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The Deterrent Effect
of Imprisonment

Steven N. Durlauf and Daniel S. Nagin

1.1 Introduction

This chapter 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 summary 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 the magnitude of deterrent effects depends critically on the specific form of the sanction policy. In particular, there is little evidence that increases in the severity of punishment yield strong marginal 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 increases in the certainty of punishment produce substantial deterrent effects. 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. Many studies show that the police, if mobilized in ways that materially heighten the risk of apprehension, can exert a substantial deterrent effect. There is also evidence that if the parole and probation systems are similarly deployed they too can exert a substantial deterrent effect. Thus, one policy relevant implication of our conclusions is that lengthy prison sentences, particularly in the form of mandatory minimum type statutes such as California's Three Strikes Law cannot be justified based on their deterrent effect on crime. In fact, our review suggests a stronger implication: it is possible that crime rates can 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 and parole and probation monitoring systems. 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. We take the perspective that the "economic way of looking at behavior" (Becker 1993) has much to commend it for the study of crime and for interpreting psychological and sociological ideas in ways to enhance the perspective.

The chapter is organized as follows. We begin by laying out what we refer to as the "baseline" economic model of crime due to Gary Becker. The Beckerian model provides a framework for our discussion of empirics. We then turn to a review of the literature and our interpretation of implications. Our discussion closes with an assessment of policy implications and directions for future research including expansion of the baseline model.

1.2   The Economic Model of Crime

In order to provide a conceptual framework for our discussion, we employ a version of 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 approach. Much of the criticism of Becker's model, especially by noneconomists, amounts to criticisms of the ways in which the crime choice is delineated. In fact we will argue that what might, given the existing deterrence and imprisonment literature, appear to be empirical limitations of the economic approach to

crime are remedied in a straightforward fashion by alternative formulations of the same choice-based logic that is the basis of Becker's model.

A very simple variant of the Becker model may be constructed as follows. In formulating this baseline model we think of a single cross section of choices made across a population at a fixed point in time; the fact that sentences are served over time and crime/no crime choices are made throughout the life course will be ignored. We will discuss the implications of dynamic versions of the model later. 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; that is, a choice between C and NC.[1] 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

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

From the perspective of criminal sanctions, this elementary calculation highlights the two distinct aspects of crime sanction policy that are the appropriate focus of scholarly research on deterrence: p, 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; we treat the sentence length as a sufficient statistic for the penalty associated with conviction and do not explicitly account for the fact that a sentence is served over time. We return to this issue later. This allows us to rewrite the condition for commission of a crime as

(2) $$p(U_C(Z_i, L) - U_{NC}(Z_i)) + (1 - p)(U_C(Z_i, 0) - U_{NC}(Z_i)) > 0.$$

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. We can think of individual crime choices as binary functions $\omega(Z_i, p, L)$, with 1 denoting crime and 0 no crime such that

1. We abstract away from richer descriptions of the crime decision such as Becker's original (1968) formulation, which considers the number of offenses a potential criminal will commit in a time period, and Ehrlich (1973) who explicitly considers the allocation of available time between criminal and noncriminal activity.

(3) $\omega(Z_i, p, L) = 1$ if equation (2) holds; 0 otherwise.

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

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

For this simple specification, 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

(5) $$\phi(\Pr(C|p, L)).$$

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

(6) $$\lambda(p) + \mu(I)$$

where the variable I, defined as

(7) $$I = \Pr(C|p, L)pL$$

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 imprisonment.

How should a policymaker choose among possible (p, L) pairs? Rather than solve for the optimal pair that requires consideration of a budget constraint for total law enforcement expenditure, 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, conditioning on this value 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 concave function of L; that is, 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." An agent who chooses to commit a crime faces an expected sentence length pL and will prefer to trade p against L when the marginal disutility of sentence length is increasing in the level of the length. Further, since a lower p reduces policing costs $\lambda(p)$ and must also reduce prison costs as it minimizes $\Pr(C|p, L)$ given constant pL, it hence minimizes $\mu(I)$. This is the basis of Becker's conclusion that efficient sanction policy leads to relatively low punishment probabilities and long sentences. In terms of interpreting

the relationship between sentence policy and deterrence, Becker's analysis concludes that, for a locus defined by $pL = K$, deterrence effects are greater, ceteris paribus, for higher L values *so long as criminals are risk averse along this locus.*

Becker's conclusion about optimal sanction policy should not be interpreted as meaning that severity is more important than certainty in deterrence; it is obvious from the structure of the decision problem that the two interact nonlinearly. When we evaluate evidence on the effects of marginal changes in severity and certainty, it is important to keep these interactions in mind. In particular, differences in estimated magnitudes of marginal deterrence effects from severity may be explained by differences in the background certainty levels; the converse may also hold.

In referring to this model as a baseline, we do Becker a partial injustice in that there are dimensions along which one can alter the structure we have described, while at the same time fully preserving the choice-based logic that underlie Becker's analysis. A simple example is the concavity of $U_C(Z_i, L)$; this assumption has no bearing on the interpretation of crime choices as determined by expected utility maximization.[2] While alterations in various assumptions in the baseline may change conclusions concerning the relationship between certainty, severity, and efficient punishment regimes, they do so via the same reasoning pioneered by Becker.

1.3   Empirics

There have been three 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 statewide 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; Tittle 1969; 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 Blumstein, Cohen, and Nagin (1978), 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

2. Polinsky and Shavell (1999) provide a comprehensive analysis of how optimal sanction policy changes according to whether $U_C(Z_i, L)$ is concave, linear, or convex.

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 first generation literature, for example, assumed that demographic or socioeconomic characteristics such as percentage of males aged fourteen to twenty-four or mean years of schooling of persons over twenty-five or per capita public safety expenditures lagged one year causally affected sanction levels but did not causally affect crime rates. Studies that fall under this criticism include Avio and Clarke (1976), Carr-Hill and Stern (1973), and Ehrlich (1973). The examples we have listed are examples of what Sims (1980) dubbed "incredible" identifying assumptions and are now recognized as an inadequate basis for making causal claims in social science. 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, that is, crimes per capita, and the denominator of the measure of certainty of punishment, that is, prison admissions per crime (Nagin 1978).

   In response to these deficiencies, two subsequent waves of crime/deterrence research emerged, each of which is an ongoing literature. First, starting in the 1990s a number of authors began to use time series methods developed in the econometrics literature to understand the temporal relationship between imprisonment and crime. This new group of studies continued to use states as the unit of observation but unlike the first generation studies that primarily involved cross-sectional analyses of states, this second generation of studies had a longitudinal component. 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. Another distinct modern research program also emerged that focuses on the effect of police resources on crime rates, 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 review these two modern literatures separately by first considering studies that have attempted to link aggregate crime and imprisonment rates, and second, considering studies that have considered the effects of criminal sanction policy on crime.

1.3.1   Aggregate Studies Relating Imprisonment Rate to the Crime Rate

An important recent review by Donohue (2009, table 9.1) identifies six published articles that examine the relationship between aggregate crime rates and imprisonment rates. Each of these studies finds a statistically significant negative association between imprisonment rates and crime rates, and each has 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 (Liedka, Piehl, and Useem 2006),[3] to an elasticity of –0.4 (Spelman 2000). 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.

Donohue (2009), in the context of generating a cost-benefit analysis of imprisonment, discusses the heterogeneity of elasticity estimates. He argues that values in 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 percent of the incarcerated population was 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 overstatement of the magnitude of the crime prevention effect of the state level imprisonment rate. Second, Donohue is sympathetic with the arguments of Liedka, Piehl, 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, Piehl, and Useem, but is also mirrored in parameter estimates based on constant coefficient models in which the absolute magnitude of 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 the estimated parameter that is observed.

While the literature relating crime rates to imprisonment rates has served

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3. Liedka, Piehl, and Useem (2006) explicitly allow for the marginal effect of imprisonment on the crime rate to depend upon the scale of imprisonment. They do this by regressing crime rate on quadratic and spline functions of the lagged imprisonment rate. Their analysis implies that by the 1990s the preventive effect of imprisonment in some states (e.g., California) had diminished to a negligible level and perhaps was even criminogenic.

the valuable purpose of resuscitating interest in the crime prevention effects of imprisonment, we are less sanguine about the usefulness of this body of literature than Donohue. Our more critical stance stems from both statistical and theoretical considerations. Four 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 and arguably launched the literature on which we focus.[4] 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 does the word causality as understood in microeconometrics and elsewhere in economics and criminology. 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 definition of causality that is relevant to understanding policy effects.[5] 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,

4. In terms of delineating the joint time series properties of crime and imprisonment, Spelman (2008) is state of the art, using the data series studied by Marvell and Moody and subjecting them to a wide range of time series analyses. That said, the Spelman paper follows the tradition established in Marvell and Moody in terms of focusing on the joint stochastic process of crime and imprisonment and from this drawing conclusions about policy.

5. Marvell and Moody recognize that there are interpretation issues regarding Granger causality. They argue that their results can be interpreted in the sense of counterfactuals. However, their arguments in this regard are informal and mathematically incorrect. For example, they remark on "the possibility that omitted variables could create a spurious regression, but the presence of lagged values of the dependent variable presumably controls for such variables" (122–3). This claim is incorrect except for the nongeneric case where the omitted variables are linearly dependent on the lagged dependent variables; in general, all lagged variables can be useful in constructing empirical proxies for the omitted variables and hence both sets of lags will have nonzero coefficients in the bivariate VAR. It is straightforward to construct examples where this occurs.

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 that the criminal justice system creates simultaneity between the two series.

As we have said, these criticisms do not uniquely apply to Marvell and Moody (1994). Becsi (1999) runs panel regressions of state level crime indices normalized by the national US level against a set of controls including a one year lagged measure of the state population share of convicts relative to the national level.[6] Spelman's (2000) analysis employs Granger causality ideas, arguing two things. First, Spelman (2000, 456) states that "Because the Granger test is explicitly a test of causality, it is critical that exogenous variables be somehow controlled for" and subsequently produces results (458) titled "Controls Improve Interpretability of Granger Test Results." Second, he argues that the differencing to address trends (Spelman 2000, 456) "Although . . . not a perfect solution . . . is probably sufficient to clarify the general direction of causality." Neither proposal addresses the distinction between Granger causality and causality that is understood in policy counterfactuals. Liedka, Piehl, and Useem (2006) employ Granger tests to justify causal interpretations of regressions of crime rates against lagged imprisonment rates and controls; while they make a valuable contribution in assessing the constancy of parameters in these regressions, they follow the same approach to causality as Marvell and Moody.

Even when the limitations of nonstructural time series methods are explicitly acknowledged, the proposed solutions are inadequate. Spelman (2005) attempts a different strategy for using aggregate regressions to study crime by focusing on counties in Texas. In this analysis, changes in county-specific crime rates are regressed against changes in public order arrest and incarceration rates and some set of controls. The arrest and incarceration rates are then instrumented using lagged values of variables such as police resources, republican voting, and jail capacity. No explanation is given as to why these are valid instruments; that is, why they should not appear in the original crime regression. We will address this issue in more detail later.

Levitt (1996) is the single aggregate study reviewed by Donohue that constructively addresses the simultaneous interdependences between crime

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6. Becsi (1999) acknowledges that his analysis suffers from interpretation problems. His footnote 17 states "Using lagged variables is perhaps the simplest way of dealing with the simultaneity bias inherent in empirical time series analysis." Yet he follows this with, "One problem with this method is that it may not adequately represent dynamic interrelationships in the data and may in particular miss serial correlation effects." But these remarks ignore the post hoc ergo propter hoc problem associated with nonstructural time series regressions of the type he employs.

and imprisonment rates through a principled argument on instrumental variable validity. 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. Even here one can question instrument validity. Liedka, Piehl, and Useem (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. A natural response to this criticism is that Levitt's analysis includes tests that bear on instrument validity. Levitt's analysis is based on multiple forms of the overcrowding instrument that reflect the stage to which the overcrowding litigation had progressed, which allows for tests of over identifying restrictions. These tests support Levitt's contention that the overcrowding litigation has no direct effect on crime rate but only work through the level of imprisonment. Klick and Tabarrok (2010) assert that Levitt's tests of over identifying restrictions have low power, so that the validity of his instruments can only be assessed on a priori grounds. However, in fairness to Levitt, Klick and Tabarrok do not empirically demonstrate any lack of power in Levitt's analysis; in our view a priori arguments on instrument validity tend to be stronger than a priori arguments about test power, as power depends on details of the data generating process whereas instrument validity often involves economic or other social science theory.

In our view, the primary concern with the Levitt analysis involves the question of what the findings tell us about the ability of sanction policies 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 composition of the prison and civilian populations via a change in sanction regime (Nagin 1998). What Levitt establishes, in our view persuasively, 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, let alone establish the mechanism by which the reduction occurred from the policy that was in fact implemented. For example, it is reasonable to believe that criminals responded directly to the litigation as signaling a reduction in either the certainty or severity of punishment or both; without knowing how beliefs changed, it is difficult to assess exactly what is learned from Levitt's exercise relative to the question of deterrence versus incapacitation.[7]

7. Even if one interprets Levitt's findings exclusively in terms of incapacitation, extrapolation of his findings is difficult. It may be the case 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. This possibility is plausible. It is very difficult to parole old prisoners who have committed very serious crimes because of objections by the victim or the victim's family and the public in general.

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 outcome of the interplay of sanction policies dictating who goes to prison and for how long with all other determinants of the crime/no crime decision. 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 1.2, 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. Heckman (2000, 2005), for example, has famously (and we believe correctly) remarked that "causality is a property of a model of hypotheticals . . . A model is a set of counterfactuals defined under the same rules" (2005, 2).[8] For us, "the same rules" constitute a description of individual decisionmaking and the counterfactuals refer to fully delineated punishment regimes.[9]

This problem is evident when one specifically considers the limits of 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

8. 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 decision making are necessary to make claims about the effects of alternative policies.

9. 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 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.

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 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 greater than –1, an increase in either of these variables will result in an increase in prison population, whereas if the elasticity is less than –1, an increase will result in a reduced prison population. The indeterminacy in the sign of the relationship between p, L, and I implies the possibility of a Laffer curve-style relationship between a given sanction variable 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.

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 to provide 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 particular choice of variables. For example, indicators of state level economic conditions were not included, yet these are natural proxies for an individual's opportunities if he chooses not to commit a crime. To be clear, Marvell and Moody are not alone in making arbitrary variable choices on what to include and not include in the model. The basic problem is what Brock and Durlauf (2001b) have called theory openendedness. In the imprisonment case, theory openendedness means that the prediction that criminal sanctions affect crime rates is consistent with many theories of criminality, so that empirical evidence of the importance of one explanation can only be 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 finding must be evaluated against every variable that enters a researcher's imagination. Judgments are inevitable in empirical

work, including judgments on the plausibility of various controls. Rather our claim is that because there is no settled theory on the causes of crime let alone the appropriate way to quantify these causes, choices about control variables in the deterrence literature are necessarily ad hoc to some degree and so the influence of such judgments needs to be assessed.

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 form 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 context 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.[10] The trend variables employed in crime regressions 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 of the presence of persistence in the model's residuals; that is, 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 provides 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 from trend can be justified when there is no theoretical basis for the modeling of crime trends. Further, there can be substantive prior information that implies that stationarity is violated. 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 presumably affects the dynamic correlation structure between crime rates and other variables beyond simply introducing a correlated unobservable. Yet another source of ad hocness concerns the use of linear regressions to model discrete decisions; Durlauf, Navarro, and Rivers (2010) discuss how the aggregation of individual crime decisions into linear regressions that are used to explain crime rates requires strong assumptions on the details underlying individual decision problems. Aggregation issues further turn out to call into question the interpretation of instrumental variables for aggregate crime regressions, an issue we do not pursue here.

10. This claim is distinct from the question of cycles in crime rates; Philipson and Posner (1996) is an example of a model that produces equilibrium crime rate cycles. Our argument concerns time-varying deterministic components.

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, and is indeed rejected by Spelman (2005) for his joint time series analysis of crime and prison rates. Further, 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]).[11] 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; that is, nonlinearities are rarely systematically examined, with Liedka, Piehl, and Useem (2006) representing an important exception. The importance of nonlinearities for deterrence is also suggested by Shepherd (2005) who found that the signs of state level estimates of capital punishment effects depended on the level of executions that are carried out.

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 (e.g., 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 (2008) for conceptual

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11. 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 an overview.

discussion of the role of assumptions in crime regressions and Cohen-Cole et al. (2009) 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. For example, Lott's (1998) response to Black and Nagin (1998) and Dezhbakhsh 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. One limitation of many randomized experiments 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.[12]

Our discussion of obstacles to making valid causal inferences about the effects of sanctions on crime from panel data on heterogeneous geographic units should not be interpreted to mean that we view such studies as having no value. To the contrary, as we indicated earlier, the studies relating imprisonment rates to crimes have served the extremely valuable purpose of reopening research on the deterrent effect of imprisonment. We do, however, hold the position that the uncertainties that are inherent in most inferences based on panel data across heterogeneous geographic units such as states are sufficiently large that conclusions from such studies should be treated with great caution until they are confirmed by alternative study designs that are less subject to the inferential challenges inherent to panel studies of aggregate-level crime data.

Studies of the effects of imprisonment exist against a background of large sustained increases in imprisonment. Blumstein and Beck (1999) and Raphael and Stoll (2009) have closely scrutinized the primary sources of the increases in imprisonment over the past four decades. Both reviews conclude that the primary reason for the growth in prison populations during this period has been increased punitiveness. For the last twenty-five years, Raphael and Stoll conclude that only 15 percent to 20 percent of the increase in the overall US incarceration rate is due to increased crime rates. Blumstein and Beck (1999) who focus on state imprisonment rates from 1980 to 1996 conclude that none of the increase in state level incarceration rates for nondrug-related

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

offenses during this period is due to increased crime rates. Both papers find that increased certainty and severity each played a major role in the rise in incarceration. We therefore turn to studies that examine particular mechanisms by which the criminal sanction regime affects crime rates.

1.3.2   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. However, these studies are of great value because of their focus on the effects of individual policies. Studies based on individual policies do not require that policy effects are constant across locations and, depending on the nature of the policy change, provide evidence that is more likely to be uncontaminated by the presence of unobserved heterogeneity. We note that for the case of severity, aggregate crime regressions are infeasible for the simple reason that aggregate data on the severity of punishment is unavailable.

The earliest post-1970s attempts to measure severity effects analyzed the deterrent impact of sentence enhancement for gun crimes. A series of studies conducted by Loftin, McDowall, and colleagues (Loftin and McDowall 1981; Loftin, Heumann, and McDowall 1983; Loftin and McDowall 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 fail to uncover evidence of a deterrent effect (but see McDowall, Loftin, and Wiersema 1992).[13] The generally null findings may reflect that gun-using criminals did not respond to the incremental increase in severity. However, Loftin, McDowall, 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, which mandated a minimum sentence of twenty-five years upon conviction for a third strikeable offense. Zimring, Hawkins, and Kamin (2001) conclude that the law at most reduced the felony crime rate by 2 percent. 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

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13. McDowall, Loftin, and Wiersema (1992) combine data from the different locations they had previously studied for evidence of a deterrent effect of sentence enhancements. While none of the individual site analyses produced evidence of a deterrent effect, the combined analysis did. For several reasons we are skeptical of the combined analysis. First, it is vulnerable to many of the criticisms we have leveled at aggregate regression analyses. Second, their finding that at the individual sites the laws were ineffective in increasing sentence length suggests that the null findings at the individual sites were not a result of a lack of statistical power that might be remedied by combining data across sites. Third, the combination of results from different studies involves ad hoc statistical assumptions that are a separate source of possible nonrobustness.

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, Hawkins, and Kamin (2001) finding of a potential deterrent effect among individuals with two strikeable offenses accords with the results of Helland and Tabarrok (2007), a study that we regard as particularly well crafted. This analysis focuses exclusively on 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 nonstrikeable 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 in studies of this type, the interpretation of the findings in terms of the marginal deterrence effects of the three strikes law is contingent on the comparability of the two groups 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.[14] Another reason for noncomparability may be that those convicted of a nonstrikeable offense are simply better criminals than those convicted of strikeable offenses in the sense that they are better able to generate alibis, avoid leaving evidence, and so forth, and so were convicted of lesser offenses than those of which they were, in fact, guilty. Our own view is that the concerns raised by these possible sources of heterogeneity are sufficiently speculative that we find the Helland and Tabarrok results to still be persuasive. 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

14. We thank Philip Cook for this observation.

policing that primarily affects the certainty of punishment are likely to yield far larger crime reduction benefits. We return to this observation later.[15]

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 1990s. 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 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 percent decline in crime attributable to deterrence in the first year after enactment. Within five to seven years the effect grows to a 20 percent reduction. The longer term estimate includes incapacitation effects. Indeed, Kessler and Levitt acknowledge that the incapacitation effect may dominate the deterrent effect.

Webster, Doob, and Zimring (2006) challenge the basic finding of any preventive effects. Kessler and Levitt examine only data from every other year. When all annual data are used, Webster, Doob, and Zimring (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. This critique has not been resolved: see Levitt (2006) for a response, and further commentary supportive of aspects of Webster, Doob, and Zimring by Raphael (2006). In our view, the strongest critique of the Kessler and Levitt analysis concerns the assumption that the time series properties of either crime rates unaffected by Proposition 8 in California or the equivalent Proposition 8 crime rates for other states can, via comparison with the crime rates in California for crimes affected by Proposition 8, be used to uncover the effect of Proposition 8. To be fair, both Kessler and Levitt and Levitt are very clear that comparability is a judgment call. Our judgment is that Raphael, in particular, makes a strong argument against comparability.[16]

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15. Shepherd (2002) 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.

16. We also come to this conclusion because Kessler and Levitt do not provide a clear conceptualization of what is meant by a comparable data series. It appears that they are assuming that if a group of states follow a similar crime trend to California pre-Proposition 8 they are comparable. Variance, however, may be just as important as trend. Suppose that both series are white noise, but that shocks to the California series are two times the value of shocks to the other states. If Proposition 8 had no real effect; that is, the reduction in California's crime rate was a function of a series of draws of shocks, then under the assumptions of this example, the reductions would be twice the other states. One can construct similar examples if the degree of dependence in the series is different.

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 eighteen, 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. First, 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, something that Lee and McCrary note. Second, the Lee and McCrary analysis is based on individual level data and so avoids interpretation problems that can arise from aggregation (Durlauf, Navarro, and Rivers 2010). Further, the individual-level data employed by Lee and McCrary are of particular interest because of the common discontinuity in severity faced by all individuals at age eighteen and the fact that the exact ages of arrested individuals are identified, allowing one to pinpoint very short-term effects of the discontinuity on criminal behavior.

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 must be negative consequences. Much research in the perceptual deterrence literature, which surveys individuals on their sanction risk perceptions and intentions to offend, finds that perceived severity of sanction consequences are inversely related to self-reported offending or behavioral intentions to offend (Nagin 1998). This research, however, also makes clear that perceptions of severity are tied in complex ways to attachments to family, friends, and the legal labor market (Nagin and Paternoster 1991, 1993). It also finds that unlike perceptions of the risk of apprehension, perceptions of sentence length are generally not associated with self-reported offending.

It is important to note that most research on sentence length involves increases in already long sentences. There is some evidence that Massachusetts' Bartley-Fox gun law mandating a one year prison sentence for unlawful carrying of a gun may have been a deterrent (Wellford, Pepper, and Petrie 2005). Further, we will discuss experiments that show short but certain incarceration deters. We thus see a need for research on the likely nonlinear relationship between deterrence and severity.

1.3.3   Studies of the Effect of Certainty of Punishment

Severity alone, of course, 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 that follows on evidence pertaining to the certainty of punishment focuses mainly 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.[17]

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 postcrime 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 involves 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 Andenaes (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 three distinct literatures. One set of studies 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 body of work, based on regression discontinuity designs, examines the effects of abrupt changes

17. Several studies conducted in the 1970s examined the deterrent effect of conviction risk, usually measured by the ration of convictions to changes (Avio and Clark 1976; Carr-Hill and Stern 1973; Sjoquist 1973). These studies suffered from a number of important methodological limitations including, most importantly, their treating conviction risk as exogenous.

in police presence. A third research program has focused on the crime 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.

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 data sets, one composed of forty-nine states for the years 1968 to 1993 and the other of fifty-six large cities for the years 1971 to 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 comes 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 policymakers 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 fifty-nine large cities for the years 1970 to 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 and finds large preventive effects of police on violent crime and smaller, but still significant, effects on property crime. However, in a reanalysis of Levitt's data, McCrary (2002) corrects technical problems in Levitt's analysis and finds no significant preventive effect of police on crime. In a reply and new analysis, Levitt (2002) uses an alternative identification strategy based on the number of firefighters and civil service workers and obtains similar elasticity estimates to his original analysis. More recently, Evans and Owens (2007) examine the crime prevention effects of police by analyzing hiring and crime reduction effects associated with federal subsi-

dies 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.

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—is associated with reductions in crime. This negative association is interpreted as reflecting the deterrent effect of police presence. 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.

How should these aggregate police/crime regressions be evaluated in light of our criticisms of the aggregate crime and imprisonment regressions? On some dimensions, the police/crime regressions are clearly more persuasive. Unlike the imprisonment regressions, all of the studies we cite that employ aggregate police regressions ask a meaningful policy question; changes in policing are subject to policy choice in a way that the imprisonment rate is not. Further, there is less emphasis on Granger causality in the police regressions literature than the imprisonment literature; so while Marvell and Moody (1994) explicitly use Granger causality notions and Corman and Mocan (2000) do so implicitly, neither Levitt (1997, 2002) nor Evans and Owens (2007) fall into the misinterpretation of marginal time series predictive power as evidence of causality in a counterfactual sense. It is true that the studies we have cited suffer from issues of theory openendedness and lack of attention to robustness with respect to the way variables are measured, the possibility of parameter heterogeneity across geographic units, theory, and the difficulties of distinguishing between deterrence and incapacitation. Still, it is noteworthy that with the important exception of McCrary's reanalysis of the data used in Levitt (1997), panel-based studies fairly consistently find evidence of a preventive effect of the police. So, read in isolation, the evidentiary strength of the studies is limited by inadequate attention to model uncertainty.

On the other hand, the findings of aggregate police regressions are consistently replicated in a number of tests of police effects of crime via particular policy changes. This is not true for imprisonment/crime studies in the sense that the time series evidence is not systematically matched by evidence that particular penalty enhancements are efficacious. As in the case of targeted studies of severity, we are well disposed to these targeted studies of police, in this case because they provide a more transparent test of the effect of police presence on crime, are less subject to biases that may attend analyzing data across a highly heterogeneous set of cities, and are less likely to measure incapacitation effects.

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 2010), 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 (2009) 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 Tabarrok (2005) study examines the effect on crime of the color-coded alert system devised by the US Department of Homeland Security in the aftermath of the September 11, 2001, terrorist attack to denote the terrorism threat level. Its alert system's 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) is especially interesting because of its use of 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. One limitation of their finding concerns general equilibrium effects, which we raised in the context of experiments. Their evidence of lower crime during higher police presence 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.

Cohen and Ludwig (2003) take a third approach by studying the outcomes

of policies by the Pittsburgh Police Department, which 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 accords with the conclusions of hot spots policing literature discussed later.

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. So in this sense, they are something of a black box. 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 that 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.

In considering the effect of police on apprehension risk, it is important to recognize that there is heterogeneity in the effects of alternative police deployment tactics on apprehension risk. As will be discussed, some tactics appear to be very effective whereas others seemingly have no effect. Even more important, apprehension risk itself is a heterogeneous quantity. A given police deployment strategy may differentially affect offender types (e.g., gang members versus nongang members) or crime types (drug dealing versus robbery).

One class of strategies for affecting apprehension risk involves the way the police are mobilized once a crime is reported. 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.

Another strategy in this class involves the implementation of mandatory actions when the police are called onto the scene of a crime. 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, Schmidt, and Rogan (1992) and Sherman et al. (1992) propose a theoretical explanation called defiance theory to explain the status-based heterogeneity in response to mandatory arrest.

A second class of strategies involves the deployment of police resources in a city. We distinguish this class of policies from the first class as the latter involves the way the police respond to a reported crime, whereas this strategy is one that establishes police locations and procedures in light of the characteristics of crime in the area under consideration. 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.[18] 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 a variety of ways, but Weisburd and Eck give special attention to policing strategies that 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, Gartin, and Buerger

18. An implication of this type of strategy is that 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 that were not acted upon by potential offenders because the risk was deemed too high (see Cook 1979 for more discussion).

(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. (2006) 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, Gersh, and Taylor (2000), and Roncek (2000). As 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.[19]

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 to 13 percent. In another randomized experiment, Weisburd and Green (1995) found that hot spots policing was similarly effective in suppressing drug markets and Weisburd et al. (2006) found no evidence that hot spots policing simply displaced crime to nearby locations. It is important, however, to note that these experiments do not test long-term effectiveness. Even if in the short term there is no displacement, over the long-term new hot spots may emerge in response to the suppression of prior hot spots.

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 US 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 nontargeted locales. We also note that the findings from the previously described econometric studies of focused police actions, for example in response to terror alert

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19. Zenou (2003) provides a theoretical analysis that explains the spatial concentration of crime via the interplay of social interactions and economic opportunities and provides corroborating empirical evidence.

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 (POP). Problem-oriented policing comes in so many different forms that (like pornography) it is regrettably hard to define, but the essence of POP is devising strategies for increasing apprehension risk or reducing criminal opportunities (see Cook and MacDonald, chapter 7, this volume) that are tailored to address the crime problem at a specific location or involving a specific type of activity (examples include targeting open air drug markets or focusing on the protection of adolescents being victimized going to and coming from school).

Weisburd et al. (2010) conduct a review of the POP evaluations and report overwhelming support for its effectiveness. While the great majority of evaluations are of very low quality—little more than before and after studies—they identified ten studies with credible designs (i.e., randomized experiments or quasi-experiments with credible control comparisons). Eight of the ten studies report statistically significant reductions in crime. For several reasons the findings are notable for our purposes here. First, effect sizes vary considerably across interventions, a finding that reinforces our argument that police-related deterrent effects are heterogeneous—they depend on how the police are used and the circumstances in which they are used. A second and related point is that two of the interventions involved monitoring of probationers to avert probation revocation due to reoffending or violation of conditions of parole. This highlights the point that police can be effectively used to deter crime not only at high-risk locations but also among high-risk individuals.

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 the 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.

The observation that police can be used to affect the criminality of high-risk individuals brings us to another relevant literature—field interventions

in which sanctions are specifically focused on high-risk groups. Like POP tactics, all of the interventions are multifaceted but deterrence-based tactics are a core feature of each. In all cases the deterrence component of the intervention involved an attempt to make sanction risk certain and salient to a selected high-risk group. In our judgment these interventions deserve special attention because they provide a useful perspective on the promise and uncertainties of such focused deterrence-based interventions.

We begin by summarizing the findings of an underappreciated randomized experiment by Weisburd, Einat, and Kowalski (2008) that tests 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 common feature of treatment conditions involving incarceration was a high certainty of imprisonment for failure to pay the fine. However, the fact that Weisburd, Einat, and Kowalski 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.

The deterrence strategy of certain but nondraconian sanctions has been applied with apparently great success in Project Hope, an intervention heralded in Mark Kleiman's (2009) highly visible book *When Brute Force Fails.* Project Hope is a Hawaii-based probation enforcement process. In a randomized experiment probationers assigned to Project Hope had much lower rates of positive drug tests, missed appointments, and, most importantly, were significantly less likely to be arrested and imprisoned. The cornerstone of the HOPE intervention was regular drug testing, including random tests, and certain but short punishment periods of confinement (i.e., one to two days) for positive drug tests or other violations of conditions of probation. Thus, both the Weisburd, Einat, and Kowalski (2008) fine experiment and Project Hope show that highly certain punishment can be an effective deterrent to those for whom deterrence has previously been ineffective in averting crime.

The strategy of certain punishment is also a centerpiece of field interventions in Boston, Richmond, and Chicago that are specifically aimed at reducing gun violence. However, unlike Project Hope and the fine-paying experiment, the certain punishment is far more draconian—a very lengthy prison sentence. For descriptions of a Boston intervention called Operation Ceasefire see Kennedy et al. (2001), for the Richmond intervention called Project Exile see Raphael and Ludwig (2003), and for the Chicago-based intervention see Papachristos, Meares, and Fagan (2007). A common feature of each intervention was commitment to federal prosecution for gun crimes which, upon conviction, allowed for very lengthy prison sentences. Notably,

there were also concerted efforts to communicate the threat of certain and severe punishment to selected high-risk groups (e.g., members of violent gangs). All interventions claimed to have substantial success in reducing gun crime but at least in the cases of Boston and Richmond questions have been raised about whether the declines preceded the intervention or were no different than other comparable urban centers (Cook and Ludwig 2006; Raphael and Ludwig 2003). These concerns notwithstanding, each of these interventions illustrate 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.

1.4   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. These findings do not argue against the value of the economic approach, but rather suggests dimensions along which the modeling of beliefs and preferences should be generalized relative to standard formulations.

In understanding why certainty might trump severity in criminal decision making, we first return to the fact that the implications of our formulation of the Becker crime model concerning the relative efficacy of certainty and severity depended on assumptions about the concavity of $U_C(Z_i, L)$. 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. The role of the timing of benefits and punishments to crime was recognized early in Cook (1980). Polinsky and Shavell (1999) provide a formal and very complete demonstration of the importance of timing in understanding certainty/severity tradeoffs; here we provide the basic intuition underlying the ideas in these papers. In thinking about the effects of penalties on individuals, it is necessary to consider the commission of a particular crime at a particular date 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 equation (2) are in fact a sequence of expected discounted utilities over the future, in which the commission of a crime (or lack thereof) represents one element of a dynamic choice problem. This dynamic choice problem calls into question the assumption that $U_C(Z_i, L)$ is concave since the function is appropriately understood as depending on the degree to which future utility is discounted. A marginal increase in sentence length affects utilities starting at time $t + L$. If utility between times t and $t + 1$ 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 concavity should hold for $U_C(Z_i, L)$. 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 chapter to analyze a lifetime utility model in which agents consider a sequence of crime/no crime decisions. Models of this type are developed in Imai and Krishna (2004) and Lee and McCrary (2005, 2009).[20] Imai and Krishna study an environment in which sentence length is constrained to be one time unit, which allows them to estimate the model in absence of sentencing data. The sentence length assumption means that issues of severity certainty tradeoffs cannot be addressed; on the other hand, this paper is of particular interest in terms of understanding how individual heterogeneity affects crime decisions. Lee and McCrary allow for variable sentence length and study the effects of certainty and severity via calibration of parameters to match various aggregate crime statistics. The additional theoretical richness of Lee and McCrary comes at the expense of a less rich version of individual heterogeneity. These important papers illustrate the possibility that dynamic structural analyses of crime can play a valuable role in the study of sanctions policies. The differences in the modeling assumptions between the papers also illustrate some of the defects of the current crime statistics, an issue we address later.

The embedding of our initial model into a dynamic framework is fully consistent with the view of economic actors as rational, purposeful decision makers 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 awway from the particular rationality assumptions with which we have so far worked. An example of why this may be needed is the finding in Lee and McCrary (2009) that the shift from juvenile to adult penalties has little deterrent effect; this is not explainable by the fact that the additional penalty occurs later in life, but speaks to something about the way that future consequences are considered. To formalize this intuition, we distinguish between deviations our original rationality postulates based on the ways in

20. Earlier examples of dynamic crime choice models include Polinsky and Shavell (1998) who show how, in a two-period version of Becker's model, optimal sanction policy may imply that the severity of a penalty depends on previous convictions. Polinsky and Shavell's finding reinforces the idea that sanction severity should not be reduced to a scalar in considering alternative punishment regimes.

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; White et al. 1994). One of the most prominent findings in this literature is the linkage between crime and impulsive behavior in noncriminal 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 frotal 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," individuals who attend mainly to incentives and disincentives over a short rather than long time horizon, are more prone to crime. Psychologists working in this theoretical tradition have devised many tests of impulsivity that 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 decision makers that do not naturally arise on standard geometric discounting. Lee and McCrary (2005) provide a formal analysis of the effects of hyperbolic discounting on crime choices; see the discussion in Utset (2007). As shown by Lee and McCrary, hyperbolic discounting 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 decision making.

Questions of discounting are logically distinct from those concerning the formation of beliefs about the future and the ways in which these beliefs affect decisions; in our context, the key variable being the probability of punishment p. Our baseline formulation assumed that expected utility calculations are linear in these objective probabilities. There are a number of reasons to question this assumption. One reason may have to do with bounded rationality. While there is a growing body of evidence that individuals update their sanction risk perceptions based on past experiences in successfully and unsuccessfully avoiding apprehension in a fashion that at least crudely

approximates Bayesian updating (Lochner 2007; Hjalmarsson 2008; Anwar and Loughran 2009), there is also a large body of evidence that perceptions of risk diverge substantially from actual risk with most people overestimating the actual risk.[21] This constitutes one reason why the way in which p appears in equation (2) may be empirically inadequate. In such contexts, it might make sense to work with subjective probabilities $p^i(p)$ that depend on the objective probabilities but do not equal them.

Further, in the modern decision theory literature, many arguments have been made that expected utility calculations should be replaced with nonexpected utility alternatives in order to better describe actual decision making. A number of these alternatives involve replacing p in equation (2) with a probability weighting function $\pi(p)$,[22] which means that the choice to commit a crime requires

(8) $\pi_i(p)(U_C(Z_i, L) - U_{NC}(Z_i)) + (1 - \pi_i(p))(U_C(Z_i, 0) - U_{NC}(Z_i)) > 0.$

We will focus on the implications of the use of probability weighting functions for crime decisions. While the replacement of objective with subjective probabilities can be mathematically equivalent to the use of probability weighting functions (as occurs when subjective probabilities follow $p^i(p)$), the interpretation of the two approaches is quite different.[23]

With respect to assessing the effects of sanction policy changes, the key difference between equation (8) and the baseline model equation (2) is that the marginal effect on the payoff to criminality with respect to changes in certainty of punishment is changed to

(9) $$\frac{d\pi_i(p)}{dp}(U_C(Z_i, 0) - U_C(Z_i, L)).$$

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 $d\pi_i(p)/dp$; in the baseline model this term always equals one. Hence, evaluating a sanctions policy should perhaps include consideration of how changes in objective probabilities affect choices via the probability weighting function. Evaluating a sanctions policy under both expected and nonexpected utility approaches to decision making can provide a check

21. See Apel and Nagin (2010) for a summary of evidence on sanction risk perceptions.

22. Starmer (2000) provides a very clear discussion of nonexpected utility theories that lead to the use of probability weighting functions; see also Machina (1987) for a very accessible overview of modern approaches to decision theory.

23. The nonexpected utility literature focuses on formulations of individual preferences that do not lead to equation (2) as the description of decision making under uncertainty. One example is rank-dependent expected utility thory (Quiggin 1982), in which the payoff to a particular outcome is affected by its relative ranking compared to other possible outcomes. Nonexpected utility models do not require that individuals fail to assess the consequences of actions using objectively correct probabilities. Rather, they lead to formulations of the effects of these probabilities that differ from equation (2).

on the robustness of the effects of the sanctions to uncertainty about the decision-making process.

Unsurprisingly, there is a large empirical literature that has studied the properties of $d\pi_i(p)/dp$ in a range of experimental contexts.[24] At the risk of oversimplifying a complex body of work, considerable evidence exists that many individuals tend to overweight small probabilities and underweight large probabilities, relative to standard expected utility calculations. More specifically, there appears to be good evidence that the probability weighting function follows an inverse S-shape, which means that

(10) $$\frac{d\pi_i(p)}{dp} \text{ is large if } p \text{ near 0 or 1.}$$

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 an additional candidate explanation 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 et al. (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; that is, being punished for a crime. However, as far as we know, there does not exist a body of research that focuses on the properties of probability weighting functions among that part of the population in which the decision to commit a crime is close to marginal. This strikes us as a valuable area for future work.[25] And of course, if the appropriate deviation from equation (2) is bounded rationality rather than nonexpected utility, one needs to know the functional form that relates subjective beliefs to objective probabilities for those whose decisions on criminality are near the margin in order to draw conclusions about the effects of changes in certainty.

While impulsivity, discounting, and generalizations of the role of probabilities in determining individual decisions 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

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24. Starmer (2000) reviews the experimental evidence.

25. There are findings in the behavioral economics literature that would suggest modifications of the baseline Beckerian model beyond discounting and the probability weighting functions. For example, Post et al. (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 that we possess.

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 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 and 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, Figlio, and Sellin (1972) find that through age eighteen, 6 percent 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 reimprisoned. 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 seventy-five largest cities, at the time of arrest 32 percent of defendants had an active criminal justice status, such as probation (15 percent), release pending disposition of a prior case (10 percent), or parole (5 percent). Further, 76 percent of all defendants had been arrested previously, with 50 percent having at least five prior arrest charges.

There are two very different interpretations of these statistics. One is that the high concentration of recidivists 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 system, 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. The difficulties in disentangling them may be seen in a recent study by Drago, Galbiati, and Vertova (2009) of Italy's Collective Clemency Bill. In May of 2006, this bill resulted in the release of more than 20,000 inmates from Italian prisons. The release came with the condition that individuals convicted of another crime within five

years of their release would have to serve the residual of the sentence that was suspended in addition to the sentence for the new crime. The residual sentence length varied between one and thirty-six months. Drago, Galbiati, and Vertova (2009) find that each month of residual sentence was associated with 1.2 percent reduction in the propensity to recommit crime. The authors interpret this finding as a deterrent, but an alternative and equally valid interpretation is that each additional month of imprisonment increases the propensity to offend by 1.2 percent.[26] The respective roles of these distinct explanations cannot be identified.

Moving beyond interpretation problems for a particular study, there are good reasons to think that the severity of a punishment does a poor job of summarizing the effects of incarceration on an individual. 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. 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 increases postrelease, noncriminal income earning opportunities (Layton MacKenzie 2002). Other types of rehabilitation are designed to increase the capacity for self-restraint in the presence of situations such as a confrontation that might provoke a criminal act such as violence (Cullen 2002).

There are, however, a number of reasons for theorizing that the experience of punishment might increase an individual's future proclivity for crime. One argument relates to the effect of the experience of crime on expectations about the prison experience. 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. Other reasons have to do with the social interactions induced by imprisonment. Prisons might be "schools for crime" where inmates learn new crime skills even as their noncrime human capital depreciates. Associating with other more experienced inmates could lead new inmates to adopt the older inmate's deviant

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26. We thank Philip Cook for this important insight on the alternative interpretation of the Drago, Galbiati, and Vertova (2009) study.

value systems or enable them to learn the tricks of the trade (Hawkins 1976; Steffensmeier and Ulmer 2005). Being punished may also elevate an offender's feelings of resentment against society (Sherman, Schmidt, and Rogan 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 (1989a, 1989b)). 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 (1993) have called attention to the role of legal sanctions in what they call the process of cumulative disadvantage. Official labeling 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; Pager 2003). Sampson and Laub (1993) 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 1994; 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

$$(11) \quad p(U_C(Z_i, CR_i, L) - U_{NC}(Z_i, CR_i))$$
$$+ (1 - p)(U_C(Z_i, CR_i, 0) - U_{NC}(Z_i, CR_i)) > 0,$$

which is 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 equation (3)

$$(12) \qquad \omega(Z_i, CR_i, p, L) = 1 \text{ if equation (11) holds; 0 otherwise.}$$

The equilibrium crime rate will now equal

$$(13) \qquad \Pr(C|p,L) = \int \omega(Z,CR,p,L)dF_{Z,CR} =$$
$$\iint \omega(Z,CR,p,L)dF_{Z|CR}dF_{CR}.$$

The double integral in equation (13) is a mixture density and represents averages (based on population weights) across the crime probabilities at each level of prior criminal record in the population; that is, probability weighted averages of the criminal record-specific crime rates $\int \omega(Z,CR,p,L)dF_{Z|CR}$.[27] By allowing the expected utility of a crime choice to depend on an individual's criminal record, it is evident that a criminal record can 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 individual.

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; rather, the possibility is implicitly embedded in $\omega(Z,0,p,L)$. Therefore, the presence of experience of punishment effects does 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 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.[28] The condition under which a crime is chosen is in this case

$$(14) \quad p(U_C(Z_i,L,I) - U_{NC}(Z_i)) + (1 - p)(U_C(Z_i,0,I) - U_{NC}(Z_i)) > 0.$$

27. Notice that the distribution of Z will typically differ between subpopulations with different criminal records. Any causal argument that a criminal record is criminogenic needs to account for this heterogeneity, which may not be observable to the analyst.

28. In addition to Sirakaya (2006), which is noteworthy for its econometric sophistication, a number of papers have suggested the presence of social interactions in crime in which the criminal choice of one person depends on the criminal behavior of others within one's community; Glaeser, Sacerdote, and Scheinkman (1996) is an early eample while Bayer, Hjalmarsson, and Pozen (2009) is a recent analysis based on an unusually detailed data set. This type of interaction may involve stigma as well. In terms of formal modeling, one way to model this

If stigma means that $U_C(Z_i, L, I)$ 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 $I = P(C|p,L)pL$, 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 [2001c] and Durlauf and Ioannides [2010] 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 provides 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 relative efficacy of certainty versus severity. That said, we believe there is good intuition why experience of imprisonment should 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 no more than 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, $I = P(C|p,L)pL$ is a nonlinear function of p and L, a differential may simply follow from this. Another reason why stigma leads to this differential effect 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.[29] 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 others make

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type of interaction, following Brock and Durlaf (2001a), 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 in determining interdependences in crime choices. See Calvó-Armengol, Verdier, and Zenou (2007) and Ballester, Calvó-Armengol, and Zenou (2010) for theoretical analyses of how network structure can affect aggregate crime and Patacchini and Zenou (2009) for empirical evidence that network structure matters.

29. 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.

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 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.

1.5   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 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 specific policy-relevant implication of this general conclusion is that lengthy prison sentences, particularly those that take 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. They must be justified based on either incapacitation benefits or along retributive lines. While we have not surveyed 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 and also probation and parole monitoring systems designed along the lines of Project Hope. However, even such an apparently self-evident conclusion may be difficult to translate into a defensible operational plan beyond 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 nonhuman capital, better training, and so forth. The econometrics literature on police resources and crime rates provides very little guidance on how those resources should be utilized. Likewise, there has yet to be a demonstration that probation and parole monitoring systems designed along the lines of

Project Hope can be replicated with comparable results. The success of the monitoring system clearly depends upon the enthusiastic support and coordinated efforts of judges, parole/probation officers, and the police. The question of the mechanism by which the resources would be transferred has also not been addressed. Corrections are, by and large, a state and federal function whereas policing is, by and large, a local function.

Put differently, the details of the policy for cutting back on sentences and shifting the resources to policing and probation and parole supervision 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 greatly. This literature has assembled an impressive body of evidence that the so-called standard model of policing that involves the nonstrategic use of preventive patrols, rapid response to calls for service, and improved investigation methods is not effective in deterring crime (National Research Council 2004; Weisburd and Eck 2004).

However, more strategic use of police—hot spot policing for example, 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 that in our judgment will be most effective in delineating the details of a policy that will achieve this objective.

One area that warrants more research concerns the explicit analysis of the costs and benefits of different combinations of certainty and severity. As noted earlier, if the elasticity of the crime rate with respect to either certainty or severity is less than –1, then one can simultaneously reduce both crime and imprisonment by increasing the policy variable. Additional routes emerge when the policies are considered together. Recall from equation (7) that the elasticity of I, the imprisonment rate, with respect to p is

$$(15) \quad \frac{d \log I}{d \log p} = \frac{d \log C \cdot p \cdot L}{d \log p}$$

$$= \frac{d \log C}{d \log p} + \frac{d \log p}{d \log p} + \frac{d \log L}{d \log p} = \frac{d \log C}{d \log p} + 1,$$

and that elasticity of the imprisonment rate with respect to L is

$$(16) \quad \frac{d \log I}{d \log L} = \frac{d \log C \cdot p \cdot L}{d \log L}$$

$$= \frac{d \log C}{d \log L} + \frac{d \log p}{d \log L} + \frac{d \log L}{d \log L} = \frac{d \log C}{d \log L} + 1.$$

Suppose that one increases L by 1 percent and increases p by 1 percent. Equations (15) and (16) imply that crime rate and the imprisonment rate change by the same amount:

$$(17) \qquad \frac{d \log C}{d \log p} - \frac{d \log C}{d \log L}.$$

But this means that so long as the elasticity of the crime rate with respect to certainty is smaller than the elasticity of the crime rate with respect to severity, the crime and imprisonment rates will *both* decrease under this policy change. Now, this calculation does not account for the relative costs of this shift from certainty to severity and so does not answer the question of whether this shift from severity to certainty is overall efficient. But the example illustrates how differential crime elasticities may be exploited to reduce both crime and imprisonment rates.

What types of deployment strategies are good candidates for reducing both crime and imprisonment? In terms of increases in certainty, 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 in Kennedy (2009) and Kleiman (2009) have the common feature of targeting enforcement resources on selected places or people. 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. As we have already noted, the fine payment experiment conducted by Weisburd, Einat, and Kowalski (2008) and the Project Hope experiment make clear that the imminent threat of incarceration is a powerful incentive for paying delinquent fines and for conforming with conditions of probation. These experiments suggest that sanction need not be draconian to deter proscribed behavior. As we noted earlier, there may be a nonlinear relationship between the magnitude of deterrent effects and sentence lengths. Sentence lengths in Western European countries tend to be far shorter than in the United States. For example, over 90 percent of sentences in the Netherlands are less than one year (Nieuwbeerta, Nagin, and Blokland 2009). Research based in European data on the deterrent effect of shorter sentence length should be a priority.

These speculations indicate the importance of additional research on the crime rate elasticities for different policies. In addition, the costs of tradeoffs between various policies are very much underresearched.

Our review made brief references to a large literature on sanction risk perceptions. Most of this literature is outside of economics (but see Lochner 2007; Hjalmarsson 2008). Economic research on crime would benefit from giving closer attention to the origins and development of sanction risk perceptions as they relate to experience with committing crime, frequency of contact with the criminal justice system, the objective characteristics of the quality of a criminal opportunity (e.g., proximity of the police), and the punishments prescribed by criminal statutes. Returning to a distinction we drew earlier between this probability and $p^i(p)$, the subjective probability of potential criminal i of imprisonment, one can imagine that different policies that have equivalent effects on p inducing different deterrent effects. For example, raising the speed of responses to 911 calls may have a lower effect on subjective probabilities than greater street presence even if each has the same effect on p. In cases where changes in p are not accompanied by effects on $p^i(p)$, 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.

As emphasized in Nagin (1998) deterrent effects are ultimately determined by perceptions of sanction risk and severity. We have already shown how allowing for a divergence between perceived and actual probability of punishment in the economic model of crime provides an interesting theoretical explanation for why the deterrent effects of certainty changes might be larger than in the standard model. The noneconomic literature on sanction risk perceptions shows that there is little correspondence between perceptions and reality. This is not surprising for at least two reasons. First, for most people knowledge of actual sanctions is not relevant because for moral, social, and/or economic reasons they are not even remotely close to the margin of committing crime. Second, sanction risks and severity are not posted like most market prices. Instead, for the criminally inclined, they must be learned from experience or word of mouth. This is why the work of Lochner (2007), Hjalmarsson (2008), and Anwar and Loughran (2009) on Bayesian updating of sanction risk perceptions is so important and should be extended. There is also a small body of research which examines how the characteristics of criminal opportunities affect sanction risk perceptions (Klepper and Nagin 1989a, 1989b). More work of this type would also be desirable particularly as it relates to how police deployment affects perceptions of apprehension risk.

Research on the deterrent effect of sentence length and more generally about the effects of changes in sentencing statutes on crime rates and imprisonment rates is seriously hampered by the lack of data on the distribution of sentences lengths and time served by different types of offenders across states. Without such evidence, it is impossible to assess the effects of features of the punishment regime, for reasons we have discussed and that flow

immediately from the Beckerian crime model.[30] Such data can be assembled for selected states from prison census data. Prison census data should be expanded to include all fifty states and should be made available in an easily accessible and manipulable format.

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. Thus, we 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 et al. (2009), the distribution of deterrent effects across a space of seemingly second-order changes in regression specification can lead to estimates of net lives saved per execution 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

30. It is also remarkable that aggregate studies of the deterrent effect of capital punishment are conducted without data on the sentences served by those convicted of a potential capital offense but who are not sentenced to death.

as in medicine where a portfolio of treatments is required to address hetero-geneous 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 stud-ies 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 earlier, we do not believe such studies are valueless. They clearly can help buttress qualitative conclusions about certainty versus severity. But for aggregate studies to pro-vide 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.[31] We do not raise Snow for reasons of pedantry; his work is a lasting example of how careful nonexperimental data analysis can produce successful policy interventions and is a useful standard against which to think about regres-sion 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 decision making under ambiguity. But independent of this, the uncertainty about particular policies does not exist in a vacuum; in particu-lar, 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 decision making 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 decision making 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.

31. David Freedman, a deep and often harsh critic of contemporary empirical practice in social science, regarded Snow's work as an exemplar of good research; see Freedman (1991).

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