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A Process and Outcome Evaluation of an Agricultural Crime Prevention Initiative

ABSTRACT

Agricultural crime victimization—including theft of farm crops, livestock, pesticides, and equipment—and efforts to address it remain largely ignored despite the potential of such crime to affect adversely the lives and businesses of farmers and increase the costs of food to consumers. The Agricultural Crime, Technology, Information, and Operations Network (ACTION) initiative, located in California's Central Valley, was recently developed to combat such crime. We present findings from a process and outcome evaluation of this program. The findings are limited by the design of the study but nonetheless suggest that efforts to increase guardianship measures among farmers (e.g., marking equipment and livestock and using surveillance equipment), to "harden targets" (e.g., locking tractors and storing chemicals in locked storage sheds), and to arrest and prosecute offenders, may help reduce agricultural crime victimization.

INTRODUCTION

Despite large-scale changes in the American economy over the past 200 hundred years, agriculture remains one of its central pillars. Indeed, although farmers today comprise only 1 percent of the U.S. population, "the entire agricultural sector accounts for 17 percent of all employment and 13 percent of gross domestic product" (Lobao and Meyer 2004:17). Not surprisingly, agriculture, like any business enterprise, experiences crime. Theft of livestock, crops and commodities, equipment, and chemicals and pesticides all take a toll on farmers, with estimated costs of $5 billion in losses annually (Swanson et al. 2002:628). Evidence from other countries suggests that the prevalence and costs of agricultural crime have been increasing in recent years (National Community Crime Prevention Programme 2004). Even so, agricultural crime remains largely ignored by criminologists and criminal justice policymakers (Weisheit and Donnermeyer 2000; Barclay 2001).

Recently, however, the U.S. Department of Justice's Bureau of Justice Assistance facilitated the development of an agricultural crime prevention program in California. The effort, called the Agricultural Crime Technology Information and Operations Network (ACTION), consists of collection and analysis of agricultural crime data, education of law enforcement and farmers, use of equipment marking, deployment of surveillance equipment, aggressive law enforcement and prosecution, and collaborative communication and efforts among law enforcement agencies within and across counties. As the program elements indicate, ACTION assumes that agricultural crime can be deterred and, similar to other opportunity theory-guided property crime prevention programs in urban areas (Eck 2002), that increased guardianship and target hardening reduces the likelihood of victimization. With its multi-dimensional approach, including activities consonant with the recent trend towards problem-solving and "intelligence-led policing" (Ratcliffe 2003; Peak and Glensor 2004), ACTION stands as one of the first systematic, proactive efforts in the United States to address agricultural crime. Insights gleaned from this pilot initiative thus should prove instructive in facilitating the study of such crime and how to effectively combat it.

Drawing on data from a study funded by the National Institute of Justice, we present results

from a process and outcome evaluation of ACTION. We begin first by describing the context in which the program emerged, research on agricultural crime, and the evaluation design, which relies on a "dose" model strategy for assessing the program's impact. We then discuss the results, which suggest that the program contributes to intended intermediate program outcomes, to reduced victimization at higher levels of dose, and to arrests and prosecutions that otherwise likely would never have occurred. However, the impacts on longer-term end outcomes, such as reduced crime, remain unclear, primarily for reasons that plague evaluations of ecological-level criminal justice programs, such as community policing—specifically, the lack of randomization of the intervention and the questionable appropriateness of inferring results from quasi-experimental designs. We discuss these findings and issues, as well as the study's implications for research and policy.

BACKGROUND

It is difficult to understate how little attention has been given to the study of agricultural crime.[1] Recent reviews (e.g., Weisheit and Donnermeyer 2000; Barclay 2001) point primarily to a collection of anecdotal findings. Examination of the few empirical studies, which typically consist of county or state surveys of farmers, suggests several generalizations: Agricultural theft is more common than thought (some estimates suggest that upwards of 60 percent of farmers experience agricultural theft—Dunkelberger et al. 1992); most agricultural crime involves property theft; farmers are reluctant to report crimes, in part because of a belief that law enforcement can not or will not take it seriously; and few farmers use sophisticated security measures (see, e.g., Bean and Lawrence 1978; Donnermeyer 1987; Farmer and Voth 1989; Peale 1990; Deeds et al. 1992; Dunkelberger et al. 1992; Saltiel et al. 1992; Sugden 1999; Barclay and Donnermeyer 2002). Despite the limited research to date, the identified patterns suggest a reasonable, if tentative, foundation for developing programs and policies aimed at reducing agricultural crime victimization. For example, such efforts might focus on improving law enforcement responsiveness to theft on farms and implementing educational efforts aimed at helping farmers understand the steps that they can take to protect their property.

To non-farmers, agricultural crime may seem either esoteric or trivial. Indeed, why distinguish farm crime from any other kind? The simple answer is the one that criminologists give when they study any type of crime, such as homicide or burglary, rather than all crime—namely, the causes of specific crimes may vary (Osgood et al. 2002). Similarly, interventions may have differential effects on different kinds of crimes, just as specific types of offenders may respond differently to similar treatments (Cullen et al. 2000). To farmers, of course, the answer is more mundane—agricultural crime targets their property and directly affects their livelihood. For example, the replacement value for a single theft of herbicide can be up to $70,000, sometimes more; Weisheit and Donnermeyer (2000:337) cited a case in California in which officials prosecuted an individual who had fenced $1.5 million in stolen herbicides. Such theft will likely worsen as farmers increasingly rely on expensive equipment, herbicides, pesticides, fertilizers, and production technologies (Barclay 2001; Lobao and Meyer 2001). As but one example, cattle breeding costs can be reduced substantially by purchasing frozen bull semen. Technologies for freezing semen have improved in recent years, and so the likelihood of such theft increases, especially given the considerable profits involved—for example, a single tank of frozen bull semen can be worth $75,000 or more (Hernandez 2005:B3). Other types of agricultural theft are also relatively common. By some estimates, approximately 20,000 cattle and 50,000 horses are stolen annually (Swanson et al. 2002:636). Equipment theft, too, can be relatively easy to commit and quite lucrative. As one typical account noted, "A $60,000 backhoe can be hauled off at night, have its serial number changed, and be sold without a title at an out-of-state auction" (Malone 2005:A1).

There is, indeed, substantial reason to believe that crime in rural areas and on farms may differ not only in kind but in the factors that contribute to it (Farmer and Voth 1989; Weisheit and Donnermeyer 2000; Barnett and Mencken 2002; Wells and Weisheit 2004). Not least, for example, is the sheer size, as well as isolation, of many farms, which creates numerous opportunities for theft due to limited guardianship (Donnermeyer and Barclay 2005). Also, law enforcement offices and prosecutors traditionally have not taken agricultural crime seriously (Barclay 2001), thus removing a formal legal source of deterrence. Even if such crimes were

prioritized, catching criminals or obtaining sufficient evidence can be challenging enough to all but eliminate the possibility of successful convictions. At the same time, research suggests that farmers are reluctant to contact law enforcement or to adopt security measures that might help prevent or reduce theft (Weisheit and Donnermeyer 2000; Barclay and Donnermeyer 2002).

Against this backdrop, few efforts to address agricultural crime at the local, state, or national level have been documented in the United States (Saltiel et al. 1992; Barclay 2001; Barclay and Donnermeyer 2002). Indeed, Donnermeyer and Barclay (2005:4) recently observed that "studies of policing and farm crime are nearly non-existent." To our knowledge, the sole exception is the ACTION project, which began in Tulare County and quickly expanded to include San Luis Obispo, on the coast, and seven Central Valley counties (Fresno, Kern, Kings, Madera, Merced, San Joaquin, and Stanislaus). These counties include large numbers of agricultural producers, some with over 5,000 farms. Collectively, the counties produce several billion dollars of products in a given year; Tulare alone accounts for roughly 10 percent of the total value of farm products in the State of California (California Agriculture Statistics Service 2004).

The origins of ACTION lie with the Rural Crime Prevention Demonstration Project developed by the Tulare County District Attorney in 1995. Several years later, in 1999, the California Legislature provided $3.5 million to Tulare and seven other counties for what was called the Rural Crime Prevention Program, later renamed the Central Valley Rural Crime Prevention Program (CVRCPP). This effort entailed the creation of a regional task force comprised of county agricultural commissioners, district attorneys, county sheriffs, and interested property owner groups or associations, and required participants to contribute their agricultural crime records to a centralized database. (Traditionally, law enforcement agencies in these counties, as with most counties nationally, have not coded such crime separately from, say, general property crime.) At the same time, Tulare County received federal funds to expand on the program to create a more holistic and integrated approach to preventing and reducing agricultural crime.[2] (Another county, San Luis Obispo, was added to ACTION in 2004 through a parallel effort to the CVRCPP, called the Coastal Valley Rural Crime Prevention Program.)

It was not, however, until 2002 that ACTION matured into the program, which consists of

five sets of activities, it is today. First, developing a database of reported agricultural crime was a central activity of the program, but it required several years of effort both to create it and then to educate all participating counties about the process for entering and accessing data. Today, ACTION hosts a web-based records management system on their own secure server, which serves as the central repository for any agricultural crime incident record occurring within the nine-county region. Information that is recorded in the database includes details of the number, types, and geographically specific locations of offenses. Its accuracy, like that of any law enforcement data repository, depends not only on the extent to which farmers report crime but also the extent to which law enforcement agents in each county enter agricultural crime information that they receive or learn about in their county.

Second, efforts to raise public and law enforcement awareness about agricultural crime also changed. Over time, ACTION's efforts to increase general awareness have evolved to include such activities as meeting with farmers at county farm bureau meetings, hosting meetings with law enforcement officers and prosecutors to share information regarding county- and program-level activities, maintaining the ACTION web site, participating in the annual agricultural exposition in Tulare, and promoting ACTION and its services in farm bureau publications and other media. Each of these efforts aim to educate the diverse groups about the prominent impact of agricultural crime on farmers, stress the importance of reporting incidents, and present solutions to help prevent victimization.

Third, counties varied in the extent to which they promoted the use of a numbering and tracking system and application of owner-applied numbers (OANs) to assist with crime investigations and recovery efforts. Some variation still exists, but the basic approach has developed into one in which ACTION staff, when contacted and requested by farmers, go directly to farms to stamp equipment. Some counties maintain their own marking equipment; satellite ACTION staff or officers in these counties assist farmers.

Fourth, considerable effort went into identifying and modifying surveillance equipment that could effectively be used in farm settings. Initially, staff realized that much of the equipment they needed either did not exist or had to be modified substantially to be effective in farm

settings. For example, a conventional surveillance video camera can not be placed in a field without easily being seen. The ACTION staff thus created different "disguises" for the equipment. They might, for instance, place a camera inside an old can or a replica stump. They also worked to build an inventory of equipment to keep up with demand for it.

Fifth, and finally, the program has focused on educating law enforcement and prosecutors about how to collect evidence and successfully prosecute agricultural crimes. Counties are given funds and in-kind contributions to support part- or full-time dedicated staff in crime units in each of Sheriffs' department as well as dedicated prosecutors in each District Attorneys' office who specialize in the investigation and prosecution of agricultural crime. Vertical prosecution, in which agricultural crime cases are handled by specific prosecutors who work in conjunction with similarly specialized law enforcement officers is the main strategy promoted by the program.

It should be emphasized that a central pillar of the program was also an emphasis on improving communication among law enforcement, prosecutors, and agricultural agencies within and across counties, but achieving this goal, too, required several years to achieve. The literature and analysis of our interviews with practitioners document that communication between law enforcement agencies, as well as between these agencies and prosecutors' offices, frequently is problematic and minimal. ACTION has promoted communication in several ways—educating law enforcement and prosecutors about agricultural crime, enabling counties to assign law enforcement officers and prosecutors to specialize in such crime, convening meetings, and, not least, providing the officers with Nextel® phones that enable the officers to more easily talk directly with one another about cases they are working. The latter have reportedly been especially useful in enabling officers to talk frequently, to identify common offending patterns across counties, and to coordinate between-county efforts to target and arrest offenders.

Because the program depends heavily on the amount of support counties receive from local and state authorities, and because prosecutor priorities can vary among counties, implementation of the different components of ACTION can vary. Thus, at one extreme, a county might contribute little to the database, make few efforts to raise public or law enforcement officer awareness about agricultural crime, do little to promote farm equipment marking, encourage or

enable the use of surveillance equipment, or aggressively prosecute agricultural crimes, and , communicate little with other counties or ACTION. At the other extreme, a county might actively promote each activity. Although ACTION encourages implementation of all components, we found that counties varied considerably in their implementation of the activities, even after the program had more or less fully evolved.

Collectively, the goals of these diverse activities are to "harden" targets (i.e., make them more difficult to steal) and to improve crime-solving capacities through more effective criminal investigation and prosecution efforts to deter agricultural theft. Two points bear emphasis. First, the target-hardening goal is central to the program, and ultimately hinges on whether the program's activities change farmers' crime prevention behaviors, what we discuss below as intermediate outcomes—that is, behaviors, such as marking equipment and taking various guardianship measures, that might ultimately reduce victimization. Second, the program's goals are consonant with the emphasis in recent years on "intelligence-led policing" (Ratcliffe 2003; Peak and Glensor 2004). ACTION's approach reflects this trend, especially its emphasis on local partnerships and using analysis to identify how and where to strategically allocate resources to prevent crimes. In this regard, then, the program has relevance not only for other efforts to reduce agricultural crime, but also those that incorporate similar policing strategies.

Prior research suggests that programs guided by a theoretical logic tend to be more effective (Rossi et al. 2004). Notably, ACTION is guided by several well-established crime theories. Primarily, it builds on opportunity theory through its focus on guardianship measures and on steps farmers can take to reduce exposure. It also is guided by rational choice and deterrence theories in its emphasis on aggressive prosecution and punishment of agricultural crime.

Although some evaluations have been conducted of situational crime prevention strategies to reduce specific types of crime (Clarke and Felson 1993a-b; Hope 1995; Felson 1998; Weisburd and McEwen 1998), few have focused on agricultural crime or on programs to reduce such crime through situational crime strategies. Regardless, ACTION has taken steps to help reduce different kinds of theft (e.g., equipment, livestock, crop, chemical/pesticide) that parallel those that have been recommended for reducing an analogous crime, car theft, including the use of

markings and serial numbers (Felson 1998:168).

In short, reasonable grounds exist to anticipate that the theoretical logic of the program is sound. At the same time, agricultural crime may be distinct from offenses that have been the subject of tests of opportunity theory and situational crime prevention strategies. It can, for example, encompass a considerable diversity of stolen products, ranging from fuel to livestock to steel irrigation pipes. In addition, farms are typically located in rural areas and are not only extremely exposed but difficult to guard in any consistent or comprehensive manner. To be clear, we do not directly test the above criminological theories. Rather, our purpose here is simply to emphasize that ACTION is guided by a clear and well-tested theoretical rationale.

DATA AND METHODS

Overview

The present study draws on several types of data, including a survey of farms, interviews with ACTION staff, law enforcement officers, and prosecutors, and official (law enforcement) records on farmer equipment marking (i.e., use of OANs). This study employs a range of what we term "dosage" measures—that is, measures that reflect the level of program implementation in each county. These county-level measures are used to predict individual-level (farm-level) agricultural crime victimization as well as other dimensions, such as the quality of life among farmers, relevant to evaluating the success of the program.[3] For the process evaluation, we examine whether higher county-level doses of ACTION contribute to greater farm-level involvement in activities, such as increased guardianship of property, that comprise the program's intermediate process outcomes. We then link the process and outcome evaluations by examining whether inclusion of the process measures reduces or eliminates any observed association between the dose and outcome measures. The causal logic, stated simply, is that ACTION's activities (e.g., educating farmers about steps they can take to fight crime) will contribute to intermediate process outcomes (e.g., farmers actually taking recommended steps for protecting their property) and, in turn, to "end" (longer-term) outcomes (e.g., less victimization).

Given that multiple counties participate in ACTION, it is unrealistic to assume that program

treatment is provided equally across them. Rather, one would anticipate, and the program staff verified, that even though ACTION has been in existence for several years, some counties have implemented ACTION to a greater degree than others. As a result, we can expect that outcomes among farmers would be influenced by the county in which they reside, and, in particular, the level of program implementation in that county. For this reason, we have designed this study to reflect potential differences in dosage across the counties. In essence, then, the study uses multiple comparisons. As described in greater detail below, we use logistic, Poisson, and ordinary least squares (OLS) regression analyses to test our central hypothesis, one flowing directly from the causal logic of the program—namely, farmers in higher dosage counties should have improved intermediate outcomes (e.g., greater use of guardianship measures) and improved end outcomes (e.g., less victimization). We also test a related hypothesis—namely, has the presence of ACTION contributed to the identification and arrest of agricultural crime suspects?

Data

Using lists of farms provided by Agricultural Commissioners in each of nine counties—eight Central Valley counties in California and one coastal county—we conducted a paper-and-pencil, self-administered survey of farmers. The lists included most farms in the study area, since farms must register with the Commissioners if they use pesticides, on which all but the smallest operations typically rely. However, the study's findings may not generalize to all farms, especially livestock and other such operations that do not use pesticides.

We created the survey instrument with the assistance of farmers, who provided insights prior to and during a field test.[4] We also relied on prior studies (e.g., Bean and Lawrence 1978; Farmer and Voth 1989; Cleland 1990). Administered in fall 2004, the survey asked farmers about crime victimization experiences in the prior 12 months and characteristics of their farms. Following Dillman's (2000) recommendations for surveying, we gave farmers advance notice of the study through postcards. Surveys then were mailed in three waves, with one-month intervals between each. The final sample was 823 and the response rate was 43.2 percent[5], a rate comparable to other studies involving surveys of farmers (see, e.g., Cleland 1990; Peale 1990;

Ballweg 1991; Abaidoo and Dickinson 2002; Barham et al. 2004; Donnermeyer and Barclay 2005).

This study used the survey data as well as two other sources to create county-level dosage measures, which then were assigned to farms residing in the respective counties, for predicting agricultural crime victimization among farms. Five ACTION staff members and an insurance agent familiar with the program and actively involved in efforts to reduce claims in the region were asked in a survey to rank order the nine counties according to each county's level of program implementation. In addition, ACTION provided OAN records (i.e., information about which farms were marked with OANs) for calendar years 2003 and 2004.

Dependent Variables

As shown in Table 1, we examine the following dependent variables, computed at the farm unit of analysis: A global measure and five specific types of victimization[6], including small equipment theft (e.g., tools), serious theft (e.g., tractors, large equipment, livestock, poultry, and commodities, such as grain, feed, seed, fruit, or vegetables), chemical or fuel theft (e.g., insecticide, herbicide, fungicide, fertilizer, diesel fuel, or gasoline), vandalism of machinery, fences, fields, or other property, or dumping of cars, dead livestock, or trash, and burglary of farm buildings (0=not victimized, 1=victimized); a victim diversity index (0=no victimization, 4=4 or more types of victimization)[7]; a measure of perceived change in victimization over the past year (1=increased a lot, 7=decreased a lot); loss from victimization in dollars (logged); quality of life (0=no change in quality of life due to fear of or actual victimization, 1=some type of change); and a measure of whether intermediate process outcomes (IPO) were achieved.[8]

The IPO measure merits discussion. As previously mentioned, the ACTION program consists of several efforts designed to promote certain crime prevention activities, termed here intermediate outcomes, among farmers. These activities—such as increased reporting of incidents to law enforcement, marking equipment, using surveillance technology, and adopting other proactive steps to protect property—in turn are held to cause reductions in victimization, the primary end, or long-term, goal of the program. A factor analysis of farmers' survey

responses led to the identification of a single IPO measure used here to capture the extent to which farmers in counties where ACTION has been more aggressively implemented have been more likely to undertake the various recommended crime prevention activities.[9] A higher IPO factor score indicates that a given respondent reported undertaking more such activities. It should be emphasized that while some of the measures used to create the IPO variable overlap with dose, most do not. Those that do, such as use of OANs, are measured at the farm, not county, level. For example, the percent OAN dose measure reflects the activities the ACTION program has taken, by county, to mark farms. By contrast, the IPO OAN measure captures whether individual farms have marked equipment. The latter may occur as a result of ACTION going to a farm to mark equipment, but it also, and more likely, given the limited ACTION staffing, results from farmers independently marking their equipment. In fact, the logic of the program's activities is that their educational efforts should lead farmers to seek ACTION's assistance in marking equipment or to mark equipment themselves. In sum, the dose and IPO measures capture different sets of activities, but to the extent that the causal logic underlying ACTION is realized in practice, they should be correlated (as shown in Table 3).

Finally, as we detail below in our discussion of Table 5, we also assess several other outcomes. Specifically, we examine the number of surveillance deployments and whether these in turn contribute to identification of suspects (measured as the number of suspects identified as a result of the deployments) and arrests (measured as the number of arrests occurring as a result of the deployments). Here, we are less interested in dose effects. Rather, our presumption, based on interviews with staff, is that the baseline comparison in all counties is zero, so any identification of suspects or any arrests can be taken as an impact of the program. Of course, some higher dose counties may (and did) have more deployments, suspect identifications, and arrests. Our focus, here, however, is on the program in aggregate. Although using zero as a counterfactual might appear to be too generous, we were apprised by program staff and county law enforcement officers, as well as prosecutors, that prior to the program, agricultural crimes were rarely if ever investigated or prosecuted in any of the counties. That pattern certainly comports with reviews of the agricultural crime literature (e.g., Weisheit and Donnermeyer

2000). Similarly, we were told that in cases involving deployment of surveillance equipment, virtually no action would otherwise be taken by law enforcement for the simple reason that conducting stakeouts in, say, a farm building or field would be too costly or impractical. Hence, for this one dimension, the comparison is an absolute one—that is, without the surveillance equipment, law enforcement would have taken virtually no action.

Independent (Dosage) Variables

Three measures of county-level dosage were used as independent variables: Staff ranking, percent of farms using OANs, and police responsiveness. For each of the measures, county-level values were computed and then assigned to farmers residing in each respective county.

For the first dosage measure, ACTION staff and an insurance agent familiar with the program and actively involved in efforts to reduce claims in the region were asked in a survey to rank order the nine counties according to each county's level of program implementation.[10] All responses were averaged and are used here as one measure of each county's overall level of program implementation (10=least implemented, 90=most implemented).[11] We relied on staff assessments for three reasons. First, few other sources of data exist to provide valid indicators of program implementation. Second, and related to the first, ACTION consists of many different activities, with some counties more aggressively implementing certain activities as compared to others. Third, ACTION staff are involved on a daily basis with all counties and have little obvious incentive to give biased assessments of one county over another.

A second measure of dose—county-level percentages of farms with OANs—was calculated by dividing the number of farms in each county recorded by ACTION staff as having used OANs in 2003 and 2004 by the total number of farms per county. The latter counts were obtained from U.S. Department of Agriculture (2004) for the most recent year (2002) in which an agricultural census was conducted.[12]

The final dosage measure came from the survey, which asked respondent to rate the change in the responsiveness of local law enforcement in the prior twelve months (0=same or worse, 1=improved). County-level averages were computed and then assigned to each farmer. The

logic was that victimization among farmers should be lower among those residing in counties where law enforcement responsiveness was greater.

Each of these measures of dose capture different aspects of the program. The first, staff rankings, reflects a global assessment of program implementation; the second, percent OANs, reflects an assessment of the extent of implementation of only one program activity, albeit a prominent one; and the third, like the second, yet again focuses more narrowly than the first, in this case capturing law enforcement proactiveness. Given the varying dimensions that these measures capture and the possibility that their influence on victimization may vary, we examine their effects separately. Analyses in which all three dose measures and the associated quadratic terms were simultaneously included produced similar results, although the statistical or substantive significance of some estimates diminished or were altered, likely resulting from the fact that introducing six terms (three dose and three quadratic terms), in addition to the presence of the many controls, placed excessive demands on the data. Also, two of the dose measures (percent OAN and police responsiveness) were correlated, while neither was strongly correlated with the third.[13] By examining each dose measure separately, we are better able to assess whether similar patterns emerged, regardless of the dose measure used, and to identify potential differential effects of the various types of dose on the outcomes.

Controls

Several variables were included in each regression model to control for county- and farm-level effects. The county-level control consisted of the 2003 reported property crime (burglary, motor vehicle theft, and larceny theft over $400) rate (per 1,000 residents) (California Criminal Justice Statistics Center 2005). County-level values were assigned to each individual farm. At the farm level, controls included the number of acres farmed (per 1,000 acres), the age group of operators (1=less than 30 years, 2=30 to 50 years, 3=greater than 50 years old)[14], whether fruits/nuts were the primary product, and whether an operation was family-operated.

Methods

The analyses were performed with SAS PROC GENMOD (SAS version 9.1), a procedure that fits generalized linear models (GLM). The GENMOD procedure is an extension of traditional linear models that can accommodate a wider range of data situations, including logistic and normally distributed response probabilities (SAS Institute 1999). This procedure was chosen because of its wide applicability to the different types of dependent variable distributions in our study. For example, it accommodates those variables with a binomial distribution (e.g., any victimization, small equipment theft, serious theft, chemical or fuel theft, vandalism, burglary, and change in quality of life), those that are Poisson-distributed counts (e.g., the victim diversity index), and those that are normally distributed and typically examined using ordinary least squares (OLS) regression (e.g., change in victimization, loss from victimization, and the IPO measure).

In each regression model, the independent dosage variable and its squared version are used to predict the ten dependent variables. Inclusion of the squared term allows for the detection of potential curvilinear relationships, which we anticipate may be evident in a dose model.[15] (For example, program effects may be stronger at higher rather than lower levels of dose.) Across all models, we computed "pseudo-R^2" statistics to measure model goodness of fit. Each model's log likelihood was subtracted from the minimum achievable log likelihood (determined by modeling only the intercept) of each and then divided by the sample size (N). Computing the exponent of the negative of this quantity and subtracting that from 1 provides a pseudo-R^2 measure that ranges between 0 and 1 (Allison 1995:248); the closer the model fit statistic is to 1.00, the better the model predicts the dependent variable.

We then calculated predicted probabilities for dependent and independent variable pairings. In each case, control variables were set to the grand mean across all counties and independent variables were set to each county's mean. For each measure of dose, the predicted probabilities (for logistic models) and predicted outcomes (for Poisson and OLS models) for statistically significant models were calculated and their values plotted for visual inspection of curvilinear relationships in cases where the quadratic (squared) dosage term was statistically significant.

All tests of statistical significance were conducted using a modified sandwich variance

estimator to calculate robust standard errors, which are presented in tables 2, 3, and 4. Sandwich variance estimates are commonly used in econometrics and statistics when researchers are unsure about the complete specification of the distribution in a fully parametric model but are relatively sure that the mean value is well-specified (Huber 1967; White 1980; Wooldredge 2002). The estimator used in this paper further adjusts the sandwich estimates for the possibility that there may be unobserved but persisting heterogeneity within clusters (e.g., counties).[16]

Before proceeding, our evaluation strategy should be reiterated: In the absence of appropriate pre-intervention data, and in a situation where a program has changed and expanded over time and where no other obvious "no treatment" counties exist, an analtyic approach is needed that can provide some foothold in assessing whether an ecological-level program has been effective. In the present context, it was not until 2002 that ACTION appeared to fully crystallize. And even then, each county had not fully embraced the program. Thus, we devolved on the following strategy: Create measures of the "dose" (i.e., amount) of the program implemented in each county, then survey farmers in these counties about crime they experienced. The central premise is that farms in counties with higher doses of the ACTION program in the years immediately prior to 2004 should experience less victimization, net of other factors.

In short, our paper attempts to estimate the effects of increased dosage on intermediate and end outcomes of an agricultural crime prevention program. As in all such analyses, a crucial identification issue is that of causality. Clearly, establishing causality from regression-based studies requires some way of controlling for the effects of unobserved but correlated factors on the outcomes of interest. In our analysis, this problem would surface if we believed that there existed some unobserved aspect of the farms or counties that are correlated with the dose they receive as well as the intermediate and end outcomes of interest. For example, it is possible that the reason why a county receives higher dosage is because farms there are more prone to victimization. Hence, it is victimization that explains dosage rather than the other way around. As we discuss at length in our discussion of the findings, without more detailed longitudinal data, or the ability to randomize, establishing causality is typically very difficult. At best, one can minimize this problem. In our analysis, we have attempted to mitigate the biasing effects of

unobserved attributes in three ways. First, we include individual-level predictors that could explain such unobserved heterogeneity. We also include in all models the county crime rate as a control. This variable was included to provide a proxy for the unobservable propensity to receive dosage that could potentially bias any inferences about the dosage variable. Hence, the dosage effects we report are net of this potentially reverse causal effect. This strategy, of course, does not mean that we have accounted for all unobserved factors. As a third precautionary measure, we also use adjusted sandwich estimators, discussed above, in deriving inferences. These estimators yield more conservative (larger) standard errors on the coefficients of interest to account for unobserved but persisting heterogeneity among farmers in the same county. All tests of significance use these adjusted standard errors.

FINDINGS

Like those in other studies, our analyses suggest that agricultural crime is relatively common. Inspection of Table 1 shows, for example, that over 60 percent of farms in this study experienced some type of victimization in the year prior to the study. Closer inspection shows that the bulk of such crime involved vandalism (49 percent of farms) and small equipment theft (30 percent). Nonetheless, a non-trivial percentage of farms, ranging between 11 and 14 percent, experienced serious theft, chemical or fuel theft, or burglary.

Insert Table 1 about here

The victimization diversity index indicates that farms on average experienced 1.11 types of victimization. Among those victimized, however, the mean was 1.78 (s.d. .95), suggesting that such farms typically experience two different types of victimization. Specifically, although 51 percent experienced one type of victimization, 26 percent experienced two types, 15 percent experienced three types, and 7 percent experienced four. Farmers as a group reported that there had been little change in victimization during the prior year. The annual per-farm loss due to victimization was $3,020, with $0 and $305,000 representing the lowest and highest claims, respectively, made by farmers (the logged version of this variable, presented in Table 1, is used in the subsequent analyses). Close to half of all respondents reported a change in their quality of

life in the prior twelve months. (We examine the IPO variable below when discussing the analyses. The factor score is standardized, so there is no intuitive interpretation of the mean.)

Examining the dose measures, we see that the staff ranking mean lies exactly in between the low and high rankings. That results from asking the staff to rank order the counties from lowest to highest in implementation. Examining the dose measures, we see that few farms mark their equipment. The average is 1.3 percent of all farms per county, with a low of 0.0 percent and a high of 3.0, indicating marked variation. For the third dose measure, we see that, on average, 18 percent of farmers reported that police responsiveness had improved in the prior year, with a low 12 percent in one county and a high of 30 percent in another.

Finally, counties varied considerably in their property crime rates. The average was 2.3 crimes per 1,000 residents, but the rates ranged from a low of 1.3 to a high of 3.1. Farms in the sample varied in size from less than 100 acres to 30,000 acres, with a mean of 540 acres. Almost three-fourths of the farms produced fruits and nuts as their primary commodity and 78 percent were family-owned operations. The average age of farmers was roughly 40 years.

We turn now to the question of whether higher doses of ACTION correspond to better outcomes (e.g., lower rates of victimization). Table 2 presents analyses for ten outcomes regressed on three measures of dose, controlling for such factors as property crime rates. Logistic regression results are presented for each of the dichotomous victimization outcomes as well as the change in quality of life measure; Poisson regression is used for the victim diversity index outcome; and ordinary least squares (OLS) regression is used for the change in victimization and victimization loss outcomes. Review of the table shows that all three dose measures are statistically significant for five of the outcomes, including any victimization, chemical or fuel theft, vandalism, the victim diversity index, and victimization loss. One or more of the dose measures is statistically significant in all but two cases—change in victimization and change in quality of life. In addition, the quadratic term is statistically significant in most models. Together, these patterns suggest that dose is consistently associated with the diverse set of outcomes and that the dose-outcome relationship is non-linear, signifying that the effect of dose on the outcomes varies depending on the level of dose.

Insert Table 2 about here

Interpretation of the linear and quadratic dose terms is not entirely straight-forward. The linear term indicates how unit changes in dose translate into unit changes in the outcome, but such an effect is modified by the quadratic term, depending on the level of dose.[17] For this reason, graphing predicted probabilities for varying levels of dose can facilitate interpretation. Figure 1 illustrates the point, using as the outcome whether farms were victims of any type of agricultural crime.[18] Unexpectedly, the general pattern suggests a counter-intuitive interpretation—namely, as dose increases (i.e., as we progress from counties with low levels of program implementation to those with high levels of implementation), the risk of victimization actually increases. However, after dose exceeds a certain threshold, as depicted with the dashed vertical line, the risk of victimization begins to drop. The latter finding suggests that with fuller implementation the program might well contribute to lower rates of victimization. However, the former raises important questions since one would not logically expect increases in an intervention to produce worse outcomes. Notably, this pattern was not anomalous—in plotting the predicted probabilities for the other outcomes, the same general pattern consistently emerged. In the next section, we discuss this pattern and explain how and why it might arise.

Insert Figure 1 about here

Finding that a relationship indeed exists between dose and outcomes, the next step is to determine whether this relationship can be explained by ACTION's anticipated intermediate outcomes. Table 3 presents analyses showing that higher levels of all three measures of dose in fact are associated with higher levels of the intermediate outcomes, a pattern confirmed by plotting the predicted probabilities.[19] In short, farms in counties where ACTION was more aggressively implemented were more likely to undertake activities—such as locking equipment and livestock in protected or guarded buildings, asking neighbors to watch one's property when away—promoted by the program. Viewed from the perspective of a process evaluation, such a finding is important in its own right because it suggests that the program has been effective in achieving behavioral changes among farmers.

Insert Table 3 about here

Given that higher levels of dose are associated with intermediate outcomes (Table 3), the logical question to address is whether the observed relationship between dose and the end outcomes (Table 2) can be explained by inclusion of the IPO variable, which itself is correlated with each of the outcomes.[20] If inclusion of this variable in the models presented in Table 2 can reduce or eliminate the dose effects, we have grounds to believe that the program's logic is sound—namely, the activities it undertakes may produce the desired intermediate outcomes (i.e., changes in farmers' behaviors) and these changes in turn likely contribute to the end outcomes.

Table 4 presents the results of regression analyses in which the IPO variable is included as a control. In almost all models, the dose effects are either eliminated or substantially reduced. Observe, for example, that the linear and quadratic dose terms are statistically insignificant in most models or, in those models where one or both are significant, the coefficients generally are considerably smaller than in Table 2. Thus, ACTION appears to be producing changes in farmers' behaviors and these behaviors in turn appear to contribute to the likelihood of victimization. Notwithstanding the seemingly paradoxical dose effects discussed earlier, this pattern lends support to the general causal logic that guides ACTION. A reasonable supposition, which will require empirical evaluation, is that continued and aggressive implementation of ACTION might well create significant reductions in agricultural crime over time.

Insert Table 4 about here

Finally, we turn to ACTION's deployment of surveillance technology to farmers' properties. Recall that an absence of such deployment is expected to result in no identification of suspects or, subsequent to identification, any arrests. The basis for this counterfactual is the fact that in all instances in which surveillance equipment was deployed, no action (e.g., overnight stakeouts) would otherwise have been taken because of the expense involved. Empirically verifying whether that in fact is how reality would otherwise unfold is difficult. Would, for example, a farmer's call to the police otherwise end with nothing more than mention in a database that a crime was reported? Or would the incident result in further action? Reliance on official records alone to assess this possibility would not be sufficient unless the data included information sufficient to identify "like" cases and mention of whether such activities as stakeouts were

undertaken. Here, we have accepted the assessment of law enforcement agents with whom we spoke who uniformly said that the cost of stakeouts as an alternative to collecting evidence through the use of surveillance equipment is cost-prohibitive and thus typically they are not conducted. By contrast, deploying video cameras requires little time or expense.

As inspection of Table 5 shows, over a two-year period ACTION deployed surveillance equipment 69 times, which led to the identification of 35 more suspects and 15 more arrests than would otherwise likely have occurred. Informally, we were told in interviews with prosecutors that they were able to successfully obtain convictions and that without the surveillance equipment evidence, and ACTION's assistance more generally, few if any successful convictions would have been undertaken during the study period. The findings suggest a non-trivial impact of ACTION, given the difficulty of obtaining arrests or convictions in agricultural crime cases. Ultimately, however, the veracity of this assessment rests on whether the assumption that no arrests would otherwise have occurred is correct.

Insert Table 5 about here

DISCUSSION

In sum, the analyses provide evidence of the following: (1) Higher program dose (i.e., program implementation) is positively, and unexpectedly, associated with agricultural crime victimization, though at higher levels of dose victimization decreases; (2) this general pattern holds across many types of victimization; (3) dose is positively associated with intermediate outcomes consisting of a range of farmer-undertaken crime prevention steps; (4) the effects of dose on the end outcomes are largely explained by the intermediate process outcomes, lending support to the causal logic of the program (i.e., program activities contribute to changes in farmers' behaviors, which in turn are associated with victimization); and (5) ACTION has increased the identification and arrest of agricultural crime suspects and possibly the successful prosecution of agricultural crimes. The first two findings bear particular discussion.

Clearly, it appears illogical that greater levels of dose would be associated with more crime or worse outcomes, especially given that dose is positively associated with intermediate

outcomes. Such relationships are, of course, possible. For example, some medical treatments actually contribute to increased disease, which is precisely why, in these cases, clinical trials are stopped (see, e.g., Kolata 2002). However, empirical research on opportunity theory, which guides ACTION's efforts, does not suggest that decreased opportunities increase victimization.

Viewed more closely, however, a relatively simple two-part explanation exists. First, counties with the highest amounts of agricultural crime may have more strongly embraced and implemented ACTION, and, second, insufficient time may have passed for the program implementation to have resulted in lower crime rates. Program staff confirmed that in their view the counties that most embraced ACTION were indeed those with larger perceived crime problems. Because of the absence of valid, over-time measures of agricultural crime, we cannot, however, empirically assess that impression. Our preliminary assessment was that the program had sufficiently matured over time, and that enough time had elapsed for a substantial program effect to emerge. But those assumptions could be incorrect. For example, although it appears that the program largely matured into a coherent effort by 2003, clearly—as the dose model itself implies—not all counties equally embraced ACTION.

These considerations give rise to the following possible scenario. On the one hand, counties with low rates of agricultural crime were not aggressive in implementing ACTION and thus, by the time of the survey, continued to have low levels of implementation and agricultural crime relative to other counties. On the other hand, counties with higher rates of agricultural crime aggressively implemented ACTION and continued to do so up to and through the survey. Even if the program reduced rates of agricultural crime among all participating counties and even more so among the higher dose counties, crime might well still be substantially higher in the latter counties precisely because their baseline rates of agricultural crime were so much higher. Observe that in such a scenario, program implementation still might well contribute to the intermediate outcomes and these in turn might contribute to improved outcomes.

To illustrate, consider an analogous situation in which a school attempts to improve student high school performance by emphasizing the development of strong reading skills among freshman. The program might improve reading skills among all students, especially among

those students with the lowest baseline levels of reading proficiency. However, the results of such improvements might not be easily detectible. For example, freshmen with the least reading proficiency might receive more of the program, all participating freshmen might then see their reading proficiency improve during their sophmore year, and this change in turn might improve student performance. However, precisely because of the stark differences in the baseline levels of performance, one might discover in a cross-sectional analysis of seniors' performance that students who received more of the program had the lowest levels of performance.

In short, the problem lies in a lack of random assignment, the possibility that changes may take years to emerge, and that such changes may not be sufficiently large to detect. Importantly, these issues are implicated in many recent criminal justice policy initiatives. Community policing, for example, has become one of the most common types of crime-fighting initiatives in the last twenty years (Goldstein 1987; Grinc 1994; Skogan 2003). Yet, attempts to evaluate such efforts confront a similar problem, one not easily solved by recourse to collecting data on changes in communities, policing, or crime. Such data do not eliminate the issue that communities that are least likely to embrace community policing, and yet may benefit the most from it, may fundamentally differ from those that do not (Grinc 1994). As a result, comparisons across communities, whether using dose or treatment-vs.-control group evaluation methods, necessarily build off a flawed semblance of an experimental design in which all subjects (e.g., communities) are considered to be equal except in whether or how much treatment was received. The problem is aggravated by the very real possibility that improvements may take many years to emerge and that even substantial improvements may be difficult to detect statistically.

Some studies attempt to address these issues through the use of statistical controls. However, as Lieberson (1985) and others (e.g., Heckman and Smith 1995; Berk 2005) have emphasized, such approaches frequently generate misleading results. Indeed, in quasi-experimental designs in which controls are used, a central challenge lies in controlling for "relevant" factors. The factors that make two or more areas comparable frequently is unknown and not easily or validly captured by, say, controlling for differing levels of poverty, unemployment rates, residential mobility, percent minority, or the like. In this study, we controlled for property crime rates on

the assumption that this measure would serve as a valid proxy for ecological-level conditions that might contribute to agricultural crime. Unfortunately, there is little solid research on the ecological-level causes of agricultural crime, and so the validity of the measure as a proxy is unknown. This problem plagues evaluations of community policing and other studies of criminal justice initiatives, especially those targeting communities or larger areas, because of the difficulty in arriving at valid measures of community difference that might be related to the outcome of interest and that might in turn bias estimation of a treatment effect.

A natural question, then, is whether evaluations such as the present one should be pursued. We believe they should. First, as Gibbs (1997) has argued, theories and, in this case, evaluations, have stronger support when tested against a range of scenarios. For example, when results from cross-sectional and longitudinal studies correspond, we tend to believe that an observed relationship, such as one between unemployment and crime rates, is likely to be causal. In our study, the focus was on establishing whether a cross-sectional relationship between program implementation and agricultural crime rates could be established. The results were mixed—at the lower levels of program implementation, greater amounts of the program actually appear to contribute to a greater likelihood of victimization, but at the higher levels greater amounts of the program appear to be associated with a reduced likelihood of victimization, net of various controls. A logical next step will be to conduct longitudinal analyses to determine if within-county increases in program implementation are associated decreased rates of victimization. If such is found, a stronger case may be made for the program's effectiveness.

Second, the evaluation, and others like it, provide a foundation for assessing the causal logic of a program. Such work is especially important in areas such as agricultural crime where little prior theoretical or empirical research exists to support the logic on which a program rests. Even in the presence of such research, cross-sectional evaluations can be used to verify empirically that the program elements have indeed been implemented and that they contribute to the short- or intermediate-term outcomes that in turn are known to improve longer-term, end outcomes.

Third, rigorous evaluations need not always entail the collection of longitudinal data, a fact that assumes particular importance when we realize that few criminal justice programs have

sufficient funds to collect such data or to analyze them. The example of surveillance deployments is illustrative. Here, the baseline point of comparison could safely be assumed to be zero. Thus, if the program produced any arrests, it could be viewed as effective. Whether arrests in turn contribute to specific or general deterrence is another matter, one that a larger body of basic and applied research can address. Evaluations of many criminal justice programs and policies may not be able to adopt such a strategy, but it is likely that many more can than currently is recognized. For example, the past decade has witnessed the emergence of a plethora of new initiatives focused on enhancing prosecutorial discretion and the ability of prosecutors to take a proactive stance towards crime prevention (Wolf and Worrall 2004). To the extent that such efforts lead to successful prosecutions where previously none would not be possible, then a similar evaluation strategy could be pursued. Again, whether prosecution itself leads to specific or general deterrence does not necessarily need to be established in an evaluation, especially against a backdrop of decades of research on the effects of punishment.

CONCLUSION

Agriculture remains a central pillar of American society, and yet the study of agricultural crime, such as the theft of equipment, livestock, crops, and chemicals, remains rare. Indeed, few comprehensive programs exist to combat such crime. To the best of our knowledge, ACTION stands as the only exception. Consonant with recent calls for intelligence-led policing and the lessons gleaned from evaluations of situational crime prevention programs, the program relies on a range of strategies—collection and analysis of data, education of law enforcement and farmers, equipment marking, use of surveillance technologies, aggressive prosecution, and collaborations among law enforcement agencies within and across counties—to deter would-be offenders and to limit opportunities for agricultural crime to occur.

The process and outcome evaluation results presented here suggest that the program holds the potential to reduce crime and to improve the ability of law enforcement agents to identify, arrest, and prosecute suspects. The results are by no means definitive. Indeed, given that the evaluation is of a pilot initiative aimed at preventing and reducing agricultural crime, this study

should be viewed as an exploratory attempt to determine whether such efforts can be effective. Nonetheless, the results do suggest that in areas where the program has been aggressively implemented that farmers respond by taking more crime prevention efforts and that these in turn may reduce crime, especially among counties with the highest levels of program implementation. More generally, the evaluation results underscore the importance of taking a broad view of program effectiveness and of including diverse measures of impact (Rossi et al. 2004).

These results should be of particular relevance to policymakers and practitioners interested in agricultural crime prevention. To date, researchers have been able to offer little guidance about how such initiatives might be structured or undertaken. This evaluation suggests that ACTION could be viewed as a promising program (Sherman et al. 1997). More generally, precisely because of the diverse activities the program undertakes, ACTION holds the potential to inform agricultural crime prevention efforts nationally. For example, few jurisdictions will be able to devote law enforcement officers exclusively to agricultural crime, but they nonetheless could pursue efforts to promote equipment marking, develop Rural Watch programs, or prosecute a select set of high-profile cases of agricultural crime (see, generally, National Community Crime Prevention Programme 2004). In certain respects, the bar has been set low—agricultural crime is relatively common and few jurisdictions have attempted to do anything about it. So, any effort that is premised on solid theory and sound implementation may well be able to make a substantial dent in agricultural crime for relatively little cost. That said, a central focus of ACTION is to encourage farmers to report crime, so any similar initiatives might well initially result in increased reporting of agricultural crime. For that reason, collecting an independent source of data, such as periodic surveys, on trends in such crime would be essential for assessing whether reductions in crime actually occurred.

For researchers, especially those involved in evaluation efforts, we believe the results also should be of interest. Among other things, they reinforce the importance of linking process and outcome evaluations, taking a broad view of program effectiveness, and being creative in identifying and developing appropriate measures of effectiveness. Just as clearly, they reinforce that serious limitations attend to cross-sectional evaluations of ecological-level programs—such

as community policing initiatives—not least because of the difficulty of randomly assigning some communities an intervention and the questionable utility of relying on statistical controls to approximate an experimental design. Too often, comparisons are made between an intervention site and a comparison site, where the latter cannot safely be assumed to be identical in all regards except the presence of the intervention and where statistical controls cannot produce true equivalence. At the same time, the baby should not be thrown out with the bathwater. Cross-sectional designs can, for example, help test the logic of a program, and the results can be juxtaposed against the findings from other studies, such as evaluations using longitudinal data that examine whether changes in implementation create changes in outcomes. In such cases, a cumulative body of findings from diverse methodological approaches may create a more compelling case in support of or against a program than any one approach by itself might allow.

This study also indirectly highlights an important question that remains neglected in the criminological and evaluation literature. Specifically, to what extent do marginal general deterrent effects exist, especially among crimes that typically are rarely prosecuted? For example, does prosecuting a small handful of such crimes produce the same general deterrent effect as prosecuting many more? How many exactly are needed to produce a more-than-nominal effect? After certain threshold levels of prosecution are achieved, do increases in prosecution make any difference? By and large, the literature is largely silent on this issue (see, generally, Nagin 1998; Akers and Sellers 2004). A similar question applies to other activities undertaken by ACTION and, more generally, to any components of any program or policy. That is, at what point does a particular activity produce a substantively significant effect, and is that effect in any way modified by the baseline level of the activity or of the outcome it is designed to influence? Such questions are, we submit, fundamental to evaluating criminal justice policies (Goldstein 1987). We suggest, however, that the more important issue they highlight is the potential, as Blumstein (1996) and others (e.g., Rossi 1980) have argued, for evaluation research studies to prompt social scientists to investigate basic questions about social phenomena.

ENDNOTES

[1] Greater attention has been given to the topic in Australia (see National Community Crime Prevention Programme 2004).

[2] As defined by ACTION, agricultural crime is "any property crime against a farmer, rancher, agricultural related business or other designated industry which takes place in the unincorporated rural areas of the state and impacts the victims' commercial production, distribution or economic livelihood derived from agricultural products, livestock, petroleum, chemicals, farm implements, and equipment."

[3] Packing houses and suppliers could be considered part of agricultural production, but we focused exclusively on farmers because they produce commodities and thus are the foundation for all other agricultural-related business.

[4] We thank Dr. Joseph Donnermeyer, an expert on rural crime who has conducted several studies of agricultural crime, for his comments and assistance in creating the survey.

[5] We initially mailed 2,286 surveys, of which 64 were returned due to bad addresses, leaving 2,222 for our eligible sample size. We received 960 completed surveys (43.2 percent response rate), but 134 were not farmers and 3 did not complete the survey, leaving a final sample of 823. Response rates varied from between 30 and 60 percent among the counties.

[6] Indexing is not always defensible if offenses are unique and if theory provides no guidance (Osgood, McMorris, and Potenza 2002). Similarly, if factors that predict specific crimes differ in kind or degree, indexing is not recommended (Osgood, Finken, and McMorris 2002).

[7] This index is not designed to capture a single underlying "agricultural crime" construct (for discussions of index creation, see Osgood, McMorris, and Potenza 2002). Rather, the aim is to capture the diversity of types of agricultural crime farms experience.

[8] Environmental crimes (e.g., dumping toxic waste) were not included because they do not typically or directly affect agricultural production. Also, ACTION does not target such crimes.

[9] The variables included surveillance equipment usage (0=yes, 1=no); prevalence of locking items up at night (aggregate mean of items locked away each night, including chemicals,

tractors, livestock, tools, equipment, and commodities); percent of respondent's equipment marked with an OAN number; use of traditional protection measures such as owning a dog and/or gun or asking neighbors to watch property (0=no steps taken, 10=all such steps taken); use of more modern protection measures such as installing lights around property, locks on doors and fuel tanks, using alarms or security cameras (0=no steps taken, 10=all such steps taken); reporting victimization of five specific crimes incidents to law enforcement, including small equipment, serious theft, chemical/fuel theft, vandalism, and burglary (1=yes, 0=no), and attending a meeting about agricultural crime (0=yes, 1=no). The analysis yielded one distinct factor (eigenvalue=1.58); the individual-level factor scores were used as the IPO variable.

[10] ACTION consists of several activities, and program staff pointed out that some counties appear to be more aggressive in implementing some (e.g., OAN-marking) rather than others (e.g., prosecution). We requested that staff make such allowances in arriving at their overall assessment of county-level implementation. As with many criminal justice initiatives, unpacking the precise activities that are part-and-parcel of the program or policy and that give rise to various outcomes stands as a central challenge. In this particular case, little measurable variation existed to conduct analyses of the full range of specific activities; hence, we enjoined staff to arrive at general assessments. That said, analyses of how each staff member ranked each county's performance with respect to several different activities revealed that a similar county ranking emerged, regardless of whether the activity-specific or general performance measures were used. The other two dose measures examine more specific program activities.

[11] The responses were initially coded 1 to 9, where 1 was equivalent to the county with the most implementation while 9 indicated the county with the lowest amount of program implementation. The inverse of this scale was taken so that an increase in staff ranking would be associated, or so we hypothesized, with a reduction in agricultural crime victimization. We then scaled the responses up by a factor of ten to produce coefficients large enough to easily denote in tables.

[12] One county emerged as an outlier in that the percentage of farmers using OANs, though relatively small, was substantially greater than that of the other counties. We therefore omitted

this county in the models in which percent OAN served as a measure of dose. The results were largely similar to those obtained when the county was included.

[13] The correlation between percent OAN and police responsiveness was .69; the correlations between these two measures and the staff ranking measure were .17 and .00, respectively.

[14] We used age categories as a basis for capturing generational differences between operators. Younger farmers may be more likely than older farmers to adopt new methods of security.

[15] Although inclusion of quadratic term increases the collinearity among the predictors, we have included these terms because we wish to investigate non-linearity in the relationship between dose and the outcomes. Multicollinearity only causes the precision of parameter estimates to drop relative to a model with non-collinear predictors. Given our sample size, this drop in precision is not problematic. Moreover, the ability to detect curvilinear relationships between the dosage and the outcomes is more important to our analyses than is the minimal gain in precision that we would obtain from not including the quadratic term.

[16] Analytical derivatives are available from the authors upon request.

[17] At first glance, the coefficients on the quadratic term for the dosage measures seem small. However, the scale of the dose measures should be kept in mind when interpreting these effects. The standard deviations of the squares of average staff rankings and the police responsiveness measures are 3,095 and 378 (across all farmers), respectively. This means that standardized coefficients on the two respective quadratic terms would be of the magnitude 3,095 and 378 times larger than the unstandardized coefficients displayed in the tables. Hence, for example, a coefficient on the quadratic term for average staff ranking of -0.001 in a logit model (for a binary outcome) represents a 3.095 reduction in the log-odds ratio. As such, it represents a relatively strong quadratic effect. These effects can be seen clearly in the plots.

[18] Our binary choice models were estimated using logistic regression. The general equation for predicting probabilities in these cases can be found in Menard (1995). However, the probabilities predicted for the figure and the other logit models involve equations that are different for each farm. Hence, to display graphically the effects of just one of these

predictors—in this case curvilinear dose effects—we have held fixed all other characteristics of the farmers to the sample average. In other words, these curves are plotted for the "average" farmer. As such, they plot the ceteris paribus (all else being equal) relationship between dosage and the probabilities of the various outcomes of interest.

[19] At a general level, the results indicate an association between dose and the IPO variable. The fact that the pattern may be non-linear, as evidenced by the statistically significant quadratic terms, may help account for the non-linear patterns identified in the Table 2 outcome analyses.

[20] The correlations between the IPO and outcome measures are statistically significant ($p < .001$) and as follows: any victimization (.41), small equipment theft (.51), serious theft (.33), chemical or fuel theft (.43), vandalism (.35), burglary (.59), victim diversity index (.69), change in victimization (-.44), victimization loss (.61), and change in quality of life (.28).

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Table 1. Descriptives

	N	Min - Max	Mean (S.D.)
Dependent Variables			
Any Victimization (0=not victimized, 1=victimized)	812	0.0 - 1.0	0.62 (.48)
Small Equipment Theft (0=not victimized, 1=victimized)	782	0.0 - 1.0	0.30 (.46)
Serious Theft (0=not victimized, 1=victimized)	810	0.0 - 1.0	0.14 (.35)
Chemical or Fuel Theft (0=not victimized, 1=victimized)	785	0.0 - 1.0	0.13 (.34)
Vandalism (0=not victimized, 1=victimized)	766	0.0 - 1.0	0.49 (.50)
Burglary (0=not victimized, 1=victimized)	796	0.0 - 1.0	0.11 (.31)
Victim Diversity Index (0=no victimization, 4=4 or more)	812	0.0 - 4.0	1.11 (1.15)
Change in Victimization (1=increased a lot, 7=decreased a lot)	779	1.0 - 7.0	3.89 (1.55)
Victimization Loss (log $)	811	0.0 - 12.63	3.71 (3.80)
Change in Quality of Life (0=no, 1=yes)	796	0.0 - 1.0	0.45 (.50)
Intermediate Process Outcomes (I.P.O.)	663	-1.4 - 5.3	0.00 (1.00)
Independent Variables			
Average Staff Dosage Ranking (10=lowest, 90=highest)	9*	12.0 - 81.9	51.50 (21.06)
Percent of Farms using OANs	9*	0 - 3.0	1.29 (.74)
Police Responsiveness (0=same or worse, 1=improved)	9*	11.6 - 30.0	18.38 (6.40)
Control Variables			
Property Crime Rate (per 1,000 residents)	9*	1.3 - 3.1	2.32 (.61)
Acres Operated (x 1,000)	758	0.0 - 30.0	0.54 (1.86)
Age of Operator (1= <30 yrs, 2= 30-50 yrs, 3= >50 yrs)	823	1.0 - 3.0	2.48 (.71)
Fruit/Nut as Primary Product (0=not primary, 1=primary)	728	0.0 - 1.0	.70 (.46)
Type of Operation (0=not family-owned/operated, 1=family)	770	0.0 - 1.0	0.78 (.42)

* County-level values were computed and then assigned to farmers in each respective county.

Table 2. Regression Analyses of Program Dosage Effects on End Program Outcomes

	Dose = Avg. Staff Ranking				*Dose = Pct. Farms Using OANs*				*Dose = Police Responsiveness*			
	Intercept (S.E.)	X (S.E.)	X^2 (S.E.)	Pseudo R2	Intercept (S.E.)	X (S.E.)	X^2 (S.E.)	Pseudo R2	Intercept (S.E.)	X (S.E.)	X^2 (S.E.)	Pseudo R2
Dependent Variables												
Any Victimization	.088	.085***	-.001***	.036	.480	2.439***	-.980**	.038	-2.280*	.318**	-.008*	.027
	(.560)	(.022)	(.000)		(.581)	(.653)	(.330)		(1.699)	(.149)	(.004)	
Small Equipment Theft	-1.170*	.063*	-.001*	.021	-.775	2.274**	-1.021**	.025	-1.626	.129	-.004	.016
	(.612)	(.025)	(.000)		(.643)	(.736)	(.365)		(1.772)	(.154)	(.004)	
Serious Theft	-1.619*	.030	-.000	.019	-.950	1.878*	-.923*	.022	-3.267	.208	-.006	.020
	(.794)	(.033)	(.000)		(.787)	(.916)	(.460)		(2.357)	(.204)	(.005)	
Chemical or Fuel Theft	-2.50**	.090**	-.001*	.021	-1.720	3.302**	-1.281*	.024	-6.814***	.546**	-.014**	.021
	(.888)	(.035)	(.000)		(.966)	(1.115)	(.533)		(2.319)	(.197)	(.005)	
Vandalism	-.670	.069**	-.001*	.031	-.367	1.554*	-.414	.035	-3.982**	.395**	-.009**	.029
	(.562)	(.022)	(.000)		(.588)	(.664)	(.332)		(1.668)	(.146)	(.004)	
Burglary	-2.800**	.083*	-.001	.013	-1.990	1.942	-.423	.015	-9.546***	.746***	-.018***	.016
	(.982)	(.039)	(.000)		(1.041)	(1.189)	(.569)		(2.643)	(.223)	(.005)	
Victim Diversity Index	-.233	.046***	-.000***	.051	.101	1.453***	-.529***	.056	-1.948***	.229***	-.006***	.041
	(.264)	(.011)	(.000)		(.276)	(.315)	(.154)		(.737)	(.063)	(.002)	
Change in Victimization	3.551***	-.016	.000	.021	3.277***	-.388	.096	.024	4.495***	-.104	.002	.020
	(.384)	(.015)	(.000)		(.395)	(.426)	(.219)		(1.262)	(.111)	(.003)	
Victimization Loss (log $)	2.989***	.150***	-.001***	.061	4.378***	5.256***	-2.157***	.066	-1.840	.619**	-.015**	.046
	(.916)	(.035)	(.000)		(.943)	(1.039)	(.531)		(2.883)	(.249)	(.006)	
Change in Quality of Life	-1.275*	.033	-.000	.018	-.848	.701	-.183	.015	-2.109	.119	-.003	.016
	(.551)	(.022)	(.000)		(.557)	(.631)	(.317)		(1.632)	(.142)	(.003)	

Note: Unstandardized coefficients are presented (standard errors are in parentheses). Logistic regression results are presented for each of the victimization outcomes as well as the change in quality of life measure; Poisson regression is used for the victim diversity index outcome; and ordinary least squares (OLS) regression is used for the change in victimization and victimization loss outcomes. Asymptotic standard errors were computed using a modified sandwich variance estimator (see discussion in text).

* $p < .05$ ** $p < .01$ *** $p < .001$

Figure 1. Predicted Probabilities of Farmers Being Victims of Agricultural Crime, Based on Program Staff Assessments of County-Level Program "Dose" (Level of Implementation)

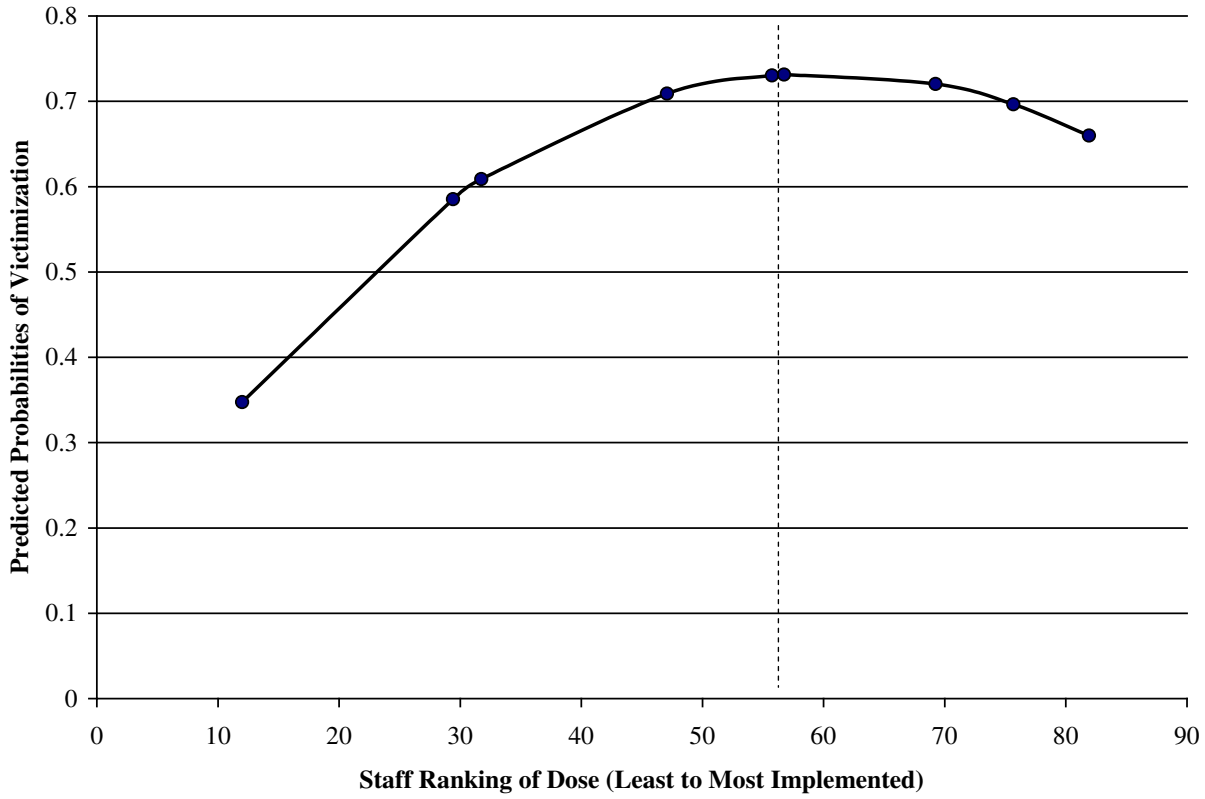

Table 3. Ordinary Least Squares Regression of Intermediate Program Outcomes on Program Dosage

	IPO Model 1 (Dose=Avg. Staff Ranking)	IPO Model 2 (Dose=Pct. Farms Using OANs)	IPO Model 3 (Dose=Police Responsiveness)
Intercept	-.551*	-.147	-2.930***
	(.248)	(.260)	(.897)
X (Dose)	.039***	1.599***	.265***
	(.010)	(.306)	(.080)
X^2 (Dose x Dose)	-.000**	-.684***	-.007***
	(.000)	(.157)	(.002)
Pseudo R2	.061	.062	.049

Note: Unstandardized coefficients are presented (standard errors are in parentheses). Asymptotic standard errors were computed using a modified sandwich variance estimator (see discussion in text). The IPO measure is the dependent variable in each model; only the measure of dose varies.

* $p < .05$ ** $p < .01$ *** $p < .001$

Table 4. Regression of End Program Outcomes on Program Dosage, Controlling for Intermediate Process Outcomes

	Dose = Avg. Staff Ranking				*Dose = Pct. Farms Using OANs*				*Dose = Police Responsiveness*			
	Intercept (S.E.)	X (S.E.)	X^2 (S.E.)	Pseudo R2	Intercept (S.E.)	X (S.E.)	X^2 (S.E.)	Pseudo R2	Intercept (S.E.)	X (S.E.)	X^2 (S.E.)	Pseudo R2
Dependent Variables												
Any Victimization	1.308	.055*	-.001	.130	1.376	1.218	-.410	.129	.174	.165	-.004	.127
	(.693)	(.026)	(.000)		(.696)	(.774)	(.394)		(2.056)	(.180)	(.004)	
Small Equipment Theft	-.987	.018	-.000	.131	-.600	.855	-.454	.131	.481	-.114	.002	.132
	(.775)	(.030)	(.000)		(.771)	(.877)	(.442)		(2.325)	(.202)	(.005)	
Serious Theft	-1.838	.006	-.000	.060	-1.791	.562	-.543	.063	-.652	-.110	-.002	.062
	(.925)	(.037)	(.000)		(.929)	(1.082)	(.553)		(2.974)	(.262)	(.006)	
Chemical or Fuel Theft	-2.437*	.058	-.001	.072	-1.542	2.458	-1.198	.083	-5.368	.372	-.010	.075
	(1.096)	(.043)	(.000)		(1.151)	(1.345)	(.664)		(2.949)	(.251)	(.006)	
Vandalism	-.527	.056*	-.000	.074	-.127	.888	-.123	.089	-2.705*	.273*	-.001*	.074
	(.645)	(.025)	(.000)		(.665)	(.755)	(.380)		(1.914)	(.167)	(.004)	
Burglary	-2.477	.032	-.000	.127	-2.275	-2.422	1.529*	.129	-9.951*	.689*	-.015	.134
	(1.329)	(.052)	(.001)		(1.274)	(1.575)	(.783)		(3.820)	(.318)	(.008)	
Victim Diversity Index	-.207	.025*	-.000*	.195	-.052	.548	-.196	.200	-1.010	.107	-.003	.193
	(.287)	(.011)	(.000)		(.292)	(.345)	(.169)		(.818)	(.070)	(.002)	
Change in Victimization	3.369***	.015	-.000	.118	3.327***	.736	-.382	.119	3.029**	.050	-.001	.117
	(.379)	(.016)	(.000)		(.382)	(.424)	(.215)		(1.206)	(.106)	(.003)	
Victimization Loss (log $)	4.045***	.075*	-.001	.240	4.704***	2.225*	-.941	.236	4.898*	.004	.005	.235
	(.827)	(.033)	(.000)		(.833)	(.952)	(.481)		(2.479)	(.214)	(.005)	
Change in Quality of Life	-.904	-.003	.000	.068	-.706	-.192	.181	.052	-1.430	.049	-.001	.070
	(.620)	(.025)	(.000)		(.617)	(.707)	(.357)		(1.866)	(.162)	(.004)	

Note: Unstandardized coefficients are presented (standard errors are in parentheses). Logistic regression results are presented for each of the victimization outcomes as well as the change in quality of life measure; Poisson regression is used for the victim diversity index outcome; and ordinary least squares (OLS) regression is used for the change in victimization and victimization loss outcomes. Asymptotic standard errors were computed using a modified sandwich variance estimator (see discussion in text).

* $p < .05$ ** $p < .01$ *** $p < .001$

Table 5. Surveillance Equipment and Identification and Arrest of Suspects, 2003-2004

County	No. Deployments	No. Suspects Identified as Result of Deployments	No. Arrests Occurring as Result of Deployments
County 1	4	2	1
County 2	7	8	4
County 3	3	0	0
County 4	8	2	0
County 5	1	0	0
County 6	3	8	4
County 7	3	0	0
County 8	7	5	1
County 9	33	10	5
Total	69	35	15