

Revisiting Incapacitation: Can We Generate New Estimates?

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Abstract A substantial body of empirical research examines how the huge expansion in incarceration in the United States since the early 1970s has influenced crime. These studies merge the effects of three conceptually distinct paths by which incarceration might reduce crime: general deterrence, specific deterrence and incapacitation. This issue of the Journal focuses specifically on the incapacitation path. This Introduction reviews the individual papers and offers the editors' judgment as to the plausibility of progress using different research strategies. It emphasizes the potential for using individual level data to take advantage of natural experiments.

Keywords Incapacitation · Natural experiments · Risk assessment

Introduction

A substantial body of empirical research examines how the huge expansion in incarceration in the United States since the early 1970s has influenced crime. (Kuziemko and Levitt 2004; Levitt 2004; Johnson and Raphael 2007; Piehl et al. 2006; Spelman 2000). These studies merge the effects of three conceptually distinct paths by which incarceration might reduce crime: general deterrence, specific deterrence and incapacitation.

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This issue of the *Journal* focuses specifically on the incapacitation path. We believe that research on the topic had stagnated during the last ten years and it is both important and possible to instigate new research on incapacitation. This special issue offers reviews of prior research from the perspective of criminologists and economists, some new empirical contributions and a methodological note raising a basic point about the related effort to maximize incapacitation effects by selective incarceration. This Introduction and the review papers offer suggestions for future research strategies.

Are Incapacitation Effects Estimable?

There are two basic approaches to the study of incapacitation that fall along basic disciplinary lines. The first, based in criminology, relies on individual level data to generate estimates of λ , the frequency of offending. These estimates are then used to generate simulated estimates of the amount of crime averted by specific imprisonment policies. This approach is made difficult by (1) the plethora of assumptions it requires about the relationship of the data to sentenced offenders on those who might be sentenced and (2) by a failure to generate estimates that can be directly mapped onto any given policy, such as extending all sentences by ten percent or reducing the severity of offense needed for admission to prison. The second approach, based in economics, relies on aggregate level data to generate estimates of the impact of prison on crime. This approach, which tends to focus on breaking the simultaneous link between prison policy and crime rates, is generally unable to separate the incapacitative impact of prison from the deterrent impact.

Although research on the crime reduction effects of incapacitation flourished in the period 1975 to 1995, there has been an almost complete absence of research from either camp over the past ten years, reflecting a shallow consensus that there are serious shortcomings to both approaches. This issue of the *Journal of Quantitative Criminology* was motivated by the desire to revisit this apparent deadlock. The continued growth in per capita imprisonment rates a decade after crime rates started their precipitous decline creates an ever more pressing need to better understand the impact of prison policy on crime rates. Incapacitation is potentially less efficient than deterrence, since one bed will only incapacitate one criminal at a time; in theory one prison bed might *deter* multiple potential criminals. But incapacitation is also more concrete than deterrence, which is notoriously difficult to measure. Therefore, incapacitation effects are often viewed as providing lower bounds on the crime-reduction benefits of prison. This is particularly true in political settings. Thus incapacitation seem to us to be a reasonable place to start a discussion about the impact of prison on crime. Deterrence, if it exists, is a bonus to be added to the concrete incapacitation effects.

The first two articles in this issue are commissioned review articles that are, respectively, by two criminologists and by two economists. Not surprisingly, each article reflects the authors' disciplinary heritage, with the article by criminologists Piquero and Blumstein championing the individual level approach to the study of incapacitation, and the article by economists Miles and Ludwig championing the economic approach. Each pair, in their own way, however, also goes beyond the current literature to at least hint at future directions. Miles and Ludwig are the most striking in their position. They argue that reliable and valid estimates of incapacitation are too difficult to obtain, and that time would be better spent generating estimates of the aggregate effects of prison using natural experiments. Although these aggregate measures would not enable the researcher to isolate

the mechanism by which crime declined, it would give policymakers clear guidance with which to conduct cost benefit analyses of prison. From Miles and Ludwig' perspective, generating a clean estimate of the crime reducing benefits of a policy would then provide room to concentrate on generating good estimates of the costs and benefits of such a policy. Worrying about the relative impacts of incapacitation and deterrence is time consuming, and does not directly contribute to this cost benefit analysis. They believe that criminologists, with their institutional knowledge, could help reinvigorate the economist's now stagnant search for new instrumental variables with which to study the aggregate costs and benefits of incarceration.

While Piquero and Blumstein no doubt agree that the work of economists could be improved through collaboration with criminologists, they strongly disagree with the sentiment that research on incapacitation at the individual level should be abandoned. They start with a review of the basic critique of classic incapacitation research—namely that its assumption of a constant λ ignores the reality of considerable heterogeneity in offending rates across age, and within the population. They point to the need for research to start to generate estimates of λ that reflect this heterogeneity, and then can be used to simulate the effects of policies which affect different parts of the offender distribution. They also suggest, without providing much detail, that methods now common in developmental criminology, like trajectory analysis, could be used to generate new estimates of incapacitation.

The three empirical papers in this issue essentially take up where Piquero and Blumstein leave off by attempting to generate new estimates of the incapacitation effect using longitudinal datasets of both self reported (Apel and Sweeten) and official individual level data (Bhati and Blokland and Nieuwbeerta). Each of the three papers uses a different dataset and a different approach to generate estimates of incapacitation. Blokland and Nieuwbeerta use a major national data set on offenders in the Netherlands, tracking a four percent sample (4,615 individuals) of all those convicted in 1977 over the following 25 years. They take a simple approach and simply count the number of offenses committed during an imaginary incarceration spell of length x that would have accompanied any given arrest. This approach is justified in this sample by the very low levels of incarceration during this time period in the Netherlands. Blokland and Nieuwbeerta explicitly assume that there is no deterrence or any other change in behavior. While this is a stark assumption, it does generate a lower bound on the potential benefits of incarceration. Empirically, they find that much more severe punitive policies would have modest effects on crime in the Netherlands. In their model, a 25% crime reduction through selective incapacitation would generate a prison population 45 times that currently in Dutch prisons.

Apel and Sweeten take a different approach and study the self-reported offending of a group of individuals in a contemporary US sample. In contrast to most prior research, they do not generate estimates by relying on the reports of offending before incarceration by the same respondents. Rather, they rely on self-reported data from other people who are otherwise similar but not incarcerated. That is, they use an altogether new counterfactual—the offending rates of a matched control group. To the best of our knowledge, this is the first time that a matched sample approach has been used to generate estimates of incapacitation. Since it is likely that those who are incarcerated are different from those who are not in unobservable ways, it is reasonable to assume that the Apel and Sweeten estimates also underestimate the incapacitative benefits. However this cost comes with the benefit of avoiding the many well documented problems raised by the short term increases in offending which usually precedes a spell of incarceration. Their approach also has the merit of being tied to a specific change in imprisonment policy, namely increasing the

numbers who are given prison as a punishment as opposed to extending the lengths of those currently incarcerated.

Bhati's approach, which is formally the most complicated, relies on individual trajectories of offending, which are generated from information from all of the individuals in the sample. He takes advantage of a large Bureau of Justice Statistics data base on 38,000 offenders in 15 states who were released in 1994 and then followed in official records for three years. Bhati estimates what would have happened if the person had not been in prison using information both from the individual himself and the other people in the sample. Since it is based on official records, it does not include offenses that do not result in arrest. To generate the total crime reduction one has to use a multiplier reflecting the share of specific crimes that are actually reported.

This counterfactual is somewhat less obvious than the counterfactual in the Apel and Sweeten paper, but has the advantage of using information from the individual's own offending, as in the Nieuwbeerta case where the individual serves as his own counterfactual. Because of the size of his data base, Bhati is able to offer separate estimates of incapacitation effects for each of 13 states for specific crime types and specific sex/race groups. He finds substantial variation in the state specific estimates by crime type. Bhati's approach, or some variant of it, can easily be adapted for policymakers seeking to estimate the impact of some given policy.

Of course, Bhati's paper, like the other two papers, ignore any other potential effect of incarceration on the individual trajectory of offending, and his estimates are not explicitly causal. In other words, simultaneity problems and selection effects could still affect his results. But the three papers offer new estimates of incapacitation that go beyond the current literature.

Natural Experiments and Longitudinal Data

The National Institute of Justice grant that supported the review articles also funded a new study by Owens (2007), not in this issue, which adds to these contribution with a different approach. She fashioned an identification strategy that should control for both unobserved and observed differences between those who are incarcerated and those who are not. It also fits nicely between the two review papers, because she uses individual level data and a natural experiment to estimate the causal effect of incapacitation for a subsample of people affected by the policy. She takes advantage of a technical change in Maryland sentencing guidelines, not driven by changes in crime rates,¹ that nonetheless had a substantial effect on a subset of sentenced offenders: males aged 23–25 with juvenile records.

The change involved the use of juvenile records in sentencing decisions. Until 2001 these records were included in the criminal history of all individuals up to the age of 25; after 2001, the age for which juvenile histories counted was lowered to 22. Thus some of those aged 23–25 received shorter sentences than they would have received in the earlier years. Owens estimates that this reduced the average sentence under the Maryland guideline system by one eighth to one quarter (about nine to eighteen months). During the time period they were at liberty compared to their unlucky pre-2001 counterparts, they were arrested on average 2.5 times per annum. Taking account of the specific offenses for which they were arrested, and the ratio of recorded arrests to recorded offenses of the same

¹ The Maryland Sentencing Commission made the change because it felt that the difference between Maryland and surrounding states was unfair; in other states the juvenile record did not count after age 22.

type, she estimates that they were responsible for 1.5 index crimes per annum. This provides a relatively precise estimate of their recorded criminal activity during a period when they would have been incarcerated under the previous rules. This estimate is unique because it uses information from other offenders to generate a counterfactual incarceration spell, rather than a counterfactual offending rate. Then, the individual's own offending during this time period AFTER their initial period of incarceration is used to estimate the incapacitation effect. Owens runs a number of tests to control for other changes between the period 1997–2000 and 2002–2004 (e.g. police arrest intensity) that might have affected the observed number of offenses; she argues that none of the tests suggest that there is a problem with the estimates.

The estimate that she develops of crimes averted is smaller by an order of magnitude than the consensus estimate of 16 to 20 Index I crimes previously cited in the literature. It is also smaller than any of the estimates generated by the other three papers. It is important to note that over half of Maryland's commitments to prisons are convicted of drug offenses; Owens does not attempt to estimate the number of drug offenses averted through incapacitation, in part because replacement is so much more likely to occur for these as compared to property or violent crimes. But the most important reasons for the lower estimates are (a) Owens is the first researcher who exclusively uses behavior after rather than before the incident spell and (b) Owens has in all likelihood captured the lower tail of the distribution.

One of the main contributions of Bhati, Sweeten and Apel, and Blokland and Nieuwbeerta is that they all generate estimates that reflect the heterogeneity of offending in the population using fairly broad distributions of offenders. Owens' paper focuses on a small group of offenders—23 to 25-year-old offenders in MD who were served less than 3 years. As a result, it is likely she captured fairly low risk offenders. She has also explicitly captured the cost, in crimes, of the change in policy by the sentencing commission. Given the minor nature of the change, it would not have been possible to study the effects of such a policy change on aggregate data as suggested by Miles and Ludwig.

Taking advantage of such "natural experiments" is a well established research strategy in empirical microeconomics. *Freakonomics* provides a popular account of a whole series of such studies carried out by Steve Levitt; many of them concern criminal justice interventions. For a still more recent example see Kuziemko (2006) who took advantage of court orders to examine the effect of early parole discharge in Georgia. It is clear that many such experiments are indeed available for analysis, and contrary to the claim of Miles and Ludwig, they can be usefully studied with individual level data.

Future Research

A fair question is whether any but academic purposes are served by attempting to separate a distinct incapacitation effort from deterrence. For decision makers the most important question is the extent to which incarceration reduces crime; the path generating the reduction is irrelevant for those purposes. Incapacitation has acquired prominence primarily because of the claim that it is more readily estimated than the total effect. The RAND study showed heterogeneity across states and pointed to the likelihood that lambdas varied by age. The current research once again highlights the dramatic heterogeneity of offending across the population offenders. Given that heterogeneity, there has been a pressing need for replication in more jurisdictions and at various times. We were struck by the Michigan policy maker at an American Society of Criminology presentation of these

papers who pleaded for Michigan-specific estimates that would help them inform their own policy. But thirty years have gone by since the RAND second Inmate Survey. Even though that was seen as a landmark study from the time it was first published, there has been no effort to replicate it on a large scale.²

Bhati's paper with readily available administrative data gives us the first working model of the type of large scale effort that would be required for policy analysts considering a change in policy. However, we are hesitant to put too much causal weight on the estimates from Bhati's model. The causal modeling in Owens' paper represents stronger identification, but for only a very narrow part of the population. We suspect that this kind of tradeoff between generalizeability and strong internal validity is endemic in this type of study. It is yet to be seen whether stronger causal inference can be applied to broader ranges of the population. One approach would be comparing estimates from models like those developed by Bhati with the estimates generated by small scale experiments like that described by Owens using the same data. While such an approach was not utilized with these data, it is clearly feasible, given that both papers used readily available administrative data.

Our focus on the source of causal inference and identification potentially puts us at odds with the Piquero and Blumstein research agenda, which outlines nine disparate and very demanding items based on the criminal career perspective. This perspective tends to focus on deriving parameter estimates from various sources that can then be entered into a simulation model rather than generating estimates from one sample. Given the heterogeneity of the available estimates, we are skeptical of any approach that does not work from one known sample to generate estimates with a clear identification strategy. In other words, we think that a heavy reliance on the Shinar and Shinar approach is no longer a wise strategy. The movement to longitudinal datasets and methods mean that we can now generate direct estimates of the incapacitation effect of current policy, which means that we do not have to generate simulation estimates based on average parameter estimates.

But the criminal career approach is not irrelevant. Blokland and Nieuwbeerta, Owens, and Bhati use official record data, and must therefore estimate the number of offenses associated with each conviction. This number is not estimable from their data, so they must rely on estimates generated in the criminal career literature. Moreover, the criminal career framework provides a valuable conceptual framework for evaluating these estimates. For example, Piquero and Blumstein emphasize the need to generate estimates of residual career lengths. At first blush this seems odd—none of the empirical papers needed estimates of residual career length to generate their estimates. But residual career length estimates are relevant if they are created to simulate an incarceration policy that goes beyond their data. To the extent that residual career length is not perfectly correlated with offending rates, residual career length may need to be a separate point of focus for policymakers and researchers.

We also agree with Piquero and Blumstein's call for more research into the correlates of the various offending rates discovered in the data. The obvious and striking heterogeneity of offending rates across offenders (but not generally by offense type) in each of the empirical papers simply begs for policymakers to consider how the prison time should be spread among offenders to maximize the incapacitation effect. Of course, the issue of selective incapacitation is well traveled ground in criminology. And there is substantial agreement that we can reliably predict the identity of the bottom end of the distribution

² Small scale efforts at replication include DiIulio (1990), covering Wisconsin prisoners, and Piehl and DiIulio (1995) on a sample of New Jersey offenders.

(Auerhahn 1999). Therefore, existing data, including that of the included papers, can be used to craft policy that limits the incarceration of low rate offenders. But there are distinct limits to how far one can go based on current research, given the equally strong consensus that it is difficult to predict the highest rate offenders.

A final Research Note in this special issue, by Bushway and Smith deals with this difficult issue. Analysis of selective incapacitation policies is complicated by the fact that the criminal justice system may affect the offending of the incarcerated individuals through specific deterrence, stigmatization or other causal mechanisms. And these treatments are being assigned in a non-random way to the convicted population. In this context, in which a regime is already trying to implement a treatment, Bushway and Smith make it clear that it is hard to evaluate the impact of any variable on subsequent offending without an explicit model of what the criminal justice actors are already trying to accomplish. To the extent we know what the actors are trying to do, we can more easily interpret the causal impacts of the various actions. However, in most administrative datasets, such information is not available, and we need to make strong assumptions to make much progress on the question of risk assessment and selective incapacitation, particularly for those offenders who are heavily involved in the criminal justice system—the highest risk offenders. While there is a substantial literature on risk prediction, very little of this research takes this problem—that the decisions are endogenous with respect to the risk—into account. Bushway and Smith’s research note at least hints at a future research agenda that might more definitively shed light on the prospects of treatment rules that attempt to capitalize on the wide variation in offending to more efficiently use prison resources.

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