forte, Gordon, Kunkel & Moriarty (2000)	177 male domestic violence misdemeanants	Adult	Prior arrest or conviction (y/n)	Logistic	18-24 months	Domestic Violence	Jail sentence	Rearrest	+, p=.75
								Reconviction	+, p=.75
Sabol, Adams, Parthasarathy & Yuan (2000)	215,263 federal prisoners recidivists	Adult	None	Descriptive	3 years	Any	Time served in federal prison	Reincarceration	+, p<.001

<b>STUDY</b>	<b>SAMPLE</b>	<b>POP</b>	<b>CRIMINAL HISTORY CONTROL</b>	<b>STATS</b>	<b>FOLLOW-UP PERIOD</b>	<b>ORIGINAL OFFENSE</b>	<b>OFFICIAL SANCTION</b>	<b>RECIDIVISM MEASURE</b>	<b>FINDINGS</b>
Yu (2000)	415 respondents w/ 1+ drunk driving arrests	Adult	None	OLS	Cross-sectional	Drunk driving	Sentenced to jail (y/n)	Total # drunk driving offenses	-, p>.05
	352 respondents w/ 1+ drunk driving arrests			Logit				Arrested for 2nd drunk driving offense (y/n)	-, p>.05
	150 respondents w/ 2+ drunk driving arrests							Arrested for 3rd drunk driving offense (y/n)	-, p>.05
	80 respondents w/ 3+ drunk driving arrests							Arrested for 4th drunk driving offense	-, p< .01
Ulmer (2001)	516 felony offenders	Adult	Prior convictions (weighted for severity)	Logit	2 years	Any	Incarceration v. probation	Rearrest	-, p>.05
								Severity of rearrest	-, p>.05

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS				
Bernburg & Krohn (2003)	529 7th and 8th grade males	Juvenile	# prior self-reported serious delinquent activities	Poisson Regression	3 years	Any	Police intervention	Self-reported serious crime ages 19-20	+, p<.05				
							Juvenile justice intervention		+, p<.01				
							Police intervention	Self-reported drug sales ages 19-20	+, p<.01				
										4 years	Juvenile justice intervention		+, p<.01
					Police intervention	Self-reported general crime ages 21-22	+, p<.01						
					Juvenile justice intervention		+, p<.01						
					Police intervention	Self-reported drug sales ages 21-22	+, p<.01						
Juvenile justice intervention		+, p<.01											

STUDY	SAMPLE	POP	CRIMINAL HISTORY CONTROL	STATS	FOLLOW-UP PERIOD	ORIGINAL OFFENSE	OFFICIAL SANCTION	RECIDIVISM MEASURE	FINDINGS
Fagan, Kupchick & Liberman (2003)	2382 adolescents charged with a felony	Juvenile	# prior arrests, age at 1st arrest, prior incarceration (y/n)	Survival	7 years	Robbery, assault, or burglary	Sentence length	Time to arrest (any)	+, p<.001
								Time to arrest (violent)	+, p>.05
								Time to arrest (property)	-, p>.05
								Time to arrest (weapon)	-, p>.05
								Time to arrest (drug)	+, p<.05
								Time to incarceration	-, p>.05
								Juvenile court action	
								Time to arrest (any)	+, p<.001
								Time to arrest (violent)	+, .001<p<.05
								Time to arrest (property)	+, .001<p<.05
								Time to arrest (weapon)	+, .001<p<.05
								Time to arrest (drug)	+, p>.05
								Time to incarceration	+, p>.05



<b>STUDY</b>	<b>SAMPLE</b>	<b>POP</b>	<b>CRIMINAL HISTORY CONTROL</b>	<b>STATS</b>	<b>FOLLOW-UP PERIOD</b>	<b>ORIGINAL OFFENSE</b>	<b>OFFICIAL SANCTION</b>	<b>RECIDIVISM MEASURE</b>	<b>FINDINGS</b>
Sung (2003)	263 drug sales offenders	Adult	# prior adult arrests	Logit	3 years	Drugs	Incarceration time	Rearrest	+, p>.05
Ventura & Davis (2005)	519 domestic violence charges	Adult	# prior domestic violence charges	Logit	1 year	Domestic violence	Conviction v. dismissal	Rearrest	-, p<.05
Bernburg, Krohn & Rivera (2006)	870 7th and 8th grade adolescents	Juvenile	# prior self-reported serious delinquent activities	Logit	18 months	Any	Juvenile justice intervention	Self-reported serious delinquency	+, p<.05

\*Table Abbreviations:

POP= population sampled

STATS= statistical technique used

Logit= logistic regression

ANOVA= analysis of variance

Survival= Survival regression (proportional hazards)

OLS= ordinary least squares regression

SEM= structural equation modeling

p= one-tailed significance level

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