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Daniel S. Nagin

Criminal Deterrence Research at the Outset of the Twenty-First Century

ABSTRACT

Evidence for a substantial deterrent effect is much firmer than it was two decades ago. However, large gaps in knowledge on the links between policy actions and behavior make it difficult to assess the effectiveness of policy options for deterring crime. There are four major impediments. First, analyses must estimate not only short-term consequences but also calibrate long-term effects. Some policies that are effective in preventing crime in the short term may be ineffective or even criminogenic in the long run because they may erode the foundation of the deterrent effect—fear of stigmatization. Second, knowledge about the relationship of sanction risk perceptions to policy is virtually nonexistent; such knowledge would be invaluable in designing effective crime-deterrent policies. Third, estimates of deterrent effects based on data from multiple governmental units measure a policy's average effectiveness across unit. It is important to understand better the sources of variation in response across place and time. Fourth, research on the links between intended and actual policy is fragmentary; a more complete understanding of the technology of sanction generation is necessary for identifying the boundaries of feasible policy.

The criminal justice system threatens punishment to law breakers—through the police power to arrest and investigate, the judicial power to adjudicate and sentence, and the corrections agencies' power to administer punishments. Since Jeremy Bentham and Cesare Beccaria, scholars have speculated on the deterrent effects of official sanctions,

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but sustained efforts to verify deterrent effects empirically did not begin until the 1960s.

In 1978, the National Academy of Sciences released the report of the Panel on Research on Deterrence and Incapacitation, of which I was a coauthor. The panel's conclusion was guarded but affirmative on the existence of a deterrent effect: "The evidence certainly favors a proposition supporting deterrence more than it favors one asserting that deterrence is absent" (Blumstein, Cohen, and Nagin 1978, p. 7). The report was followed by a widely cited review in this series by Philip J. Cook that reached a similar but less guarded conclusion: "my assessment is that the criminal justice system, ineffective as it may seem in many areas, has an overall crime deterrent effect of great magnitude" (1980, p. 213). In this essay I review the current state of the evidence on deterrence but with a focus on research since 1980, identify important gaps in knowledge, and suggest a research agenda for the outset of the twenty-first century.

Deterrence research has evolved in three distinctive and largely disconnected literatures—interrupted time-series, ecological, and perceptual studies. Interrupted time-series studies examine the effect of targeted and specific policy interventions such as police crackdowns on open-air drug markets. Here the evidence suggests that such interventions have at least a temporary effect, although decay is commonplace (Sherman 1990).

The ecological studies use natural variations in crime rates and sanctions levels across time and space as the test bed for estimating deterrent effects. These studies search for a negative association between crime rates and sanction levels that can plausibly be interpreted as a deterrent effect. I am convinced that a number of studies have been successful in doing this (e.g., Sampson and Cohen 1988; Kagan 1989; Levitt 1996).

Prior to 1980, those two kinds of studies were the mainstay of the deterrence literature. Since that time, another large deterrence literature has emerged that focuses on the links between perceptions of sanction risk and severity to self-reported crime and delinquency. The data for these studies are assembled from surveys. Thus, perceptual studies differ from ecological and interrupted time-series studies both in terms of the unit of observation—individuals rather than places—and the source of the data—surveys rather than official records. With few exceptions, the perceptual studies find that self-reported criminality is lower among people who perceive that sanction risks and costs

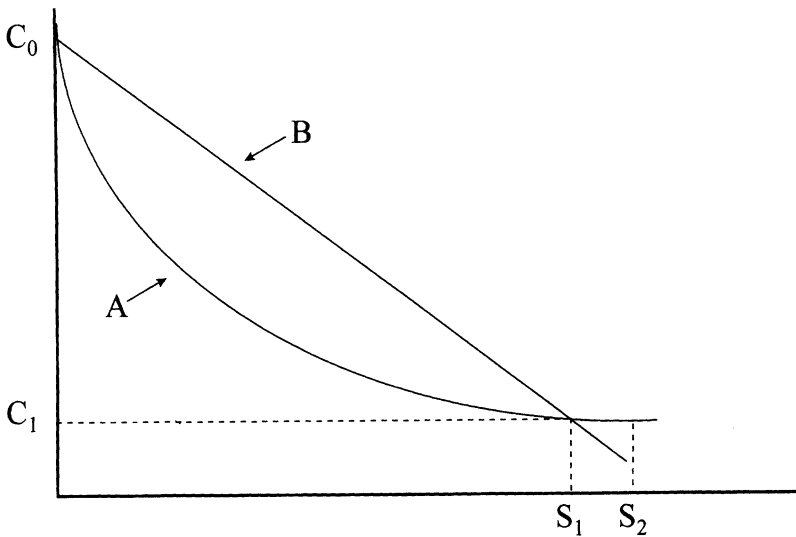


FIG. 1.—Marginal versus absolute deterrent effects

are higher (e.g., Grasmick and Bursick 1990; Bachman, Paternoster, and Ward 1992; Paternoster and Simpson 1997). Thus, my review leads me to conclude that the evidence for a substantial deterrent is much firmer than it was fifteen years ago. I now concur with Cook's more emphatic conclusion that the collective actions of the criminal justice system exert a very substantial deterrent effect.

That said, it is also my view that this conclusion is of limited value in formulating policy. Policy options to prevent crime generally involve targeted and incremental changes. So for policy makers the issue is not whether the criminal justice system in its totality prevents crime but whether a specific policy, grafted onto the existing structure, will materially add to the preventive effect. Here I draw on the distinction between absolute and marginal deterrence. Figure 1 depicts two alternative forms of the response function relating crime rate to sanction levels. Both are downward-sloping, which captures the idea that higher sanction levels prevent crime. At the status quo sanction level, S_1 , the crime rate, C_1 , is the same for both curves. The curves are also drawn so that they predict the same crime rate for a zero sanction level. Thus, the absolute deterrent effect of the status quo sanction level is the same for both curves. But because the two curves have different shapes, they also imply different responses to an incremental increase in sanction

level to S_2 . The response implied by curve A is small. Accordingly, the response would be difficult to detect and likely not sufficient to justify the change as good policy.¹ By comparison, the response depicted in curve B is large, thus more readily detectable, and also more likely to be justifiable as good policy.

While the distinction between absolute and marginal deterrence is useful, it implies an underlying analytical simplicity of the relationship between crime rates and sanction levels that belies the complexity of the phenomenon. Contrary to the implicit suggestion of figure 1, no one curve relates sanction levels to crime rates. The response of crime rates to a change in sanction policy will depend on the specific form of the policy, the context of its implementation, the process by which people come to learn of it, differences among people in perceptions of the change in risks and rewards that are spawned by the policy, and feedback effects triggered by the policy itself (e.g., a reduction in private security in response to an increase in publicly funded security). Further, the magnitude and possibly even the direction of the response to a policy may change over time. Thus, while I am convinced that a number of studies have credibly identified marginal deterrent effects, it is difficult to generalize from the findings of a specific study because knowledge about the factors that affect the efficacy of policy is so limited. Specifically, I see four major impediments to making confident assessments of the effectiveness of policy options for deterring crime.

First, while large amounts of evidence have been amassed on short-term deterrent effects, little is known about long-term effects. Evidence from perceptions-based deterrence studies on the interconnection of formal and informal sources of social control point to a possibly substantial divergence between long- and short-term effects. Specifically, these studies suggest that the deterrent effect of formal sanctions arises principally from fear of the social stigma that their imposition triggers. Economic studies of the barriers to employment created by a criminal record confirm the reality of this perception. If fear of stigma is a key component of the deterrence mechanism, such fear would seem to depend on the actual meting out of the punishment being a relatively rare event. Just as the stigma of Hester Prynne's scarlet "A" depended on adultery being uncommon in Puritan America, a criminal

¹ The shape of this response curve is also instructive for making another point: just because the response to an increase in sanctions from S_1 to S_2 is small, it does not follow that response to a reduction in sanction levels from S_1 will be small.

record cannot be socially and economically isolating if it is commonplace. Policies that are effective in the short term may erode the very basis for their effectiveness over the long run if they increase the proportion of the population who are stigmatized. Deterrence research has focused exclusively on measuring the contemporaneous effects of sanction policies. Long-term consequences have barely been explored.

The second major knowledge gap, which was also emphasized by Cook more than fifteen years ago, is that we know little about the connection of risk perceptions to actual sanctions policy. The perceptual deterrence literature was spawned by the recognition that deterrence is ultimately a perceptual phenomenon. While great effort has been committed to analyzing the links between sanction risk perceptions and behavior, comparatively little attention has been given to examining the origins of risk perceptions and their connection to actual sanction policy.

For several reasons this imbalance should be corrected. One is fundamental: the conclusion that crime decisions are affected by sanction risk perceptions is not a sufficient condition for concluding that policy can deter crime. Unless the perceptions themselves are manipulable by policy, the desired deterrent effect will not be achieved. Beyond this basic point of logic, a better understanding of the policy-to-perceptions link can also greatly aid policy design. For instance, nothing is known on whether the risk perceptions of would-be offenders for specific crimes are formed principally by some overall sense of the effectiveness of the enforcement apparatus or by features of the apparatus that are crime specific (e.g., the size of the vice squad or the penalty for firearms use). If it is the former, seemingly targeted sanction policies will have a generalized salutary effect across crime types by heightening overall impressions of system effectiveness. If the latter, there will be no such generalized effect. Indeed would-be offenders may substitute nontargeted offenses for targeted offenses (e.g., committing burglaries in response to increased risk for robbery).

Third, the effect of specific policies—for example, increasing the number of police—will depend on the form of their implementation across population units. Yet estimates of the deterrent effect of such policies from the ecological literature are commonly interpreted as if they apply to all units of the population from which they were estimated. In general this is not the case. Rather, the estimated deterrent effect should be interpreted as the average of the “treatment” effect across population units. For instance, the deterrent effect of more po-

lice in any given city will depend on a host of factors including how the police are deployed. Consequently, the effect in any given city will not be the same as the average across all cities; it may be larger, but it could also be smaller. Similarly, it is not possible to make an all-purpose estimate of the effect of prison on crime. There are many ways to increase prison population by a given amount, ranging from broad-based policies such as across-the-board increases in sentence length to targeted policies like “three-strikes” statutes. It is likely that the magnitude of the preventive effect will vary materially across these options. The implication is that, even though there are credible estimates of average deterrent effects of at least some broad classes of policies, the capacity to translate the average estimates into a prediction for a specific place is limited. This is a major shortcoming in the evidence because crime control is principally the responsibility of state and local governments. It is the response of crime to policy in that city or state that is relevant to its population, not the average response across all cities and states.

A fourth major gap concerns the link between intended and actual policy. Generally, laws are not administered as intended. For example, mandatory minimum sentences can be circumvented by plea bargains or selective prosecution. Commonly, the popular press and political process attributes the noncorrespondence to malfeasance. The reality is more complicated but not well understood. A better understanding of the technology of sanction generation is required to delineate the boundaries of feasible policy as prescribed.

Here is how this essay is organized. Section I provides an overview of the principal points that I make about the interrupted time-series, perceptual-deterrence, and ecological studies. The principal findings of these literatures are summarized in Sections II, III, and IV. In Section V the links between prescribed and actual policy are examined, and Section VI offers conclusions.

I. Overview of the Interrelationship of Crime Rates, Sanctions, and Policy

Figure 2 depicts the interrelationship of three variables that form the focus of this essay—crime rates (C_t), sanction levels (S_t), and policy (P_t) (e.g., number of police). Each variable is subscripted by t to account for changes over time. The major points I want to make are motivated by the interrelationship of these variables.

The first point involves the observation that spawned the perceptual

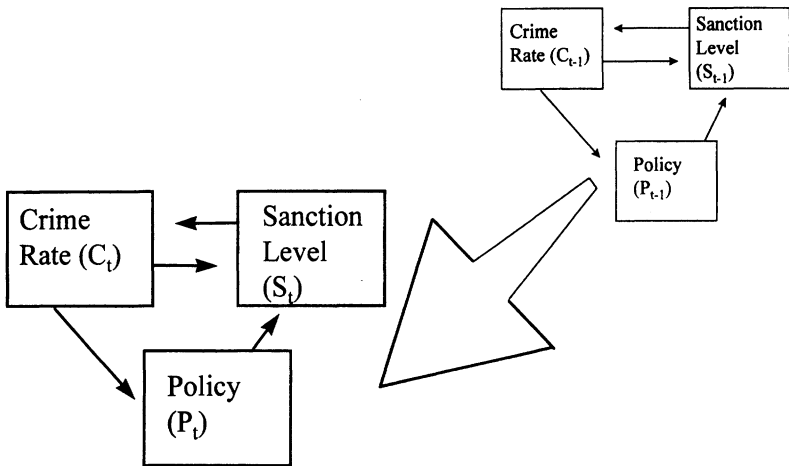


FIG. 2.—The interrelationship of crime rate, sanction levels, and policy

deterrence literature—the deterrent effect of sanctions ultimately depends on perceptions of their certainty and severity. Thus, S_t should be interpreted as perceived sanction certainty and severity at time t . In my judgment, the evidence amassed by perceptual deterrence researchers points overwhelmingly to the conclusion that behavior is influenced by sanction risk perceptions—those who perceive that sanctions are more certain or severe are less likely to commit crime. But for crime control policy to be effective it must alter these perceptions. Evidence on whether and how policy in current and prior time periods (P_t, P_{t-1}, \dots) affects sanction perceptions is fragmentary. Ecological and interrupted time-series studies have focused only on the relationship of policy to crime. In so doing these studies have treated the intervening policy-to-perceptions linkage, depicted by the arrow from policy to perceived sanction level, as a black box. While these studies generally find that policy has at least a temporary effect on crime and thereby somehow influences perceptions, the dearth of evidence on the policy-to-perceptions linkage is a major gap in knowledge of the etiology of deterrence. As is discussed in Sections II and III, such knowledge would be of great value in designing effective deterrence policies.

Another important dimension of the policy-to-perceptions linkage concerns how quickly policy affects perceptions. Figure 2 includes a pointer from time $t - 1$ to time t as a reminder that policy, perceptions, and behavior are connected over time. Perceived sanctions at

time t , S_t , are likely to be a function not only of actual sanction policy at t , P_t , but also of sanction policy in prior periods, P_{t-1} , P_{t-2} , Yet nothing is known about the most basic aspects of the process by which sanction perceptions adjust to policy change—the speed with which it occurs and the mechanism by which people form their perceptions. Relatedly, sanction perceptions may also differ across two locations with the same sanction policy because of differences in population characteristics and context. For instance, the effect of policing tactics on risk perceptions may depend on the population characteristics of a neighborhood, such as ethnic and age composition.

Another of my major points stems from the simultaneous relationship between crime rates and sanction levels that is depicted in figure 2 (i.e., arrows going in both directions between S_t and C_t). In econometric parlance such variables are called “endogenous.” The deterrent effect of sanctions is reflected in the arrow from S_t to C_t . But the level of crime may also affect sanction levels. For instance, increased crime may overwhelm the criminal justice system’s capacity to process cases. This effect is depicted by the arrow from C_t to S_t . To partial out the deterrent effect requires that the analysis also take into account the effect of crime rate on sanction level, whatever its cause. In Section IV, I discuss a few studies that in my judgment have plausibly dealt with the simultaneity problem. I also discuss important limitations to the generalizability of these studies.

The final arrow links crime to policy. For a sanction policy to be effective it must be credible. Credibility in turn depends on the capacity of the criminal justice system to administer official policy. Ironically, this capacity in turn depends on the level of crime, the very phenomenon that the policy is intended to affect. The interplay of policy, credible threat, and crime rate is the subject of Section V.

II. Interrupted Time-Series Studies

Interrupted time-series studies examine the effect of targeted policy interventions such as police crackdowns or effectuation of statutes changing penalties. The best-designed studies attempt to incorporate important features of a true experiment—a well-defined treatment regime, measurement of response before and after treatment, and a control group. Two classic studies of this genre are Ross’s studies of the effect on drunk driving of the British Road Safety Act (Ross 1973) and of Scandinavian-style drunk driving laws (Ross 1975).

The great proportion of interrupted time-series studies have exam-

ined the effect of drunk driving laws or of police crackdowns on drug markets (Kleiman 1986, 1988; Reuter et al. 1988), disorderly behavior (Sherman et al. 1986), and drunk driving (Ross 1982). A less extensive literature has also examined the effect of gun-control laws and ordinances (cf. Loftin and McDowell 1984; Loftin, Wiersema, and Cottey 1991; McDowell, Loftin, and Wiersema 1992). Excellent reviews of these studies are already available from Sherman (1990) and Ross (1982), so I only summarize their conclusions. My primary objective for this section is to use the conclusions of these two experts as a springboard for offering further observations on the importance of gaining better knowledge of the determination of sanction risk perceptions.

Both Sherman and Ross conclude that interventions are generally successful in generating an initial deterrent effect. For instance, in drunk-driving interventions this is evidenced by a reduction in fatalities in which a driver is intoxicated or in drug market crackdowns by reduced dealing. One exception may be increases in sentence severity that are not accompanied by at least the maintenance of the status quo level of certainty. If judges or juries believe that the penalties are too harsh, they may respond by refusing to convict guilty defendants with the result that the policy increases rather than deters the targeted behavior. Indeed, Ross (1982) concludes that efforts to deter drunk driving with harsher penalties commonly fail for precisely this reason. I return to this conclusion in Section V. Sherman and Ross are also in agreement that the effect is generally only transitory: the initial deterrent effect typically begins decaying even while the intervention is still in effect. However, in some instances the decay is not always complete even following the end of the crackdown.

Sherman (1990, p. 10) offers some useful nomenclature for labeling these effects: “initial deterrence decay,” which he describes as the reduction in the deterrent response as “potential offenders learn through trial and error that they had overestimated the certainty of getting caught at the beginning of the crackdown,” and “residual deterrence,” which is a crime suppression effect that extends beyond the intervention until offenders learn by experience or word of mouth that “it is once again ‘safe’ to offend.”

There are at least two explanations for deterrence decay and residual deterrence. One is incorporated directly in Sherman’s definition *qua* explanation of these two concepts—would-be offenders initially overestimate the increase in sanction risk posed by the intervention. Deter-

rence decays as they learn that they were fooled. This explanation is also endorsed by Ross. A related but distinct explanation is also suggested by Sherman (1990). It involves a concept from behavioral decision theory called “ambiguity aversion.”

Expected utility theory assumes that probabilities of outcomes are known whereas subjective expected utility theory does not make this strong assumption. Rather, subjective expected utility allows that people may have a subjective probability distribution over the unknown probability. Camerer and Weber (1992) observe that it is hard to think of a real-world decision problem where probabilities are known with certainty. This observation certainly applies to crime. As discussed in the prior section, the probability of successful completion of a crime depends on the kind of crime committed, the circumstances in which the crime is committed, the skill of the offender, and a litany of other contingencies.

In subjective expected utility theory, this distinction is analytically unimportant because the expected (mean) value of the subjective probability distribution is substituted for the single objective probability in expected utility theory. The problem is that people do not behave as if they make this simple substitution. They seem to care also about the variance of the distribution. Specifically, people prefer gambles in which they know the probabilities exactly to “ambiguous” gambles where they only know the distribution of probabilities. To illustrate, consider the following two lotteries: (1) a 0.5 probability of winning \$10 and 0.5 probability of winning nothing, versus (2) a two-stage lottery where, in the first stage, the probability of winning \$10 is determined by a draw from a 0-1 uniform distribution and, in the second stage, the lottery is “played-out” based on the probability drawn from the first stage. In both lotteries the chance of success is 0.5, but the evidence is overwhelming that people prefer lottery 1 to lottery 2 (Camerer and Weber 1992). This aversion to uncertainty about the relevant probability is what behavioral decision theorists call “ambiguity aversion,” a label that Camerer and Weber attribute to Daniel Ellsberg (1961).

Ambiguity aversion offers an explanation for initial deterrence and its subsequent decay that is distinct from the overreaction hypothesis. The difference is illustrated with an extreme example. Suppose that intervention did not alter people’s mean estimate of risk, as depicted in figure 3, but only increased their uncertainty about its exact value. Such increased uncertainty is reflected in the larger variance of the

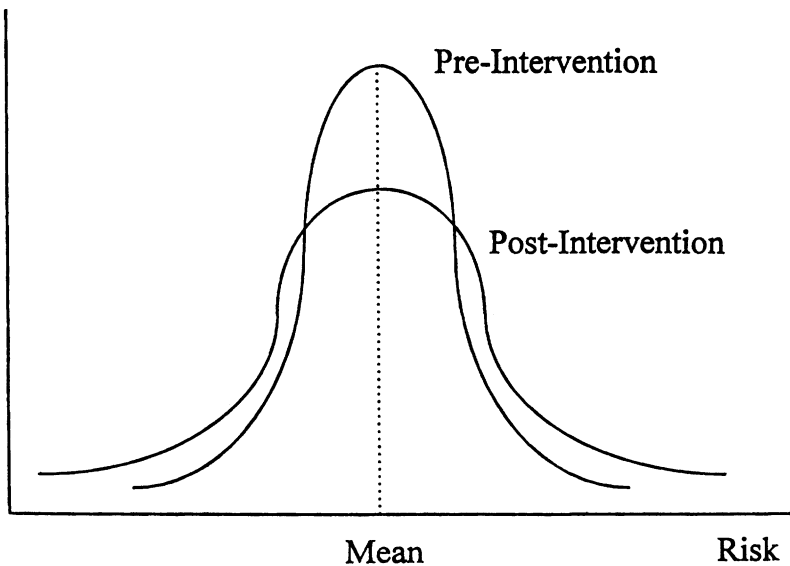


FIG. 3.—Subjective risk distributions: pre- and postintervention

postintervention subjective risk distribution. Even though the mean of the pre- and postintervention risk distributions are the same, the ambiguity aversion hypothesis predicts an initial increase in deterrence. As people learn more about the actual effects of the postintervention enforcement regime, the variance of the postintervention risk distribution will decrease. This reduction in ambiguity in turn will result in deterrence decay.

I contrast these two explanations for deterrence decay because they again serve to illustrate how little is known about the formation of sanction risk perceptions. We currently have no basis for distinguishing these two explanations, and the question of which is the more credible explanation is more than academic. Sherman (1990) has suggested that initial deterrence can be made permanent by constantly experimenting with novel police deployment strategies or enforcement priorities. The idea is to avoid stability and predictability. The large body of evidence suggesting that ambiguity aversion is deeply embedded in human decision making supports Sherman's recommendation. Even when subjects are made aware of the equivalence of gambles with the same expected probabilities, ambiguity aversion persists; ambiguity avoidance does not appear to be a decision response that is easily "un-

learned.” However, if the initial deterrence is attributable to an overreaction to the effect of the intervention on actual risk, it seems less likely that people will be repeatedly fooled.

III. Perceptual Deterrence Studies

The perceptual deterrence literature examines the relations of perceived sanction risks to either self-reported offending or intentions to offend. This literature was spawned by a cadre of researchers (Meier and Johnson 1977; Minor 1977; Tittle 1977, 1980; Grasmick and Bryjak 1980) interested in probing the perceptual underpinnings of the deterrence process. They were motivated by the observation that ultimately deterrence depends on perceptions of the risks (and rewards) of offending and by skepticism that perceived sanction risks are very closely tied to actual risks.

A. Summary of Findings

The first perceptual deterrence studies appeared nearly thirty years ago (Jensen 1969; Waldo and Chiricos 1972), but perceptual research did not begin in earnest until the late 1970s. Paternoster (1987) and Williams and Hawkins (1986) provide excellent reviews of this literature. I focus on highlights and on drawing connections to other components of the deterrence literature.

Perceptual deterrence studies have been based on three types of data: cross-sectional survey studies, panel survey studies, and scenario-based studies. In cross-sectional survey studies, individuals are questioned about their perceptions of the certainty and severity of sanctions and about either their prior offending behavior or their future intentions to offend. For example, Grasmick and Bryjak (1980) queried a sample of city residents about their perceptions of the risk of arrest for offenses such as a petty theft, drunk driving, and tax cheating and also about whether they thought they would commit each of these acts in the future.

In panel survey studies, the sample is repeatedly surveyed on risk perceptions and criminal behavior. For example, Paternoster et al. (1982) followed a sample of students through their three-year tenure in high school and surveyed them on the frequency with which they engaged in various delinquent acts and their perceptions of the risks and consequences of being caught for each such act.

In scenario-based studies, individuals are questioned about their perceptions of the risks of committing a crime described in a detailed

crime vignette and about their own behavior if they found themselves in that situation. Bachman, Paternoster, and Ward (1992), for instance, constructed a scenario describing the circumstances of a date rape. They then surveyed a sample of college males about their perceptions of the risk of the scenario male being arrested for sexual assault and also about what they themselves would do in the same circumstance.

The cross-sectional and scenario-based studies have consistently found that perceptions of the risk of detection and punishment have negative, deterrent-like associations with self-reported offending or intentions to offend (cf. Jensen 1969; Minor 1977; Grasmick and Bryjak 1980; Grasmick and Bursik 1990; Bachman, Paternoster, and Ward 1992; Paternoster and Simpson 1997). Such deterrent-like associations with perceived severity are somewhat less consistent, but when individual assessments of the cost of such sanctions are taken into account, statistically significant negative associations again emerge (Grasmick and Bryjak 1980). Only in the panel-based studies have null findings on deterrent-like effects been found (Paternoster et al. 1982, 1983*a*, 1983*b*; Saltzman et al. 1982).

In panel-based studies respondents were typically interviewed on an annual cycle. In analyzing these data researchers have examined the relationship of self-reported offending in the year period between survey administrations to sanction risk perceptions at the outset of the measurement period. Generally, these studies have found only weak evidence of deterrent-like associations (Saltzman et al. 1982; Paternoster et al. 1982, 1983*a*, 1983*b*).

Researchers using panel data focused on the relationship of behavior between years t and $t + 1$ to risk perceptions at the outset of the year t to avoid the problem of causal ordering: is a negative association a reflection of the deterrent effect of risk perceptions on crime or of the effect of criminal involvement on risk perceptions (Greenberg 1981)? Paternoster et al. (1982, 1983*a*, 1983*b*) and Saltzman et al. (1982) argued that it was the latter. Specifically, they argued that the criminally uninitiated had unrealistically high expectations of sanction risks and that experience with offending caused them to lower their unrealistically high expectations. This experiential effect, they contended, accounted for the negative association between contemporaneous measurements of risk perceptions and behavior.

The results of Paternoster and colleagues generated a spirited debate on the appropriate time lag between measurements of sanction risk perceptions and criminal involvement. Piliavin et al. (1986), Williams

and Hawkins (1986), and Grasmick and Bursik (1990) argued that ideally the measurements should be made contemporaneously because perceptions at the time of the act are what determines behavior. Further, if risk perceptions are highly unstable, a long temporal lag between measurement of such perceptions and behavior could introduce substantial measurement error, which in turn would attenuate estimates of the deterrent effect. As Piliavin et al. (1986, pp. 115–16) observe, “The effective assessments of risk are to some extent situationally-induced, transitory, and unstable. . . . If true, this could help explain the ineffectiveness of our risk variables—that is, if persons’ perceptions of risk are unstable over time, and causally-relevant perceptions are those more proximate to crime, our distal measures of perceived risk may be irrelevant to behavior.”

The argument for temporal proximity is compelling, but the challenge is its practical achievement. People cannot be queried on their risk perceptions on a real-time basis as they encounter criminal opportunities in their everyday lives. The scenario method offers one solution. With this method respondents are not questioned about their actual behavior or intentions but are instead offered a scenario that describes in detail an event involving law breaking. The respondents are then queried about their perceptions of sanction risks confronting the scenario character and also about the likelihood they would commit the act depicted in the scenario. In my research I have found this method to be a productive approach for studying illegal behaviors as diverse as tax cheating and sexual assault (Klepper and Nagin 1989*a*, 1989*b*; Nagin and Paternoster 1993, 1994).²

The scenario method has the additional benefit of specificity about the circumstances of the crime. Research on situational crime prevention makes clear what few would doubt (Clarke 1995). Perceptions of sanction threats are affected by the context in which the crime is committed, such as the presence of witnesses, escape opportunities, and so on, and these perceptions materially affect behavior. Unless the circumstances are well described, questions about sanction risk are ill-

² The principal weakness of the scenario approach is that an expressed intention to offend is not synonymous with actual performance. Fishbein and Ajzen (1975) argue that there will be a close correspondence between intentions and behavior when intentions are measured with the same specificity as the behavior that is being predicted, when there is stability of the expressed intention, and when the individual is able to willfully carry out the intention. In my judgment the scenarios used in my own research meet these criteria, but still I must acknowledge that the link between intentions and behavior remains problematic.

posed. For instance, the risk of arrest for larceny is negligible if the property is completely unprotected and untraceable and nearly certain if it is guarded and its owner easily identified.

Scenario-based research has consistently found deterrent-like relationships in the data. On average, persons who perceived that sanctions were more certain or severe reported smaller probabilities of their engaging in the behavior depicted in the scenario, whether it be tax evasion (Klepper and Nagin 1989*a*, 1989*b*), drunk driving (Nagin and Paternoster 1993, 1994), theft (Nagin and Paternoster 1993, 1994), sexual assault (Bachman, Paternoster, and Ward 1992; Nagin and Paternoster 1993, 1994), or corporate crime (Paternoster and Simpson 1997). Also, in my collaboration with Raymond Paternoster (Nagin and Paternoster 1993), we reanalyzed the panel data he used in his earlier deterrence research. In this later analysis we found clear evidence of deterrent-like effects even in model specifications in which risk perceptions are lagged. Thus I believe that a consensus has emerged among perceptual deterrence researchers that the negative association between sanction risk perceptions and offending behavior or intentions is measuring deterrence. This conclusion reframes the question of the deterrent effect of sanctions from the issue of whether people respond to their perceptions of sanction threats to the issue of whether those perceptions are manipulable by policy. This brings me to the issue of the formation of sanction risk perceptions.

B. The Formation of Sanction Risk Perceptions

The perceptual deterrence literature was motivated by skepticism that perceived and actual sanction threats were tightly linked. Thus it is curious that perceptual deterrence researchers have given only modest attention to the factors influencing risk perceptions and to the dynamic processes by which they are formed. Consider the experiential argument of Paternoster and colleagues: why is it that those without experience in offending have higher risk perceptions than those with experience? Minor and Harry (1982), Tittle (1980), and Paternoster and colleagues attribute it to the naïveté of inexperienced offenders who overestimate the effectiveness of enforcement apparatus. Tittle (1980, p. 67) describes this naïveté as the “shell of illusion” about the consequences of law breaking. These arguments are plausible but remain untested hypotheses.

Embedded in these explanations is the presumption that perceptions are updated based on experience. As Paternoster et al. observe: “People

who engage in illegal acts without getting caught may be expected to lower their estimate of the probability of getting caught” (1983*a*, p. 458). They are suggesting that offenders have prior estimates of decision-relevant quantities, such as the risk of getting caught, which they do not abandon completely based on new data. Instead they update their prior estimates based on the new information. Thus, their current estimate is an amalgam of the prior estimate and information gleaned from recent experience. Presumably the experience of being caught should result in an increase in perceived probability of apprehension but not to a probability of one, which defines certain apprehension, only to some higher probability estimate.

The process that I have just described of updating rather than completely abandoning perceptions of risks (and rewards) based on new information is not only commonsensical but, as Bayesian decision theorists argue, formally rational (DeGroot 1978). A few studies have attempted to test whether offenders are good Bayesians (my label, not theirs)—Cohen (1978), Parker and Grasmick (1979), Richards and Tittle (1981, 1982), Paternoster et al. (1985), and Horney and Marshall (1992). Results have been mixed. Only the final three cited studies find that offenders do appear to adjust risk perceptions in a Bayesian-like fashion. However, it is notable that this group includes the one study, by Horney and Marshall, which is based on serious offenders. Specifically, the Horney and Marshall study is based on a sample of more than 1,000 convicted felons. Within their sample, subjects who had higher arrest ratios, self-reported arrests to self-reported crime, also reported higher risk perceptions.

In my scenario-based research, my coauthors and I have given some attention to the effect of situational factors in risk perceptions (Klepper and Nagin 1989*a*; Nagin and Paternoster 1993). We have done this by experimentally varying scenario conditions (e.g., length of the drive home) and examining the effect of such variation on perceived risk (e.g., the probability of arrest for drunk driving). Results have been mixed. For offenses such as date rape and drunk driving, we find little evidence of risk perceptions being affected by context, but for tax evasion the link was strong. Perceptions of the risk of detection increased with the amount of noncompliance and varied by type of noncompliance (e.g., were higher for deductions than for cash income). For tax compliance, at least, perceptions mirrored the realities of the enforcement process.

Kagan (1989) provides a complementary perspective on the findings

for tax evasion. He argues that the visibility of income to the IRS exerts an enormously powerful effect on compliance rates. Compliance rates are very high for very visible sources of income such as wages, dividends, and interest for which the IRS receives information reports from payers. Compliance declines substantially for less visible sources of income for which the IRS does not receive information reports but for which there are other practical methods for tracing the income like bank or business records. Examples of this sort of income are proprietorship and partnership incomes. Finally, compliance rates are negligible for income sources like cash income earned in the informal, underground, and illegal economic sectors, which are virtually untraceable. As Kagan points out, visibility is simply an evocative synonym for detectability. For highly visible sources of income it is easy for the IRS to assemble the accounting information necessary to prove noncompliance; it comes to them on a computer tape. Thus, the threat of detection is very high. People recognize this and compliance is correspondingly high. For invisible sources of income it is extremely costly to assemble the required accounting information to prove noncompliance, and here again people seem to respond accordingly, by reporting very little of such income.

The literature on the formation of sanction risk perception is small and narrow in scope. Arguably, measuring the linkage between sanction policies and sanction risk perceptions is of secondary importance to measuring the linkage between sanction policy and behavior. Knowing the effect of policy on risk perceptions serves only to clarify the basis for the relationship of policy to behavior but has little value in and of itself. This argument assumes that the linkage between policy and behavior can be firmly established. In fact, evidence on the policy-to-behavior linkage will never be “airtight” even if it is based on data from an experiment. For instance, suppose it was found that a policy of presumptive arrest for spousal assault was associated with a decline in various indicators of spousal abuse in the population at large. One interpretation of such a finding is that it reflects a general deterrent effect. But if there were no evidence that men were generally aware of this policy, the deterrence interpretation would be undercut. Alternatively, if survey evidence showed a general awareness of the policy, the case for the deterrence interpretation would be bolstered.

The dearth of evidence on the policy-to-risk-perceptions linkage also leaves unanswered a key criticism of skeptics of the deterrent effects of official sanctions. Even if crime decisions are influenced by

sanction risk perceptions, as the perceptual deterrence literature strongly suggests—absent some linkage between policy and perceptions—behavior is immune to policy manipulation (Jacob 1979). In this sense behavior lacks rationality, not because individuals fail to weigh perceived costs and benefits, but because the sanction risk perceptions are not anchored in reality. Cook (1979) attempted to answer this criticism with a simulation in which a robber's perception of the risk of arrest and punishment is influenced only by readily available information—his own experience and that of a few compatriots. In this simulation a would-be robber's rate of offending is based on his latest perception of risks, which in turn is based on his own experience as well as that of a small circle of friends. (Cook's robbers are indeed good Bayesians!) His simulation shows that a policy-to-perceptions linkage can be created, albeit very noisy, based on very limited information—one's own experience and that of a small network of comrades. Cook's attempt is useful, but by his own acknowledgment it is not based on empirical evidence.

I view two generic categories of questions about risk perceptions as particularly important. One is whether sanction risk perceptions are well formed at the level of the specific offense—for example, burglary versus robbery—or do would-be offenders have only a generalized sense of the effectiveness of the enforcement apparatus? For instance, are perceptions of apprehension risk formed principally by broad-based impressions of the police being proactive in suppressing disorder, as suggested by Sampson and Cohen (1988) and Wilson and Bolland (1978), or are they more crime-specific and determined by the rate at which police actually solve specific types of crime?

The answer to this question is important for policy. Rational choice models of criminal behavior, such as those posed by economists, predict that escalation of penalties for a specific crime—such as robbery with a firearm—will have the desired effect—fewer gun robberies—but the models also predict an undesirable side effect—an increase in nongun crime, such as burglary and robberies with knives. These predictions require potential offenders to have crime-specific impressions of sanction risks that vary independently of one another, but there is no research on whether this is true. If it is substantially incorrect and impressions of risk for all crime types are closely tied to an overall impression of effectiveness, there may be no substantial crime substitution effects. Indeed a seemingly targeted sanction policy may have a generalized deterrent effect that extends beyond targeted crimes.

The second category of questions that deserve special attention con-

cerns the dynamics of the risk formation process. How do would-be offenders combine prior experience with the criminal justice system and new information on penalties? How long does it typically take for persons to become aware of new sanctioning regimes? How do they become aware of changes in penalties, and what information sources do they use in updating their impressions? How do novices form impressions of sanction risks? These questions speak to the broader issue of whether sanction risk impressions are easily manipulable. The Bayesian model assumes that with the right information they are, but the model has not been tested.

Assembling evidence on sanction risk perceptions will not be easy, particularly for groups including a large representation of marginal offenders, individuals who are neither strongly committed to crime nor to legal conformity. But the research of Horney and Marshall (1992), which was based on a sample of high-risk offenders, and successes in research on situational deterrence and tax evasion make me confident that the effort will be profitable and that headway is possible.

C. The Linkage between Formal and Informal Sanction Processes

In my judgment the most important contribution of the perceptual deterrence literature does not involve the evidence it has amassed on deterrence effects per se. Rather, it is the attention it has focused on the linkage between formal and informal sources of social control. Recognition of this connection predates the perceptual deterrence literature. For instance, Zimring and Hawkins (1973, p. 174) observe that formal punishment may best deter when it sets off informal sanctions:

“We must recognize that there are other aspects of the administration of criminal justice which, while forming no part of the formally prescribed punishment, must nevertheless be regarded as part of the threatened consequences. It would be illogical to restrict the definition of threatened consequences in such a way as to exclude such aspects of the enforcement process which are integral parts of the system and may often be as significant as the formally prescribed punishment themselves. . . . Official *actions* can set off societal *reactions* that may provide potential offenders with more reason to avoid conviction than the officially imposed unpleasantness of punishment. [Emphasis in original]

See also Andenaes (1974), Gibbs (1975), and Blumstein and Nagin (1976) for this same argument.

Early perceptual deterrence studies did not consider the connection between formal and informal sanctioning systems, but a review by Williams and Hawkins (1986) prompted a broadening of the agenda to consider this issue. In a nutshell, their position was this: community knowledge of an individual's probable involvement in criminal or delinquent acts is a necessary precondition for the operation of informal sanction processes. Such knowledge can be obtained from two different sources: either from the arrest (or conviction or sentencing) of the individual or from information networks independent of the formal sanction process (e.g., a witness to the crime who does not report such knowledge to the police). Williams and Hawkins observe that deterrent effects may arise from the fear that informal sanctioning processes will be triggered by either of these information sources. They use the term "fear of arrest" to label deterrent effects triggered by the formal sanction process and the term "fear of the act" to label deterrent effects triggered by information networks separate from the formal sanction process. The crux of their argument is that all preventive effects arising from "fear of arrest" should be included in a full accounting of the deterrent effect of formal sanctions. For example, if an individual refrains from committing a criminal act because she fears that an arrest will bring the transgression to the attention of others, and thereby jeopardize valued social relationships, the preventive mechanism is ultimately the result of formal sanctions and, therefore, "part of the general deterrence process" (Williams and Hawkins 1986, p. 561).

I concur, and much of my scenario-based research confirms their argument. This research has consistently found that individuals who report higher stakes in conventionality are more deterred by perceived risk of exposure for law breaking. My most salient finding in this regard is for tax evasion. Civil enforcement actions by tax authorities are a private matter unless the taxpayer appeals the action. Because tax authorities are scrupulous about maintaining this confidentially, for civil enforcement actions noncompliers are gambling only with their money, not their reputations. In Klepper and Nagin (1989*a*, 1989*b*) a sample of generally middle-class adults were posed a series of tax non-compliance scenarios. The scenarios laid out the essential features of a tax report—income from different sources, number of exemptions, and various deductions. We then experimentally varied the amount and type of noncompliance (e.g., overstating charitable deductions or understating business income) across tax-return line items. We found that a majority of respondents reported a nonzero probability of taking

advantage of the noncompliance opportunity described in the scenario. Plainly, our respondents were generally willing to consider tax noncompliance when only their money was at risk. They also seemed to be calculating; the attractiveness of tax noncompliance gamble was inversely related to the perceived risk of civil enforcement.

The one exception to the rule of confidentiality of enforcement interventions is criminal prosecution. As with all criminal cases, criminal prosecutions for tax evasion are a matter of public record. Here we found evidence of a different decision calculus; seemingly all that was necessary to deter evasion was the perception of a nonzero chance of criminal prosecution. Stated differently, if the evasion gamble also involved putting reputation and community standing at risk, our middle-class respondents were seemingly unwilling to consider taking the noncompliance gamble.

This finding also provides some fresh perspective on the old question whether it is the certainty or the severity of punishment that is the greater deterrent. If the social and economic costs of punishment are strictly proportional to the punishment received—for example, if the cost to the individual of a two-year prison term is twice that of a one-year sentence—certainty and severity will equally affect the decision making of a would-be offender who is an expected utility maximizer. This is because expected cost is simply the multiplicative product of certainty, P , and severity, S . The value of the product, $P * S$, is equally affected by proportional changes in P or S . For example, the effect on expected value of a 50 percent increase in P is the same as a 50 percent increase in S . However, my tax evasion research suggests that people do not perceive that costs are proportional to potential punishment. Instead, it seems that they perceive that there is fixed cost associated with merely being convicted or even apprehended if it is public record.

While my tax evasion research does not pin down the specific sources of these costs, other research on the effect of a criminal record on access to legal labor markets suggests a real basis for the fear of stigmatization (Freeman 1991; Grogger 1992; Lott 1992; Waldfogel 1994; Nagin and Waldfogel 1995; Bushway 1996). Freeman estimates that a record of incarceration depresses probability of work by 15 percent to 30 percent; Waldfogel (1994) estimates that conviction for fraud reduces income by as much as 40 percent; and Bushway (1996) concludes that even an arrest for a minor offense impairs access to legal labor markets at least in the short run.

I emphasize the link between formal and informal sanctions because over the long run a policy may erode the foundation of the deterrent effect—fear of stigmatization. For an event to be stigmatizing it must be relatively uncommon. As I pointed out earlier, Hester Prynne's ostracism depended on a proscribed behavior, adultery, being a rare event in Puritan America. To illustrate how a policy may cannibalize the basis for its effectiveness, consider the following example. Suppose a policy had the effect of increasing the probability of imprisonment for committing a crime, $P(I)$, by 10 percent and this policy was effective in reducing the number of offenders, N , by 5 percent. *Ceteris paribus*, is it reasonable to assume this reduction in N can be sustained over the long run? I think not. In steady state, the incarcerated population, I , equals $(P(I) * S) * (N * \lambda)$, where S is the average time served in prison and λ is the average rate of offending. The two product terms, $P(I) * S$ and $N * \lambda$, respectively, can be interpreted as the expected prison price per crime committed and the total number of crimes committed. Thus, their product equals the size of the incarcerated population. Assume for simplicity that the 10 percent increase in $P(I)$ has no effect on λ or S . Under these circumstances, the 5 percent reduction in N will reduce the crime rate by 5 percent. However, it will also increase the incarcerated population by 5 percent— N declines by 5 percent, but $P(I)$ increases by 10 percent. The increase in prison population will in turn result in an increase in the proportion of the population with a prison record. Here lies my reservation about the sustainability of the 5 percent reduction in crime. If in fact fear of stigmatization is a prominent factor in a full accounting of the deterrent effect of formal sanctions, this policy may erode the basis for its effectiveness by making prison records more commonplace.

More generally, such erosion in effectiveness seems likely to occur when a policy's preventive effect is not sufficiently powerful to reduce crime by enough to reduce rather than increase the proportion of the population with criminal records. To illustrate, suppose that the 10 percent increase in $P(I)$ reduced N by 15 percent—that is, the elasticity of N with respect to $P(I)$ is 1.5—each 1 percent increase in $P(I)$ reduces N by 1.5 percent. For an elasticity of 1.5, both crime rate and prison population would decline, the former by 15 percent and the latter by 5 percent. In this case, the 15 percent reduction in crime may be sustainable. Indeed, it may even increase over time because the policy decreases rather than increases the population rate of criminal records.

These examples illustrate that the long-term preventive effect of a

policy may depend critically on the magnitude of the response. If the elasticity of the crime rate with respect to the sanction policy variable is great enough to reduce the proportion of the population that is stigmatized, the effect may be sustainable. However, if the policy increases the proportion stigmatized, the deterrent effect is less likely to be sustainable.

At least with regard to prison sanctions the evidence suggests we are currently in the latter situation. Mauer and Huling (1995) examined recent trends in the proportion of the population under the control of the criminal justice system—incarcerated or on parole or probation. They estimated that these proportions are growing and have reached extraordinarily high levels, particularly for young African-American men. In 1989, 6.2 percent of white males ages twenty to twenty-nine were under the control of the criminal justice system. By 1994 this control percentage had increased to 6.7 percent, or to one in fifteen young adult white males. The statistics for young adult African-American males are even more startling. In 1989 their criminal justice system control rate was 23 percent. By 1994 it had grown to nearly one-third of the population, 30.2 percent, with more than 10 percent of this group incarcerated.

My concern about stigma erosion also provides a complementary argument in support of Braithwaite's plea for sanctioning systems that reintegrate rather than isolate punished offenders. In *Crime, Shame and Reintegration*, he argues that conscience is a more potent deterrent threat than punishment by the criminal justice system (Braithwaite 1989). In Braithwaite's view, pangs of conscience depend on the individual's social integration. Therefore formal sanction processes that do not reintegrate the punished exacerbate misconduct. Here I am suggesting that reintegration may serve to preserve deterrent effects that depend on stigma. Research that models and calibrates the long-term feedback effects of sanction policy is urgently needed.

IV. Ecological Studies

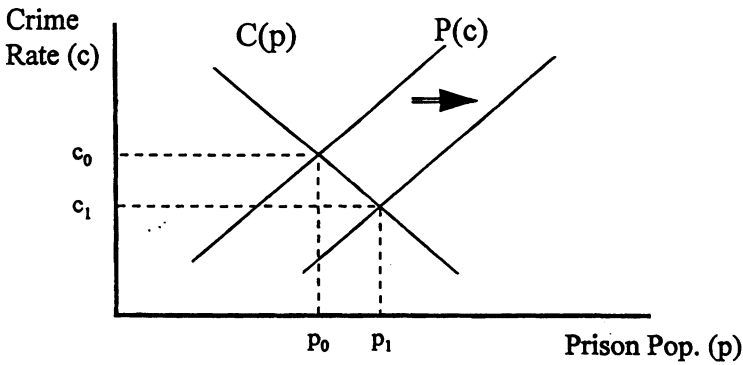
The obstacles to making valid causal inferences from analyses of natural variations are many—incomplete specification of relevant causal factors, measurement error, unmeasured persistent heterogeneity, and endogeneity of regressors (i.e., simultaneously determined regression variables), to name just a few. In the case of deterrence studies, the endogeneity problem, described in Section II, stands out as probably the most important and certainly the most salient obstacle to making

inferences about the deterrent effects. To isolate the deterrent effect requires that the analysis also take into account the effect of crime rate on sanction level. This requires the imposition of so-called identification restrictions. There are many forms of identification restrictions, but the most common is the assumption that some factor or set of factors affects only one of the endogenous variables of interest. Thus, to identify the deterrent effect of sanctions on crime requires that the statistical model assume that some factor, such as court orders to reduce prison overcrowding, directly affects sanction levels but only affects crime through its effect on sanction levels. A major focus of the 1978 academy report (Blumstein, Cohen, and Nagin 1978) and my contributions to its commissioned papers (Fisher and Nagin 1978; Nagin 1978) was the veracity of the restrictions that were imposed. Accordingly, a primary focus of my review is the strategies that have been used to deal with simultaneity. A second major focus is the interpretation of the estimated deterrent effect of a specific policy lever. I argue that, while the extant evidence provides useful guidance on the average effect of specified policies across all implementations, it is of limited value for predicting the effect of any specific implementation of the policy. Two broad classes of ecological analyses are considered—studies of the deterrent effect of prison and of the police.

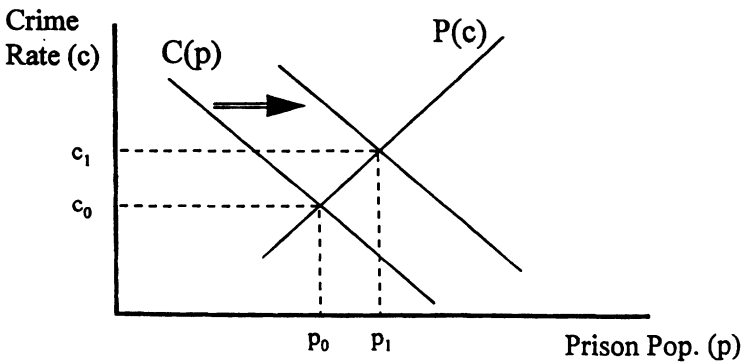
A. The Effect of Prison Population on Crime Rate

Between 1974 and 1994 the number of people incarcerated in state or federal prisons grew at an average annual rate of 7.9 percent; the result has been a near quintupling of the prison population—218,000 to 1,016,000 (Maguire and Pastore 1996). Whether this run-up in prison population has materially affected the crime rate has profound implications for public policy, yet there has been surprisingly little analysis of this question. The few studies that have been done produce a range of conclusions from that of Zimring and Hawkins (1995) that the effect has been negligible to an estimate by Levitt (1996) that each additional prisoner averts about fifteen index crimes. In between are the estimates of studies by Marvell and Moody (1994) and Spelman (1994).

The paucity of studies is probably attributable to the problem of identification that I wrote about nearly twenty years ago (Nagin 1978). Figure 4 depicts the problem graphically. In each panel there are two curves—a crime rate function, $C(P)$, and a prison population function, $P(C)$. The crime rate is depicted as a declining function of the prison



a



b

FIG. 4.—The identification problem. *a*, The effect of a policy that increases average punishment per crime. *b*, Contamination by the influence of increases (decreases) in the prison spawned by exogenous increases (decreases) in the crime rate.

population. The downward-sloping crime function reflects the preventive effects of imprisonment through some combination of deterrence and incapacitation. The upward-sloping prison population function captures the effect of crime on the size of the incarcerated population. For any given level of sanction threat—average incarceration time per crime committed—more crime will generate larger prison populations.

Studies attempting to measure the effect of prison population on crime rate that do not take into account the mutual interaction of $C(P)$ and $P(C)$ depicted in figure 4 will confound the preventive effect with the effect of crime on prison populations. To see this, consider first panel *a*. This panel depicts the effect of a policy that increases average

punishment per crime. An example is a policy that curtails parole boards powers. Such policies will result in a rightward shift of $P(C)$ that results in an increase in the prison population from p_0 to p_1 , which is accompanied by a reduction of the crime rate from c_0 to c_1 . In the parlance of econometrics, the exogenously induced shift of $P(C)$ “identifies” $C(P)$ under the assumption that the influence causing the shift in $P(C)$ does not directly affect crime behavior by also shifting $C(P)$. In the real world, $C(P)$ is also shifting. Suppose a change in demography, such as an increase in the number of young men, causes a rightward shift in $C(P)$. Such a shift could induce the same p_0 - p_1 increase in the prison population depicted in panel *a*, but here the increase in prison population is accompanied by an *increase*, not a decrease, in crime rate. Consequently, studies that do not take into account the mutual determination of crime rate and prison populations are likely to underestimate the preventive effects of prison sanctions because the preventive effect depicted in panel *a* will be contaminated by the influence of increases (decreases) in the prison spawned by exogenous increases (decreases) in the crime rate as depicted in panel *b*.

This identification problem is not limited to technically sophisticated multivariate regression studies. Quite to the contrary. Any approach that simply associates crime rates with prison population will suffer from the contamination problem. This includes seemingly straightforward approaches intended to appeal to our common sense, such as graphical comparisons of crime rates and prison population and comparisons of average changes in crime rates and in prison population.

At least one study has plausibly dealt with the simultaneity problem. Levitt (1996) employs a clever strategy for identifying $C(P)$: shifts in $P(C)$ resulting from court orders to reduce prison overcrowding. The Levitt analysis is based on a panel data set of states for the years 1971–93. For some part of this period the entire prison systems of twelve states were under court order to reduce overcrowding. Levitt finds that in the three years prior to the initial filing of overcrowding litigation in these states their prison population growth rates outpaced the national average by 2.3 percent. In the three years following the initial filing of the overcrowding litigation, their prison population growth lagged the national average by 2.5 percent per year. The effect was even more dramatic following the handing down of the final court order, a 4.8 percent lower growth rate than the national average.

Levitt argues that overcrowding litigation affects the crime rate only

through its effect on prison population. That is, such litigation shifts $P(C)$ but does not shift $C(P)$. His arguments and supporting evidence are plausible, and, more generally, the analysis is thorough. Thus his estimate of the effect of prison population on crime rate, fifteen index crimes averted for each additional man-year of imprisonment, deserves close attention.

Levitt does not attempt to partition his estimate of the preventive effect between incapacitation and deterrence, but his estimate is not much larger than the estimated rates of offending of incarcerated populations reported in various studies of the incapacitation effects (Visser 1986; Blumstein, Cohen, and Canela-Cacho 1993). This suggests that incapacitation effects make a substantial contribution to his overall effect estimate.

From a policy perspective the distinction between deterrence and incapacitation is academic; the central question is how much a given policy will affect the crime rate. The answer to this question can be partitioned into two parts: the effect of the policy on the prison population and the effect of that prison population on crime rate. Can a study such as that conducted by Levitt provide an all-purpose estimate for calibrating the second effect? The answer, I believe, is no. Specifically, there is good reason for believing that policies producing equivalent changes in the prison population will not result in the same change in crime rate.

To make this argument I again return to the Levitt analysis to provide a concrete point of reference, but my arguments are generic to all regression-based studies. Levitt's estimate is based on the identifying power of reductions in prison populations forced by court orders to reduce overcrowding. In the parlance of experimental design, this study measures the treatment effect on the crime rate of reductions in prison population achieved principally by early release of prisoners.

Figure 5 is a two-dimensional taxonomy of sanction policies affecting the scale of imprisonment. One dimension labeled "Type" distinguishes three broad categories: policies regulating certainty of punishment, such as laws requiring mandatory imprisonment; policies influencing sentence length, such as determinate sentencing laws; and policies regulating parole powers. The second dimension of the taxonomy, "Scope," distinguishes policies that cast a wide net, such as a general escalation of penalties for broad categories of crime, compared to policies that focus on targeted offenses (e.g., drug dealing) or offenders (e.g., "three-strikes" laws).

		Scope	
		General	Targeted
Type	Certainty	Increasing the number of police	Police crackdown on drug dealing
	Severity	Broad-based mandatory minimums	"Three-Strikes" laws
	Parole	Parole abolition	No parole for violent offenders

FIG. 5.—Taxonomy of prison sanction policies

The nearly 500 percent growth in prison population over the last two decades is attributable to a combination of policies belonging to all cells of this matrix. Parole powers have been greatly curtailed, sentence lengths increased, both in general and for particular crimes (e.g., drug dealing), and judicial discretion to impose nonincarcerative sanctions has been reduced (Morris and Tonry 1990; Cohen and Canela-Cacho 1994; Tonry 1995). Consequently, any effect on the crime rate of the increase in prison population reflects the effect of an amalgam of potentially interacting treatments. By contrast, the treatment effect estimated in the Levitt study measures the preventive effect of reductions in the imprisonment rate induced by the administrative responses to courts orders to reduce prison populations. Thus, his estimate would seem only to pertain directly to policies affecting parole powers.

Is this treatment effect generalizable to the whole range of sanction policy options shown in the figure 5 taxonomy? I suspect not. Increased incarceration of individuals convicted of drug offenses has been a major factor contributing to the growth in prison population in the past decade (Cohen and Canela-Cacho 1994; Mauer and Huling 1995; Tonry 1995; Zimring and Hawkins 1995). This reflects the effect of statutory changes that require the incarceration of drug offenders (cell 2 of figure 5) and increase the length of that incarceration (cell 3). It is not likely that these drastic increases in penalties for drug dealing have had any material effect on the drug trade (Rydel and Everingham

1994). Indeed they may have actually increased the rate of other income-generating crime such as robbery, burglary, and larceny by making them comparatively more attractive than dealing.

Further, my research (Cohen et al. 1995) suggests large differences in the nondrug felony offense rates of drug dealers sentenced to prison compared to other types of offenders in prison. Specifically, this study finds that persons convicted of dealing have distinctly lower nondrug felony offending rates than those convicted of robbery and burglary. The implication is that Levitt's work overstates the preventive effects of such "War on Drugs" statutes. More generally, Levitt's estimate is not likely to be informative about policies affecting prison sanctions for specific types of offenses (e.g., longer sentences for armed robbers). Levitt (1995) himself argues that the response to such targeted policies will include a combination of suppression of the targeted offense and substitution to other types of offenses. Other examples of sentencing policies to which the Levitt study estimate is unlikely to generically apply are mandatory sentence enhancement for weapon use, "three-strikes" laws, and laws mandating incarceration of individuals who would otherwise be diverted.

B. The Effect of Police on Crime Rate

The largest body of evidence on deterrence in the ecological literature focuses on the police. The earliest generation of studies on the deterrent effect of police examined the linkage of crime rate to measures of police resources (e.g., police per capita) or to measures of apprehension risk (e.g., arrests per crime). These studies were inadequate because they did not credibly deal with the endogeneity problem (Nagin 1978; Wilson and Boland 1978). If the increased crime rates spur increases in police resources, as seems likely, this endogeneity must be taken into account to obtain a valid estimate of the deterrent effect of those resources. By the same logic depicted in figure 4, if the endogeneity is not taken into account, the estimate of the deterrent effect of police resources is likely to be underestimated. Alternatively, if the focus is on the effect of the arrest per crime ratio (hereafter, the arrest ratio), failure to properly account for endogeneity may overstate the deterrent effect. Here the argument is that increased crime may swamp police resources with the arrest ratio declining as a consequence.

Wilson and Boland (1978) conducted the first study that in my judgment plausibly identifies the deterrent effect of the arrest ratio. They

argued that the level of police resources per se is, at best, only loosely connected to the apprehension threat they pose. Rather, the crucial factor is how the police are mobilized to combat crime; Wilson and Boland argue that only proactive mobilization strategies will have a material deterrent effect. In their words (1978, p. 373), "By stopping, questioning, and otherwise closely observing citizens, especially suspicious ones, the police are more likely to find fugitives, detect contraband (such as stolen property or concealed weapons), and apprehend persons fleeing from the scene of a crime."

In Wilson and Boland's analysis, identification is achieved by the assumption that proactive and aggressive policing contributes to the determination of apprehension threat, as measured by the arrest ratio, but has no direct effect on the behavior of criminals except through the effect on this ratio. Their identification strategy also depends on the assumption that the choice of policing strategy is independent of the crime rate. In support of this assumption they point out that patrol strategy can not be predicted by the crime rate. They also appeal to Wilson's own seminal work on policing (Wilson 1968) to argue that patrol strategy is a consequence of political and bureaucratic features of the local environment rather than the crime rate. Their cross-sectional analysis of thirty-five cities, in which police aggressiveness is measured by moving violation citations per patrol unit, concluded that the arrest ratio has a substantial deterrent effect on robbery.

The Wilson and Boland study spawned a small flurry of studies (Jacob and Rich 1981; Decker and Kohfeld 1985; Sampson and Cohen 1988). I will focus on the Sampson and Cohen study, for it is notable in two important respects. First, it expands the Wilson and Boland conception of the deterrent effect of policing. Second, it is the only ecological deterrence study I know of that attempts to estimate deterrent effects across subpopulation groups.

The Sampson and Cohen study is based on a 1980 cross-section of 171 cities. Their key premise is that "hard" policing of "soft" crime—such as prostitution, drunkenness, and disorderly conduct—deters serious criminality. More recently, this policing strategy, which involves proactive efforts to suppress disorder by, for example, breaking up congregations of idle young men or making "random" safety checks of vehicles with suspicious drivers, has been credited as a key factor in recent large reductions in the New York City crime rate (Gladwell 1996). Sampson and Cohen build on Wilson and Kelling (1982), Greenberg, Rohe, and Williams (1985), Sherman (1986), Skogan

(1986), and others who have examined the negative externalities of urban disorder and fear of crime and on Sampson's own work on social control (Sampson 1986). They explore two alternative mechanisms through which "hard" policing of disorder may deter crime. The first is the Wilson and Boland model: aggressive policing of public disorder deters serious crime *indirectly* by shifting the arrest ratio function in a way that is equivalent to the shift of $P(C)$ in panel *a* of figure 4. This shift will in turn reduce crime. Alternatively, separate from any effect on the arrest ratio, suppression of "soft" crime may make public spaces more desirable and secure and thereby encourage law-abiding citizens to reoccupy these public spaces and reassert informal sources of social control. The result may be fewer attractive crime opportunities.

Sampson and Cohen, like Wilson and Boland, find strong evidence of the arrest ratio deterring robbery in a simultaneous equations model. The model is identified by their expanded measure of police aggressiveness in suppressing incivilities. In a reduced-form format they also find a negative association between the robbery rate and their measure of aggressiveness. This reduced-form estimate captures the combined effect of aggressiveness from all sources—shifts in the arrest ratio function, changes in the crime opportunity structure due to informal social control, and altered offender perceptions.

While the estimate from the reduced-form equation does not isolate the deterrent effect of the arrest ratio, it is actually more policy relevant. It measures the effect of the policy lever that the police can directly control. The police cannot directly manipulate the arrest ratio, but they can choose how aggressive to be in suppressing incivilities.

A second important innovation in Sampson and Cohen is that they estimate not only a population-wide deterrent effect but disaggregate this effect across segments of the population—white juveniles, black juveniles, white adults, and black adults. They do this by using arrest rates as surrogate measures of demographic group-specific offense rates. They find a negative deterrent-like association between aggressiveness and arrest rate for all groups, but they also find significant differences by race and age in the magnitude of the effect. For robbery, at least, adults seem to be more deterred by police aggressiveness than juveniles, with black adults seemingly more deterrable than white adults.

Because the results for specific demographic groups are based on arrest rates, they must be qualified in a number of obvious ways. Notwithstanding, the efforts of Sampson and Cohen to disaggregate are

laudable and where feasible should become standard in deterrence studies. The differences in response across demographic groups identified in this study are still another reminder that regression coefficients are only measuring an average effect. A priori we would not expect all people or segments of the population to respond in the same way to police aggressiveness. Indeed there are good reasons for believing the response will vary in the population. For instance, I am not surprised that adults seem to be more deterrable than juveniles because the consequences of apprehension are graver for adults.

Two other noteworthy studies of the effect of police on crime are Levitt (1997) and Marvell and Moody (1996). Both use similar data and attempt to estimate the effect of officers per capita on the index crime rate and its constituent components. The Levitt study is based on a panel data set of large U.S. cities for the period 1970–92. The Marvell study is based on a state panel for 1973–92 and a panel of large cities for the same years.

The major difference in the studies is that they deploy very different statistical modeling strategies. Marvell and Moody use Granger causality testing, which, stripped to the bare essentials, involves regressing levels of police resources in time periods $t - 1$ and earlier on the crime rate in period t , and vice versa. The idea is to test whether, controlling for the levels of other potentially relevant factors, resource levels in prior periods predict the crime rate in the current time period. Levitt uses structural equation modeling and again makes use of a clever identification restriction—the timing of mayoral elections. He shows that increases in the size of police forces are disproportionately concentrated in election years and argues that there is little reason to think that elections will otherwise be correlated with crime.

When these very different statistical methods are applied to essentially the same data set, both analyses reach similar conclusions. Both find evidence of a negative (deterrent-like) association between officers per capita and index crimes. Levitt's estimate of the elasticity of the violent crime rate to sworn officers is about -1 , and for property crime his elasticity estimate is about -0.2 . Marvell's estimates of elasticities also vary across crime type but average about -0.4 .

These studies also provide still another reminder that regression coefficients are only measuring average treatment effects. The elasticities cited above apply to all places and times only under the condition that the treatment effect is invariant over place and time. The studies of Wilson and Boland (1978) and Sampson and Cohen (1988) and of in-

errupted time-series analyses of police deployment (Sherman 1990) all point to the not surprising conclusion that the treatment effect of police presence is not constant but rather is contingent on the way the force is mobilized. Consequently, for any given locale the Levitt and Marvell/Moody deterrent estimates may either greatly overstate or understate the effect of a change in the size of the police force.

I know of only one study that provides direct evidence on cross-jurisdiction variation in such response. McDowell, Loftin, and Wiersema (1992) conducted a study of the deterrent effect of mandatory sentencing laws for gun crimes. They note (1992, p. 385) that the effect of the law will not be constant but will “vary because of differences in the details of the laws, implementation, publicity and other factors specific to a given setting.” More broadly, the magnitude of deterrent effects may be dependent on the social and economic context in which a sanction policy is imposed. For example, the magnitude of the deterrent effect may be contingent on the availability of legal work opportunities (Fagan 1994). McDowell et al. estimate a model that makes it possible to calibrate not only the magnitude of the mean treatment effect but also its variation. Their analysis estimates that mandatory sentencing laws *on average* reduced gun homicides by about two-thirds of a standard deviation. However, except by extraordinary coincidence, this average does not measure the effect in any specific city. As McDowell et al. themselves point out, their statistical model implies that some cities “might register an increase in gun homicides following the law” (1992, p. 390) even though the analysis implies that across the population of all cities there would be a reduction. Future ecological research based on interjurisdiction variations in crime rate and sanction levels should follow the lead of McDowell et al. and attempt to calibrate the magnitude of the cross-jurisdiction variation in the response to a law enforcement treatment.

V. The Link between Prescribed and Actual Policy: The Technology of Sanction Delivery

The history of policy implementations is littered with examples of supposedly major reforms having no apparent effect and even counterproductive effects. Crime control policy has had its fair share of failed attempts to alter sanction threats. Tonry (1995) offers a long list of examples—New York’s Rockefeller Drug Laws, which increased statutory penalties for illicit drug dealing; broad-based systems of mandatory minimum penalties at both the federal and state levels; and

targeted penalty enhancements for firearms use in Michigan and Massachusetts—all of which were largely unsuccessful in altering sanction threats or altered them in ways that were not intended. Actual policy bears little resemblance to intended policy because the exercise of discretion by the key actors of the criminal justice system drives a wedge between the reality of the policy and its intention as expressed by its formulators, generally elected officials. Police are selective in enforcement, prosecutors are selective in whom to prosecute and for what, judges and juries decide who to convict and for what, and judges rationalize wide leeway in sentencing.

However, all efforts at altering threats are not futile. The Internal Revenue Service has had enormous success in increasing compliance for specific types of income, such as dividends and interest, by requiring payers to provide them records of such payments. Airport security procedures have been very effective in averting hijacking. What then distinguishes successful efforts to affect sanction threats from those that are not?

Credibility is assuredly crucial. If a sanction threat is not credible it will not be effective. Penalties for unreported income apply equally to high-visibility and low-visibility income but are only credible for the former. But this observation begs the question—what then determines credibility? Economic feasibility certainly plays a decisive factor. It has long been appreciated that resource constraints have pronounced and far-reaching effects on the functioning of the criminal justice system—the tactics and deployment of the police, case-processing and plea-bargaining decisions of prosecutors, sentencing decisions of judges, and release decisions by parole boards are all shaped in major ways by resource constraints. Yet surprisingly little attention has been given to pinning down the role of cost in determining the success of policies to alter the sanction risks posed by the criminal justice system. The problem is that cost is endogenous; it depends on the response of would-be offenders to policy.

In the tax compliance arena, some valuable headway has been made on this problem. Here economists have developed models of strategic interaction between taxpayers and the tax collectors (Graetz, Reingaum, and Wilde 1986; Beck and Jung 1989; Erard and Feinstein 1994). These models nicely illustrate that credibility and effectiveness are substantially affected by the cost of projecting the sanction threat. Compliance rates are high for income sources subject to information reporting; taxpayers know that the Internal Revenue Service can easily

detect nonreporting of such income, so cheating is uncommon. Stated differently, the threat of detection is credible because the cost to the Internal Revenue Service of projecting the threat is low relative to the revenue gain that is returned. The result is not only high rates of compliance but also negligible enforcement costs to the Internal Revenue Service precisely because cheating is infrequent. At the other end of the compliance spectrum is cash income. Here the Internal Revenue Service cannot project a credible threat because costs are high relative to gain. As a result, compliance rates are low, but so are enforcement expenses. The Internal Revenue Service makes no substantial effort to enforce compliance because it is not worth the candle. Taxpayers know this, which is precisely why they cheat with impunity.

Another factor that will affect credibility is the size of the would-be offender population: those who could conceivably be motivated to offend. If this population gets too large, it may overwhelm the system's capacity to project a credible enforcement threat. The proliferation of abusive tax shelters during the 1970s and 1980s seems to have overtaken the Internal Revenue Service's capacity to effectively regulate them. This consideration was a major factor in the near abolition of tax shelters in the Tax Reform Act of 1986 (Nagin 1989).

The key lesson of these models of strategic interaction is that a sanction threat cannot be effective unless it can be administered economically. While the specific forms of the models of strategic interaction between taxpayers and the tax collector are not transferable to capturing the interaction between the criminal justice system and would-be criminals, the concepts of credible threat and strategic interaction are applicable. For instance, consider "three-strikes" type statutes that threaten draconian punishments to individuals with multiple convictions. The credibility of such sanction threats to repeat offenders is likely to be undermined in at least two ways. First, competition among elected officials to be toughest on crime creates pressure to widen the population of repeat offenders either by broadening the types of offenses that count as "strikes" or reducing the number of "strikes" to be subject to the penalty enhancement. This political version of "king of the mountain" dilutes the economic feasibility of such supposedly targeted policies by widening the net of applicability. The penalty enhancement is simply too costly to impose on too broad a segment of the offender population. My bet is that offenders come to know this and respond accordingly. Second, draconian penalties increase the incentives for defendants to demand trials rather than plea bargaining.

The result may be that the criminal justice system will be overwhelmed—again with the effect of making the threatened sanction a paper tiger just as was the case with the Rockefeller Drug Law of the 1970s.

Another important factor that is likely to be important in determining the capacity of the criminal justice system to translate policy into a credible threat is perceptions of fairness. If the threatened penalty so offends the sensibilities of juries, they may engage in jury nullification and refuse to convict. Alternatively, prosecutors may themselves nullify the case by dropping or altering charges. Indeed Andreoni (1991) makes just this argument and advances a model predicting that the probability of conviction will be inversely related to statutory penalties. In Andreoni (1995), he goes on to provide evidence that higher penalties may so reduce probability of conviction that the deterrent effect of the penalty enhancement is nullified. His finding is reminiscent of Ross's (1976) conclusion that tough penalties for drunk driving were ineffective because they were not administered as intended and of Tonry's (1995) account of the unwillingness of juries and judges to enforce the litany of capital offense laws in eighteenth-century England.

In summary, effective use of sanction policy levers to deter crime requires that the policy be administered as intended, yet experience demonstrates policies are commonly not administered as planned. Research on sanction policy implementation is fragmentary and incomplete. To define the boundaries of feasible policy we must gain a better understanding of the process of sanction generation.

VI. Conclusions

Our knowledge about deterrent effects is vastly greater than in 1980 but, as is so often the case, the more we learn the more we come to appreciate that prior conceptions of the key questions were oversimplified. Thus, while I am confident in asserting that our legal enforcement apparatus exerts a substantial deterrent effect, four major knowledge gaps limit our capacity to make confident predictions about what works in specific circumstances: First, it is necessary to know about more-than-average effectiveness; we need a better understanding of how and why responses to policy vary across time and space. Second, analysis must go beyond estimating only short-term consequences to calibrating long-term effects. Third, knowledge about the relationship of sanction risk perceptions to actual policy is virtually nonexistent; such knowledge would be invaluable in designing effective crime-

deterrent policies. Fourth, research on the linkage between intended and actual policy is fragmentary; a more complete understanding of the process of sanction generation is necessary for identifying the boundaries of feasible policy. This then is the outline of my agenda for research on deterrence for the outset of the twenty-first century.

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