down defensive structures usually associated with cities (Weber 1950), and incorporating them at the community level. Decreasing neighborhood permeability, however, may negatively affect intercommunity relations. Presumed crime-prevention benefits to be gained by such physical shifts need to be weighed against adverse social impacts at a higher level.

One reason it is necessary to be so cautious in our conclusions derives simply from the independent variables in question. It is exceedingly difficult, using an experimental or rigorous quasi-experimental design to simultaneously change a large number of PE features on a block or in a neighborhood. Case studies (Dayton, Miami Shores) and two quasi-experiments (Hartford, Oakland) showed that PE changes resulted in improved safety. One longitudinal study suggested physical features contribute independently to crime changes over time. Given the scale of many of the features at issue here, research is unlikely to learn precisely about causal connections as features change. Once neighborhood layout is established, or a suburban development built, it cannot be changed wholesale.

But there is also the suggestion in some of these works that improvements may be contingent on local social and political processes. We can better understand these contingencies by more closely integrating work on PE and crime with the broader theoretical perspectives that have been described above.

15

Deterrence

Steven D. Levitt

Opportunities to commit crime are everywhere: Each house is a potential burglary target; every store is a place to shoplift; and each person passed on the street offers the prospect of a robbery. In practice, however, crime is extremely rare. Crime is infrequent for many reasons, but one of the most important factors is likely to be fear of punishment (especially in a country like the United States, where over 2 million people are incarcerated at any given time). Deterrence, which is the subject of this chapter, is the formal term for the idea that criminal behavior can be affected by the threat of punishment.

Deterrence can operate in many ways. For instance, increases in the number of police on the streets may raise the likelihood that a crime will lead to an arrest. Stiffer prison sentences also will increase the costs of crime. But deterrence need not operate only through the criminal justice system. If a criminal knows that a potential robbery victim is armed with a gun, the criminal may be afraid to attempt the robbery. Neighborhood watch groups that actively report crimes to the police will increase the chances that a criminal is caught, reducing the attractiveness of the illegal act.

The chapter is structured as follows. First, I examine the theoretical underpinnings of deterrence. Although the theory of deterrence is straightforward, misconceptions nonetheless persist. For instance, many criminologists mistakenly believe that criminals must be "rational" for deterrence to exist.

I then discuss the reasons why testing for deterrence in the data is not nearly as straightforward as it might initially appear. There are three main reasons. First, the deterrence model is a description of individual behavior, but most tests of the model rely on aggregate data due to data limitations. Second, it is often difficult to separate correlation and causality. To the extent that public policy on crime is itself affected by the level of crime (for

example, more police are hired in response to a crime wave), standard statistical approaches like ordinary least squares regression do not reliably test the effectiveness of deterrence. Third, it is often extremely difficult to differentiate between deterrence and other competing hypotheses. In particular, incapacitation—a reduction in crime due to the fact that prisoners are quarantined from society at large and thus cannot commit crimes while incarcerated—is in most cases indistinguishable from deterrence. Thus, most purported tests of deterrence are in reality joint tests of deterrence and incapacitation.

The bulk of the chapter is devoted to assessing the existing empirical evidence on deterrence. In particular, I focus on four different settings where deterrence might be expected to arise: increases in the size of the police force, the scale of imprisonment, the death penalty, and victim precaution. Although there is an enormous empirical literature on the subject, many of these studies shed little light on deterrence as a consequence of failure to deal adequately with one or more of the pitfalls described above. As a result, the emphasis in this chapter is on a highly selective set of papers which I believe are most successful in overcoming these inherent empirical difficulties. More exhaustive surveys of the empirical research on issues related to deterrence can be found in Cameron (1988) and Nagin (1998). The best available evidence suggests that deterrence plays a nontrivial role in explaining differences in crime rates across time and space, but by itself deterrence falls far short of providing a complete explanation for the observed patterns of crime.

THE THEORY OF DETERRENCE

The idea underlying deterrence—that criminals respond to the costs and benefits of crime—featured prominently in the thinking of classical writers such as Bentham and Beccaria. In the modern era, however, deterrence received little attention from those studying crime until the seminal contribution of Becker (1968), which provided the first formal mathematical model of deterrence. Since that time, deterrence has been the focus of a great deal of research, both theoretical and empirical, especially among economists (for example, Stigler 1970, Ehrlich 1973, Posner 1977, Polinsky and Shavell 1984, Kaplow and Shavell 1999). Because the Becker model has so greatly influenced the way economists think about crime, it is sometimes simply referred to as the "economic model of crime."

Becker's model is one in which potential criminals are rational utility maximizers (in the sense that they use all information available in optimally choosing actions). Furthermore, there is complete information, that is, potential criminals know precisely the likelihood with which they will be caught if they commit a crime, as well as the punishment. The decision to commit crime is weighed against the best legitimate job opportunity. If the payoff to crime exceeds that of the best legitimate opportunity, then the crime is committed.

The Becker model is not intended to be an accurate, literal reflection of the world. In real life, people are not perfectly rational in their actions (especially when under the influence of drugs and alcohol as are many criminals at the time of their crimes). Individuals have less than perfect information about the probability they will be caught and the punishment they will receive if convicted.

Indeed, many of the specific predictions of the Becker model do not match real-world experiences well. For example, the model predicts that fines will be used as punishment rather than imprisonment and that all crimes will be punished with the maximum penalty, but with varying probabilities of apprehension. The primary contribution of the Becker model was not its predictions, but rather that it provided researchers with a simple framework for thinking about how the costs and benefits of crime influence criminal behavior. As a consequence, extensions of the Becker model do succeed in generating predictions that are consistent with various stylized facts in criminal justice: stiffer penalties for more severe offenses (Stigler 1970), harsher penalties for repeat offenders (Polinsky and Rubinfeld 1991), less than maximal penalties (Andreoni 1991), the use of prisons rather than fines (Levitt 1997b). Clotfelter (1978) and Shavell (1991) further extend the economic model of crime to incorporate a role for victim precaution (for example, installing car alarms or carrying concealed weapons).

In the simplest versions of deterrence, the key determinant influencing crime is the "expected punishment," which is simply the amount of prison time an offender will serve on average for committing a particular crime. The expected punishment, expressed mathematically, is the probability that a criminal is caught multiplied by the punishment if caught. Certainty and severity of punishment play equal roles in deterring crime in such models. Although not discussed as frequently, the swiftness of punishment is also likely to influence crime. The benefits of crime accrue immediately, but punishment is delivered with a delay. Because individuals tend to discount the importance of future events relative to the present, the faster punishment can be administered, the greater the predicted reduction in crime.

Because formal models of deterrence generally assume both that criminals are rational and that they have detailed knowledge of the certainty and severity of punishment, a misconception has arisen that deterrence cannot work in the absence of these restrictive assumptions. This claim, however, is patently false. The only requirement for deterrence to be present empirically is that criminals *on average* must respond to changes in the costs and benefits of crime. Some potential criminals may greatly understate the true likelihood of punishment and therefore go undeterred; other criminals may have exaggeratedly high estimates of the probability of detection and thus be "excessively" deterred. It is not rationality that is required for deterrence, but rather that people—including criminals—are capable of responding to incentives. This would seem to be a minimal hurdle to clear as incentives are a ubiquitous element of everyday life: People study to get good grades, work hard to get promoted, consume more of a good when the product's price falls, and so on. Even pigeons and rats show the ability to respond to incentives in laboratory experiments.

DIFFICULTIES IN TESTING THE DETERRENCE HYPOTHESIS

Despite the simplicity of the logic underlying deterrence, it has proven surprisingly difficult to estimate the magnitude of deterrence empirically. As noted briefly at the beginning of this chapter, there are three primary obstacles that need to be overcome. First, deterrence is a model of individual-level behavior, but almost all relevant crime data are at the aggregate level such as those for cities, counties, states, or nations. When individual-level data on criminal activity are available, for example in the longitudinal data sets analyzed by Wolfgang et al. (1972) and Sampson and Laub (1993), all of the subjects are drawn from localized geographic areas. For example, in the classic Glueck and Glueck (1950) data set reanalyzed by Sampson and Laub, all of the subjects are from the Boston metropolitan area. Consequently, all individuals in the sample presumably face similar punishment regimes, making it difficult to test for deterrence. Tauchen et al. (1994) attempted to overcome this problem in data on a cohort of Philadelphia youths collected by Wolfgang et al. (1972) by using a particular individual's own arrest history as a proxy for expected punishment. This approach, however, is subject to criticism because past punishment history is likely to be correlated with unobserved factors that also influence an individual's criminal propensity. A similar criticism can be leveled at Witte (1980). The overwhelming majority of studies, however, have used aggregate data. While there is nothing inherently flawed in such an approach, aggregate data necessarily provide only an indirect test of individual-level behavior, leading to possible misinterpretation. This is especially true given that the available aggregate data based on reported crime statistics suffer from important weaknesses: For example, many crimes are never reported to the police and thus are not included in official records (O'Brien 1985).

The second difficulty in testing deterrence is differentiating between correlation and causality. If two variables are correlated, it simply means that they tend to move together. It does not necessarily demonstrate the presence of a causal relationship, or identify the direction of causality. Both for the purposes of testing a hypothesis and developing good public policy, only causal relationships are relevant. Standard empirical approaches typically used in studies of deterrence (for example, simple correlations or ordinary least squares regression analysis), however, identify correlations between variables, not causal relationships. The issue of correlation versus causality proves to be especially important in measuring deterrence because crime policy emerges from a political process. When crime is high, more money tends to be spent on the criminal justice system. As one example, large cities tend to have both many more police officers per capita and higher crime rates than smaller cities. Thus, there is a positive correlation between police and crime in a cross-section of cities, but it cannot plausibly be argued that the greater numbers of police *cause* the high crime. Rather, the reverse is true. If one wishes to determine the impact of police on crime, an alternative approach must be identified.

A third factor making it hard to test adequately the deterrence hypothesis is that empirically deterrence and incapacitation are very difficult to distinguish. Unlike deterrence, incapacitation is not a behavioral theory of crime, that is, incapacitation can exist even if no individual alters his or her behavior in response to incentives. Incapacitation simply asserts that repeat offenders will be unable to commit further crimes when imprisoned because they are isolated from society at large. The number of crimes prevented through incapacitation will be closely related to the expected punishment for crime, just like deterrence. As a consequence, most studies of the impact of criminal justice resources (which are hypothesized to impact crime through increases in expected punishment) are not tests of deterrence, but rather are joint tests of the combined crime-reducing impact of deterrence *and* incapacitation. Given that there is overwhelming evidence that many prisoners are frequent offenders when not incarcerated (Chaiken and Chaiken 1982, DiIulio and Piehl 1991), studies that attrib-

ute all of the crime reduction associated with increased punishment to deterrence are likely to overstate the true magnitude of the deterrent effect.

THE IMPACT OF POLICE ON CRIME

A logical place to begin a discussion of the impact of the criminal justice system on crime is with police, who represent the first line of defense against crime. An enormous literature on this topic emerged in the 1970s. Some of the earliest evidence comes from a quasi-randomized experiment conducted in Kansas City (Kelling et al. 1974). Five "proactive" beats were assigned two to three patrol cars per beat; five "reactive" beats were given no routine preventative patrols, although police of course responded to calls for service; and five beats maintained the standard level of patrol: one car per beat. The experiment ran for one year. A number of outcome variables were analyzed including reported crime rates, victim-derived crime rates, and citizen perceptions. Few statistical differences were found across the beats. The experiment, however, has been criticized on numerous grounds by Larson (1976). Perhaps most troubling is the fact that police response times were unaffected by the experiment. Thus, the practical differences in police services provided to the treatment and control beats, at least along one of the most important dimensions, was negligible. As a consequence, most researchers view this experiment as inconclusive.

A great number of studies were published that compared the relationship between crime rates and the number of police across cities or states at a given point in time (known as cross-sectional studies). Of the twenty-two studies of this type that Cameron (1988) surveys, eighteen find either no relationship or a *positive* relationship between the number of police and the crime rate. A positive relationship between police and crime means that places with more police also tend to have more crime. This result is contrary to the prediction of deterrence. The clear weakness of these studies is the failure to distinguish correlation from causality, a point made quite definitively by Fisher and Nagin (1978). Places with more severe crime problems respond by hiring more police. This induces a positive correlation between police and crime, but says little about the causal impact of hiring more police. The research conducted by Fisher and Nagin (1978) was part of an extremely influential National Academy of Sciences panel on deterrence that called into question the validity of all of the empirical work on the subject up to that time. Following the release of the scathing report of this panel, research on the impact of the number of police on crime virtually disappeared for almost two decades. Sherman (1992), in

surveying the literature on police and crime, relegates discussion of this entire question to a footnote, devoting his attention instead to studies of policing strategy—that is, how to most effectively use a fixed number of officers.[1]

It was not until the 1990s that researchers returned to the task of estimating the impact of police on crime with a range of solutions to the difficulties raised by Fisher and Nagin (1978). Levitt (1997a) attempts to solve the question of reverse causality using an "instrumental variables" approach. The goal in such research is to identify a "natural experiment" which systematically affects the size of the police force, but is otherwise unrelated to crime. Levitt (1997a) uses mayoral and gubernatorial elections as such a source of variation in police forces. Empirically, it is shown that increases in the size of big-city police forces are disproportionately concentrated in election years, presumably engineered by politicians seeking the maximum electoral benefit. Having controlled for other government spending, demographic factors, and the state of the local economy, it seems plausible to argue that the impact of elections on crime through channels not taken into account in the analysis will be negligible, as is necessary for the instrumental variables strategy to yield interpretable results. Although imprecisely estimated, Levitt (1997a) finds additional police to be extremely effective in reducing crime: A 10 percent increase in the size of the police force is associated with a decrease in crime of 3–10 percent.

Marvell and Moody (1996) attack the same problem in a very different manner, but uncover similar results. Marvell and Moody (1996) employ a Granger-causality approach. One variable is said to "Granger-cause" another if changes in the first variable are associated with future changes in a second variable. Thus, Granger causality does not reflect causality in the typical sense of the word, but rather a temporal relationship between changes in two variables.[2] Using panel data for both U.S. states and large cities, Marvell and Moody (1996) find strong evidence that increases in police lower crime with a lag.[3] The magnitude of the effects are comparable to Levitt (1997a). While not direct causal evidence, the analysis of Marvell and Moody is nonetheless quite compelling.

A recent study, Corman and Mocan (2000), adopts yet another approach to answering this question. Corman and Mocan (2000) note that the problem of distinguishing correlation and causality in studies of police and crime arises partly as an artifact of using annual data. Police hiring occurs only with a lag after a rise in crime (empirically, they find the lag to be approximately six months). Using annual data, however, a rise in crime in the beginning of the year can trigger an increase in police later in the year,

leading to spurious results. On the other hand, if one uses monthly data, a rise in crime will not trigger an increase in police immediately, allowing one to identify a short-run causal impact of police on crime. Corman and Mocan (2000) use monthly data for New York City covering the period 1970 through the 1990s. Their results suggest that a 10 percent increase in the size of the police force reduces crime by roughly 10 percent—the same magnitude as found in Levitt (1997a) and Marvell and Moody (1996).

Although each of the studies described above has limitations, it is remarkable that three papers employing such different approaches and data sets yield such consistent results. Together, these studies provide convincing evidence that increases in the number of police reduce crime.

Is the Decline in Crime Associated with More Police due to Deterrence?

The extent to which the decline in crime from more police is attributable to deterrence (as opposed to incapacitation, for instance) is an issue that has not been the subject of much research. If more police lead to more arrests and higher rates of imprisonment, then the impact of police need not be due to deterrence. Note, however, that the deterrence model has an ambiguous prediction, which may surprise the uninitiated, as to whether more police will lead to an increase or decrease in *arrests*. If criminals know that the probability of arrest for any particular offense has risen because of more police, fewer crimes should be committed. Thus, depending on the relative magnitude of those two effects (more arrests per crime, fewer crimes committed), the absolute number of arrests made may either rise or fall as the number of police rises.

The most compelling (albeit indirect) evidence to date on this issue comes from a clever study by McCormick and Tollison (1984), which uses a change in the number of referees in college basketball as a natural experiment for analyzing the response of behavior to increases in the probability of punishment. In this context, more referees correspond to more police, and the number of fouls called parallels the number of arrests made. McCormick and Tollison (1984) demonstrate that the switch from two to three referees per game is associated with a reduction in fouls called of over 30 percent. This result strongly suggests a deterrent effect in basketball, although it is unclear whether this result can be generalized to police.

Another study that attempts to disentangle the deterrence and incapacitation effects of arrests is Levitt (1998a). In general, it is difficult to do so because an increase in the expected punishment for a given crime, say

robbery, is associated with a reduction in that crime through both channels, making it difficult to distinguish the two effects. Levitt (1998a), however, points out that when the expected punishment for robbery rises, deterrence predicts an *increase* in crimes that are substitutes for robbery (like burglary), but incapacitation predicts burglary should *fall*, as long as criminals are generalists.[4] Thus, by looking at how changes in punishment for one crime affect other crimes, deterrence and incapacitation can be isolated. Levitt (1998a) finds that deterrence is more important than incapacitation in explaining the link between arrest rates and crime, particularly for less serious offenses.

EXPECTED PUNISHMENT AND CRIME RATES

It is critical to the deterrence hypothesis that longer prison sentences be associated with reductions in crime. Because of both conceptual and data issues regarding the measurement of expected sentences, much of the research to date, however, has relied on other, less explicit tests of the link between expected punishment and crime. In particular, the focus of research has been on the relationship between the overall size of the prison population and crime rates.

The early literature on the subject, which focused exclusively on time-series patterns in the data, emphasized the fact that in spite of enormous increases in the U.S. prison population in the United States since the 1970s, crime rates did not begin to systematically fall until the 1990s (for example, Zimring and Hawkins 1991). Based on this evidence, these authors argue that both deterrence and incapacitation effects must be inconsequential and call for a dramatic reduction in the prison population. These analyses, however, fail to distinguish correlation from causality.[5] Assuming that the expected punishment per crime remains constant, any increase in the crime rate (for example, due to changing demographics [Wilson and Herrnstein 1985], economic fluctuations [Freeman 1995], or a decline in stable families and communities [Wilson 1987]) will translate one-for-one into an increase in the prison population. There is an essentially mechanical relationship binding the crime rate to the prison population. Thus, it is not at all surprising that aggregate crime and imprisonment rates might be positively correlated in some time periods.

More recent research has been far more careful in analyzing this issue. Marvell and Moody (1994), utilizing a panel-data set of U.S. states, demonstrate that the positive relationship between imprisonment rates and crime disappears when looked at within a state over time, as opposed

to using cross-sectional data. Marvell and Moody (1994) estimate that a 10 percent increase in the size of the prison population reduces crime by about 1.5 percent.

While the research methodology employed in the preceding paragraph is superior to that of earlier studies, it goes only part of the way in isolating a causal link between prisons and crime. To more convincingly identify causality, a natural experiment approach has been utilized. Levitt (1996) uses prison-overcrowding litigation filed by the American Civil Liberties Union (ACLU) as an "exogenous" shock to the state prison population. In response to widespread prison overcrowding, the ACLU filed lawsuits against state prison systems on the basis of cruel and unusual punishment inflicted by the poor prison conditions. Years later, when these lawsuits had finally wound their way through the legal process, courts ordered these states to ameliorate prison overcrowding. This was often accomplished by releasing prisoners who otherwise would have been held. As Levitt (1996) demonstrates, crime rates jump sharply coincident with the release of prisoners in states affected by the lawsuits. The magnitude of the estimated relationship between the number of prisoners and crime is two to three times greater than that obtained by Marvell and Moody (1994).

The results above, while consistent with a deterrent effect of imprisonment, are extremely weak tests of deterrence because of the potential incapacitation effects of prisons (Chaiken and Chaiken 1982, DiIulio and Piehl 1991, Spelman 1994). The distinction between deterrence and incapacitation is of paramount importance in this context because of the differing implications that these two channels have for the effectiveness of policies such as "three strikes and you're out." Under such policies, repeat offenders are sentenced to extremely long prison terms designed to keep them off the streets permanently. If incapacitation is the operative factor, "three strikes" is not an efficient use of prison space. Eventually, prisons will be overflowing with aging inmates, most of whom no longer pose a threat to society as a result of the natural declining age crime profile (Blumstein et al. 1986). Greenwood et al. (1994), for instance, projected that annual prison expenditures in California would almost triple if "three strikes" were fully implemented. Such doomsday scenarios, however, completely ignore the role of deterrence. If deterrence works, then "three strikes" is an extremely attractive policy because the threat of punishment deters the potential criminal from committing the crimes in the first place. Since fewer crimes are committed, the prison population may actually decline when "three strikes" is implemented. The experience of California since the adoption of "three strikes" certainly appears to be more consistent with the predictions of deterrence than with the projections of Greenwood et al. (1994). Between 1994 and 1998, California's prison population grew at a rate only slightly above the national average (29 percent vs. 23 percent) and California's violent crime rate per capita fell 30 percent, compared to 20 percent for the rest of the nation.

Two further studies provide evidence for a deterrent effect of increases in expected punishment. Kessler and Levitt (1999) evaluate the impact of Proposition 8 in California, which instituted sentence enhancements for particular crimes, for example, crimes committed with a gun. Because those convicted of such crimes virtually always received prison sentences even without the sentence enhancements, Proposition 8 had no immediate effect on incapacitation. Only after the conventional prison term expires and the longer sentence due to the enhancement takes effect will the incapacitation effect begin to operate. Thus, any immediate decline in crime associated with passage of Proposition 8 must be due to deterrence, not incapacitation. Consistent with a deterrent effect, Kessler and Levitt (1999) find that crimes covered by Proposition 8 appear to fall immediately by about 4 percent relative to other states, whereas crimes not covered by Proposition 8 are unaffected. Crimes covered by the law continue to fall over time, suggesting that incapacitation is reducing crime once the sentence enhancements take hold.

A second study suggesting deterrence from increased punishment is Levitt (1998b), which looks at changing crime involvement with the transition from the juvenile to the adult criminal justice system. In those states in which the adult system is substantially more punitive than the juvenile system, criminal activity falls sharply when the age of majority is reached, whereas this is not the case in states where the juvenile and adult systems are similar in severity. The immediacy of the drop in crime when juveniles become subject to more severe adult sentences strongly suggests deterrence is at work, especially since juvenile records are sealed in most states so that criminals making the transition to the adult system start with clean records.

The studies discussed above attempt to infer the presence of deterrence based on observed criminal behavior. An alternative research strategy is to ask potential criminals about how the threat of punishment affects their behavior. This survey approach has the attractive feature of being quite direct. Survey responses, however, may not adequately reflect true behavior if criminals provide intentionally misleading answers, or if they are simply unaware of the reasons why they do or do not engage in certain patterns of behavior. By and large, survey responses are consistent with a

deterrent effect of punishment, although the relationship is sometimes weak and deterrence is typically not the most powerful explanatory variable (see, for example, Saltzman et al. 1982). As an example of how deterrence appears in these surveys, two-thirds of prisoners interviewed by Van Voorhis et al. (1997) say that they agree or strongly agree with the statement that being sent to prison has "taught them a lesson" and that they "have given up criminal activity forever." A slightly different approach is adopted by Glassner et al. (1983), which reports the findings of a series of interviews with adolescents in New York who self-report a dramatic reduction in criminal involvement at the age of majority, that is, the age at which they become subject to the punishments of the adult court. As one youth who recently turned sixteen (the age of majority in New York) says, "When you are a boy, you can be put into a detention home. But you can go to jail now. Jail ain't no place to go." Another adolescent, interviewed in a juvenile detention facility, saw his one-to-four year sentence as "easy. I'll just do my year and get out; it ain't nothing." But he advised his friend who continued his criminal activity to stop "because he's 16 now; he'll go to jail."

CAPITAL PUNISHMENT

The deterrent effect of capital punishment has been the subject of extensive, spirited academic debate for more than two decades.[6] On the surface, capital punishment would appear to be an ideal means of testing deterrence. Because the alternative to execution is likely to be life in prison without parole, incapacitation is not an issue. Whether the prisoner is executed or not, he is unlikely ever to be released from prison.

On further reflection, however, it is clear that the rarity with which executions occur and the long lag between the crime and the eventual execution suggest that a rational criminal's decision-making is unlikely to be greatly affected by the presence of the death penalty. In 1997, seventy-four prisoners were executed in the United States—the highest total in thirty years. At the end of 1997, there were 3,335 inmates under a sentence of death, meaning that only about 2 percent of those on death row were executed. The execution rate on death row is only slightly greater than the rate of accidental and violent death for black males between the ages of 15 and 34. Among the subsample of individuals engaged in illegal activities, the death rates outside of prison are likely to be at least as high. Levitt and Venkatesh (2000) report a death rate of 7 percent annually for street-level drug sellers in the gang they analyze. Kennedy, Piehl, and Braga (1996) estimate violent death rates to be 1–2 percent annually among all gang members in Boston. Based on these figures, it is hard to believe that in modern America the fear of execution would be a driving force in a rational criminal's calculus. Furthermore, given the high discount rates of many criminals (Wilson and Herrnstein 1985) and the fact that many homicides are committed by individuals under the influence of alcohol or drugs that further foreshorten time horizons, it is hard to believe that punishment with such a long delay would be effective.

Given the infrequency of executions, it is not surprising that empirical estimates of any deterrent effect are extremely sensitive to modeling assumptions. Much of the evidence arguing in favor of a deterrent effect of capital punishment is based on the work of Ehrlich (for example, Ehrlich 1975, Ehrlich 1977, Ehrlich and Liu 1999). Ehrlich (1975), using national time-series data for the period 1932–1970, estimates that between one and eight lives are saved per execution. These results, however, depend critically on the years of data included in the analysis. If the sample ends in the mid-1960s, or is extended to include later years, the results often disappear (Passell and Taylor 1977, Forst et al. 1978, Leamer 1983). The importance of the late 1960s to the results is particularly worrisome because that was a time of sharp increases in all types of crime. Presumably, the death penalty only has a deterrent effect on crimes that are punished by death, so it is not plausible that the rise in property crime, robbery, and aggravated assault can be attributed to declining use of the death penalty in the late 1960s.

Ehrlich (1977) uses cross-sectional state-level data for the year 1940 and for the year 1950 to obtain estimates of the deterrent effect of capital punishment that are similar to his time-series estimates. As was noted in the earlier discussion of police, heterogeneity across states is an important concern, particularly since the use of the death penalty is heavily concentrated in a small number of Southern states. Cameron (1994) summarizes a number of articles that question the findings of Ehrlich (1977). Perhaps the most interesting of these studies is Bailey (1982), which finds no impact of the death penalty on police killings. One might expect police killings to be the type of homicide that is most sensitive to capital punishment since the death penalty is much more likely to be invoked against cop killers than murderers more generally.

Panel-data estimates of the impact of executions also yield mixed results. Lott and Landes (1999), using data from the 1970s to the 1990s, report finding that a one percentage point increase in the probability of execution for those committing murder is associated with a 7 percent

reduction in the homicide rate. Katz et al. (2000), however, find that panel data estimates of the impact of the death penalty are very sensitive to the time period examined and the way in which one controls for unobserved differences across states and time periods.

Thus, it appears that the death penalty, at least as exercised in the United States, offers little concrete evidence either for or against deterrence.[7] Even if a substantial deterrent effect does exist, the amount of variation in crime rates induced by executions may simply be too small to be detected. Assuming a reduction of seven homicides per execution (a number consistent with Ehrlich 1975), observed levels of capital punishment would have a trivial impact on overall homicide rates. Even in Texas, the state that has executed far and away the most prisoners in recent years (a total of 144 executions between 1976 and 1997), executions would be predicted to have reduced the annual number of homicides in Texas by about fifty, or 2 percent of the overall rate. Given that the standard deviation in the annual number of homicides in Texas over this same time period is over 200, it is clearly a difficult challenge to extract the execution-related signal from the noise in homicide rates.

VICTIM PRECAUTION

Although the usual focus of deterrence is government expenditures on criminal justice, any action that raises the potential costs of committing crime would be predicted to have a deterrent effect. In the case of victim precautions, a critical distinction is required between actions that are or are not observable to the criminal prior to committing the criminal act (Clotfelter 1978, Shavell 1991). In the case of observable precautions, such as enclosing one's house with a fence, putting "The Club" on a steering wheel, or posting a "beware of dog" sign, criminals are likely to substitute other victims, for example, the house next door, for the protected targets. Such precautions may have "specific" deterrence (that is, protect the person who erects the fence), but may have little or no impact on aggregate crime because of substitution by criminals to other available victims. In stark contrast, unobservable victim precaution (for example, silent alarms or carrying concealed weapons) may have "general" deterrence since the criminal will not know in advance whether a particular victim is protected or not.

The possible deterrent effect of laws allowing citizens to carry concealed weapons has been the focus of heated debate in recent years. Lott and Mustard (1997) and Lott (1998) present panel-data evidence which argues for extremely large crime reductions associated with the passage of

such laws. For instance, Lott (1998) estimates that states that allow citizens to carry concealed weapons experience a 7.7 percent decline in homicide and a 5.3 percent decline in rape (although only a 2.2 percent decline in robbery—the crime one might expect to fall the most).

The results in Lott and Mustard (1997) and Lott (1998) have been strongly challenged by a series of critics. Ludwig (1998) notes that juveniles are never allowed to carry concealed weapons. Thus, in states that pass laws allowing adults to carrying concealed weapons, criminals should substitute away from adult victims toward juvenile victims. In fact, however, Ludwig demonstrates that juvenile victimizations actually fall more with the passage of such laws than does adult victimization. Black and Nagin (1998) and Duggan (2000) both find that the evidence in favor of concealed weapons is sensitive to the construction of the sample, the precise econometric approach used, and the way the law is coded in the analysis. Duggan (2000), for instance, demonstrates that the statistical significance of the Lott and Mustard (1996) estimates disappears when one takes into account that data from counties within a state are not independent of one another as Lott and Mustard (1997) and Lott (1998) implicitly assume in their analysis. Ayres and Donohue (1999) find that extending the original sample, which ended in 1992, to include more recent data reverses the results: The passage of laws allowing for the carrying of concealed weapons appears to be associated with increasing rather than declining crime.

Unfortunately, outside of concealed weapons, there has been little empirical research on the issue of victim precaution. One exception is Ayres and Levitt (1998), which analyzes a different form of unobservable victim precaution: an anti-theft radio transmitter known as "Lojack." This device, hidden inside a vehicle, allows specially equipped police vehicles to remotely track a vehicle that has been stolen. There is no indication anywhere on the vehicle that Lojack has been installed, making it a prototypical example of unobservable victim precaution. Ayres and Levitt (1998) find that auto theft falls sharply when Lojack is introduced into a city, consistent with deterrence. The key to Lojack's effectiveness is attributable to the fact that Lojack leads police directly to "chop shops," where large numbers of stolen vehicles are stripped for parts. Without Lojack, finding such chop shops is extremely difficult for police. Other crimes do not appear to be strongly affected by Lojack, suggesting that auto thieves are not substituting into other crimes in large numbers. Nor is there evidence that the auto theft is being displaced geographically. If the estimates in the paper are correct, the number of cars equipped with Lojack is far

below the social optimum—little of the benefit of deterrence accrues to the owner of the actual vehicle that has Lojack installed (the car may still be stolen because Lojack is unobservable). Instead the benefits are dispersed across all car owners.

CONCLUSION

Empirical evidence from a wide range of settings is consistent with the presence of a substantial deterrent effect. There is evidence that increases in police, the number of prisoners, sentence lengths, and victim precaution are all associated with reductions in crime through a deterrence channel. Deterrence alone, however, cannot adequately explain the differences across place and time in crime rates. Given the most reliable estimates of the impact of the criminal justice system on crime rates and the fraction of that impact that is attributable to deterrence, it seems unlikely that any more than 25 percent of the observed fluctuations in crime rates either across geographic areas or over time is due to deterrence. Thus, while deterrence is an important consideration in the development of public policy, it appears that other factors—family, community, demographics, and so on—are at least as influential. Nonetheless, to the extent that deterrence is a factor that can be readily influenced by public policy through changes in the criminal justice system, it may represent the quickest and most efficient way for government to influence criminal activity in the short run.

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Prisons: A Policy Challenge

Alfred Blumstein

ESCALATING PRISON POPULATIONS

In a precursor to this volume, I began my essay[1] on prisons with the observation that "the most critical administrative problem facing the United States criminal justice system through the 1980s will be that of crowded prisons. Pressure will continue to mount for more and harsher prison sentences, seriously straining the already limited capacity of penal institutions."

That essay from which I quoted was written in 1981, and the period of the 1980s—and continuing into the 1990s—certainly bore out that forecast. That impact is displayed most vividly in Figure 16.1, which depicts the United States' incarceration rate (prisoners per capita) from 1924 to 1999. The fifty-year period from the early 1920s to the early 1970s was characterized by an impressively stable incarceration rate averaging 110 per 100,000 of general population (or 0.11 percent of the population) in prison at any time, with a coefficient of variation (the standard deviation of the series of annual observations divided by the mean of the series) of only 8 percent.

Indeed, that stability through a period that included such disruptive events as the Great Depression (when incarceration rates increased somewhat) as well as World War II (when the nation had greater needs for men of imprisonment age, and perhaps even better means for controlling them) was so striking (and was consistent with similar stability in a number of other countries) that it suggested a homeostatic process leading to this stability.[2]

Especially in light of what has happened more recently, the homeostatic process proposed seems to have been a reasonable characterization of that period, when incarceration policy was largely within the control of functionaries within the criminal justice system: When prisons got too