The Silence of the Lambdas: Deterring Incapacitation Research

Thomas J. Miles · Jens Ludwig

Abstract   This essay provides an economist's perspective on criminological research into incapacitation effects on crime. Our central argument is that criminologists would do well to substantially scale back the enterprise of trying to estimate the various behavioral parameters central to a micro-level approach to measuring incapacitation effects, including the annual rate of offending outside of prison (λ) and the lengths of criminal careers. One problem with this line of research is practical: for example, mean estimates of self-reported criminal activity by incarcerated prisoners are quite sensitive to reports by the most criminally active offenders. But the larger concern is conceptual—the incapacitation effects from a given change in sentencing policy may be undermined by the possibility of replacement effects, and at the same time omit other benefits that may arise from deterrent effects on crime. A more promising approach is to identify plausibly exogenous changes in sentencing policy in order to estimate the net impact on crime from the combined effects of incapacitation, deterrence and replacement.

Keywords   Incapacitation · Deterrence · Replacement · Incarceration · Sentencing

T. J. Miles (✉)
The University of Chicago Law School, 1111 East 60th Street, Chicago, IL 60637, USA
e-mail: tmiles@law.uchicago.edu

J. Ludwig
University of Chicago, 969 E. 60th Street, Chicago, IL 60637, USA
e-mail: jludwig@uchicago.edu

J. Ludwig
NBER, 1050 Massachusetts Avenue, Cambridge, MA 02138, USA
e-mail: jludwig@uchicago.edu

Introduction

This essay provides an overview of how economists (or at least how two economists) think about research on incapacitation effects. We assume that the primary objective of research in this area should be to help improve the efficacy of criminal justice policy. Perhaps the most important criminal justice question of our time is whether the current scale of incarceration in the United States is optimal, given the nearly seven fold increase in the US prison population over the past 30 years (Sentencing Project, 2005). The prevailing view of both economists and criminologists is that at the present level of incarceration, the costs may exceed the benefits. The benefits from adding one more prisoner to the system decline as the incarceration rate increases, because the system tends to prioritize the worst offenders and the stigma of punishment may erode as prison becomes more common. In addition concerns about the costs of incarceration are only likely to increase given the growing pressures on state and federal budgets. The social costs of incarceration may be even greater than the direct budget costs given the disproportionate representation in prisons of young minority males.

With the goal of guiding criminal justice policy in mind, the central argument of this essay is that criminologists should scale back the enterprise of trying to estimate the behavioral parameters central to a micro-level approach to measuring incapacitation effects, including the annual rate of offending outside of prison (λ or lambda) and the lengths of criminal careers. One problem with this line of research is the difficulty of estimating these parameters (Zimring and Hawkins 1995, Piquero et al. 2003, Piquero and Blumstein 2007). Moreover for policy purposes a single "lambda" is not enough, since the effects of a change in sanction policy are probably contingent on the specific policy intervention and target population (Nagin 1998). Whether these practical challenges can ever be solved is very much an open question.

But the larger concern with incapacitation research is conceptual—its objective is inadequate. Estimates of incapacitation effects from a given change in sentencing policy may be undermined by the possibility of replacement effects (Cook 1986a, Ehrlich 1981, 1996), and at the same time omit other benefits that may arise from deterrent effects on crime. In their companion essay, Alex Piquero and Alfred Blumstein argue that "the overall goal of this paper is to not only jump-start the discussion of [how to better estimate] incapacitation effects, but also to stimulate new [incapacitation] research efforts" (2006). Our goal is the opposite of this.[1]

A more promising path to policy-relevant information is to search for plausibly exogenous changes in sentencing policy and then focus on estimating the net benefits and costs of these policy shifts, which at least in principle capture the combined effects of incapacitation, deterrence and replacement. In most settings, an estimate of the net effect of the three mechanisms speaks more effectively to policymakers than a well-expressed estimate of one mechanism and silence on the other two. Generating unbiased estimates along these lines is extremely difficult in its own right, as discussed in gruesome detail nearly 30 years ago by the National Research Council's review of the early deterrence literature (Blumstein et al. 1978). But, these challenges are not insurmountable. They seem less daunting than those involved with estimating the micro-parameters associated with pure incapaci-

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[1] Others have also pointed out the problems with the incapacitation literature in criminology; see for example Zimring and Hawkins (1995) and Donohue and Siegelman (1998). Yet the interest in estimating pure incapacitation effects in criminology persists, perhaps in part because criminologists do not accept the conceptual challenges posed by the possibility of deterrence and replacement effects.

tation effects, and, ultimately, evidence for overall net impacts is much more useful for policy.

We agree with incapacitation researchers that the design of policy is improved by understanding the degree to which the effects of changes in prison policy operate through the distinct mechanisms of incapacitation, replacement and deterrence. But if forced to choose, for policy purposes we would prefer "black box" estimates of the net effects of a prison policy on crime rather than incomplete estimates of these benefits in the form of incapacitation effects alone. Fortunately in some applications incapacitation and deterrence effects yield different empirical predictions that permit decomposition of the net effect into the relative contributions of deterrence and incapacitation.

Our belief in a tradeoff between estimating lambda versus net incarceration effects is itself a reflection of our economics orientation. After all, a criminologist might argue: Why not pursue *both* lines of research? Our answer is that research time, funding and talent are scarce. There is only one National Institute of Justice and one Alfred Blumstein. Criminal justice policy would be better served if the former funded the latter to estimate the net effects on crime from some specific change in sentencing policy rather than to launch a massive new effort to estimate lambda. A difficult-to-estimate but useful parameter in our view dominates a difficult-to-estimate less useful parameter.[2]

The paper proceeds as follows. Section "Conceptual Framework" presents our conceptual framework that describes the different mechanisms of action through which changes in prison policy may affect criminal behavior. Section "Incapacitation Research in Criminology" briefly reviews the fundamental problems of estimating lambda, with a focus on the key conceptual limitations. Section "Incarceration Research in Economics" discusses the more promising alternative "natural experiment" approach, including a review of the small number of economic studies conducted to date that try to distinguish deterrence and incapacitation effects. Part V concludes.

Conceptual Framework

Economists typically proceed from the assumption that the appropriate framework for the analysis of criminal justice policies is generally benefit-cost analysis (BCA). Even economists recognize that some criminal justice applications involve questions of constitutional rights and human dignity which lie outside the scope of BCA. Economists also recognize that criminal law and criminal justice implicate many phenomena such as expressive effects that are exceedingly difficult to quantify in the common metric required by BCA (dollars).[3] For the types of policies that we discuss here—the scale of incarceration and the

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[2] Jeffrey Smith reminds us that if there are diminishing marginal returns to any activity then the optimal research portfolio will include both pure incapacitation studies and "natural experiment" studies. Given the relative rarity of criminological studies that attempt to estimate the net effects on crime from incapacitation, deterrence and replacement using plausibly exogenous natural experiments, there is room for a substantial shift in research activities from the former to the latter.

[3] BCA requires that benefits and costs be converted to a common dollar metric. This is the approach that Becker (1968) took in articulating the modern economic framework for analyzing social policy toward crime and that Ehrlich (1981) took in articulating the first economic model distinguishing deterrence and incapacitation. All subsequent economic research on crime has also adopted this approach (e.g., Donohue and Siegelman 1998). This metric is imperfect because important aspects of human experience, such as emotions, do not translate well into monetary equivalents.

length of sentences—BCA seems a reasonable evaluative framework; however benefits and costs that cannot be monetized should not be ignored.

A complete social welfare analysis considers all mechanisms through which a public policy may furnish benefits and all the costs incurred by the policy. The benefit that criminal justice policies typically seek to provide is a reduction in crime. Research in criminology and economics has identified three primary mechanisms through which prison policies may affect crime rates: (1) incapacitation; (2) replacement; and (3) deterrence. We argue that studies of prison policy should consider all three mechanisms.

Incapacitation

Incapacitation is the inability of an incarcerated person to commit additional offenses. Incapacitation provides a social benefit of reduced crime when the incarcerated person would commit additional offenses if not confined (and if replacement effects are not complete). The incapacitation benefit of a prison policy is the product of two quantities: the change in the period of confinement and the rate at which the incarcerated would have offended during that period. A policy can change the aggregate period of confinement by changing the length of time that current offenders remain incarcerated, or by altering the number of offenders confined while holding the length of sentences constant, or some combination of the two. The second variable—what criminologists commonly refer to as lambda—is the counterfactual rate at which inmates would have offended had they not been incarcerated. As discussed below, criminologists have devoted considerable efforts to measuring lambda, showing that—unsurprisingly—lambda varies with offender demographics, especially age, as well as with the rate of incarceration in the population as a whole.

Deterrence

A second potential benefit of a prison policy is deterrence. The magnitude of a general deterrent effect depends on the elasticity or responsiveness of offending in the population to punishments, and also on the specific policy intervention used to change punishment (Zimring and Hawkins 1973). Economists and criminologists contest not only the magnitude of deterrence, but also its very existence.

For deterrence to operate requires that *some* members of the population of potential offenders respond to incentives and be aware of relevant changes in the incentives to commit crime. It is not necessary that all criminals be "perfectly rational" or avidly track the Federal Register for changes in sentencing rules.[4] Evidence that many or even most people at risk for criminal behavior are drunk, ignorant, or dim is not sufficient to categorically dismiss the possibility of a deterrent effect.

The notion that deterrence can be ignored in the evaluation of changes in sanction policy is also inconsistent with a large body of evidence suggesting that as a general matter, people respond to incentives at least in the aggregate.[5] But is this true for crime-

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[4] For example as Cook (1980) notes, experience within one's social network provides a plausible alternative mechanism for deriving information about sentencing policies.

[5] When the Earned Income Tax Credit increases the after-tax wage at the bottom of the income distribution there is a resulting change in labor supply by low-income workers (Eissa and Liebman,1996). When parents

prone people as well? As Cook (1986a, p. 18) notes in a related context, denying that criminals respond to incentives "is tantamount to claiming that potential criminals … are fundamentally different from everyone else, if indeed there is anyone who can be excluded from the 'potential criminal' category."

As we discuss below, the evidence for deterrence in the context of incarceration is far from conclusive, but cannot be dismissed entirely. The evidence for the deterrent effect of policing is particularly suggestive, as some of this evidence comes from randomized experiments (Sherman 2002, Levitt 2002). Moreover even if none of the current evidence on deterrence could withstand scrutiny, the presumptive rejection of the possibility of deterrence would be inappropriate because it would risk misguiding policy.

Replacement

A third mechanism may diminish the benefits of longer prison terms: replacement effects. This possibility is most intuitive within the context of drug offenses. So long as there are users who demand illegal drugs, some suppliers will be enticed to enter the trade by available profit opportunities.

The economic model for the "market" for criminal offenses developed by Cook (1986a) and others suggests that replacement may be a concern for criminal activity more generally. In this model the "supply" of criminal activity is an upward sloping function of the net payoff (loot minus expected punishment), while the "demand" for criminal activity slopes downward as potential victims increase protective activities in response to increased risks of victimization. The removal of some offenders from a neighborhood shifts the supply for offenses schedule inward, which expands the criminal opportunity set for other potential or current offenders. The more lucrative criminal opportunities that the now-incarcerated offenders would have taken become available, raising the net returns per offense for non-incarcerated persons (Cook 1986a; Ehrlich 1981, 1996). A higher incarceration rate in a particular neighborhood may make offending relatively more attractive for remaining residents. Whether the crimes committed by newly entering offenders completely replace those committed by the incarcerated is uncertain, and depends in part on the elasticity of the demand for offenses schedule and the lambda of the marginal inmate relative to the marginal offender.

To a first approximation, the net benefits of a prison policy are therefore the incapacitation and deterrent benefits, less the replacement costs.[6] The next section focuses on the importance and difficulties of measuring the net impact of these three mechanisms. But we pause here to acknowledge that, except for replacement effects, we have not dwelled on the costs of a prison policy, and yet consideration of costs is crucial to setting policy. The usual practice is to focus on the direct monetary costs to the government of the marginal prison bed. But also relevant are less apparent costs, such as the deadweight losses resulting from raising tax revenue to finance these expenditures, the social costs to families and

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Footnote 5 continued

have a strong tax incentive to have their babies before January 1, there is an increase in the likelihood that children are born during the last week of December rather than first week of January (Dickert-Conlin and Chandra 1999). When malpractice liability declines, doctors are less likely to practice defensive medicine (Kessler and McClellan 1996). When the costs of obtaining an abortion are reduced through legalization, abortion rates increase (Ananat et al. 2006).

[6] Note also that another potential mechanism by which a prison policy may influence the crime rate is the extent of private precautions (Philipson and Posner 1996). The evidence on this point is extremely limited.

communities from the mass incarceration of potential husbands and fathers, the opportunity costs including the forgone income that an offender would have earned from the legitimate sector, and the negative effect that confinement has on his human capital or even health (Johnson and Raphael 2006).[7]

Incapacitation Research in Criminology

The cost benefit framework is conceptually clear but difficult to implement. One problem is that social scientists tend to focus exclusively on the operative mechanisms favored by their discipline (itself evidence that individuals respond to incentives). In examining the effect of prison policy, economists often focus exclusively on deterrence, while criminologists often restrict their attention to incapacitation. Some criminologists, such as Doob and Webster (2003), reject the evidence that more severe sanctions have crime-reducing effects. More frequently, criminologists focus solely on the incapacitation effect of incarceration and especially the estimation of lambda.[8]

Difficulties of Estimating Lambda

Piquero and Blumstein's (2007) companion essay discusses in detail the practical problems associated with lambda and other incapacitation micro-parameters. Here we highlight just a few of the problems that seem practically insurmountable.

Incapacitation research cannot avoid relying on self-reports of criminal activity by people who, by virtue of their criminal conviction and imprisonment, have revealed that they are willing to engage in anti-social and untrustworthy behavior.[9] The problem of misreporting is particularly acute since the measure of central tendency most relevant for BCA, the mean lambda, is highly sensitive to the treatment of high-risk offenders—some of whom report literally hundreds of offenses per year. As a result, predictions about how many crimes are prevented by pure incapacitation effects hinge on a relatively small number of observations. It is not obvious how more research funding or attention will ever be able to solve this problem.

Another fundamental challenge comes from drawing inferences about the effects of marginal changes in sanction policy. Criminologists have recognized that inmates are a non-random sample of the offending population, because irrespective of any targeting of enforcement to high-risk offenders, more frequent offending increases a criminal's exposure to the risk of arrest and imprisonment (Canela-Cacho et al. 1997). This

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[7] Perhaps too obvious to mention is that the largest opportunity cost is often the net benefit of the alternative policy that could have been pursued in lieu of the chosen policy. If for example the marginal $1 billion in the federal government goes to new prisons rather than the NIH then the opportunity cost of the marginal expansion to federal prisons could in principle be the failure to cure some dreadful disease.

[8] Not all criminologists believe that research on lambda is useful. For example, Laub and Sampson (2001) characterize this research as having attained a "point of stagnation ... because of its narrow focus on measurement and policy."

[9] The alternative approach of relying on administrative arrest data is widely recognized to under-state criminal activity, and any effort to project from arrests to crimes will employ some inflation factor that necessarily requires some use of survey reports about criminal activity.

"stochastic selectivity" implies the offending rates of arrestees are biased estimates of the offending rate of the general population and of the benefits of incapacitation.[10]

These patterns of selection have several implications about the care with which lambda estimates should be used in recommending policy. First, as incarceration levels rise, the offending rate of the marginal offender falls (Zimring and Hawkins 1988). Second, when prisons have excess beds, the relevant lambda for BCA is that of the marginal convict. Third, when prisons are filled to capacity, the relevant lambda is that of the marginal releasee.[11]

Even if the difficulties of self-reports, skewness, sampling, and selection could be overcome, lambda is only one of several parameters necessary to guide policy. Lambda is one aspect of criminology's conception of a life-cycle pattern of offending, or the criminal career. Other dimensions include the onset of offending, escalation to more serious offenses, and ultimately the termination of offending or desistence. For each of these dimensions of criminal careers a similar set of measurement difficulties arise (Laub and Sampson, 2001).

Relevance of Lambda for Public Policy

The primary policy implication of the measurement of lambda is selective incarceration, or the identification and incapacitation of high-risk offenders or so-called "career criminals." Even if estimates of lambda could accurately identify the highest risk offenders with sufficient speed to permit the criminal justice system to prevent the commission of crimes, lambda estimates would still have limited use to decision-makers.

Most sanction policies have the potential to produce a bundle of effects—deterrence, incapacitation, and replacement. These effects manifest themselves as both costs and benefits of the policy. For a decision-maker contemplating a choice between competing policies or the modification of an existing policy, it is typically the entire bundle of effects, rather than a single effect, that is relevant. A failure to consider the full bundle of effects may be counterproductive. For example, Cook (1986b) demonstrated that an incarceration policy focused on selective incapacitation can lead to more crime compared to a regime that allocates punishment more uniformly across convicted offenders. If most crime is committed by infrequent offenders, selective incapacitation of the highest-rate offenders may have the effect of reducing the perceived deterrent threat faced by the majority of criminals. Consequently, offending by "low-lambda" offenders may increase and partially or entirely offset any incapacitation benefits from selectively incarcerating the "highest-lambda" offenders.

General estimates for incapacitation effects are unlikely to provide information about the combined effects from deterrence, incapacitation and replacement, since the bundle of impacts produced is likely specific to the policy change in question (Nagin, 1978). For example, policy that requires 6 months in prison for every 20-year-old convict will have different effects if eligibility is limited to violent felons rather than all felons. Similarly, a policy that doubles the odds that 20-year-olds convicted of violent felonies spent 6 months

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[10] Donohue and Siegelman (1998) make the parallel point about comparing the stock of inmates and the flow of inmates who are about to be released. Offenders with shorter sentences are those more likely to be released, and their offending rates are surely lower than those who receive longer sentences. The average lambda of inmates will therefore be a biased estimate of the offending rate of the marginal releasee and will overstate the benefit of delaying release dates.

[11] While some criminologists, such as Canela-Cacho et al. (1997), have focused on selection as offenders enter the criminal justice system and filter through its stages, less often have criminologists developed estimates for offenders who are filtering out of the system.

in prison should generate a larger deterrent effect than a policy that doubles the average sentence length for 20-year-old violent felons from 6 months to 12 months, because criminals appear to be more sensitive to changes in the certainty rather than severity of punishment. The effects of either of these policy changes may depend in part on how they are implemented, such as whether law enforcement officials conduct a public relations campaign to notify offenders of the sentencing changes. Also, their effects may vary across communities, such as Chicago versus downstate Illinois, and this point is especially relevant for state legislators considering how to allocate the next dollar in the state prison budget. A single estimate for lambda—or even a series of age-adjusted lambdas—would provide incomplete and potentially even misleading information for policymakers forced to choose among these different policy options.

Incarceration Research in Economics

Given the practical and conceptual problems associated with a focus on estimating pure incapacitation effects, we argue for broadening the focus of criminological research to rely more on finding policy experiments or other sources of exogenous variation in prison policies or practices to estimate the bundle of impacts.

To believe that the economics approach is more promising for policy than the effort to identify pure incapacitation effects in criminology is not to absolve the economics research of its sins. Economists sometimes attributed all of an observed impact of incarceration on crime to deterrence, and were often quick to conclude causal impacts from correlational evidence.[12] And economists and their allies sometimes take an overly narrow view of the costs and benefits associated with incarceration.

But more recently economists have attempted to correct these shortcomings by focusing on the natural experiment approach (Cook 1980), where the goal is to "exploit situations where the forces of nature or government policy have conspired to produce an environment somewhat akin to a randomized experiment" (Angrist and Krueger 2001, p. 73). Put differently, the goal is to mimic true randomized experiments by searching for changes in criminal justice "treatments" (sentencing policies) that are plausibly unrelated to the other determinants of criminal activity.

Angrist and Krueger (2001, p. 73) argue that good natural experiments "often come from detailed knowledge of the economic mechanism and institutions determining the regressor [i.e., treatment] of interest."[13] Because the average criminologist has far superior institutional knowledge of how the criminal justice works compared to the average economist studying crime, identification and estimation of "natural experiments" to uncover the causal effects of incarceration in general, and incapacitation versus deterrence effects in particular, represents an unusually productive possibility for collaboration across these two disciplines (see Bushway and Reuter 2005).

Another challenge to conducting research in the natural experiment tradition comes from the quality of the available data. Many of the most commonly used sources of aggregate data suffer from well-known problems, including measurement error as with the

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[12] See for example the discussion of Ehrlich's (1973) seminal work by the National Research Council's report on the early deterrence literature (Blumstein et al. 1978).

[13] As Meyer (1995, p. 151) notes, "[t]he natural experiment approach emphasizes the general issue of understanding the sources of variation used to estimate the key parameters ... If one cannot experimentally control the variation one is using, one should understand its source."

UCR (Maltz 1999, Pampel and Williams 2000, Williams and Flewelling 1987) or limited information about the nature of criminal events, such as the Vital Statistics (Wiersema et al. 2000). However the natural experiment approach does not require the use of aggregate rather than individual data to estimate the combined effects on crime of incapacitation, deterrence and replacement.

And regardless of whether analysts are working with aggregate or micro-data, an important specification check for any research employing the natural experiment approach is to ensure that any estimated effects of a given policy change are derived from examination of the people most affected by the policy. For example when studying the effects of federalizing the prosecution of eligible gun cases (as in Raphael and Ludwig 2003), we would ideally focus on gun offenders and gun offenses that are eligible for federalization.

Despite the gradual resurgence of the economics literature in this area, the number of studies that focuses specifically on disentangling deterrence from incapacitation is exceedingly modest—four, by our count. We organize our review of these studies around common sources of credible natural experiments.

Court Decisions

The first truly credible estimate for the overall effects of incarceration on crime is by Levitt (1996), who noted that in many states prison overcrowding lawsuits generated sharp changes in incarceration rates. Levitt's estimates focused on the 12 states where the entire prison system eventually came under court order, and used repeated cross-sections of state-level data to control for unmeasured state and year fixed effects.[14] Within the key set of 12 states, the *timing* of when the overcrowding lawsuits were decided was plausibly random and so unrelated to other determinants of crime trends.

Levitt demonstrated that in the 12 states where the entire prison system was under court order the growth rate in incarceration rates was about 2% above the national average before the courts took over the system, but this growth rate slowed to about 2–4% below the national average afterwards. Levitt related variation in prison populations over time within states that was induced solely by changes in the status of these overcrowding lawsuits to variation in crime rates, and he estimated elasticities of violent and property crime with respect to incarceration equal to −0.40 and −0.30, respectively. His estimates were much larger than ordinary least squares estimates from these same data (−0.10).

This study is particularly important because it provides something of an "existence proof" that the careful examination of natural experiments in sanction policy can overcome the identification problems that plague this area of research. With that said, the total effect from a change in incarceration policy is (as with the pure incapacitation effect) likely to be contingent on the specific policy (Nagin 1998). For example Levitt's (1996) estimates are most relevant for changes in prison release and parole policies, particularly among the rural Southern states that account for most of the identifying variation in his study. Moreover, for policy there is also considerable value in understanding the degree to which the

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[14] Those states in which overcrowding lawsuits have been filed at all are not systematically different from other states in the sense that such cases have been filed in almost every state in the nation (plus DC).

operative mechanism through which increased incarceration reduces crime is incapacitation versus deterrence.[15]

Variation in Public Policies

Another common source of natural experiments in economics research comes from changes in state or local public policies, or differences in policies across jurisdictions. For example, to distinguish incapacitation from deterrence effects, Kessler and Levitt (1999) used the insight that increased sentence enhancements for particular crimes should have no incapacitation effect on crime in the short term. If there is no change in the propensity of the criminal justice system to sentence convicted offenders to prison, there should be no change in the incarceration rate between the date that the new policy goes into effect and the date that the first batch of prisoners subject to the new policy reach the end of what their sentences would have been under the old sentencing regime. The sentence enhancements therefore have no immediate, additional incapacitation effect. However if the prospect of longer prison sentences deters criminal behavior among those offenders who are still at large, crime may decline immediately after the new sentence enhancement policy goes into effect. If other things are held equal, this prediction offers a potential for distinguishing deterrent and incapacitation effects.

Kessler and Levitt (1999) tested this prediction using a 1982 referendum in California, known as Proposition 8, which enhanced sentences for particular crimes. The legislated sentence enhancements were large in magnitude; they extended each eligible offender's sentence by the greater of either an extra 5 years for each prior conviction for an eligible "serious" offense or an extra 1 year for each prior prison term. Before the adoption of Proposition 8, the affected offenses were serious crimes for which a conviction almost always garnered a prison term.

Using a "difference-in-differences-in-differences" framework that compares trends in eligible and ineligible offenses inside and outside California, Kessler and Levitt reported that the rate of crimes covered by Proposition 8 dropped in California relative to other states by 4–8% during the first few years after the new law went into effect. The incidence of felonies that were not covered by Proposition 8 in California to other states was unchanged. After the expiration of the standard prison terms, the rate of the affected crimes continued to fall, and Kessler and Levitt argued that these later declines indicated that the full impact of the sentence enhancements included both deterrence and incapacitation effects. Their estimates suggested that incapacitation explained more than half of the reduction in crime attributable to the sentence enhancements.

Doob and Webster (2003) and Webster et al. (2005) challenged these conclusions. They argued that Kessler and Levitt's causal interpretation was unwarranted because the rate of eligible offenses in California peaked about 2 years before the passage of Proposition 8. In addition, they questioned the validity of the control groups. They observed that the eligible felonies are far more serious than the ineligible ones and that offending rates were much higher in California than in other states. They argued that the absence of a trend break coincident with the adoption of Proposition 8 in the rate of eligible felonies in California, as well as seemingly substantial differences between the treatment and control groups, render the Kessler and Levitt (1999) estimates implausible.

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[15] Federal as well as state court decisions can sometimes provide sources of useful natural experiments, as for example occurred when the US Supreme Court's *Roe v. Wade* decision changed the "cost" of abortion nationwide. See Ananat et al. (2006) for a summary of this research.

Levitt (2006) responded to these criticisms by noting that Levitt and Kessler's (1999) claim did not rest on comparisons in the rate of eligible offenses in California before and after the passage of Proposition 8. Rather, their approach rested on the differential time-series patterns between eligible and ineligible offenses in California and the rest of the country. He also argued that the absence of a pre-existing trend in the triple differences estimates buttresses the plausibility of the identification strategy.

Regardless of the ultimate bottom line from California's Proposition 8 experience, Kessler and Levitt have provided a useful insight of how sentence enhancements might be used to distinguish deterrence and incapacitation. Other sentence enhancements might profitably be subject to similar analyses, in part because the potential specificity of responses suggested by Nagin (1998) implies that analyses of any single enhancement policy will not be definitive.

The second economic study that attempts to distinguish deterrence and incapacitation takes advantage of the fact that states vary in their criminal justice policies and practices generally and with respect to particular offense categories (Levitt 1998a). In this case deterrence and incapacitation might be distinguished because the mechanisms offer different predictions about whether offenders will substitute away from offenses which carry relatively larger sanctions. For example, if the length of sentences for robbery rises, deterrence predicts that criminals switch to commit more burglaries, but incapacitation predicts that the rates of both burglary and robbery would fall. This prediction rests on the assumption that criminals are generalists who participate in a wide range of offenses, and some support exists for this assumption (Beck 1989).

Applying this design to city-level data for the period 1970–1992, Levitt (1998a) finds evidence that deterrent effects outweigh incapacitation effects for some types of crime, namely aggravated assault and property offenses. His estimates suggest that where deterrence matters, it may matter a great deal: for property crimes the findings imply that three-quarters of the relationship between arrest rates and these offenses is due to deterrence rather than to incapacitation effects.

Researchers using state- or local-level variation in sanction or other policies need to be aware that some applications may be susceptible to "policy endogeneity." In cross-section designs, the jurisdictions that enact a given policy of interest may be systematically different from other jurisdictions in terms of the level of crime or other factors that affect crime rates. Even in panel data designs that enable researchers to control for local or state fixed effects (unobserved time-invariant factors that influence crime within each locality or state), the timing of when jurisdictions change their policies may itself be related to determinants of crime or other social outcomes of interest. For example states seem to adopt permissive concealed gun-carrying laws in part in response to increases in local crime rates (Nagin and Black 1998, Donohue 2003). However, federal policy changes can sometimes provide a useful source of natural experimental variation, particularly when the impact of these federal policies differ across states or population sub-groups to provide comparison groups for help in isolating policy impacts.[16]

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[16] For example Ludwig and Cook (2000) take advantage of the fact that the Brady Act's requirements for gun-dealer background checks and waiting periods took effect in 1994 in some states but not others. State fixed effects help account for the fact that states affected and not affected by Brady may be systematically different with respect to permanently higher or lower rates of crime, while the fact that the timing of the Brady Act's provisions were determined by the federal government rather than local states helps overcome concerns about endogeneity in the timing of the policy implementation.

Treatment Assignment Rules

A third common source of credible natural experiments comes from sharp cutoffs in eligibility for "treatments," which could include beneficial social services or punitive measures like extra punishments.

Levitt (1998a) used this approach to try to distinguish deterrence from incapacitation effects, and began with the observation that the relative severity of punishments in state juvenile and adult criminal justice systems may differ. If punishments in the adult system are more severe than in the juvenile system, deterrence predicts that juveniles should reduce their criminal activity upon reaching the age of majority. One complication in testing this hypothesis comes from the possibility that juvenile justice systems may release a disproportionate share of their detainees at the age of majority, in which case comparing offending rates for people just above versus below the age of majority may conflate compositional effects with deterrence.

To circumvent this problem, Levitt (1998a) took advantage of variation across states in the relative severity of the state's juvenile versus adult systems, and focused on comparing states with the same age of majority. He measured the differential severity of punishment in the adult system relative to the juvenile system as the ratio of inmates-to-offenses in each system. In states that had the most lenient juvenile justice systems according to this measure, crime rates declined by around 4% when youth reached the age of majority, compared to an increase in crime of around 23% in those states with the most punitive juvenile justice systems.[17]

In contrast, Lee and McCrary (2005) examined a longitudinal database of individual-level arrest records in just one state (Florida). They take advantage of the fact that they have data on exact date of birth of arrestees and look for discontinuous changes in offending right at the age of cutoff using a regression discontinuity (RD) design. This design assumes that other measurable and unmeasurable determinants of the outcome of interest will vary smoothly by date of birth around the age of majority, and that the effects of these determinants can be adequately modeled with a sufficiently smooth parametric function or a non-parametric function of the "running variable" (in this case date of birth). The only reason outcomes (arrests) would then reveal a discontinuous jump right at the age of majority would be from the enhanced penalties associated with shifting from the juvenile to adult system. They found no sizable declines at the age of majority, although in principle any deterrent effects here could be obscured by discontinuous changes around the age of majority in the number of crime-prone young people no longer subject to inca-pacitation effects because of the possibility of juvenile detention releases at this age as mentioned above.

The fact that Lee and McCrary use data from just one state means that their estimate is identified exclusively by variation in offending as youth age across the age of majority (a single difference), while Levitt uses differences across states with different juvenile justice policies in offending as youth age as a way to net out the release-at-age-of-majority effect (essentially a difference-in-differences). Determining how much weight should be placed on the estimates of Levitt (1998b) versus Lee and McCrary (2005) would seem to depend in part on obtaining empirical evidence on the degree to which state juvenile justice systems actually release detained youth at the age of majority.

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[17] Why crime rates would increase at the age of majority in systems with the most punitive juvenile justice systems is not clear, but could be driven in part by whatever factors cause offending rates to increase in general by age starting in early adolescence.

Conclusion

The economic evidence for deterrence—as distinguished from incapacitation—is small, contested, beset by difficulties, but potentially of great use to criminal justice policy makers. By contrast, the criminological evidence for lambda is large, contested, beset by difficulties, and ultimately of limited use to criminal justice policy makers. A key question is not which literature, the criminologic or economic, as it currently exists is more problematic. Rather, it is which approach holds the greater promise for identifying policy-relevant information about incarceration and perhaps more importantly, about incarceration's effects. In addition, the difficulties confronting both approaches are daunting, but those facing the "economic" approach to understanding the net effects of changes in sanction policy seem more surmountable than those involved in estimating the micro-data parameters necessary to identify pure incapacitation effects.

We believe the most promising way to identify the net impacts of incarceration from the combination of incapacitation, deterrence and replacement effects is from the careful study of natural experiments. Credible natural experiments require "shoe leather" research to identify cases where state or federal policy changes, court decisions or "treatment" assignment rules generate differences in punishment experiences across populations that are plausibly unrelated to other determinants of criminal activity (Angrist and Krueger 2001). Because on average criminologists have far superior institutional knowledge of the criminal justice system compared to economists, this type of research would seem to provide an unusually fruitful opportunity for collaboration across the two disciplines. Importantly, in some cases natural experiments can also provide leverage in distinguishing incapacitation from deterrence effects because of different testable predictions made by the two theoretical mechanisms.

Another virtue of the natural experiment approach is to avoid confusing policymakers with the idea that there is a single multi-purpose estimate of lambda. Nagin (1998) observed that the effects of changes to sanction policy hinge critically on the nature of the particular policy change itself and the context in which it is implemented. This observation implies that there may be additional value in conditioning policymakers who are considering new policy innovations to search for evidence from similar policy changes in the past that have been targeted at similar populations.

Another theme developed in our essay is that if the goal is to help inform criminal justice policy, researchers should aspire to generate estimates that are as complete as possible in terms of capturing the overall benefits and costs of different candidate sanction policies. In this sense, a third advantage of the research approach we advocate here is that the same natural experiments that are identified by researchers to study sanction policy effects on "benefits" (crime reduction) can also in principle be used to evaluate impacts on the social costs of mass incarceration such as marriage rates or the prevalence of single-parent households, particularly in low-income minority communities. These types of social costs have been largely ignored in empirical research to date yet could be as large—perhaps even larger—than more readily measured costs of incarceration such as government expenditures per prison bed.

The final step in implementing the economic approach to understanding incapacitation and incarceration effects more generally requires converting estimates for benefits and costs into a single metric (dollars) so that they can be compared. To evaluate the benefits from crime reduction the appropriate perspective is ex ante—what is the benefit to society (what are citizens willing to pay?) to reduce the volume of crime in the community by

some specified amount next year (Cook and Ludwig 2000)? This question corresponds to the resource-allocation problem facing policymakers, and leads to a very different measurement strategy compared to an ex post perspective that seeks to measure tangible and intangible costs to crime victims after the fact. If research on measuring the dollar benefits from crime control is in its infancy (Cook and Ludwig 2000, Cohen et al. 2004),[18] then research on monetizing the social costs from mass incarceration is in the pre-natal stage. But stepped-up efforts to assign dollar values to the benefits and costs of different incarceration policies strikes us as being at least as important as efforts to understand the impacts of different sanction regimes on crime.

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[18] Recently, economists have taken a step in the direction of evaluating the ex ante benefits of crime control by employing contingent valuation methods to estimate the willingness-to-pay for crime reductions. In this approach, a survey asks individuals how much they would be willing to pay in order to achieve a particular policy outcome, here reductions in crime, by essentially constructing a hypothetical "market" situation. An advantage of this approach is that the responses reflect ex ante expectations of the benefits that are anticipated to accrue rather than ex post attempts to compensate victims. Also, the responses also measure the respondent's fear of victimization as well as concern for others, such as family and the broader community, which may not correlate with the risk of victimization. Using this method, Cook and Ludwig (2001) estimated that the average household was willing to pay more than $200 to reduce gun violence by 30%, and this implies a willingness to pay about $1 million to prevent each gun injury. Cohen et al. (2004) using similar methods estimated willingness to pay to prevent crime of approximately $25,000 per burglary, $232,000 per armed robbery, and $9.7 million per murder.

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