The Deterrent Effect of Imprisonment

Steven N. Durlauf
University of Wisconsin

Daniel S. Nagin
Carnegie Mellon University

12/22/09

1. Introduction

This essay is designed to provide an overview of the state of knowledge on the deterrent effects of imprisonment. Much of what we say constitutes a selective synthesis of existing research. At the same time, we provide some general critiques of the state of knowledge on imprisonment and deterrence and identify some implications for policy.

Our reading of the current empirical literature is that there is overwhelming evidence of substantial deterrent effects across a range of contexts. Therefore, a well-balanced crime-control portfolio must necessarily include deterrence-based policies. Yet deterrent effects depend on the specific form of the sanction policy. In particular, there is little evidence that increases in the severity of punishment yield strong deterrent effects; further, credible arguments can be advanced that current levels of severity cannot be justified by their social and economic costs and benefits. By contrast there is very substantial evidence that certainty of punishment has a large deterrent effect. In this regard the most important set of actors are the police since, in the absence of detection and apprehension, there is of course no possibility of conviction or punishment. Studies of the relationship between crime rates and various indicators of police presence consistently find that greater police presence is associated with reduced crime, presumably because of the threat they pose of apprehension. Thus, one policy relevant implication of this conclusion is that lengthy prison sentences, particularly in the form of mandatory minimum type statutes such as California's Three Strikes Law, are difficult to justify on a deterrence-based crime prevention basis. Another key policy implication of our review is that crime rates can likely be reduced without an increase in the resource commitment to crime control; such a reduction may be achieved by shifting resources from incarceration via reducing sentence severity and shifting these resources to policing. These conclusions, to be clear are tentative, and we will discuss why firm claims of this form are difficult.

Our review also has suggestions for the importance of generalizing the economic model of crime in a number of directions; in particular we address psychological and sociological aspects of criminal behavior whose integration into the standard economic crime model would, in our view, enhance its explanatory power.

The essay is organized as follows: We begin by laying out what we refer to as the "baseline" economic model of crime due to Gary Becker. We use it to provide a framework for our discussion. We then turn to a review of the literature and our interpretation of implications. We close with discussion of policy implications and directions for future research including expansion of the baseline model.

2. The Economic Model of Crime

In order to provide a conceptual framework for our discussion, we employ the economic model of crime pioneered by Gary Becker (1968). Becker's analysis of crime, particularly at the time of its publication, is a fundamental theoretical contribution because it conceptualizes the commission of a crime as a purposeful choice, one that reflects a comparison of costs and benefits. While Becker's formulation, as well as subsequent "rational choice" crime models describe individual choices by way of particular formulations of a potential criminal's beliefs, preferences, and constraints, it is the notion of crime as a choice that is an irreducible requirement of the Becker perspective. Much of the criticism of Becker's model amounts to criticisms of the ways in which the crime choice is delineated; in fact we will argue that what appear to be limitations of the economic approach to crime are in fact remedied by alternative formulations of the same choice-based logic.

A very simple variant of the Becker model may be thought of as follows. Denote individuals by i and distinguish heterogeneity across them by the vector Z_i. Each individual faces a binary choice as to whether or not commit a crime, i.e. a choice between C and NC. If the criminal commits a crime, there is a probability p of being caught and punished. This means that a potential criminal will, depending on his choice, experience one of three utility levels: the utility of not committing a crime, $U_{NC}(Z_i)$, the utility of committing a crime and being punished, $U_{C,P}(Z_i)$ and the utility of committing a crime and not being punished, $U_{C,NP}(Z_i)$. Individual i chooses to commit a crime if the expected utility from commission of a crime exceeds the utility from not committing a crime. A crime is therefore committed if

$$pU_{C,P}(Z_i)+(1-p)U_{C,NP}(Z_i)>U_{NC}(Z_i) \tag{1}$$

From the perspective of criminal sanctions, this elementary calculation highlights the two distinct aspects of crime sanction policy that should be the focus on scholarly research: p_i, the probability of being punished, and $U_{C,P}(Z_i)-U_{C,NP}(Z_i)$, which will depend upon (among other factors) the nature of the punishment. Suppose that the nature of the punishment is summarized by length of imprisonment; assuming this is the only source of the utility loss in being caught, one can simplify the analysis by treating the utility of crime as $U_C(Z_i,l)$ where l denotes the length of the sentence served having committed the crime. This allows us to rewrite the condition for commission of a crime as

$$p\left(U_C(Z_i,L)-U_{NC}(Z_i)\right)+(1-p)\left(U_C(Z_i,0)-U_{NC}(Z_i)\right)>0 \tag{2}$$

From this perspective, commission of a crime is analogous to the purchase of a lottery ticket. The distribution of the heterogeneity Z_i induces an equilibrium aggregate crime rate. Letting

$$\omega(Z_i) = 1 \text{ if (2) holds; 0 otherwise} \tag{3}$$

and dF_z denote the cross-population probability density of the heterogeneity Z in the population, the aggregate crime rate $\Pr(C|p,L)$ is defined by

$$\Pr(C|p,L) = \int \omega(Z) dF_Z \tag{4}$$

For this simple model, the decision problem facing a policymaker is the choice of a sanction regime, which is described by the pair (p,L). Formally, a policymaker assesses the benefits of a given policy via some function of the crime rate

$$\phi\left(\Pr(C|p,L)\right) \tag{5}$$

In turn, the cost of the policy pair may be represented as a function

$$\lambda(p) + \mu(I) \tag{6}$$

where the variable I, defined as

$$I = \Pr\left(C \middle| p, L\right) pL \tag{7}$$

is the expected per capita imprisonment rate in the population. In equation (6), the overall cost of the sanction regime, $\lambda(p)$ captures the cost of law enforcement needed to achieve a particular apprehension rate for crimes while $\mu(I)$ captures costs of incarceration. Additivity of the two types of costs seems a natural first-order approximation since it distinguishes between police activity and prison activity.

How should a policymaker choose among possible (p, L) pairs? Rather than solve for the optimal pair, it is more insightful to solve for the conditionally optimal levels of p and L under the constraint that the product pL is constant. Since pL equals the expected sentence length for a criminal who is caught, our calculation provides a clean way of interpreting the respective roles of certainty of punishment and severity of punishment in influencing the individual crime decisions and hence the aggregate crime rate when the expected sentence length is fixed. Suppose that $U_C(Z_i, L)$ is a convex function of L, i.e. the marginal disutility of a marginal change in sentence length is increasing in the level of the sentence. This increasing marginal disutility of sentence length is equivalent to assuming that a potential criminal is risk averse with respect to the sentence "lottery". Just as a risk averse agent facing a bet with expected value X and probability of winning P will prefer bets with higher P to lower P, for a fixed level of the product pL, i.e. a constant expected sentence length, the expected utility of crime commission will be decreasing in L. Further, notice that a lower p reduces policing costs $\lambda(p)$, and must also reduce prison costs as it minimizes $\Pr\left(C \middle| p, L\right)$ given constant pL and hence minimizes $\mu(I)$. This is the basis of

Becker's conclusion that optimal sanction policy leads to relatively low punishment probabilities and long sentences. In referring to this model as a baseline, we mean that there are a number of assumptions associated with the model that can be relaxed while preserving the choice-based logic. While alterations in the assumptions may change conclusions concerning the relationship between certainty and severity, they do so by using the same choice-based reasoning pioneered by Becker.

3. Empirics

There have been two distinct waves of studies of the deterrent effect of imprisonment. The first wave was conducted in the 1960s and 1970s. The best known study, conducted by Ehrlich (1973), examined the relationship of state-wide crime rates to the certainty of punishment, measured by the ratio of prison admissions to reported crimes, and the severity of punishment as measured by median time served. Ehrlich, however, was not alone in employing this or closely related methods for measuring the certainty and severity of punishment (cf. Gibbs, 1968; Sjoquist, 1973; Forst, 1976). These studies consistently found that certainty was inversely related to crime rate which was interpreted as a deterrent effect. By contrast, the severity measure was generally not systematically related to crime rate which was interpreted as indicating that severity was not an effective deterrent.

These studies suffered from a number of serious statistical flaws that are detailed in Nagin (1978) and Fisher and Nagin (1978). The two most important problems involved endogeneity and measurement error. This generation of studies typically failed to account for the endogenous relationship between crime rates and sanction levels predicted by Becker's model. Alternatively, those that attempted to account for endogeneity used implausible identification restrictions to

parse out the deterrent effect of sanction levels on crime rates from the effect of crime rates on sanction levels. (papers in this literature, for example, assumed that demographic or socio-economic characteristics such as percentage of males aged 14-24 or mean years of schooling of persons over 25 or per capita public safety expenditures lagged one year causally affected sanction levels but did not causally affect crime rates). These are examples of what Sims (1980) dubbed "incredible" identifying assumptions and are now fully understood to be a fundamental limitation to causal claims both in criminology and in social science more generally. The second problem arose from measurement error in crime counts, of which there are many sources. It can be shown that these errors can artificially induce a negative correlation between the crime rate and the certainty of punishment because the measured level of crimes form the numerator of crime rate, i.e. crimes per capita, and the denominator of the measure of certainty of punishment, prison admissions per crime (Nagin, 1978).

In response to these deficiencies, a second generation of studies emerged in the 1990s. This new group of studies also used states as the unit of observation but unlike the first-generation studies which primarily involved cross-sectional analyses of states, this second-generation of studies had a longitudinal component in which data were analyzed not only across states but also across time. The panel structure of these studies allowed for the introduction of state and time specific fixed effects and the use of various differencing strategies to control for some forms of unobserved heterogeneity. Another important difference is that this wave of studies did not attempt to estimate certainty and severity effects separately. Instead they examined the relationship between the crime rate and rate of imprisonment as measured by prisoners per capita. A still more recent wave of studies has returned to the study of certainty and severity effects in that its focus is on particular statutory changes in criminal penalties (severity) or abrupt changes in the level of police presence arising from events such as terror alerts (certainty). Some of these studies may also be

distinguished from the first generation by their use of quasi- or natural experiments to uncover deterrence effects.

In organizing our survey of the state of the literature, we first consider studies that have attempted to link aggregate crime and imprisonment rates and second consider studies that have considered the effects of criminal sanction policy on crime.

i. Aggregate Studies Relating Imprisonment Rate to the Crime Rate

An important recent review by Donohue (2007) identifies six studies that examined the relationship between crime rates and imprisonment rates. Each of the studies in this review finds statistically significant negative associations between imprisonment rates and crime rates, and have been interpreted as implying a crime prevention effect of imprisonment. However, the magnitude of estimates of the parameter varied widely; from nil at current levels of incarceration for a study that allowed for the possibility of diminishing returns (Liedka et al. 2006), to an elasticity of –0.4 (Spelman, 2000).[1] The highest elasticity estimate is –0.4 (Spelman, 2000). Like all other studies except Liedka et al. (2006), Spelman's estimate is based on a model in which the parameter relating the imprisonment rate to the crime rate does not depend on the level of imprisonment in the population as a whole. It is important to note that these studies are actually measuring a combination of deterrent and incapacitation effects. Thus, it is impossible to decipher the degree to which crime prevention is occurring because of a behavioral response by the population at large or because of the physical isolation of crime-prone people.

[1]Liedka et al., (2006) attempt to explicitly allow for the possibility that the effect of imprisonment on crime rate depends upon the scale of imprisonment. They do this by regressing crime rate on quadratic and spline functions of lagged imprisonment rate. Their analysis implies that by the 1990's the preventive effect of imprisonment in some states (e.g., California) had diminished to a negligible level and perhaps was even criminogenic.

Donohue (2007), in the context of generating a cost-benefit analysis of imprisonment discusses the heterogeneity of elasticity estimates. He argues that the lower range of the estimates, -.15 to -.20, are most plausible, but concedes that this judgment is highly uncertain. He favors the lower range estimates on two grounds. First, while the majority of prisoners are confined in state prisons, it is only a near majority. In 2004, for example, 42% of the incarcerated population were confined in federal prisons and local jails. If, as one would expect, federal and jail inmate populations are negatively correlated with crime rates and positively correlated with state prison populations, the exclusion of the federal and jail imprisonment rates from the regression will cause an over statement of the magnitude of the crime prevention effect of the state level imprisonment rate. Second, Donohue is sympathetic with the arguments of Liedka, Phiehl, and Useem (2006) that the parameter relating the imprisonment rate to the crime rate is not constant but instead declines in absolute magnitude with the scale of imprisonment. As an empirical matter, he points out that this conclusion is not only consistent with the findings of Liedka et al. but also is mirrored in parameter estimates based on constant coefficient models in which the parameter estimates decline as data from more recent years are added to the analysis; these more recent data involve higher imprisonment rates and so implicitly (if parameters are not constant) would intuitively suggest the reduction of estimated parameter that is observed.

While the literature relating crime rates to imprisonment rates has served the valuable purpose of resuscitating interest in the crime prevention effects of imprisonment, we are less sanguine about the usefulness of this body literature than Donohue. Our more critical stance stems from both statistical and theoretical considerations. Five of the six analyses are based on the application of time series analyses that in essence look for contemporaneous and dynamic correlations between the levels of crime rates and imprisonment rates (or on changes in the two

series). Unfortunately, any claims that these correlations imply a counterfactual-based causal relationship between imprisonment rates and crime rates are, in our judgment, not valid.

To see why these studies are not informative about the presence (or absence) of a causal mechanism that links imprisonment policy to crime, we focus on Marvell and Moody (1994); we single out this study because it has been quite influential. Marvell and Moody in essence establish two facts about the time series for imprisonment and crime. First, they establish that imprisonment levels Granger cause crime levels. This finding has no logical bearing on whether changes in imprisonment policies will alter crime rates. The term "causality" has a different meaning in the phrase Granger causality than in causality as understood in microeconometrics, and elsewhere. Granger causality simply means that lagged imprisonment levels help forecast current crime levels, even when lagged crime rates have been accounted for. This marginal utility in forecasting has no counterfactual implications, which is the key idea in the notion of causality that is relevant to understanding policy effects. Second, moving from levels to first differences, Marvell and Moody, for a panel of states, regress changes in crime rates against changes in the contemporary imprisonment rate and some additional controls. This regression does not, under any interpretation of causality of which we are aware, provide a policy relevant measure of the effects of imprisonment. Here the problem is simply that changes in contemporary imprisonment and crime rates are simultaneously determined, and so the Marvell and Moody finding makes no advance over the first generation of studies in terms of dealing with endogeneity. This type of criticism, in fact applies to any of the time series studies as the presence of correlated unobservables that simultaneously affect crime and imprisonment will lead to spurious dynamic correlations, a problem that is exacerbated by the fact the criminal justice system creates simultaneity between the two series (Fisher and Nagin, 1978).

Only one study, Levitt (1996), addresses the endogeneity problem, doing so via the use of instrumental variables. Levitt employs court orders requiring reductions in prison populations as an instrument, reasoning that such orders will cause a reduction in the imprisonment rate that is unrelated to the endogeneity of the imprisonment rate. He goes on to argue that crime rates will only be affected through the court order's effect on imprisonment rates. Levitt's identification has been challenged on a number of grounds. Klick and Taborrok (forthcoming) argue that Levitt's tests of over identifying restrictions have low power, so that the validity of the instruments can only be assessed on a priori grounds.[2] Liedka et al. (2006) challenge Levitt's identification on the grounds that the court orders themselves are endogenous because prison overcrowding is itself a function of the crime rate. We also note that it is plausible that criminals in fact responded directly to the litigation as signaling a reduction in either the certainty or severity of punishment or both.

However, our primary concern with the Levitt analysis involves the question of what the findings tell us about the ability to use sanction policy to affect crime rates. There is an important distinction between a policy that forces the release of a set of current prisoners as opposed to one that alters the population via different sanctions policies Nagin (1998). It is possible that the imprisonment reductions induced by court orders were not rationally responded to by the freeing of those prisoners whose recidivism probabilities were especially low. What Levitt establishes is that exogenous court orders to reduce imprisonment levels appear to lead to short term increases in crime rates. This is not equivalent to establishing that changes in p or L will affect crime rates, which brings us to our more general reservations about the literature relating imprisonment rates to crime rates.

[2]Levitt's analysis is based on multiple forms of the overcrowding instrument that reflect the stage to which the overcrowding litigation had progressed. This allows for tests of over identifying restrictions. These tests support his contention that the overcrowding litigation has no direct effect on crime rate but only work through the level of imprisonment. Of course, this lack of power has no bearing on the logical validity of Levitt's instruments.

Beyond the specific issue of the handling of endogeneity, a number of fundamental criticisms may be raised concerning the literature relating aggregate crime rates to imprisonment rates. In our judgment this style of research suffers from two important conceptual flaws that limit its usefulness in devising crime control policy.

First, this literature generally ignores the fact that prison population is not a policy variable, but rather is an equilibrium outcome of sanction policies dictating who goes to prison and for how long. Changes in the size of prison populations can only be achieved by changing policies affecting the imprisonment/no imprisonment outcome or the length of incarcerations for those sent to prison. As discussed in Section 2, all incentive based theories of criminal behavior, including most importantly Becker's model, are posed in terms of the certainty (p) and the severity (L) of punishment not in terms of the imprisonment rate, I. The policy relevant variables p and L are not the control variables that are directly employed in the crime and imprisonment studies. Put generally, the imprisonment regression literature are not grounded in microeconomic theory in the way that makes clear the distinction between exogenous and endogenous variables; by implication the way in which endogenous and exogenous variables are interrelated is not specified. As a result, the statistical crime/imprisonment models that are typically estimated are not amenable to counterfactual analysis of the type needed for policy comparison since they do not represent instantiations of the aggregate consequences of individual decisions. This is not a minor conceptual quibble; it lies at the heart of the modern approach to policy evaluation, e.g. Heckman (2000, 2005) who has famously (and we believe absolutely correctly) remarked that "causality is a property of a model"[3].[4]

[3]This issue is well understood by philosophers and is known as the Duhem-Quine thesis; see Quine (1951) for the classic formulation. Judgment is intrinsic to the scientific enterprise and for our purposes, judgments about how to model criminal decisionmaking.

This problem is evident when one considers the limits on the informational content in I with respect to the policy choices p and L. Given that p and L may be thought of as distinct aspects of the lottery associated with commission of a crime, one obvious problem is even efforts to use instruments to account for the endogeneity of I cannot uncover the respective roles of the policy variables. Further, there is no guarantee that there exists a monotonic relationship between I and the policy choices. The lack of such a unique relationship which was first demonstrated in Blumstein and Nagin (1978) extends to the theoretical indeterminacy of the sign of the derivative of of I with respect to a change in either p or L. If the elasticity of $\Pr(C|p,L)$ with respect to p or L is less than 1, an increase in either of these variables will result in an increase in prison population whereas if the elasticity is greater than 1 an increase will result in a reduced prison population. This is analogous to the *J*-curve effect in international trade. The indeterminancy in the sign of the relationship between p, L and I implies the possibility of a Laffer curve-style relationship between sanction variables and the imprisonment rate. If there were no sanction threat there would be no one in prison even though crime rates would be very high. Alternatively, if sanctions could in practice be made sufficiently severe and certain, there would again be nobody in prison because everyone would be deterred. We return to the policy implications of the possibly "inverted U" relationship between imprisonment rate and crime rate in the discussion of policy implications and future research.

---

[4]A deterrence skeptic might counter that our criticisms do not apply if the estimates are interpreted as solely measuring incapacitation effects. This argument cannot be sustained. The point that Granger causality tests do not have a counterfactual interpretation still applies. Further, the magnitude of the incapacitation effect depends upon the mean rate of offending of the incarcerated population which in turn will depends on the types of criminals a policy regime incarcerates. For example, policies resulting in the incarceration of aged criminals likely have small incapacitation effects.

Second, we observe that all of the statistical models of crime we have discussed suffer from the problem of ad hoc model specifications. Focusing again on Marvell and Moody by way of providing a concrete example but not to single them out, their crime rate/imprisonment rate regression includes variables for the proportions of the population in different age groups, year fixed effects, and the first lagged value of the crime rate. No principled basis is given for this choice of variables. It is odd, for example to ignore state level economic conditions since these are natural proxies for a indivdual's opportunities if he chooses not to commit a crime. To be clear, Marvell and Moody are hardly alone in making arbitrary variable choices.. The basic problem is what Brock and Durlauf (2001a) have called theory opendendness: in the imprisonment case, theory opendendness means that the prediction that criminal sanctions affect crime rates is consistent with other theories of criminality, so so that empirical evidence of the importance of one explanation can only be assessed when explicitly assessed against the full background of competing explanations. For our context, some of these explanations have to do with the opportunity cost of crime; one example is the state of the economy in which a potential criminal resides, which naturally is informative about his individual prospects in the (legal) labor market. Others involve the composition of the population in a locality; while Marvell and Moody focus on age, one could just have easily focused on more subtle descriptions of the characteristics of the population in a state or other locality that account for gender as well as age shares. We emphasize that this is not a cynical suggestion in the sense that we are arguing that an empirical findings must be evaluated against every variable that enters a researcher's imagination. Judgments are inevitable in empirical work, we are only arguing that the extent they are necessary can be delimited.

Ad hocness occurs for reasons beyond questions of control variables. A second problem concerns the nature of the time series under study. This is evident, at one level, in the choice of the model of time trend made in various empirical studies. According to which paper one reads,

one finds the use of linear trends, quadratic trends or perhaps more sophisticated spline approaches. The choice of time trend has been shown to matter in the shall issue concealed weapons contexts in that Black and Nagin's (1998) use of quadratic trends reduced the evidence of a crime effect from shall issue laws versus the use of a linear trend by Lott and Mustard (1997). As far as we know, there does not exist *any* theory as to the appropriate formulation of trends in crime regressions. These trends are not formulated as ways to capture population growth or technological change (goals which for theoretical and empirical reasons motivate the use of linear deterministic trends or unit roots in macroeconomics) but rather are included because the presence of persistence in the model's residuals, i.e. the presence of some set of temporally dependent unobservables that the regressions under study cannot explain and for which there is no behavioral theory that has implications for the form of the dependence. Further, conditional on the choice of trend, the data are typically assumed to be stationary in either levels or first differences. We do not see how assumptions of stationarity can be justified in light of the lack of any understanding of the nature of and, hence, lack of any justification for the deterministic model of crime trends that is employed. To give a concrete example, the Mariel boatlift is known to have induced first order changes in the crime and imprisonment rates for Florida (Black and Nagin 1998). Such an event should affect the dynamic correlation structure between crime rates and variables of interest, beyond simply introducing a correlated unobservable.

A final source of ad hocness concerns parameter heterogeneity. It is typical in crime imprisonment studies to assume constant coefficients across states; parameter heterogeneity may be allowed via state-specific fixed effects, but other parameters, most importantly those linking imprisonment to crime, are assumed to be homogeneous across states. This assumption strikes us as problematic, although to be fair we have not verified that existing claims break down when the assumption is relaxed. However, as it is known, for example, that measures of the deterrent effect

of capital punishment sensitively depend on whether Texas and California are treated as having the same parameters as the rest of the United States (Dezhbakhsh, Rubin and Shepherd (2003) versus Donohue and Wolfers (2005)) and that inclusion of the state of Florida affects conclusions about shall issue concealed weapons laws (Lott and Mustard (1997 versus Black and Nagin (1998))[5]. These examples call into question the validity of cross-state studies of imprisonment. Conceptually, the problem is that states represent complex heterogeneous objects whose associated data do not naturally lend themselves to interpretations as draws from a common data generating process. One can make parallel arguments concerning the assumption of parameter constancy, i.e. nonlinearities rarely systematically examined, with Liedka et al (2006) representing an outlier.

One response to the ad hocness of model specifications is that criminal sanction policies can only be understood via quasi-randomized experiments. This is the position taken in Horowitz (2004); while his focus is on shall issue concealed weapons laws, his logic applies to crime policy in general and imprisonment policy in particular. Our view is that the randomized experiments approach is valuable, but is best treated as complementary to other studies. One reason why we see value to regression studies using observational data is that the sensitivity of statistical studies to model specification can be assessed both through sensitivity analyses and through model averaging methods (eg. Raftery. Madigan and Hoeting (1997)) that can provide ways to evaluate the robustness of a given empirical finding. Put differently, we concur with Horowitz that regression studies of criminal policy effects should be viewed with skepticism because of the many auxiliary assumptions made in formulating estimates of policy effects; in contrast, we believe the appropriate response to this problem is to explore policy effects across model spaces that are rich enough to span those assumptions the analyst deems reasonable. See Durlauf, Navarro, and Rivers (2008a)

[5]It is also now well understood that the failure to account for parameter heterogeneity can lead to misleading conclusions in cross country growth studies (see Durlauf, Johnson, and Temple (2005) for a review of relevant studies).

for more conceptual discussion and Cohen-Cole, Durlauf, Fagan, and Nagin (2008)) for an example of how one can constructively proceed. We believe the sensitivity and model averaging methods can move criminological research beyond the often vituperative debates one sees, e.g. Lott's (1998) response to Black and Nagin (1998) and Dezhbakshsh and Rubin's (2007) response to Donohue and Wolfers (2005), in which resolution is not achieved because of the failure to employ methods that integrate the model uncertainty implied by differences in assumptions across studies. At the same time, we are sympathetic to concerns that the virtues of randomized experiments have been exaggerated; see Deaton (2009) for a wideranging criticism in the context of development economics. One limitation of many randomized studies concerns general equilibrium effects. An example of this arises in the Klick and Tabarrok (2005) finding that increased police presence during terror alerts is associated with lower crime. Their finding cannot be extrapolated to a claim about the effects of a constant increase in police presence since one does not know to what extent criminals are merely adjusting the timing of activity. Further, even setting aside ethical considerations, some policies may not be amenable to experimental analysis; capital punishment is an example as it is already sufficiently freakish that any effort to randomize its use would make firm inferences impossible[6].

Studies of the effects of imprisonment exist against a background of large sustained increases in imprisonment. Blumstein and Beck (1999) and Raphael and Stoll (2007) have closely scrutinized the primary sources of the increases in imprisonment over the past four decades. Both reviews conclude that increased crime was not a major contributing factor to the five-fold increase in imprisonment rates since the early 1970's. Instead, both reviews attribute the growth in prison populations to increased punitiveness. Blumstein and Beck (1999) decompose the sources of the

[6]It is a distinct question whether capital punishment is sufficiently freakish to render regression analysis useless as well; see Donohue and Wolfers (2005) for arguments along this line.

increase in the imprisonment rate between the extensive margin, the probability of commitment given arrest, and the intensive margin, time served. The relative contribution of these two components varies by crime type. For the crimes of murder, robbery, assault, burglary, and sexual assault combined about 50% of the increase in imprisonment was attributable to an increase in the probability of imprisonment given arrest and 50% was attributable to increased time served. This background illustrates the import of our general concerns about treating imprisonment as the scalar determinant of a criminal sanctions / crime link. We therefore turn to studies that examine particular mechanisms by which the criminal sanction regime affects crime rates.

ii. Studies of the Effects of Severity of Punishment

The literature on the deterrent effect of the obvious form of severity, prison sentence length, is surprisingly small. The earliest post 1970's attempts to measure severity effects analyzed the deterrent impact of sentence enhancement for gun crimes. A series of studies conducted by Loftin, McDowell, and colleagues (Loftin and McDowell, 1981; Loftin, Heimann and McDowell, 1983; Loftin and McDowell, 1984) examine whether sentence enhancements for gun use in committing another type of crime such as robbery deter gun use in the commission of crime. While their findings are mixed, they generally point to no evidence of a deterrent effect. The generally null findings may reflect that gun using criminals did not respond to the incremental increase in severity. However, Loftin, McDowell, and colleagues also found that these laws were not effective in increasing the sentences actually received in gun-related crime prosecutions. Thus, gun using criminals may not have responded because the real incentives were not actually changed.

A large number of studies have examined the deterrent effect of California's "Three Strikes and You're Out" Law, enacted in 1992, which mandated a minimum sentence of 25 years upon conviction for a third strikeable offense. Zimring et al. (2001) conclude that the law at most reduced the felony crime rate by 2%. Only those individuals with two strikeable offenses showed any indication of reduced offending. The analysis was based on a variety of empirical comparisons designed to detect whether there was any evidence of a discontinuous decline in offending following the effective date of the statute (March, 1994) or whether there was a reduction in the proportion of crimes committed by the targeted groups, individuals with convictions for strikeable offenses. They found no indication of a drop in crime rate following enactment that could be attributable to the statute but did find some indication in reduced offending among individuals with two strikeable offenses. Other studies by Stolzenberg and D'Alessio (1997) and Greenwood and Hawken (2002) also examine before and after trends and find similarly small crime prevention effects.

The Zimring et al (2001) finding of a potential deterrent effect among individuals with two strikeable offense accords with the results of Helland and Tabarrok (2007). This analysis focuses exclusively on the whether the law deterred offending among individuals previously convicted of strike-eligible offenses. Helland and Tabarrok compare the future offending of individuals convicted of two previous strikeable offenses with that of individuals who had been convicted of only one strikeable offense but who, in addition, had been tried for a second strikeable offense but were ultimately convicted of a non-strikeable offense. The study demonstrates that these two groups of individuals were comparable on many characteristics such as age, race, and time in prison. Even so, it finds that arrest rates were about 20 percent lower for the group with convictions for two strikeable offenses. The authors attribute this reduction to the greatly enhanced sentence that would have accompanied conviction for a third strikeable offense.

As is standard is studies of this type, the interpretation of their findings in terms of the marginal deterrence effects of the three strikes law is contingent on the comparability of two groups who under study. There are reasons why unobserved heterogeneity may be present; for example those individuals who were convicted of a second nonstrikeable offense may have had better legal representation than those that were convicted of a second strikeable offense. In such a case, the incentives for further crime commission may differ for reasons outside the penalty differential.[7] Still in our judgment the Helland and Tabarrok analysis is very convincing as our view (which is admittedly a prior and not posterior belief) is that differences in the outcomes exploited by Helland and Tabarrok is most likely due to unobserved heterogeneity in the cases themselves

Helland and Tabarrok also conduct a cost-benefit analysis and conclude that the crime reduction benefits likely fall far short of the cost of the prison enhancement, twenty years or more. They go on to point out that a comparable investment in policing which primarily affects the certainty of punishment are likely to yield far larger crime reduction benefits. We return to this observation below.[8]

Kessler and Levitt (1999) examine the deterrent impact of another California sentence enhancement law, Proposition 8 passed in 1982. Proposition 8 anticipates the "Three-Strikes" type laws passed by many states in the 1990's. Their aim was to distinguish deterrent effects from incapacitation effects. Most state criminal statutes provide for a sentence enhancement for repeat offenders. Proposition 8 increased the severity of those enhancements and mandated their application. Kessler and Levitt argue that prior to enactment of Proposition 8 repeat offenders

[7]We thank Philip Cook for this observation.

[8]Shepherd (2005) also found crime prevention effects of California's Three-Strikes law, mostly from a reduction in burglaries. The aim of the analysis was to estimate the total deterrent effect of the law as reflected in the article's title "Fear of the First Strike..." The validity of the findings are difficult to judge because the statistical analysis rests on many fragile assumptions, for example that police and court expenditures are independent of the crime rate.

covered by the Proposition were still sentenced to prison, just not for as long. Thus, any short term drop in crime rate should be attributed to deterrence rather than incapacitation. They estimate a 4% decline in crime attributable to deterrence in the first year after enactment. Within 5 to 7 seven years the effect grows to a 20% reduction. The longer term estimate includes incapacitation effects. Indeed Kessler and Levitt acknowledge that the incapacitation effect may dominate the deterrent effect.

We also note that Webster, Doob, and Zimring (2006) challenge basic finding of any preventive effects. Kessler and Levitt examine only data from every other year. When all annual data is used, Webster et al. (2006) find that the decline in crime rates in the effected categories begins before Proposition 8's enactment, and the slope of this trend remains constant through implementation. But see Levitt (2006) for a response and commentary supportive of Webster et al by Raphael (2006).

For most crimes, the certainty and severity of punishment increases discontinuously upon reaching the age of majority, when jurisdiction for criminal wrongdoing shifts from the juvenile to the adult court. In an extraordinarily careful analysis of individual-level crime histories from Florida, Lee and McCrary (2009) attempt to identify a discontinuous decline in the hazard of offending at age 18, the age of majority in Florida. Their point estimate of the discontinuous change is negative as predicted, but minute in magnitude and not even remotely close to achieving statistical significance.

An earlier analysis by Levitt (1998) finds a large drop in the offending of young adults upon their reaching the age of jurisdiction for the adult courts. For several reasons we judge the null effect finding of Lee and McCrary more persuasive. Levitt's (1998) analysis is based on a differences-in-differences approach which as Lee and McCrary (2099:5) point out assumes that "changes over time in... relative (adult-juvenile) punitiveness are exogenous and uncorrelated with

unobservable determinants of crime". By contrast their regression discontinuity design is based on a much weaker "smoothness" assumption. Second, Levitt (1998) focuses on differences in age measured at annual frequencies, whereas Lee and McCrary measure age in days or weeks. At annual frequencies, the estimated effect is more likely to reflect both deterrence and incapacitation. Third, the Lee and McCrary analysis is based on individual level data. Because they find no evidence of a discontinuity in the hazard of offending at the 18th birthday, it is hard to reconcile this null effect at the individual level with a large effect based on aggregated data.

The literature on whether increases in prison sentence length serve as a deterrent is not large but there are several persuasive studies. These studies suggest that increases in the severity of punishment have at best only a modest deterrent effect. We emphasize, however, that this conclusion concerns changes in severity at margin. For deterrence to be effective there of course must be negative consequences. We thus see a need for research on the likely non-linear relationship between deterrence and severity.

iii. Studies of the Effect of Certainty of Punishment

Severity alone cannot deter. There must also be some possibility that the sanction will be incurred if the crime is committed. For that to happen, the offender must be apprehended, usually by the police. He must next be charged and successfully prosecuted, and finally sentenced by the judiciary. None of these successive stages in processing through the criminal justice system is certain. Thus, another key concept in deterrence theory is the certainty of punishment. For two reasons the discussion which follows on evidence pertaining to the certainty of punishment focuses entirely upon the deterrent effect of the police. First, the police are the most important actors in generating certainty—absent detection and apprehension, there is no possibility of conviction or punishment.

Second, there is little research on the deterrent effect stemming from the certainty of prosecution or sentencing to prison conditional on apprehension.[9]

The police may prevent crime through many possible mechanisms. Apprehension of active offenders is a necessary first step for their conviction and punishment. If the sanction involves imprisonment, crime may be prevented by the incapacitation of the apprehended offender. The apprehension of active offenders may, also, deter would-be criminals by increasing their perception of the risk of apprehension and thereby the certainty of punishment. Many police tactics such as rapid response to calls for service at crime scenes or post-crime investigation are intended not only to capture the offender but to deter others by projecting a tangible threat of apprehension. Police may, however, deter without actually apprehending criminals because their very presence projects a threat of apprehension if a crime were to be committed. Indeed some of the most compelling evidence of deterrence involve instances where there is complete or near complete collapse of police presence. In September 1944, German soldiers occupying Denmark arrested the entire Danish police force. According to an account by Andeneas (1974), crime rates rose immediately but not uniformly. The frequency of street crimes like robbery, whose control depends heavily upon visible police presence, rose sharply. By contrast, crimes such as fraud were less affected. See Sherman and Eck (2002) for other examples of crime increases following a collapse of police presence.

Research on the marginal deterrent effect of police has evolved in two distinct literatures. One has focused on the deterrent effect of the aggregate police presence measured, for example, by the relationship between police per capita and crime rates. A second has focused on the crime

[9]Several studies conducted in the 1970s examined the deterrent effect of conviction risk, usually measured by the ratio of convictions to charges (Avio and Clark, 1974; Carr-Hill and Stern, 1973; Sojoquist, 1973)). These studies suffered two important methodological limitations; they all treated conviction risk as exogenous and all were based on cross sectional data.

prevention effectiveness of different strategies for deploying police. We review these literatures separately.

Studies of police hiring and crime rates have been plagued by a number of impediments to causal inference. Among these are cross-jurisdictional differences in the recording of crime, feedback effects from crime rates to police hiring, the confounding of deterrence with incapacitation, and aggregation of police manpower effects across heterogeneous units, among others (see Nagin 1978, 1998). Of these problems, the challenge that has received the most attention in empirical applications is the endogeneity problem, namely the feedback from crime rates to police hiring.

The two studies of police manpower by Marvell and Moody (1996) and Levitt (1997) are notable for their identification strategies as well as for the consistency of their findings. The Marvell and Moody (1996) study is based on an analysis of two panel datasets, one composed of 49 states for the years 1968-1993 and the other of 56 large cities for the years 1971-1992. To untangle the causality problem they regress the current crime rate on lags of the crime rate as well as lags of police manpower. The strongest evidence for an impact of police hiring on total crime rates come from the city-level analysis, with an estimated elasticity of –0.3. In the spirit of Marvell and Moody's multiple time series analysis, Corman and Mocan (2000) conduct tests of Granger causality using a single, high-frequency (monthly) time series of crime in New York City (January 1970 to December 1996). They find that the number of police officers is negatively correlated with some crimes (robbery, burglary) but not with others. In addition, the number of felony arrests is a robust predictor of several kinds of crime (murder, robbery, burglary, vehicle theft). They conclude that policy makers can deter serious crimes by adding more police officers, and also by allocating existing police resources to aggressive felony enforcement (see also Corman and Mocan 2005).

Levitt (1997) performs an instrumental variable analysis from a panel of 59 large cities for the years 1970-1992. Reasoning that political incumbents have incentives to devote resources to increasing the size of the police force in anticipation of upcoming elections, he uses election cycles to help untangle the cause-effect relationship between crime rates and police manpower. Levitt's model produces elasticities of about –1.0 for the violent crime rate and –0.3 for the property crime rate. In a reanalysis of Levitt's data, McCrary (2002) corrected a technical problem in the Levitt's analysis and found no significant preventive effect of police on crime. However, in a reply and new analysis, Levitt (2002) used an alternative identification strategy based on number of firefighters and civil service workers and obtained similar elasticity estimates to his original analysis. Evans and Owens (2007) examine the crime prevention effects of police by analyzing hiring and crime reduction effects associated with federal subsidies disbursed through the Office of Community Oriented Policing Services for the hiring of new police officers. Their elasticity estimates of the crime rate to police expenditures per capita are –0.99 for violent crime and –0.26 for property crime.

These cross-city panel studies of the relationship of crimes to police presence are subject to all of the statistically related criticisms we have raised concerning the imprisonment to crime rate literature. These include our skepticism about the use of Granger causality testing in this context, parameter heterogeneity across very geographic units, and theory openendedness. Thus, we are particularly well disposed to a number of more targeted tests of the police-crime relationship which have appeared in recent years. These studies are important because they provide a more transparent test of the effect of police presence on crime and are less subject to biases that may attend analyzing data across a highly heterogeneous set of cities. Several of these targeted studies investigate the impact on the crime rate of reductions in police presence and productivity as a result of massive budget cuts or lawsuits following racial profiling scandals. Such studies have

examined the Cincinnati Police Department (Shi 2009), the New Jersey State Police (Heaton forthcoming), and the Oregon State Police (DeAngelo and Hansen 2008). Each of these studies concludes that increases (decreases) in police presence and activity substantially decrease (increase) crime. By way of example, Shi (2005) studies the fallout from an incident in Cincinnati in which a white police officer shot and killed an unarmed African American suspect. The incident was followed by three days of rioting, heavy media attention, the filing of a class action lawsuit, a federal civil rights investigation, and the indictment of the officer in question. These events created an unofficial incentive for officers from the Cincinnati Police Department to curtail their use of arrest for misdemeanor crimes, especially in communities with higher proportional representation of African Americans out of concern for allegations of racial profiling. Shi demonstrates measurable declines in police productivity in the aftermath of the riot and also documents a substantial increase in criminal activity. The estimated elasticities of crime to policing based on her approach were –0.5 for violent crime and –0.3 for property crime.

The ongoing threat of terrorism has also provided a number of unique opportunities to study the impact of police resource allocation in cities around the world, including the District of Columbia (Klick and Tabarrok 2005), Buenos Aires (Di Tella and Schargrodsky 2004), Stockholm (Poutvaara and Priks 2006), and London (Draca, Machin, and Witt 2008). The Klick and Taborrok (2005) study examines the effect on crime of the color-coded alert system devised by the U.S. Department of Homeland Security in the aftermath of the September 11, 2001 terrorist attack to denote the terrorism threat level. Its purpose was to signal federal, state, and local law enforcement agencies to occasions when it might be prudent to divert resources to sensitive locations. Klick and Tabarrok (2005) use daily police reports of crime (collected by the District's Metropolitan Police Department) for the period March 2002 to July 2003, during which time the terrorism alert level rose from "elevated" (yellow) to "high" (orange) and back down to "elevated"

on four occasions. During high alerts, anecdotal evidence suggested that police presence increased by 50 percent. Their estimate of the elasticity of total crime to changes in police presence as the alert level rose and fell was –0.3.

Another notable study by Cohen and Ludwig (2003) considered the outcome of policies by the Pittsburgh Police Department that assigned additional police resources to selected high-crime communities within the city. These patrols were relieved from responding to citizen requests for service (911 calls) to work proactively to search for illegally carried guns. Police contacts were initiated mainly through traffic stops and "stop-and-talk" activities with pedestrians in public areas. Carrying open alcohol containers in public and traffic violations were frequent reasons for initiating contact. These targeted patrols were directed to two of Pittsburgh's five police zones that had unusually high crime rates. Based on a "difference-in-difference-in-differences" type analysis they found that this heightened enforcement activity was associated with significant declines in shots fired and assault related gunshot injuries. The conclusion of the Cohen and Ludwig study nicely accord with the conclusions of hot spots policing literature discussed below.

To summarize, aggregate studies of police presence conducted since the mid-1990s consistently find that putting more police officers on the street—either by hiring new officers or by allocating existing officers in ways that put them on the street in larger numbers or for longer periods of time—has a substantial deterrent effect on serious crime. There is also consistency with respect to the size of the effect. Most estimates reveal that a 10-percent increase in police presence yields a reduction in total crime in the neighborhood of 3 percent. Yet these police manpower studies mainly speak only to the number and allocation of police officers and not to what police officers actually do on the street beyond making arrests. We now turn to the question of how police are used.

Much research has examined the crime prevention effectiveness of alternative strategies for deploying police resources. This research has largely been conducted by criminologists and sociologists. Among this group of researchers, the preferred research designs are quasi-experiments involving before-and-after studies of the effect of targeted interventions as well as true randomized experiments. The discussion which follows draws heavily upon two excellent reviews of this research by Weisburd and Eck (2004) and Braga (2008). As a preface to this summary, we draw the theoretical link between police deployment and the certainty and severity of punishment. For the most part, deployment strategies affect the certainty of punishment through its impact on the probability of apprehension. There are, however, notable examples where severity may also be affected.

One way to increase apprehension risk is to mobilize police in a fashion that increases the probability that an offender is arrested after committing a crime. Strong evidence of a deterrent as opposed to an incapacitation effect resulting from the apprehension of criminals is limited. Studies of the effect of rapid response to calls for service (Kansas City Police Department 1977; Spelman and Brown 1981) find no evidence of a crime prevention effect, but this may be because most calls for service occur well after the crime event with the result that the perpetrator has fled the scene. Thus, it is doubtful that rapid response materially affects apprehension risk. Similarly, because most arrests result from the presence of witnesses or physical evidence, improved investigations are not likely to yield material deterrent effects because, again, apprehension risk is not likely to be affected.

A series of randomized experiments were conducted to test the deterrent effect of mandatory arrest for domestic violence. The initial experiment conducted in Minneapolis by Sherman and Berk (1984) found that mandatory arrest was effective in reducing domestic violence reoffending. Findings from follow-up replication studies (as part of the so-called Spouse Assault Replication Program, or SARP) were inconsistent. Experiments in two cities found a deterrent effect, but no such effect was

found in three other cities (Maxwell, Garner, and Fagan 2002). Berk et al. (1992) found that the response to arrest in the SARP data depended upon social background. Higher status individuals seemed to be deterred by arrest whereas the assaultive behavior of lower status individuals seemed to be aggravated. The heterogeneity in response is important because it illustrates a more general point—the response to sanction threats need not be uniform in the population. Sherman (1993) and Sherman and Smith (1992) propose a theory to explain the status-based heterogeneity in response to mandatory arrest.

The second source of deterrence from police activities involves averting crime in the first place. In this circumstance, there is no apprehension because there was no offense. In our view this is the primary source of deterrence from the presence of police. If an occupied police car is parked outside a liquor store, a would-be robber of the store will likely be deterred because apprehension is all but certain. Thus, measures of apprehension risk based only on enforcement actions and crimes that actually occur, such as arrests per reported crime, are seriously incomplete because such measures do not capture the apprehension risk that attends criminal opportunities which were not acted upon by potential offenders because the risk was deemed too high.

Two examples of police deployment strategies that have been shown to be effective in averting crime in the first place are "hot spots" policing and problem-oriented policing. Weisburd and Eck (2004) propose a two-dimensional taxonomy of policing strategies. One dimension is "Level of Focus" and the other is "Diversity of Focus." Level of focus represents the degree to which police activities are targeted. Targeting can occur in variety of ways but Weisburd and Eck give special attention to policing strategies which target police resources in small geographic areas (e.g., blocks or specific addresses) that have very high levels of criminal activity, so-called crime hot spots.

The idea of hot spots policing stems from a striking empirical regularity uncovered by Sherman and colleagues. Sherman et al. (1989) found that only 3 percent of addresses and

intersections ("places," as they were called) in Minneapolis produced 50 percent of all calls to the police. Weisburd and Green (1995) found that 20 percent of all disorder crime and 14 percent of crimes against persons in Jersey City, New Jersey, arose from 56 drug crime hot spots. In a later study in Seattle, Washington, Weisburd et al. (2004) report that between 4 and 5 percent of street segments in the city accounted for 50 percent of crime incidents for each year over a fourteen-year period. Other more recent studies finding comparable crime concentrations include Brantingham and Brantingham (1999), Eck et al. (2000), and Roncek (2000). Just like in the liquor store example, the rationale for concentrating police in crime hot spots is to create a prohibitively high risk of apprehension and thereby to deter crime at the hot spot in the first place.

The first test of the efficacy of concentrating police resources on crime hot spots was conducted by Sherman and Weisburd (1995). In this randomized experiment, hot spots in the experimental group were subjected to, on average, a doubling of police patrol intensity compared to hot spots in the control group. Declines in total crime calls ranged from 6-13 percent. In another randomized experiment, Weisburd and Green (1995) found that hot spots policing was similarly effective in suppressing drug markets.

Braga's (2008) informative review of hot spots policing summarizes the findings from nine experimental or quasi-experimental evaluations. The studies were conducted in five large U.S. cities and one suburb of Australia. Crime-incident reports and citizen calls for service were used to evaluate impacts in and around the geographic area of the crime hot spot. The targets of the police actions varied. Some hot spots were generally high-crime locations whereas others were characterized by specific crime problems like drug trafficking. All but two of the studies found evidence of significant reductions in crime. Further, no evidence was found of material crime displacement to immediately surrounding locations. On the contrary, some studies found evidence of crime reductions, not increases, in the surrounding locations—a "diffusion of crime-control benefits" to non-targeted locales.

We also note that the findings from the previously described econometric studies of focused police actions, for example in response to terror alert level, buttress the conclusion from the hot spots literature that the strategic targeting of police resources can be very effective in reducing crime.

The second dimension of the Weisburd and Eck taxonomy is diversity of approaches. This dimension concerns the variety of approaches that police use to impact public safety. Low diversity is associated with reliance on time-honored law enforcement strategies for affecting the threat of apprehension, for example, by dramatically increasing police presence. High diversity involves expanding beyond conventional practice to prevent crime. One example of a high-diversity approach is problem-oriented policing. Problem-oriented policy comes in so many different forms that (like pornography) it is regrettably hard to define, although specific examples can of course be identified.

One of the most visible examples of problem-oriented policing is Boston's Operation Ceasefire (Kennedy et al. 2001). The objective of the collaborative operation was to prevent inter-gang gun violence using two deterrence-based strategies. One was to target enforcement against weapons traffickers who were supplying weapons to Boston's violent youth gangs. The second involved a more innovative use of deterrence. The youth gangs themselves were assembled (and reassembled) to send the message that the response to any instance of serious violence would be "pulling every lever" legally available to punish gang members collectively. This included a salient severity-related dimension—vigorous prosecution for unrelated, non-violent crimes such as drug dealing. Thus, the aim of Operation Ceasefire was to deter violent crime by increasing the certainty and severity of punishment but only in targeted circumstances, namely if the gang members were perpetrators of a violent crime. While there have been challenges to whether the decline in violence that accompanied Operation Ceasefire was attributable to the program, we concur with the judgment of Cook and Ludwig (2006) that Ceasefire seemed to play a role in the decline. Just as important, Operation Ceasefire illustrates the potential for combining elements of both certainty and severity

enhancement to generate a targeted deterrent effect. Further evaluations of the efficacy of this strategy should be a high priority.

Taken as whole, the literature on the preventive effect of policing provides a compelling scientific case that police prevent crime. It also makes clear that the effects of police on crime are heterogeneous—not all methods for deploying police are comparably effective in reducing crime, indeed some deployment strategies seem to be completely ineffective. Thus, policy recommendations for increasing police resources to prevent crime are incomplete without further elaboration on how they should be used. We are thus very sympathetic with intellectual tradition in the police deployment literature of testing the effectiveness of alternative strategies for using police resources. We return to this observation in the conclusions.

3. Interpretations: Certainty, Severity and the Economic Model of Crime

In this section, we discuss how our conclusions about certainty versus severity relate to the baseline model of crime. Here we argue that these findings do not undercut the economic approach, but rather suggests dimensions along which the modeling of beliefs and preferences should be generalized.

In understanding why certainty might trump severity in criminal decisionmaking, we first return to the fact that the Becker conclusions about the relative efficacy of certainty and severity depended on assumptions about the convexity of $U_C\left(Z_i, L\right)$. For several reasons, this assumption may be challenged. One reason is that the baseline model neglects the intertemporal dimensions of the payoffs under the different crime/no crime and punished/not punished scenarios. In thinking

about the effects of penalties on individuals, it is necessary to consider commission of a crime in the context of an individual's lifetime utility. In other words, the choice to commit a crime at time t is one element of the many decisions an individual makes over time. What this means is that the payoffs embedded in each of the terms in (2) is in fact a sequence of expected discounted utilities over the future, in which the commission of a crime (or lack) represents one element of a dynamic choice problem. This dynamic choice problem calls into question the assumption that $U_C\left(Z_i, L\right)$ is convex since the function is appropriately understood as depending on the way future utility is discounted. The effect of a marginal increase in sentence length affected utilities starting at time $t+L$. If utility at different points in time is discounted by β, then it is evident that for initially long sentences, the effects on crime decisions may have relatively little effect, especially if potential criminals have high discount rates. Thus, there is no logical reason why convexity should hold for $U_C\left(Z_i, L\right)$. Put differently, if one considers the different certainty/severity values that lead to a given value of pL, this expected value masks the time of life where changes in L become operational. Hence it is possible that the disutility effects of longer sentences are simply not that important in the calculation of lifetime utility.

It is beyond the scope of this paper to analyze a lifetime utility model in which agents consider a sequence of crime/no crime decisions. Fortunately, a model of this type is developed in Imai and Krishna (2004). We borrow from their paper to illustrate the general ideas associated with life time utility maximization. To do this, assume that each individual is now indexed by heterogeneity $Z_{i,t}$; this heterogeneity should be interpreted broadly to include any information relevant to the agent's utility both contemporaneously and in terms of forecasting utility in the future. Suppose that for a crime committed at t, there is a probability p of being caught at $t+1$; we denote the decision to commit a crime as $\omega_{i,t}$ and the outcome of being caught as $\mu_{i,t}$; as before 1 means the event (commit

a crime, get caught occurs) and 0 otherwise. An agent who chooses not to commit a crime at time t has an expected lifetime utility at t, $V_{NC,t}\left(Z_{i,t}\right)$, which fulfills

$$V_{NC,t}\left(Z_{i,t}\right) = U_{NC}\left(Z_{i,t}\right) + \beta E_t\left(V\left(Z_{i,t+1}\right)\middle| Z_{i,t}, \omega_i = 0\right) \tag{8}$$

In contrast, if a crime is committed, lifetime utility is

$$\begin{aligned} &V_{C,t}\left(Z_{i,t}\right) = \\ &U_C\left(Z_{i,t}\right) + \beta p E_t\left(V\left(Z_{i,t+1}\right)\middle| Z_{i,t}, \omega_{i.t} = 1, \mu_{i,t+1} = 1\right) + \beta\left(1-p\right) E_t\left(V\left(Z_{i,t+1}\right)\middle| Z_{i,t}, \omega_{i,t} = 1, \mu_{i,t+1} = 0\right) \end{aligned} \tag{9}$$

and so the decision to commit a crime requires that

$$\omega_{i,t} = 1 \text{ if } V_{C,t}\left(Z_{i,t}\right) > VN_{C,t}\left(Z_{i,t}\right) \tag{10}$$

Delineation and analysis of this abstract structure seems an important direction in understanding how different sanction policies matter.[10]

[10]Imai and Krishna (2004) do not take this route as they restrict their analysis to the case where penalties only occur for one period (this is done to allow them to estimate the model in absence of sentencing data).

The embedding of our initial model into a dynamic framework is fully consistent with the view of economic actors as rational, purposeful decisionmakers who follow consistent discounting procedures when weighing the present and future and whose subjective beliefs about probabilities correspond to the objective probabilities for the phenomena under question, most notably the probability of punishment if a crime is committed. Beyond the implications of the effects of increased severity in the context of a lifetime utility model, there may be reasons to believe that deviations from the baseline rational crime model provide additional explanatory power if one backs away from the particular rationality assumptions with which we have so far worked. Here we distinguish between deviations based on the ways in which individuals discount the future versus deviations based on the way probabilities are formulated in assessing uncertain outcomes.

There is a growing body of research from psychology and criminology linking criminal and delinquent behavior in adolescence and beyond to problem behaviors and cognitive deficits measured in childhood (Jolliffe and Farrington, 2009; Moffitt, 1993). One of the most prominent findings in this literature is the linkage between crime and impulsive behavior in non-criminal settings. Impulsivity is measured in many different ways in psychology. In part the differences in measurement reflect different theoretical conceptions of what constitutes impulsivity. Some traditions conceive of impulsivity as a cognitive deficit in "executive functions" such as abstract reasoning, self monitoring and self control. All of these cognitive functions are associated with the functioning of the frontal lobes of the brain. This conception of impulsivity is at the core of a theory of criminal behavior, posited by Wilson and Herrnstein (1985), that persons who are "present oriented", namely individuals who attend mainly to incentives and disincentives over a short rather than long time horizon are more crime prone. Psychologists working in this theoretical tradition have devised many tests of impulsivity which primarily focus on measuring the capacity for focused attention. Another tradition conceives of

impulsivity as a personality characteristic. In this tradition impulsivity is measured by scales designed to capture the degree to which an individual acts without forethought or planning.

Within the economics literature, ideas of impulsivity are paralleled in the development of models of hyperbolic discounting. Hyperbolic discounting is designed to explain behaviors where the temptation of the moment appears to lead to a failure to consistently evaluate future consequences. Models of this type can explain forms of regret on the part of decisionmakers that do not naturally arise on standard geometric discounting. As such, the models can produce behaviors that seem analogous to those associated with impulsivity. To be clear though, hyperbolic discounting does not directly translate into claims about the roles of strong emotions in decisionmaking.

Questions of discounting are logically distinct from those concerning the formation of beliefs about the future; in our context the key variable being the probability of punishment p. Our baseline formulation assumed that individuals work with the true probability of punishment. In another recent review of the deterrence literature, Apel and and Nagin (forthcoming) summarize the literature on sanction risk perceptions. While there is a growing body of evidence that individuals update their sanction risk perceptions based on experience with successfully and unsuccessfully avoiding apprehension in a fashion that at least crudely approximates Bayesian updating, there is also a large body of evidence that perceptions of risk diverge substantially from actual risk with most people overestimating the actual risk.

A simple generalization of our crime model to take account of the divergence between perceived and true probabilities, can be developed by by replacing these objective probabilities with individual-specific subjective ones, p_i^e. Assuming that these subjective probabilities are functions of the true probabilities so that a crime is committed if

$$p_i^e(p)\left(U_C(Z_i,0)-U_{NC}(Z_i)\right)+\left(1-p_i^e(p)\right)\left(U_C(Z_i)-U_{NC}(Z_i,L)\right)>0 \quad (11)$$

As a theoretical matter, the difference between (11) and the baseline model (2) is that the marginal effect on the payoff to criminality is

$$\frac{dp_i^e(p)}{dp}\left(U_C(Z_i,0)-U_C(Z_i,L)\right) \quad (12)$$

While an increase in p still makes crime less attractive, the magnitude of this increase on the expected utility from crime commission will now vary according to $\frac{dp_i^e(p)}{dp}$; in the baseline model this term is replaced by 1 since subjective and objective probabilities are identical. Hence, evaluating a sanctions policy should include consideration of how changes in objective probabilities affect subjective ones.

There is of course a large empirical literature that has studied the properties of $\frac{dp_i^e(p)}{dp}$ in a range of experimental contexts; see Starmer (2000) for a survey. At the risk of oversimplifying a complex body of work, considerable evidence exists that many individuals tend to overestimate small probabilities and underestimate large probabilities, i.e. there appears to be good evidence that subjective probabilities follow an inverse S shape, which means that

$$\frac{dp_i^e(p)}{dp} \text{ is large if } p \text{ near 0 or 1} \quad (13)$$

In words, the effects of an increase in the certainty of punishment are strongest, given a fixed value of $U_C(Z_i,0)-U_C(Z_i,L)$, when the punishment probabilities are relatively large or small to begin with. This suggests that one reason for the relatively robust evidence that increases in certainty of punishment lower crime in contexts such as hot spot policing is that such policing tactics are being implemented in a circumstance where standard policing practice projects only a small probability of apprehension. We note that Berns, Capra, Moore, and Noussair (2007) find an inverse S-shape is common in an experiment where the "rewards" were electric shocks, which suggests that the inverse S shape is relevant for adverse outcomes, i.e. being punished for a crime. However, as far as we know, there does not exist a body of research that focuses on the relationship between objective and subjective probabilities among that part of the population in which the decision to commit a crime is close to marginal, i.e. in which there is some chance of a crime being committed. This strikes us as a useful area for future work[11].

While impulsivity, discounting, and the distinction between subjective and objective probabilities all revolve around efforts to relax our initial assumptions about the cognition process of potential criminals, we close this discussion by considering a different dimension along which the baseline model can, we believe, be fruitfully extended. We are motivated by a consideration that, at first blush, might appear to be inconsistent with the Beckerian model of crime—namely the possibility that the imposition of sanction may be criminogenic even as it is preventive. A key conclusion of a review by Nagin, Cullen, and Jonson (2009) of the effect of the experience of

[11]There are of course findings in the behavioral economics literature that would suggest modifications of the baseline Beckerian model beyond discounting and the subjective/objective probability distinction. For example, Post, van der Assem, Balthussen and Thaler, (2008) find, for a high stakes television game show, risk aversion decreases across rounds of play. This perhaps speaks to a channel by which the fact of arrest and imprisonment might affect preferences. To push this line of argument further would require more expertise in behavioral economics than we possess.

imprisonment on recidivism is that the great majority of studies point to a criminogenic effect of the prison experience on subsequent offending. While this literature suffers from many statistical shortcomings that make this conclusion far from definitive, serious attention should be committed to extending the economic model of crime to account for the possibly criminogenic effect of the experience of punishment. Criminogenic effects may stem from either the crime inducing effects of the *experience* of punishment and/or stigma. As a prelude to discussing the types of model generalizations that might be used to account for potentially criminogenic effects of heightened sanctions, we summarize the state of relevant literature; we then consider modifications of the Beckerian model.

Much data documents that most crime is attributable to a small proportion of the population who repeatedly recidivate. In their seminal study of the criminal activity of a birth cohort of 9,945 males born in Philadelphia in 1945, Wolfgang et al., (1972) found that through age 18, 6% of the cohort accounted for over half of the cohort's total arrests. Also, rates of recidivism of former prisoners are very high. The latest available analysis for the United States as a whole is based on 272,111 individuals released from the prisons of fifteen states in 1993. Langan and Levin (2002) find that within three years 68 percent had been arrested, 46.9 percent had been convicted, and 25.4 percent had been re-imprisoned. Thus, as an empirical matter it is not surprising that most people who have contact with the criminal justice system are not novices. According to a 2006 Bureau of Justice Statistic study of felony defendants in the 75 largest cities, at the time of arrest 32% of defendants had an active criminal justice status, such as probation (15%), release pending disposition of a prior case (10%), or parole (5%). Further, 76% of all defendants had been arrested previously, with 50% having at least five prior arrest charges.

There are two very different interpretations of these statistics. One is that the high concentration of recidivist in the criminal justice system represents the ongoing failure of deterrence to suppress the criminal behavior of a small minority of the population. The other is that the experience of contact with the criminal justice, most specifically in the form of imprisonment, is criminogenic. These two diametrically opposing interpretations of the data lay at the core of much academic and public policy debate about the role of imprisonment in crime control.

Such discussions of the effect of imprisonment on crime often fail to distinguish between the potentially very different behavioral responses to the *threat* of imprisonment versus the *experience* of imprisonment. In the economic model of crime, deterrence is the behavioral response to the threat of crime. In criminology the term "specific deterrence" is used to describe the behavioral response to the experience of punishment. The logic of specific deterrence is grounded in the idea that if the experience of imprisonment is sufficiently distasteful some of the punished may conclude that it is an experience not to be repeated. The structure of the law itself may also cause previously convicted individuals to revise upward their estimates of the likelihood and/or severity of punishment for future lawbreaking. The criminal law commonly prescribes more severe penalties for recidivists. For example, sentencing guidelines routinely dictate longer prison sentences for individuals with prior convictions. Prosecutors may also be more likely to prosecute individuals with criminal histories. The experience of punishment may affect the likelihood of future crime by decreasing the attractiveness of crime itself or by expanding alternatives to crime. While imprisoned the individual may benefit from educational or vocational training that increase post-release non-criminal income earning opportunities (Layton MacKenzie, 2002). Other types of rehabilitation are designed to increase the capacity for self

restraint when confronted with situations, like a confrontation, that might provoke a criminal act such as violence (Cullen, 2002).

There also are, however, many reasons for theorizing that the experience of punishment might increase an individual's future proclivity for crime. While some individuals might conclude imprisonment is not an experience to be repeated, others might conclude that the experience was not as adverse as anticipated. Prisons might be 'schools for crime' where inmates learn new crime skills even as their non-crime human capital depreciates. Associating with other more experienced inmates could lead new inmates to adopt the older inmate's deviant value systems or enable them to learn 'the tricks of the trade' (Hawkins, 1976; Steffensmeier and Ulmer, 2005). Being punished could also elevate the offender's feelings of resentment against society (Sherman, 1992) or strengthen the offender's deviant identity (Matsueda, 1992).

The experience of imprisonment may also increase future criminality by stigmatizing the individual socially and economically. There is much evidence showing that an important part of the deterrent effect of legal sanctions stems from the expected societal reactions set off by the imposition of legal sanctions (Williams and Hawkins, 1986; Nagin and Pogarsky, 2003; Nagin and Paternoster, 1994). Prior research has found that individuals who have higher stakes in conformity are more reluctant to offend when they risk being publicly exposed (Klepper and Nagin, 1989). While the fear of arrest and stigmatization may deter potential offenders from breaking the law, those that have suffered legal sanctions may find that conventional developmental routes are blocked. In their work on the 500 Boston-delinquents initially studied by Glueck and Glueck (1950), Sampson and Laub (1997) have called attention to the role of legal sanctions in what they call the process of cumulative disadvantage. Official labelling through legal sanctions may cause an offender to become marginalized from conventionally

structured opportunities, which in turn increases the likelihood of their subsequent offending (Bernburg and Krohn, 2003). Sampson and Laub (1997) propose that legal sanctions may amplify a 'snowball' effect that increasingly 'mortgages' the offender's future by reducing conventional opportunities. Several empirical studies support the theory that legal sanctions downgrade conventional attainment (Freeman, 1996; Nagin and Waldfogel, 1995, 1998; Sampson and Laub, 1993; Waldfogel, 1993; Western, 2002; Western, Kling and Weiman, 2001) and increase future offending (Bernburg and Krohn, 2003; Hagan and Palloni, 1990).

Moving from this review of empirical work to the economic model of crime, our two channels for criminogenic effects imply somewhat different modeling strategies. The possibility that the experience of punishment affects proclivity for crime creates an important additional source of heterogeneity in the population at a given point in time. To see this, let CR_i denote the criminal record of the individual. Crime commission now requires that

$$p\left(U_C\left(Z_i, CR_i, L\right) - U_{NC}\left(Z_i, CR_i\right)\right) + \left(1-p\right)\left(U_C\left(Z_i, CR_i, 0\right) - U_{NC}\left(Z_i, CR_i\right)\right) > 0 \qquad (14)$$

which is a an algebraically trivial extension of our baseline model but in fact gives a very different view of the determination of the aggregate crime rate. To see this, consider a generalization of (3)

$$\omega\left(Z_i, CR_i, p, L\right) = 1 \text{ if (12) holds; 0 otherwise} \qquad (15)$$

The equilibrium crime rate will now equal

$$\Pr\left(C\middle|p,L\right)=\int\int\Pr\left(\omega=1\middle|Z,CR,p,L\right)dF_{CR,Z}= \int dF_{CR}\int\int\Pr\left(\omega\left(Z,p,L\middle|CR\right)\right)dF_{Z|CR} \tag{16}$$

The second integral in (16) is a mixture density, i.e. averages (based on population weights) the crime probabilities for each level of criminal record in the population. By allowing the various utility terms to depend on criminal record, it is evident that it is possible, of course, for a criminal record to increase the probability of crime commission. Experience of punishment effects, whether generated by the learning of crime-related skills in prison or by the diminution of labor market opportunities after prison introduce additional heterogeneity in the population that can raise the probability of crime on the part of an individuals.

Of course, one would expect that the lowered utility for an agent after imprisonment would work to reduce the incentive to commit a crime at t, especially among those who have never committed a crime. This possibility is masked in the formulation because we have not written an explicit intertemporal decision problem; this possibility is implicitly embedded in $\omega\left(Z,p,L\middle|CR=0\right)$. Therefore, the presence of experience of punishment effects do not provide an a priori implication for the aggregate crime rate, it could be either increased or decreased. Notwithstanding, these effects can help explain why criminal behavior is concentrated in a small fraction of the total population who repeatedly recidivate. Among those relatively few individuals who initially commit crimes, recidivism rates are high because of the changes induced in the relative costs and benefits of crime.

One way to model stigma effects is to modify the various utility functions so that the rate of imprisonment rate I is an additional argument in the utility functions when a crime is committed. This assumption (at least for previous offenders) is in the spirit of Sirakaya (2006) who found that the

time recdividism for individuals is associated with the mean time for recidivism in their communities, even controlling for a host of observed and unobserved community effects[12]. The condition under which a crime is chosen is in this case

$$p\left(U_C\left(Z_i,L,I\right)-U_{NC}\left(Z_i\right)\right)+\left(1-p\right)\left(U_C\left(Z_i,0,I\right)-U_{NC}\left(Z_i\right)\right)>0 \qquad (17)$$

If stigma means that $U_C\left(Z_i,L,I\right)$ is decreasing in I, then it is trivial to see that expected utility to commission of a crime will be higher when stigma is lower. Since $I=P\left(C\middle|p,L\right)pL$, a stigma effect means that the probability that an individual commits a crime is an increasing function of the average probability in the population. Under this modeling assumption, stigma is an example of a social interactions effect, (see Brock and Durlauf (2001b) and Durlauf and Ioannides (2009) for surveys), one consequence of which is the possibility of multiple equilibrium crime rates under a given sanction regime. This possibility is demonstrated theoretically in Rasmusen (1996), which is noteworthy as for giving clear microfoundations to a stigma effect in terms of the signal a criminal record gives about an individual's underlying type.

While experience of punishment and stigma effects can explain why increased sanctions can be criminogenic, it is less obvious that they can explain relatively efficacy of certainty versus severity. That said, we believe there is good intuition why experience of imprisonment should

[12]Other research suggests the presence of social interactions in crime in which the criminal choice of one person depends on the criminal choices of others within one's community; Glaeser, Sacerdote and Scheinkman (1996) is an early example. This type of interaction may involve stigma as well. In terms of formal modeling, one way to model this type of interaction, following Brock and Durlauf (2001c) would simply involve replacing I with p in the utility functions associated with commission of a crime. One can also imagine more elaborate network structures for social interactions, for example see Calvo-Armengol, Verdier, and Zenou, (2007).

exhibit the certainty severity differential. Long sentences, we suspect are very damaging because of the brutality of prison and so render released prisoners especially unlikely to prosper in the noncriminal world. But to be fair, this is only an intuition.

It seems less clear why stigma would imply that certainty is more effective in deterring crime than severity. One reason why there may be a differential effect is that our index for stigma, $I = P\left(C|p,L\right)pL$ is a nonlinear function of p and L, a differential may simply follow from this. Another reason why stigma leads to the differential may occur when one decomposes stigma into different types. One type of stigma may be purely psychological, so that the shame or embarrassment of punishment is lower when a higher fraction of the population has experienced imprisonment at any time[13]. This would create a complicated relationship between stigma, p and L because the relevant variable would be the stock of current and former prisoners. To be clear, neither of these arguments implies that the differential effect should be that certainty is more efficacious than severity, but these mechanisms at least allow the possibility.

A second possibility derives from a conception of stigma that is more in line with the analysis of Rasmusen (1996). Following Rasmusen, one can think of stigma as involving the inference that employers and other make about an individual given his criminal record. Suppose that there are two types of offenders, one able to function in a regular job, and one not. If both serve long prison sentences, then the fact of a criminal record does not distinguish between the two types of individuals. In other words, harsh sentencing policies may coarsen the information set by

[13]See Posner (2000) for a discussion of stigma that links the concept to that of shaming. It seems reasonable to think that shaming effects depend on the criminal history of the population as a whole. Of course, one can easily imagine a range of additional factors; Posner for example emphasizes the importance of informal sanctions.

which individuals are differentiated. The net effect on the crime rate will depend on the net effects on the beneficiaries of the coarsening (the bad types) versus those who are harmed (the good types). One cannot give an a priori sign to the net effect, but its presence could produce the certainty severity distinction we have emphasized.

5. Policy implications and future research

The key empirical conclusion of our literature review is that there is relatively little reliable evidence of variation in the severity of punishment (L) having a substantial deterrent effect but that there is relatively strong evidence of that variation in the certainty of punishment (p) has a large deterrent effect. We have further argued that these findings are consistent with the economic model of crime, so long as one distinguishes between the key behavior logic of the model as opposed to auxiliary assumptions of various types. In this section we discuss the translation of this general reading of the evidence into policy implications.

One policy relevant implication of this conclusion is that lengthy prison sentences particularly in the form mandatory minimum type statutes such as California's Three Strikes Law are difficult to justify on a deterrence-based crime prevention basis. They must be justified based on either incapacitation benefits or a retributive basis. While we have not studied the evidence on incapacitation, we are skeptical of the incapacitative efficiency of incarcerating aged criminals. For their incarceration to be socially efficient it must have a deterrent effect on other presumably younger criminals. There is no reliable evidence of such an effect.

If one takes the total resources devoted to crime prevention as fixed, then another natural implication of our evidentiary conclusion is that crime prevention would be enhanced by shifting resources from imprisonment to policing. However, even such an apparently self-evident conclusion may be difficult to translate into a defensible operational plan beyond it strongly recommending against any further escalation of sentence length. We say this because it leaves open many questions about the way the resources should be used—more police, better logistics, more non-human capital, better training etc. The econometrics literature on police resources and crime rates provides very little guidance on how those resources should be utilized. It also leaves open the question of the mechanism by which the resources would be transferred—corrections is, by and large, a state and federal function whereas policing is, by and large, a local function.

In our judgment the details of the policy for cutting back on sentences and shifting the resources to policing are critical to their efficacy in reducing crime without increasing resources committed to crime control. The literature on the crime prevention effects of different strategies for mobilizing the police makes clear that the way police resources are used matters a lot. This literature has assembled an impressive body of evidence that the so called "standard model" of policing which involves the non-strategic use of preventive patrols, rapid response to calls for service, improved investigation methods is not effective in deterring crime (National Research Council, 2004; Weisburd and Eck, 2004). However, more strategic use of police, for example hot spot policing, have been shown to be effective. Also, certain forms of so-called "problem oriented" policing have shown promise. This research, however, does not form the basis for devising a policy for shifting resources from corrections to policing that we can state with confidence will reduce crime without increasing the overall resource commitment to crime prevention. We thus close with a discussion of the type of research which in our judgment will be most effective in delineating the details of a policy that will achieve this policy objective.

In most circumstances the deterrent effect of policing works through its effect on the probability of apprehension, the key initial probability component of p, the probability of imprisonment given crime. Some police deployment strategies such as rapid response to 911 calls or more intensive investigation are probably ineffective in deterring crime because they are ineffective in altering apprehension risk. Some may be effective in increasing apprehension risk but ineffective in deterring crime because offenders do not perceive the change. In this case, crime may be reduced by the apprehension and the ultimate incapacitation of active offenders. However, the reduction in crime will necessarily be accompanied by an increase in imprisonment. More generally, if the elasticity of the crime rate with respect to p is greater than 1, both crime and imprisonment will be reduced. For a policy to have a crime elasticity of 1 or greater it must necessarily have a deterrent effect (Blumstein and Nagin, 1978). Identification of police deployment strategies which have this effect should be a top priority. While we know of no research that identifies policing strategies having such an effect, the paucity of evidence is attributable to research design. Assuming post apprehension probability of imprisonment remains unchanged, a policing strategy that is effective in reducing both crime and imprisonment will reduce both the per capita crime rate and the per capita arrest rate. Police deployment research routinely measures effectiveness by the former measure but never measures effectiveness by the latter measure. Future research should measure both.

What types of deployment strategies are good candidates for reducing both crime and imprisonment? We speculate that strategies that result in large and visible shifts in apprehension risk are most likely to have deterrent effects that are large enough to reduce not only crime but also apprehensions. Hot spots policing may have this characteristic. More generally, the types of problem-oriented policing described and championed by David Kennedy in *Deterrence and Crime Prevention* and by Mark Kleiman in *When Brute Force Fails* have the common feature of

targeting enforcement resources on selected places or people. An example of such a strategy is the previously described Operation Ceasefire in Boston. While the effectiveness of these strategies for focusing police and other criminal justice resources has yet to be demonstrated, priority attention should be given to their continued evaluation.

We also note that while there is good evidence that severity is not an effective deterrent, the literature is small and mostly focused on severity increments to already lengthy sentences. It is thus important to better understand the circumstances where severity can be an effective deterrent. An example is an experiment conducted by Weisburd, Einat, and Kowalski (2008) involving alternative strategies for incentivizing the payment of court-ordered fines. The most salient finding involves the "miracle of the cells," namely, that the imminent threat of incarceration is a powerful incentive for paying delinquent fines. The miracle of the cells, we believe, provides a valuable vantage point for considering our conclusion that the certainty rather the severity of punishment is the more powerful deterrent. Consistent with the "certainty principle," the common feature of treatment conditions involving incarceration was a high certainty of imprisonment for failure to pay the fine. However, the fact that Weisburd and colleagues label the response the "miracle of the cells" and not the "miracle of certainty" is telling. Their choice of label is a reminder that certainty must result in a distasteful consequence, namely incarceration in this experiment, in order for it to be a deterrent. The consequences need not be draconian, just sufficiently costly to deter proscribed behavior. Sentence lengths in Western European countries tend to be far shorter than in the US. For the example over 90% of sentences in the Netherlands are less than 1 year (Nieuwbeerta, Nagin, and Blokland, 2009). Research based in European data on the deterrent effect of shorter sentence length should be a priority.

Finally we emphasize the importance of recognizing the limits to knowledge faced by policymakers. To some degree gaps in empirical knowledge can be filled by more complete theory. We, thus, recommend that extending the baseline Beckerian model of crime along the lines we outline in our review be a high priority. However, even with better theory, substantial and irreducible empirical uncertainties will remain. In our judgment, far too many proposals for crime amelioration take as their basis a single study or a subset of studies from a broader literature. Perhaps the best example of this is in the literature on capital punishment. In our view, there is no reasonable basis for concluding *anything* about the magnitude of the deterrent effect of capital punishment. As shown in Cohen-Cole, Durlauf, Fagan and Nagin (2008) the distribution of deterrent effects across a space of seemingly second-order changes in regression specification, can lead to estimates of net lives saved that vary between -100 and 300, so that model uncertainty is sufficient to prevent one from even identifying the sign of the effect. The appropriate conclusion from the capital punishment literature is not that there is no deterrent effect to capital punishment but rather that the historical data are uninformative. In terms of decision theory, this is equivalent to saying that a policymaker's prior and posterior beliefs about the deterrent effect of capital punishment ought to coincide. Hence, without a principled basis for having a priori beliefs for a deterrence effect or without a retributive justification, it is difficult to imagine a strong defense of capital punishment as a deterrent strategy. We believe this sort of skeptical perspective is also appropriate for imprisonment policies. At the same time, we see a number of fruitful directions for imprisonment policy analysis.

First, we believe that policy recommendations should place particular value on evidence of the effectiveness of specific crime control treatments. In our view, this emphasis has a strong analogy to the medical literature, where evidence of the efficacy of a particular drug regimen or specific

preventive measure is of the highest value. We would also conjecture that more attention should be paid to the effects of policies on particular types of crimes. Again, in the spirit of our medical analogy, policies that are effective for one type of crime may have little effect on others. For example hot spots policing is unlikely to be effective in reducing crimes such as domestic violence or homicide that generally occur in nonpublic places. Our point is that just as in medicine where a portfolio of treatments is required to address heterogeneous diseases, a well designed crime control policy requires a portfolio of crime control treatments to address diversity in type of crimes and the people who commit them.

Second, we believe that stricter evidentiary thresholds for aggregate studies need to be established--thresholds that respect the deep limitations such studies face in terms of problems of model specification, exchangeability of data across localities, and the like. As we have indicated above, we do not believe such studies are valueless. They clearly can help buttress qualitative conclusions about certainty versus severity. But for aggregate studies to provide firm guidance on policy, much more attention to issues of robustness needs to be paid than has been the norm in the crime literature. Following our medical analogy, John Snow's classic demonstration that the spread of cholera in London in 1854 (enjoyably described in Johnson (2006) was due to transmission via the water supply did not require any experiments, but rather the careful and systematic elimination of alternative explanations. We do not raise Snow for reasons of pedantry; his work is a lasting example of how careful data nonexperimental data analysis can produce successful policy interventions and is a useful standard against which to think about regression studies of crime.

Third, we conjecture that serious thought should be given to diversified treatment regimes, in which policies are varied across time and place. We advocate this both as it will

enhance learning about effective policies, but also because it provides a form of diversification against the efficacy uncertainty associated with various policies. Manski (2009) provides a framework for optimal policy diversification in contexts where policymakers cannot assign probabilities to the possible effects of a policy; his work is an example of the analysis of decisionmkaing under ambiguity. We suspect his probability bounds approach may prove valuable in reducing the dependence of criminological studies on modeling assumptions. But independent of this, the uncertainty about particular policies does not exist in a vacuum; in particular, uncertainty about particular policies is a manifestation of uncertainty about the decision processes of criminals. So, our final conclusion really brings us back to our initial discussion of the economic model of crime. The economic model of crime, by identifying which aspects of individual decisionmaking matter for the determination of crime rates, also provides a template for understanding how uncertainty about these aspects induces uncertainty in the effectiveness of policies. Standard portfolio diversification arguments, as well as new thinking about decisionmaking under ambiguity suggest that heterogeneity in anticrime policies will reduce the degree of ignorance associated with the effects of policy choices. But as always, the devil is in the details. Hence we see much need for new research.

References

Andenaes, Johannes. 1974. *Punishment and Deterrence*. Ann Arbor, MI: University of Michigan Press.

Apel, Robert and Daniel S. Nagin. Forthcoming. "Deterrence." In *Crime* (4th ed.) edited by James Q. Wilson and Joan Petersilia . Oxford, U.K.: Oxford University Press. Also, appearing in *Handbook on Crime and Criminal Justice* edited Michael Tonry. Oxford, U.K.: Oxford University Press.

Berk, Richard A., Alec Campbell, Ruth Klap, and Bruce Western. 1992. "The Deterrent Effect of Arrest in Incidents of Domestic Violence: A Bayesian Analysis of Four Field Experiments." *American Sociological Review* 57: 698-708.

Berns, Gregory, C. Monica Capra, Sara Moore, and Charles Noussair. 2007. "A Shocking Experiment: New Evidence on Probability Weighting and Common Ratio Valuations." *Judgment and Decisionmaking* 2: 234-242.

Bernburg, Jon G., and Marvin D. Krohn. 2003. "Labeling, Life Chances, and Adult Crime: The Direct and Indirect Effects of Official Intervention in Adolescence on Crime in Early Adulthood." *Criminology* 41: 1287-1318.

Black, Dan and Daniel S. Nagin. 1998. "Do Right-to-Carry Laws Deter Violent Crimes," *Journal of Legal Studies* 27: 209-219.

Blumstein, Alfred, Jacqueline Cohen, and Daniel Nagin. 1978. *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*. Washington, DC: National Academies Press.

Blumstein, Alfred, and Allen J. Beck. 1999. "Population Growth in U.S. Prisons, 1980-1996." In *Crime and Justice: A Review of Research*, vol. 26, edited by Michael Tonry. Chicago: University of Chicago Press.

Blumstein, Alfred, and Daniel S. Nagin. 1978. "On the Optimal Use of Incarceration." *Operations Research* 26: 381- 405.

Braga, Anthony A. 2008. *Police Enforcement Strategies to Prevent Crime in Hot Spot Areas*. Crime Prevention Research Review (No. 2). Washington, DC: Office of Community Oriented Policing, U.S. Department of Justice.

Brantingham, Patricia L. and Paul J. Brantingham. 1999. "Theoretical Model of Crime Hot Spot Generation." *Studies on Crime and Crime Prevention* 8: 7-26.

Brock, William, and Steven Durlauf. 2001a. "Growth Economics and Reality," *World Bank Economic Review* 15: 229-272.

Brock, William and Steven Durlauf. 2001b. "Interactions-Based Models." In *Handbook of Econometrics, vol. 5*, J. Heckman and E. Leamer, eds., Amsterdam: North-Holland.

Brock, William and Steven Durlauf. 2001c. "Discrete Choice with Social Interactions," *Review of Economic Studies* 68: 235-260.

Calvo-Armengol, Antoni, Thierry Verdier and Yves Zenou. 2007. "Strong and Weak Ties in Employment and Crime." *Journal of Public Economics* 91: 203-233.

Cohen-Cole, Ethan, Steven Durlauf, Jeffrey Fagan, and Daniel Nagin. 2008. "Model Uncertainty and the Deterrent Effect of Capital Punishment." *American Law and Economics Review* forthcoming.

Cohen, Jacqueline and Jens Ludwig. 2003. "Policing Gun Crime." In Jen Ludwig and Philip J. Cook. (eds.) *Evaluating Gun Policy.* Washington, D.C.: Brookings Institution Press.

Cook Philip J. and Jens Ludwig. 2006. "Aiming for Evidenced-Based Gun Policy." *Journal of Policy Analysis and Management* 25: 691–735.

Corman, Hope, and H. Naci Mocan. 2000. "A Time-Series Analysis of Crime, Deterrence, and Drug Abuse in New York City." *American Economic Review* 90: 584-604.

Corman, Hope, and H. Naci Mocan. 2005. "Carrots, Sticks, and Broken Windows." *Journal of Law and Economics* 48: 235-266.

Cullen, Francis T. 2002. "Rehabilitation and Treatment Programs." In *Crime: Public Policies for Crime Control*, edited by James Q. Wilson and Joan Petersilia. Oakland, CA: ICS Press.

DeAngelo, Greg, and Benjamin Hansen. 2008. "Life and Death in the Fast Lane: Police Enforcement and Roadway Safety." Unpublished manuscript. Department of Economics, University of California, Santa Barbara.

Deaton, Angus. 2009. "Instruments of Development: Randomization in the Tropics, and the Search for the Elusive Keys to Economic Development," mimeo.

Dezhbakhsh, Hashem and Paul Rubin. 2007. "From the "Econometrics of Capital Punishment" to the "Capital Punishment" of Econometrics: On the Use and Abuse of Sensitivity Analysis," mimeo.

Dezhbakhsh, Hashem, Paul Rubin, and Johanna Shepard. 2003. "Does Capital Punishment Have a Deterrent Effect? New Evidence from Post-Moratorium Panel Data." *American Law and Economics Review* 5: 344-76.

Di Tella, Rafael, and Ernesto Schargrodsky. 2004. "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces after a Terrorist Attack." *American Economic Review* 94: 115-133.

Donohue, John. 2007. "Assessing the Relative Benefits of Incarceration: The Overall Change Over the Previous Decades and the Benefits on the Margin." Working paper. Yale Law School, New Haven, CT.

Donohue, John and Justin Wolfers. 2005. "Uses and Abuses of Empirical Evidence in the Death Penalty Debate." *Stanford Law Review* 58: 791-846.

Draca, Mirko, Stephen Machin, and Robert Witt. 2008. *Panic on the Streets of London: Police, Crime and the July 2005 Terror Attacks.* IZA Discussion Paper no. 3410. Bonn, Germany: Institute for the Study of Labor.

Durlauf, Steven and Yannis Ioannides. 2009. "Social Interactions Models," mimeo.

Durlauf, Steven, Paul A. Johnson, and Jonathan Temple. 2005. "Growth Econometrics." In *Handbook of Economic Growth*, P. Aghion and S. N. Durlauf, eds., North Holland, Amsterdam.

Durlauf, Steven, Salvardor Navarro and David Rivers, 2008, "On the Interpretation of Aggregate Crime Regressions. In *Crime Trends*, A. Goldberger and R. Rosenfeld, eds., Washington DC: National Academy of Sciences Press.

Durlauf, Steven, Salvador Navarro and David Rivers. 2008. "Understanding Aggregate Crime Regressions." *Journal of Econometrics*, forthcoming.

Eck, John, Jeffrey S. Gersh, and Charlene Taylor. 2000. "Finding Crime Hot Spots Through Repeat Address Mapping." In *Analyzing Crime Patterns: Frontiers of Practice*, edited by Victor Goldsmith, Philip G. McGuire, John H. Mollenkopf, and Timothy A. Ross. Thousand Oaks, CA: Sage Publications.

Ehrlich, Isaac. 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy* 81: 521-65.

Evans, William N., and Emily G. Owens. 2007. "COPS and Crime." *Journal of Public Economics* 91: 181-201.

Fisher, Franklin M., and Nagin, Daniel S. 1978. "On the Feasibility of Identifying the Crime Function in a Simultaneous Model of Crime Rates and Sanction Levels." In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, edited by Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academies Press.

Forst, Brian. 1976. "Participation in Illegitimate Activities: Further Empirical Findings." *Policy Analysis* 2: 477-92.

Freeman, Richard B. 1995. "Why Do So Many Young American Men Commit Crimes and What Might We Do About It?" *Journal of Economic Perspectives* 10: 25-42.

Gibbs, Jack B. 1968. "Crime, Punishment and Deterrence." *Southwestern Social Science Quarterly* 48: 515-30.

Glaeser, Edward, Bruce Sacerdote, and Jose Scheinkman. 1996. "Crime and Social Interactions." *Quarterly Journal of Economics* 111: 507-548.

Greenwood, Peter and Angela Hawkin. 2002. "An Assessment of the Effect of California's Three-Strikes Law." Greenwood Associates. Working Paper.

Glueck, Sheldon and Eleanor Glueck E. 1950. *Unraveling Delinquency*. New York: The Commonwealth Fund.

Hagan, John and Alberto Palloni. 1990. "The Social Reproduction of a Criminal Class in Working-class London 1950-1980." *American Journal of Sociology* 96: 265-299.

Hawkins, Gordon. 1976. *The Prison; Policy and Practice*. Chicago: Chicago University Press.

Heaton, Paul. Forthcoming. "Understanding the Effects of Anti-Profiling Policies." *Journal of Law and Economics*.

Heckman, James. 2000. "Causal Parameters and Policy Analysis in Economics: A Twentieth Century Retrospective." *Quarterly Journal of Economics*, 115: 45-97.

Heckman, James. 2005. "The Scientific Model of Causality." *Sociological Methodology* 35: 1-98.

Helland, Eric, and Alexander Tabarrok. 2007. "Does Three Strikes Deter? A Nonparametric Estimation." *Journal of Human Resources* 42: 309-330.

Horowitz, Joel. 2004. "Statistical Issues in the Evaluation of Right-to-Carry Laws." In *Firearms and Violence*, C. Wellford, J. Pepper, and C. Petrie, eds., Washington DC: National Academy Press.

Imai, Susumu and Kala Krishna. 2004. "Employment, Deterrence, and Crime in a Dynamic Model." *International Economic Review* 45: 845-872.

Johnson, Steven. 2006. *The Ghost Map*. New York: Riverhead Books.

Jolliffe, Darrick and David P. Farrington. 2009. "A Systematic Review of the Relationship Between Childhood Impulsiveness and Later Violence. In *Personality, Personality Disorder, and Violence,* Mary McMurran and Richard C. Howard, eds. New York: John Wiley & Sons.

Kansas City Police Department. 1977. *Response Time Analysis*. Kansas City, MO: Kansas City Police Department.

Kennedy, David. M. 2009. *Deterrence and Crime Prevention Reconsidering the Prospect of Sanction.* New York, NY: Routledge

Kennedy, David M., Anthony A. Braga, Anne Morrison Piehl, and Elin J. Waring. 2001. *Reducing Gun Violence: The Boston Gun Project's Operation Ceasefire*. Washington, DC: National Institute of Justice.

Kessler, Daniel, and Steven Levitt. 1999. "Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation." *Journal of Law and Economics* 42: 343–63.

Klepper, Steven, and Daniel Nagin. 1989. "The Deterrent Effect of Perceived Certainty and Severity Revisited." *Criminology* 27: 721-746.

Kleiman, Mark. 2009. *When Brute Force Fails How to Have Less Crime and Less Punishment.* Princton, N.J.: Princeton University Press.

Klick, Jonathan, and Alexander Tabarrok. 2005. "Using Terror Alert Levels to Estimate the Effect of Police on Crime." *Journal of Law and Economics* 48: 267-279.

Langan, Patrick A., and David J. Levin. 2002. *Recidivism of Prisoners Released in 1994.* Washington, DC: Bureau of Justice Statistics, U.S. Department of Justice.

Layton MacKenzie, Doris. 2002. Reducing the Criminal Activities of Known Offenders and Delinquents: Crime Prevention in the Courts and Corrections. In *Evidence-Based Crime Prevention,* Lawrence W. Sherman, David P. Farrington, Brandon C. Welsh, AND Doris Layton MacKenzie, eds. London: Routledge.

Lott, John 1998. "The Concealed-Handgun Debate." *Journal of Legal Studies*, 27: 221-291.

Lott, John and David Mustard. 1997. "Crime, Deterrence, and Right-to-Carry Concealed Handguns." *Journal of Legal Studies* 26: 1-68.

Lee, David S., and Justin McCrary. 2009. "The Deterrent Effect of Prison: Dynamic Theory and Evidence." Unpublished manuscript. Industrial Relations Section, Princeton University, New Jersey.

Levitt, Steven. 1996. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Legislation." *Quarterly Journal of Economics* 111: 319-352.

Levitt, Steven. 1997. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87: 270-290.

Levitt, Steven. 1998. "Juvenile Crime and Punishment." *Journal of Political Economy*, 106: 1156-1185.

Levitt, Steven. 2002. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Reply." *American Economic Review* 92: 1244-1250.

Levitt, Steven and Sudhir A. Venketesh. 1998. "An Economic Analysis of a Drug-Selling Gang's Finances." *Quarterly Journal of Economics* 115: 755-789.

Levitt, Steven. 2006. "The Case of the Critics Who Missed the Point: A Reply to Webster et al.." *Criminology and Public Policy* 5:449-460.

Liedka, Raymond V., Anne Morrison Piehl, and Bert Useem. 2006. "The Crime-Control Effect of Incarceration: Does Scale Matter?" *Criminology and Public Policy* 5: 245-276.

Loftin, Colin, and David McDowall. 1981. "'One with a Gun Gets You Two':Mandatory Sentencing and Firearms Violence in Detroit." *Annals of the American Academy of Political and Social Science* 455: 150–67.

Loftin, Colin, Milton Heumann, and David McDowall. 1983. "Mandatory Sentencing and Firearms Violence: Evaluating an Alternative to Gun Control." *Law and Society Review* 17: 287–318.

Loftin, Colin and David McDowall. 1984. "The Deterrent Effects of the Florida Felony Firearm Law."*Journal of Criminal Law and Criminology* 75: 250–59.

Manski, Charles. 2009. "Diversified Treatment Under Ambiguity," *International Economic Review* 50: 1013-1041.

Marvell, Thomas B. and Carlisle Moody Jr. 1994. "Prison Population Growth and Crime Reduction." *Journal of Quantitative Criminology* 10: 109-140.

Marvell, Thomas, and Carlisle Moody. 1996. "Specification Problems, Police Levels, and Crime Rates." *Criminology* 34: 609-646.

Matsueda, Ross L. 1992. "Reflected Appraisal, Parental Labeling, and Delinquency: Specifying a Symbolic Interactionist Theory." *American Journal of Sociology* 97: 1577-1611.

Maxwell, Christopher D., Joel H. Garner, and Jeffrey A. Fagan. 2002. "The Preventive Effects of Arrest on Intimate Partner Violence: Research, Policy and Theory." *Criminology and Public Policy* 2: 51-80.

McCrary, Justin. 2002. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Comment." *American Economic Review* 92: 1236-1243.

McCrary, Justin. **2010. This volume.**

McKenzie, Doris Layton. 2002. "Reducing the Criminal Activities of Known Offenders and Delinquents: Crime Prevention in the Courts and Corrections." In *Evidence-Based Crime Prevention*, edited by Lawrence W. Sherman, David P. Farrington, Brandon C. Welsh, and Doris Layton MacKenzie. London: Routledge.

Moffitt, Terrie. E. 1993. "Adolescence-limited and Life-course Persistent Antisocial Behavior: A Developmental Taxonomy." *Psychological Review* 100: 674-701.

Nagin, Daniel S. 1978. "General Deterrence: A Review of the Empirical Evidence." In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, edited by Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academies Press.

Nagin, Daniel S. 1998. "Criminal Deterrence Research at the Outset of the Twenty-First Century." In *Crime and Justice: A Review of Research*, vol. 23, edited by Michael Tonry. Chicago: University of Chicago Press.

Nagin, Daniel S., Francis T. Cullen, and Cheryl Lero Jonson. 2009. "Imprisonment and Re-Offending." In *Crime and Justice: A Review of Research*, vol. 38, edited by Michael Tonry. Chicago: University of Chicago Press, forthcoming.

National Research Council. 2004. *Fairness and Effectiveness in Policing The Evidence.* Washington, D.C.: National Academies Press.

Nieuwbeerta, Paul, Daniel S. Nagin, and Arjan Blokland. 2009. "The Relationship between First Imprisonment and Criminal Career Development: A Matched Samples Comparison." *Journal of Quantitative Criminology* 25: 227–257

Pager, Devah. 2003. "The Mark of a Criminal Record." *American Journal of Sociology* 108: 937-975.

Posner, Eric. 2000. *Law and Social Norms*. Cambridge: Harvard University Press.

Poutvaara, Panu, and Mikael Priks. 2006. "Hooliganism in the Shadow of a Terrorist Attack and the Tsunami: Do Police Reduce Group Violence?" Unpublished manuscript. Department of Economics, University of Helsinki, Finland.

Quine, Willard V. O. 1951. "Two Dogmas of Empiricism." *Philosophical Review* 60: 20-43.

Raftery, Adrian, David Madigan, and Jennifer Hoeting, 1997. "Bayesian Model Averaging for Linear Regression Models." *Journal of the American Statistical Association* 92: 179-91.

Raphael, Steven . 2006. "The Deterrent Effects of California's Proposition 8: Weighing the Evidence." *Criminology and Public Policy* 5:471-478.

Raphael, Steven, and Michael Stoll. 2007. "Why Are So Many Americans in Prison?" University of California, Berkeley: Unpublished Paper.

Rasmusen, Eric. 1996. "Stigma and Self-Fulfilling Expectations of Criminality." *Journal of Law and Economics* 39: 519-544.

Reuter, Peter, Robert J. McCoun, Patrick Murphy, Allan F. Abrahamne, and Barbara Simon. 1990. *A Study of the Economics of Drug Dealing in Washington, DC.*_ Washington, DC.: Rand Corporation.

Roncek, Dennis W. 2000. "Schools and Crime." In *Analyzing Crime Patterns: Frontiers of Practice*, edited by Victor Goldsmith, P. McGuire, J. Mollenkopf, and T. Ross. Thousand Oaks, CA: Sage Publications.

Shepherd, Joanna M. 2005. "Deterrence Versus Brutalization: Capital Punishment's Differing Impacts among States." *Michigan Law Review* 104: 203-255.

Sherman, Lawrence W. and David Weisburd. 1995. "General Deterrent Effects of Police Patrol in Crime 'Hot Spots': A Randomized Study." *Justice Quarterly* 12: 625-648.

Sherman, Lawrence W., and Richard A. Berk. 1984. "The Specific Deterrent Effects of Arrest for Domestic Assault." *American Sociological Review* 49: 261-272.

Sherman, Lawrence W. 1992. *Policing and Domestic Violence.* .New York: Free Press.

Sherman, Lawrence W., and John E. Eck. 2002. "Policing for Prevention." In *Evidence Based Crime Prevention*, edited by Lawrence W. Sherman, David Farrington, and Brandon Welsh. New York: Routledge.

Sherman, Lawrence W., Patrick Gartin, and Michael Buerger, E. 1989. "Hot Spots of Predatory Crime: Routine Activities and the Criminology of Place." *Criminology* 27: 27-55.

Sherman, Lawrence W. and Douglas A. Smith. 1992. "Crime, Punishment, and Stake in Conformity: Legal and Informal Control of Domestic Violence." *American Sociological Review* 57: 680-690.

Shi, Lan. 2009. "The Limits of Oversight in Policing: Evidence from the 2001 Cincinnati Riot." *Journal of Public Economics* 93: 99-113.

Sirakaya, Sibel. 2006. "Recidivism and Social Interactions." *Journal of the American Statistical Association* 101: 863-877.

Sjoquist, David. 1973. "Property Crime and Economic Behavior: Some Empirical Results." *American Economic Review* 63: 439-46.

Spelman, William, and Dale K. Brown. 1981. *Calling the Police: A Replication of the Citizen Reporting Component of the Kansas City Response Time Analysis*. Washington, DC: Police Executive Research Forum.

Spelman, William. 2000. "What Recent Studies Do (and Don't) Tell Us about Imprisonment and Crime." In *Crime and Justice: A Review of Research*, vol. 27, edited by Michael Tonry. Chicago: University of Chicago Press.

Starmer, Chris. 2000. "Developments in Non-Expected Utility Theory: the Hunt for a Descriptive Theory of Choice Under Risk." *Journal of Economic Literature* XXXVIII: 332-82.

Steffensmeier, Darrell J. and Jeffrey T. Ulmer. 2005. *Confessions of a Dying Thief*. New Brunswick, N.J.: Aldine/Transaction Publishers.

Stolzenberg, Lisa, and Stewart J. D'Alessio. 1997. "'Three Strikes and You're Out': The Impact of California's New Mandatory Sentencing Law on Serious Crime Rates." *Crime and Delinquency* 43: 457–69.

Post, Thierry, Martijn van der Assem, Guido Balthussen and Richard Thaler. 2008. "Deal or No Deal? Decision Making Under Risk in a Large-Payoff Game Show." *American Economic Review* 98: 38-71.

Waldfogel, Joel. 1994. "The Effect of Criminal Conviction on Income and the Trust 'Reposed in the Workmen'." *Journal of Human Resources* 29: 62-81.

Webster, Cheryl Marie, Anthony Doob, and Franklin Zimring. 2006. " Proposition 8 and Crime Rates in California: The Case of the Disappearing Deterrent." *Criminology and Public Policy* 5:417:448.

Weisburd, David, Shawn Bushway, Cynthia Lum, and Su-Ming Yang. 2004. "Trajectories of Crime at Places: A Longitudinal Study of Street Segments in the City of Seattle." *Criminology* 42: 283-320.

Weisburd, David, and John Eck. 2004. "What Can Police Do to Reduce Crime, Disorder, and Fear?" *Annals of the American Academy of Political and Social Science* 593: 42-65.

Weisburd, David, Tomar Einat, and Matt Kowalski. 2008. "The Miracle of the Cells: An Experimental Study of Interventions to Increase Payment of Court-Ordered Financial Obligations." *Criminology and Public Policy* 7:9-36.

Weisburd, David and Lorraine Green. 1995. "Policing Drug Hot Spots: The Jersey City Drug Market Analysis Experiment." *Justice Quarterly* 12: 711-735.

Western, Bruce. 2002. "The Impact of Incarceration on Wage Mobility and Inequality." *American Sociological Review* 67: 526-546.

Western, Bruce, Jeffrey R. Kling and David F. Weiman. 2001. The Labor Market Consequences of Incarceration. *Crime and Delinquency* 47: 410-427.

Williams, Kirk R. and Richard Hawkins. 1986. "Perceptual Research on General Deterrence: A Critical Overview." *Law and Society Review* 20: 545-572.

Wilson, James Q. and Richard Herrnstein. 1985. *Crime and Human Nature: The Definitive Study of Causes of Crime.* New York: Simon and Shuster.

White, Jennifer L., Terrie E, Moffitt, Avshalom Caspi, Dawn J. Bartusch, Douglas J. Needles, and Magda Stoutheimer-Loeber. 1994. "Measuring Impulsivity and Examining Its Relationship to Delinquency." *Journal of Abnormal Psychology* 103: 192-205.

Wolfgang, Marvin, Robert Figlio, and Thorsten Sellin. 1972 *Delinquency in a Birth Cohort.* Chicago: University of Chicago Press.

Zimring, Franklin E., and Gordon J. Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control.* Chicago: University of Chicago Press.

Zimring, Franklin E., Gordon J. Hawkins, and Sam Kamin. 2001. *Punishment and Democracy: Three Strikes and You're Out in California.* New York: Oxford University Press.