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# Imprisonment and Reoffending

ABSTRACT

Imprisonment is the most severe punishment in democratic societies except for capital punishment, which is used only in the United States. Crime prevention is its primary rationale. Imprisonment may affect reoffending in various ways. It may be reduced by some combination of rehabilitation and what criminologists call specific deterrence. Sound arguments can be made, however, for a criminogenic effect (e.g., due to antisocial prison experiences or to stigma endured upon release). Remarkably little is known about the effects of imprisonment on reoffending. The existing research is limited in size, in quality, in its insights into why a prison term might be criminogenic or preventative, and in its capacity to explain why imprisonment might have differential effects depending on offenders' personal and social characteristics. Compared with noncustodial sanctions, incarceration appears to have a null or mildly criminogenic effect on future criminal behavior. This conclusion is not sufficiently firm to guide policy generally, though it casts doubt on claims that imprisonment has strong specific deterrent effects. The evidence does provide a basis for outlining components of an agenda for substantive and policy relevant research.

Imprisonment is intended to prevent crime by incapacitation and deterrence. Incapacitation refers to crime prevention resulting from the physical isolation of offenders. Deterrence refers to a behavioral response. Criminologists have long drawn a distinction between general

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deterrence, the response to the threat of punishment in the population at large, and the response to punishment of the punished, called specific or special deterrence. This essay addresses the latter, the effect of imprisonment on reoffending. We use this more generic label because the experience of imprisonment may affect reoffending by mechanisms other than deterrence. For example, a preventive effect may arise from involvement in rehabilitation programs or a criminogenic effect may result from such mechanisms as stigma or association with fellow inmates.

Despite a growing literature on the effect of imprisonment on reoffending and previous attempts to assess this research (Gendreau, Goggin, and Cullen 1999; Villettaz, Killias, and Zoder 2006), rigorous scientific knowledge is in short supply. Given that in the United States alone over 2.3 million offenders reside in correctional institutions, this is a remarkable omission. Much work remains to be done. As part of this undertaking, this essay attempts to systematize the existing literature, to furnish a provisional statement of prisons' likely effect on individual offenders, and to identify issues for future research. We expand upon Gendreau, Goggin, and Cullen (1999) and Villettaz, Killias, and Zoder (2006) by examining a larger body of research than those analyses did, by discussing the methodological challenges to inferring imprisonment effects from nonexperimental data in detail, and by laying out an agenda for future research. Our concern is in assessing how imprisonment affects individuals' reoffending and not on how macro-level variations in imprisonment affect crime rates.

Although research on the effect of custodial sanctions on reoffending is international—with some of the best work coming from Europe and Australia—the evidence is reviewed from an American-centric perspective. This is because the scale of imprisonment in the United States dwarfs that of other democratic societies absolutely and per capita. Still, other nations incarcerate tens of thousands of offenders and must struggle with the wisdom of expanding their institutional capacity. Regardless of location, sound scientific knowledge about the effects of imprisonment is integral to an informed policy discussion of crime control.

In the early 1970s, the United States had experienced relative stability in imprisonment for at least half a century, with rates of incarceration hovering around 100 state and federal inmates per 100,000 population (Blumstein and Cohen 1973; Greenfeld and Langan 1987).

The use of prison as a mechanism of social control seemed to be on the decline. The prison population, which had risen to 220,149 in 1961, dipped over the next decade to under 200,000 (Greenfeld and Langan 1987). Scholars wrote about the inevitability of "decarceration" (Scull 1977) and the "end of imprisonment" (Sommer 1976). Talk of abolishing prisons was not seen as far-fetched (Mitford 1973). Menninger's *The Crime of Punishment* (1968) earned wide popular acclaim, suggesting a growing consensus that mean-spirited penal policies should be regarded as a relic of less civilized days (see also Toby 1964).

In Massachusetts, Jerome Miller, head of the state's Department of Youth Services, boldly emptied the state's juvenile reformatories in the early seventies. When youth crime failed to spike upward, the "Massachusetts experiment" in deinstitutionalization was heralded as showing that incarceration did not reduce lawbreaking (Miller 1991). At about this same time, observers of corrections learned of the Stanford Prison Experiment (Haney, Banks, and Zimbardo 1973). Psychologically normal students were randomly assigned roles as guards or inmates. The newly minted guards quickly began to abuse and otherwise mistreat the unlucky students assigned to be prisoners. This reinforced the view of many that prisons were inherently coercive and inhumane (Zimbardo 2007). During the prisoner insurgency in 1971 at Attica Correctional Facility, 29 inmates and 10 correctional officers were killed in retaking the institution. The word "Attica" came to symbolize not vicious inmates rioting uncontrollably but the willingness of the state to abuse its power by wantonly shooting down inmates whose protests had merit (Cullen and Gilbert 1982). When bank robber Al Pacino chanted "Attica, Attica" in the film *Dog Day Afternoon*, audiences cheered; they were not on the side of the police surrounding the bank.

Yet shortly thereafter, a sea change transformed penal policy in the United States. Over the next 4 decades, the incarceration rate for state and federal inmates rose more than fivefold from 96 per 100,000 in 1970 to 501 at year-end 2006 (Greenfeld and Langan 1987; Sabol, Couture, and Harrison 2007). Counting those housed in jails, the nation's total incarceration rate surpassed 750 per 100,000 (Liptak 2008). In absolute terms, the number of state and federal prison inmates jumped from below 200,000 in 1970 to over 1.5 million in 2008. When those held in local jails and other secure facilities are added, the daily count exceeds 2.3 million (Sabol, Couture, and Harrison 2007). This

inexorable expansion was not confined to a few states but reflects a trend that, with some variation, occurred across all states and regions (Zimring and Hawkins 1991).

The 4-decade-long rise in prison populations in the United States is unique. Some advanced Western nations, such as Great Britain and the Netherlands, increased their prison populations (Garland 2001; Tonry 2004; Downes 2007; Tonry and Bijleveld 2007). But others, such as the Scandinavian nations and, most notably, Canada just to the north of the United States, displayed substantial stability in their imprisonment rates over 30 years (Lappi-Seppälä 2007; Webster and Doob 2007). And even among those Western countries that expanded prison use, the scale of imprisonment is simply not comparable. England and Wales, for example, imprisoned around 80,000 in 2006 and had an incarceration rate of 142 per 100,000 (Newburn 2007). These figures make England and Wales "the highest incarcerator in Western Europe" (Newburn 2007, p. 435). For Western nations generally, the incarceration rate falls between 50 and 150—figures dwarfed by America's rate of 751 (Tonry 2004; Liptak 2008; Warren 2008). When all the comparative statistics are lined up, it is clear that the United States' "crime control policies" have become "much harsher than in earlier American times or in other places" (Tonry 2004, p. 23).

A lengthy roster of works seeks to document and dissect the sustained increase in incarceration in the United States (see, e.g., Zimring and Hawkins 1991; Clear 1994; Beckett 1997; Currie 1998; Blumstein and Beck 1999, 2005; Garland 2001; Wacquant 2001; Whitman 2003; Tonry 2004; Gottschalk 2006; Harcourt 2006; Lynch 2007; Raphael and Stoll 2007; Simon 2007; Useem and Piehl 2008). Diverse factors have been proposed: increased rates of crime; harsh, mandatory sentencing policies; a "war on drugs" that brought into the justice system many noncriminal drug users and low-risk offenders; the politicization of crime in which "get tough" promises enjoyed wide public support; efforts to contain and repress minority group members; the reallocation of resources to the criminal justice system from the mental health system, where large numbers of patients once housed in state facilities are now substantially deinstitutionalized; the emergence of a "culture of control" that welcomed efforts to protect citizens, especially those of the middle class; and a changed "sensibility" about crime and punishment that encourages harsh justice. As Tonry (2004) notes, these accounts are of differential merit; none is a complete explanation. At

best, they should be viewed as "risk factors" whose cumulative effect has been to sustain a 4-decade growth in prison populations (Tonry 2007).

Regardless of one's views on the rightness of the policies that have given rise to increased imprisonment, the United States now incarcerates a disturbingly large proportion of its residents. A recent report by Pew Charitable Trusts calculates that one in every 99.1 adults is in custody (Warren 2008). The differential impact of imprisonment on minorities is particularly disquieting. For African American men ages 20–34, one in nine is incarcerated (Warren 2008). It is estimated that one-third of all black males will spend time in a state or federal prison during their lives (Bonczar 2003). Concerns have been voiced about the negative effect on inner-city communities of removing into custody such a substantial segment of the male population for repeated and often lengthy periods (Clear 2007). More critically, Wacquant (2001, p. 95) proposes that the "ghetto and prison" are now so interconnected that they exist in a "deadly symbiosis."

Imprisonment is costly, exacts an economic and psychological toll on family members of the incarcerated, particularly children, and raises issues of social justice due to its differential impact on minority members and communities. Thus, beyond being an instrument for meting out just deserts, prison's justification must rest heavily on its demonstrated capacity to protect the social order. Understanding the effects of incarceration on reoffending is thus a key consideration in formulating correctional policy.

As noted at the outset of this essay, criminologists have long drawn a distinction between general deterrence and specific deterrence. The theory of general deterrence is clear and particularly well articulated in economic theory (Becker 1968; Cook 1980). It is the empirics that remain unclear. What is the magnitude of the effect? How does it vary across sanction types, crimes, and people? These are not trifling uncertainties. They are fundamental to the efficacy of crime control by the threat of formal sanctions (Nagin 1998; Doob and Webster 2003; Tonry 2007). By contrast, the very logic of special deterrence is murky. More than 30 years ago, Zimring and Hawkins (1973, p. 225) observed: "To talk of 'the impact of punishment on potential offenders' and the 'impact of punishment on the offender' as 'two kinds of deterrence' is rather like saying that a storm warning and the storm are two different kinds of disturbance. The use of the expression 'impact of punishment'

twice in one sentence to denote two quite separate and distinct processes generates a confusion of categories and obscures an important distinction." We agree.

The studies that are the focus of this essay examine how placing an offender in prison affects the person's future criminal involvement or "reoffending." We use the concept of reoffending to refer to all criminal acts committed by a person following a legal sanction, in this case imprisonment. Reoffending subsumes the concept of "recidivism," which is defined as the commission of at least one criminal act after the completion of a sentence. When computed for a given group of offenders, recidivism is typically expressed as a percentage—for example, as the percentage of a prison release cohort who commit one or more criminal acts within a designated period following their release. Reoffending is conceptually broader than recidivism because it also subsumes measures such as the rate of offending—number of crimes committed over a specified time interval. The measurement of reoffending and recidivism poses many technical challenges that are beyond the scope of this essay.[1] One involves the specification of the time window over which the reoffending measure will be calibrated (e.g., 3 years). Perhaps the most controversial is the specification of what event should properly constitute a return to crime—the self-reported criminality of the offender, arrest, conviction, or reimprisonment. Most of the studies reviewed here employ measures of rearrest or reconviction. These are "official" measures because they depend on an offender's detection by the state.

An initial reason to be skeptical of specific deterrence, as an empirical matter, is that reoffending among prison inmates is high, with rates of official recidivism often reaching 60 percent within 3 years (Langan and Levin 2002). But using these data is potentially misleading. Rates of official recidivism among those receiving community-based sanctions—especially felony offenders who might have been sent to prison—are also high (Petersilia 2002). The litmus test for assessing the impact of imprisonment on reoffending is to compare the experiences of offenders in prison with those of similar offenders given a noncustodial sanction.

Estimating the effect of imprisonment on the subsequent criminal career development of those actually imprisoned is complicated by

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[1] See Maltz (1984) for a full discussion of the measurement and calibration of recidivism.

many factors. One is that a priori, even the sign of the effect of the prison experience on subsequent criminality is indeterminate. Sound arguments can be made that the experience of imprisonment either increases or decreases criminality. Further, the effect may be contingent on prior experience with imprisonment, stage of criminal career development, and age. Imprisonment is selectively imposed. Persons who are sentenced to prison have committed more serious crimes and have more extensive prior records of offending, on average, than their counterparts who receive noncustodial sanctions. These differences must be carefully accounted for to isolate statistically the effect of imprisonment on subsequent criminal behavior.

The challenges to making this inference are discussed at length in Section II. Particular emphasis is given to accounting for age. This is important because involvement in crime is highly age dependent, and recidivism, by definition, is time dependent and thereby age dependent. Consequently, even small differences in the ages of those imprisoned relative to the ages of the nonimprisoned may significantly contaminate estimates of the effect of imprisonment on reoffending.

Most studies of the impact of imprisonment on subsequent criminality find no effect or a criminogenic effect. Only a few studies find evidence of a preventive effect. We conclude, however, that existing research is not nearly sufficient for making firm evidence-based conclusions for either science or public policy. The limitations of the evidence stem from a combination of factors. As a matter of public policy, there is a fundamental difference between concluding that imprisonment reduces or has no effect on reoffending and concluding that it exacerbates reoffending. The evidence on neither side of this dichotomy is sufficiently strong to distinguish convincingly between them. In many nonexperimental studies, insufficient control for the relationship between age and reoffending rates may be seriously biasing estimates of the effect of custodial compared to noncustodial sanctions. Many of the studies involve juveniles or compare very short periods of confinement with noncustodial sanctions, or both. Both attributes limit the relevance of a study's conclusions about the effects of imprisonment on reoffending in contemporary society. In medical parlance, it is important to understand the "dose-response" relationship between the experience of imprisonment and subsequent criminality. We lack even a crude estimate of this relationship. We have little evidence on

the mechanisms that underlie any effect imprisonment may have on reoffending.

This essay is organized as follows: Section I examines competing perspectives on the effects of imprisonment. Sociologically inspired criminology portrays imprisonment as a social experience that is criminogenic due to in-prison and postprison experiences. Conversely, economic analysts see imprisonment as a cost that, because it exacts a higher price than alternative sanctions, deters reoffending more than noncustodial sanctions. Section II discusses the statistical issues that must be addressed to estimate imprisonment effects on reoffending with nonexperimental data. In Section III, the evidence on the effect of custodial sanctions on reoffending is reviewed. We distinguish two broad categories of studies: those examining the effects of custodial compared with noncustodial sanction and those examining the effects of length of confinement. Section IV addresses policy and theoretical implications of our analysis.

## I. Perspectives on Imprisonment

In the 1820s and 1830s, the United States embarked on a bold experiment in institutionalization. The belief was widespread that prisons—optimistically called "penitentiaries"—would transform the lawbreaking into the law abiding (Rothman 1971). This reformist spirit is embodied in the current use of the term "correctional institutions" to refer to prisons. Since their invention, however, these facilities have had their critics, claiming that words such as "penitentiary" and "correctional institution" are more euphemism than reality—or, in the language of Rothman (1980), more claims to "conscience" than admissions of the "convenience" that actually prevails. Amidst this critical scrutiny has been the long-standing worry that imprisonment not only does not reform but, rather, exacerbates inmates' criminality.

Rothman (1971, p. 214) observed that in 1850s America, commentators, "convinced that confinement was inherently unnatural, and therefore injurious, . . . wanted to return convicts, with appropriate precautions and supervision, to the community as quickly as possible. The sooner the criminal reentered society, the more likely he would become law-abiding; the longer he remained secluded, the more incorrigible he would grow." Similar comments are found in every era. To highlight but one other example, we can point to "Stanley," the

subject of Shaw's (1930) life history, *The Jack-Roller: A Delinquent Boy's Own Story*. At the end of his stay in a House of Corrections—referred to by Shaw as a "House of Corruption"—Stanley took stock of what he had learned and of what awaited him:

> I tried to think of my future but more crimes and jail bars stared me in the face at every angle. There was no hope but in crime. All my friends were criminals and besides I was a criminal and nobody would trust me—only look down on me and shun me. Somehow I was different from anybody but criminals and I always felt drawn to crime. Circumstance had turned me back into jail every time before when I tried to make good. But now I had lost my ambition and didn't care for anything but crime. Was I not completely alone in the world except for my buddies in crime and did I not always feel pulled to them and to the adventures and luxuries that crime offered? I was educated in crime. (Shaw 1930, pp. 162–63)

Critics thus tend to see prisons as sources of inhumanity and, in turn, of crime. By contrast, others see experience with the correctional system as just that—correctional. The prison experience demarcates moral boundaries and teaches those within its walls the lesson that crime does not pay. Austere institutional conditions may convince inmates that imprisonment is an experience not to be repeated. Further, educational and treatment programs within the prison may provide the inmates with skills that can be used in legal labor markets or reduce the propensities such as drug addiction that were the cause for their incarceration.

It is possible that these two global views—prisons as criminogenic and prisons as a preventative deterrent—are both correct. Thus, imprisonment might have differential effects, pushing some offenders toward and others away from crime. The effect might be conditioned by characteristics of the offender (e.g., low risk or high risk), of the institution (e.g., harsh or therapeutic), or of the sanction (e.g., length). And the effect is comparative, assessed in relation to what a noncustodial sanction, if applied instead of custody, might have entailed. Current research is not sufficiently developed to address issues of this specificity.

In this section, we elaborate on these two global perspectives. This discussion is relevant because it illuminates the conduits through which incarceration might increase or decrease criminal involvement. These

are factors that potentially specify the impact of imprisonment and thus should be of concern in future research.

## A. Prisons as a Specific Deterrent

According to the economic model of crime, imprisonment is a key cost to offending (Becker 1968). Imprisonment exacts a variable price; it is more costly to the extent that the sentence assigned is longer rather than shorter or to the extent that the conditions of confinement are harsher rather than nicer. Prisons deter because they influence the expected utility of future criminal pursuits. Further if a custodial sanction is perceived to be more costly than a noncustodial sanction, the imprisonment sanction will exert a greater deterrent effect.[2]

Why, then, did we earlier indicate that the logic of specific deterrence is murky? From an economic perspective, the experience of imprisonment is only relevant to future offending decisions if it influences perceptions of the costs and benefits of future offending. The precise effects on perceptions or expectations of being in prison, however, are not straightforward and likely hinge on a number of contingencies. Thus, if the experience of imprisonment is sufficiently distasteful, some of the punished may indeed conclude that it is an experience not to be repeated. The structure of the law itself may cause previously convicted individuals to revise upward their estimates of the likelihood or severity of punishment for future lawbreaking. The criminal law commonly prescribes more severe penalties for recidivists. For example, sentencing guidelines routinely dictate more severe sentences for individuals with prior convictions. Prosecutors may also be more likely to prosecute individuals with criminal histories. These offenders might expect that an arrest would bring, with much greater certainty, a harsher prison sentence and hence be more susceptible to specific deterrence.

Other offenders, however, may respond differently to the experience of imprisonment. They may conclude that prisons were not as unpleasant as anticipated; if so, they may revise downward their expected utility loss from a future experience of imprisonment and thus be more likely to reoffend. Further, evidence from behavioral economics points to additional mechanisms by which the experience of punishment may decrease rather than increase expectations about sanction costs. For

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[2] Research on the differential effect of prison conditions is sparse and conflicting (Katz, Levitt, and Shustorovich 2003; Chen and Shapiro 2007); accordingly, we do not consider this aspect of specific deterrence in this essay.

example, research on substance abuse and driving under the influence finds a positive effect (i.e., offending becomes more likely) of experience with punishment on subsequent offending (Paternoster and Piquero 1995; Piquero and Paternoster 1998). Pogarsky and Piquero (2003) propose the idea of a "resetting" effect to explain this positive association. The resetting effect is an application of the concept of the "gambler's fallacy" (Gilovich 1983; Clotfelter and Cook 1993). Just as folk meteorology holds that "lightning never strikes twice in the same place," the gambler's fallacy holds that bad or good things do not run in quick succession. Consequently, the experience of punishment may lead to a decrease, not an increase, in the punished individuals' estimate of the certainty of being punished which, in turn, may encourage them to offend more frequently.

Beyond altering expectations, the experience of punishment may affect the likelihood of future crime by increasing or decreasing the attractiveness of crime itself or by expanding or contracting alternatives to crime. While imprisoned, the individual may benefit from educational or vocational training that increases postrelease noncriminal income-earning opportunities (MacKenzie 2002). Other types of rehabilitation are designed to increase the capacity for self-restraint when challenged by situations, like a confrontation, that might provoke a criminal act such as violence (Cullen 2002). There also are many reasons, however, for theorizing that the experience of punishment might increase an individual's future proclivity for crime beyond its impact on perceptions of the amount, cost, and likelihood of future punishment, a subject that we now turn to.

## B. Prisons as Criminogenic

Deterrence theorists conceptualize prison as a price that is calculated when making the choice to commit a crime. For those who portray prisons as criminogenic, such thinking suffers from reductionism. For them, years behind bars cut off from the community is not simply a price tag that one weighs—as one would weigh prices when, say, buying a pair of shoes. Rather, imprisonment is a social experience that places offenders in a unique social domain—the "society of captives" (Sykes 1958)—and that qualitatively restructures their lives from ones of freedom to ones of substantial constraint. Although intended to prevent crime, this unique experience in social segregation is argued to have the unintended consequence of increasing exposure to crime-

inducing influences and of decreasing exposure to prosocial influences. There are at least three perspectives, drawn from different scholarly traditions, which advance variants of this argument.

1. *Prisons as a Criminal Learning Environment.*    Whether as a therapeutic community or as a tightly run disciplinary regime, prisons are intended to provide a social environment that induces conformity and offers prosocial lessons. Classic studies of the prison community, however, have revealed that institutions were potentially marked by an oppositional inmate subculture into which offenders were socialized (see, e.g., Clemmer 1940; Sykes 1958). One approach, called "deprivation theory," saw the criminogenic prison culture as an adaptation to what Sykes (1958) called the "pains of imprisonment." Inmate solidarity and specific "argot roles" within the subculture were ways of mitigating the deprivations of prison life. Another approach, called "importation theory," saw the prison culture as a continuation of the worldview that offenders learned on the street and thus "imported" or carried with them as they moved behind bars. The "convict code" was not a response to prison life but constituted it in the sense that inmates infuse the institutional culture with their preexisting views (Irwin and Cressey 1962). More recently, researchers have documented how the process of importation has included gang membership and values as well as a more violent street culture that places a premium on toughness (Jacobs 1977; Irwin 1980, 2005; Carroll 1988; Wacquant 2001).

The key insight is that regardless of the precise mechanism, prisons are marked by the presence of cultural values supportive of crime that can be transmitted through daily interactions. It is thus a social learning environment in which criminal orientations are potentially reinforced. Consistent with social learning theory (Akers 1998), it can be expected that a custodial sentence will intensify a commitment to a life in crime.

2. *Prisons as a Labeling Effect.*    In the 1970s, labeling or societal reaction theory emerged as the dominant theory of crime and deviance (Cole 1975; see also Cullen and Cullen 1978). To a great extent, its popularity rested in what Hagan (1973) called the "sociology of the interesting"—that is, in advancing the ironic thesis that the state's efforts to stop crime have the unanticipated consequence of producing the very thing it was intended to suppress. This was often placed under the broader concept of the self-fulfilling prophecy.

Labeling theorists demarcated two broad ways in which labeling is

criminogenic. First, publicly stigmatizing and treating a person as a "criminal" inculcated in the individual this stained identity. Offenders who internalized a criminal identity thus would subsequently act in a way consistent with this self-conception. Initial experimentation with offending (sometimes called "primary deviance") could be stabilized because labeled individuals would organize their life around their core identity (sometimes called "secondary deviance"; see Lemert 1951). Prisons were especially consequential because they provided a lengthy opportunity for a criminal identity to be reinforced, to be accepted, and to govern conduct.

Second, similar to important theorists of the time (e.g., Merton, Sutherland, Hirschi), labeling theorists agreed that socially induced strain, differential association, and weak social bonds fostered criminal involvement. However, they saw these criminogenic influences not as main effects but as intervening variables between labeling and crime (see also Braithwaite 1989). It was societal reaction, especially when it involved imprisonment, that was integral to denying opportunities (strain theory), to enforcing prolonged association with offenders (differential association theory), and to eroding ties to family and to the conventional order (social bond theory). Being in prison was criminogenic because of enforced association with other offenders and because the person was removed from the labor market and from family and social relationships. But reentry into the community, the necessary by-product of imprisonment, also was criminogenic because ex-inmates faced job discrimination, the daunting challenge of trying to reestablish frayed bonds to conventional institutions, and placement into neighborhoods where criminal associations were readily available (see also Petersilia 2003; Travis 2005). No wonder, labeling theorists pointed out, that inmate reoffending was high.

3. *Prisons as an Inappropriate Treatment.*    Mainly led by Canadian psychologists, other scholars have sought to develop principles of effective correctional intervention (Andrews and Bonta 2006; Gendreau, Smith, and French 2006). This approach starts by empirically demarcating predictors of recidivism (Gendreau, Little, and Goggin 1996) and then explores which treatments are capable of altering these factors (i.e., are "responsive" to these risk factors). Interventions that are consistent with the principles of effective treatment have been found to achieve meaningful reductions in recidivism. Notably, deterrence-oriented interventions (e.g., boot camps, scared straight programs) and

mere incarceration absent a treatment component are viewed, in this approach, as "inappropriate" interventions. These scholars cite evidence that such sanctions, including imprisonment, have little effect on recidivism or are criminogenic (Andrews et al. 1990; Gendreau, Goggin, and Cullen 1999; Andrews and Bonta 2006; Gendreau, Smith, and French 2006; Smith 2006; Smith, Gendreau, and Swartz 2009; see also MacKenzie 2006; Lipsey and Cullen 2007).

This approach is particularly concerned about offenders' risk level. High-risk offenders are preferred targets for intervention because there is much change that can take place—assuming that appropriate interventions are used. This is often called the "risk principle." By contrast, low-risk offenders should receive minimal intervention. The danger is that inappropriate treatments—including imprisonment—can have a criminogenic effect on low-risk offenders, transforming those with low chances of recidivating into those destined to offend again. There is some evidence to support such a differential effect of imprisonment on offenders by risk level (Smith 2006).

## II. Estimating the Effect of Imprisonment on the Imprisoned

In this section, we discuss the primary methodological and substantive issues that will be emphasized in the following section's review of the empirical evidence. In the parlance of medical research on the effectiveness of drugs, our aim is to assess what is known about the dose-response relationship between imprisonment and reoffending. The review is divided between analyses of the impact on reoffending of receiving a custodial sentence or not and of the sentence length of those who are incarcerated.

We divide the literature in this fashion for two reasons. First and most importantly, for the reasons discussed in Section I, there are ample grounds for hypothesizing that the experience of imprisonment, independent of its length, may have an effect on the reoffending rate whether for the good or bad. The second reason is practical: the largest component of the literature assesses only the effects of confinement regardless of sentence length.

Although these two literatures examine conceptually distinct issues, most of the methodological issues we address apply to both. Thus, the discussion below does not distinguish between the two literatures un-

less otherwise noted. We use the term "treatment status" to distinguish individuals who were incarcerated or not or if incarcerated received different sentence lengths.

## A. The Basic Inference Problem

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 15 states in 1993. Langan and Levin (2002) find that within 3 years 68 percent had been arrested, 46.9 percent had been convicted, and 25.4 percent had been reimprisoned. Three-year arrest recidivism rates were even higher for two groups that as matter of public policy might be candidates for nonincarcerative sanctions—property (73.8 percent) and drug (66.7 percent) offenders. Those findings are not anomalous. In an earlier analysis of data from the United States, Beck and Shipley (1989) found comparably high recidivism rates for a 1983 prison release cohort. Similarly high recidivism rates are also found in non-American data. For example, in a cohort of individuals convicted in Dutch courts in 1997, the 3-year recidivism rate as measured by reconviction exceeded 60 percent (Nieuwbeerta, Nagin, and Blokland, forthcoming). Among individuals discharged from prison in 1996 in England and Wales, the 2-year reconviction recidivism rate was 57 percent (Cullen and Minchin 2000).

The high recidivism rate of former prisoners clearly demonstrates that the prison experience is not so aversive as to make most individuals give up crime entirely. That said, high recidivism rates tell us little about whether on the margin the prison experience makes individuals more or less crime prone. Determination of the effect of imprisonment on recidivism requires a comparison with the counterfactual—what the rate would have been had the individual not been imprisoned. More subtly, seemingly small changes in the recidivism rate may be reflective of more substantial changes in the underlying rate of offending.

Analytically, recidivism rates and offending rates are "tied at the hip." Because offending does not occur with predetermined regularity, it is useful to think of the rate of offending as a long-term average about which there are random variations over time. Thus, over short periods, offending may be above or below this average. For some periods of time, no crimes may be committed, even though the individual's long-term average rate of offending remains greater than zero. From this
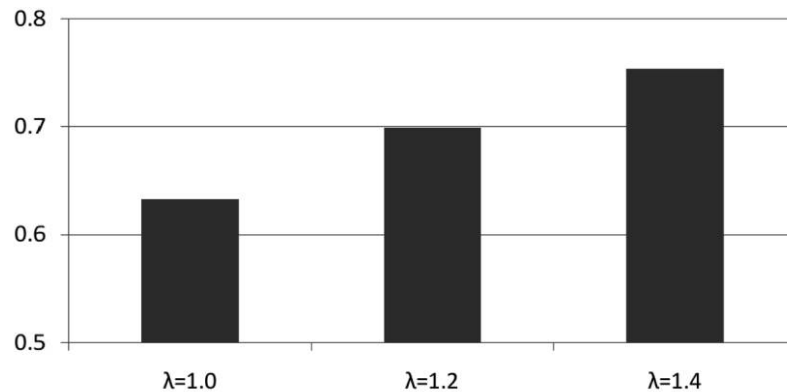
FIG. 1.—Three-year recidivism probabilities for different values of λ

perspective, recidivism is a forward-looking measure of offending risk for an individual with a record of prior offending. It is the probability that the individual will commit one or more offenses over a specified period of time.

Starting with the pioneering work of Avi-Itzhak and Shinnar (1973) on the incapacitation effect of imprisonment, it is commonly assumed that the random variations in offending over time can be modeled as a Poisson process. Avi-Itzhak and Shinnar used the now familiar notation of λ to denote the mean of this random process, namely, the mean rate of offending. Figure 1 reports recidivism probabilities for varying values of λ. Observe that for λ = 1.2 the recidivism probability is .7, which is about equal to the 3-year arrest recidivism rate reported by Langan and Levin (2002). Stated differently, under the assumption that arrests occur according to a Poisson process, if an individual's mean annual arrest rate is .4 (= 1.2/3) within 3 years, the probability of the person's having at least one arrest is .7. Also reported in figure 1 are recidivism probabilities for λ = 1.4 and λ = 1.0, which correspond to a fairly sizable 17 percent increase or decrease in rate of offending. Note, however, that the changes in recidivism probability are more modest: 10 percent or less in percentage terms—from .7 to .63 for the decline of λ to 1.0 and from .7 to .75 for the increase of λ to 1.4. The implication is that nontrivial effects of imprisonment on the mean rate of offending, whether up or down, will not necessarily be reflected in comparably large changes in recidivism probability. A

corollary point is that even though ex-prisoners have very high recidivism rates, the experience of imprisonment might still have resulted in a sizable decline in their underlying rate of offending. This is yet another important reason why high postprison recidivism rates are not prima facie evidence of the ineffectiveness of the prison experience in reducing criminality.

Recidivism rate is a muted indicator of the offending rate because the classification of an individual as a recidivist requires only the detection of a single criminal event. As a result, the recidivism rates of individuals with large differences in their underlying offending rate may be very similar. For example, the 1-year recidivism probability is nearly one for all values of $\lambda$ in excess of 5. Stated differently, the 1-year probability of reoffending one or more times is the same regardless whether the individual is offending at the rate of five crimes per year or 100 crimes per year.

The distinction between recidivism rate and rate of offending also has statistical implications for our review. Studies that use recidivism rate as their primary outcome measure will have greater difficulty in detecting differences across treatment status groups than studies that compare reoffending rate across treatment status group. Thus, one issue that receives special attention is whether the outcome is a recidivism rate or a reoffending rate.

We return now to the observation that the effect of imprisonment on reoffending rate is properly measured by the difference of two quantities: the postrelease rate of offending of the imprisoned over a specified period and their rate of offending over this same period had they not been subject to a custodial sanction. We cannot know the second quantity for the individuals who were actually imprisoned—it is their counterfactual rate of offending. It must be inferred from the behavior of others who were not imprisoned. The statistical gold standard for making this inference is a randomized experiment in which individuals from a specified population are randomly assigned between a custodial and a noncustodial sanction.

Few of the studies we review involve randomized experiments, but we give special emphasis to experiments because several important design features of an experiment provide a valuable analytical perspective for considering the much larger body of evidence based on analyses of nonexperimental data—which we also refer to in this essay as "observational data." More than 4 decades ago, Cochran (1965) reflected on

the design of studies attempting to draw causal inferences from observational data. He framed his recommendations in the context of still earlier advice by Dorn (1953), who suggested that the design of an observational study be organized around the question, "How should the study be conducted if it were possible to do it by controlled experimentation?" Certain issues are common to an experiment and an observational study, and these shared issues are brought into focus by thinking about the simpler situation of an experiment. Three specific features of an experiment are important for our purposes here: inferences about treatment effects pertain to a specified population, treatment effects are measured relative to a specified alternative, and randomization.

### B. Inferences Pertain to a Specific Population

Because randomization can occur only from a specified population, a key component of experimental design is specification of the population from which individuals will be randomly assigned between treatment and control. This design choice is of great importance because it establishes the boundary over which the results of the experiment apply. Findings about treatment effects apply only to the population from which subjects were drawn. Also important to keep in mind is that an experiment provides only an estimate of the average response to treatment. There may be large differences in the response to treatment across population members. For example, the U.S. Federal Food and Drug Administration's 2004 Blackbox warning that antidepressants may exacerbate suicidal tendencies in depressed adolescents is not inherently contradictory to the efficacy of these drugs for treating depression in the larger population. Similarly, there is no inherent contradiction between the conclusion that imprisonment exacerbates criminality within a population, on average, but for some groups within that population it reduces criminality. Because response to treatment may vary widely within the entire population, one important advantage of more narrowly defining the target population of the experiment is reduced heterogeneity in individual-level responses to treatment. The reduction in heterogeneity not only will make it easier to detect the average treatment effect but also will make the average response to treatment more representative of the response of all individuals within the sampled population. However, an important disadvantage of narrowly defining the population is that the experiment is not informative

from a statistical perspective about the response to treatment in the larger unsampled population.

In experimental analyses consideration of the issue of the population from which the data are drawn is inescapable. In analyses of observational (or nonexperimental) data consideration of this issue often receives far less attention than it should. Inattention to the source of the data has many implications for the analysis and interpretation of the findings. Analysis of observational data requires that potential sources of bias somehow be "controlled for." The most frequently used method to accomplish this task is multivariate regression analysis. We discuss some important obstacles to success in accounting for sources of bias with regression as well as other methods in the discussion of randomization. Beyond the challenges of using regression to account for sources of bias, the results of a regression may be misinterpreted in ways that are relevant to the issues just discussed.

One common misinterpretation of regression is that limiting the analysis to one specific segment of a wider population of interest somehow biases the results. To be concrete, suppose that an analysis of the imprisonment effect on recidivism rate was limited to individuals with no prior record of imprisonment. Such a sample restriction does not bias the resulting estimate of the imprisonment effect. Rather, just as in an experiment, it limits the applicability of the estimate to only one specific segment of a larger population of interest—individuals receiving custodial sentences irrespective of their prior record.

Further, just as in experiments, in a regression-based analysis of observational data there is a trade-off between the costs of narrowness and the benefits of reduced individual heterogeneity. Consider again the example of a study that is limited to data on individuals with no prior record of imprisonment. Some analysts implicitly think of a regression coefficient as measuring the effect for all population members as opposed to the average effect across the population. The regression coefficient of the imprisonment or not variable does not measure the effect of imprisonment for all individuals sentenced to prison; it only measures the average of that effect. If the effect of imprisonment on recidivism depends upon prior experience with imprisonment, the advantage of restricting the sample to individuals with no prior experience with imprisonment is a reduction of that source of treatment effect heterogeneity within the population. The disadvantage is that the treatment effect estimate applies only to a narrower population.

We emphasize the narrowness-heterogeneity trade-off because the dose-response relationship between imprisonment and reoffending may vary across offender characteristics in ways that are important for science and public policy. The relationship may depend upon whether the individual has previously been imprisoned and on age. It may also depend on conviction offense. As we discuss in Section IV, variation across conviction offense in the effect of imprisonment has important policy implications. Thus, another issue that will be emphasized in the discussion is the characteristics of the population from which the data are drawn.

## C. Specification of Treatment and Control Conditions

The design of an experiment requires a clear specification of what constitutes treatment under the experimental and control conditions. Thus, in an experimental study of the effect of custodial versus noncustodial sanctions on recidivism, it is necessary to specify the length and conditions of confinement for the custodial treatment as well as the length and conditions of the noncustodial sanction (control) treatment. The specification of the treatment and control conditions could greatly affect the outcome of the experiment. The response to spending 1 month in a well-managed jail is likely quite different than that to spending 1 year in a violence-ridden prison. The design of the noncustodial condition may have a similarly large impact on response. An interesting example is reported in Deschenes, Turner, and Petersilia (1995). This experimental study involved an attempt to randomly assign custodial and noncustodial sanctions in Minnesota. Implementation of treatment assignment proved difficult in part because the supervision requirements of the noncustodial sanction were deemed so onerous by some individuals that they refused placement into this condition and opted instead to remain in prison. Similarly, Wood and May (2003) report that for certain types of offenders, noncustodial sanctions are seen as overall being more costly than imprisonment. Ambiguity about the comparative harshness of custodial and noncustodial sanctions is likely to be largest in countries other than the United States, where sentences tend to be much shorter and prison conditions less onerous. Still another dimension of the sanction experience that may affect recidivism is whether the individual receives some sort of rehabilitative treatment in custody or in the community.

In an analysis of observational data, where the analyst does not de-

sign the experimental and control treatments, it is easier to neglect the issue of what the "imprisonment" and "nonimprisonment" conditions constitute. Imprisonment and nonimprisonment conditions not only may vary widely across studies but also may be very different across individuals within a study. Thus, the issue of what constitutes treatment and control also receives special attention in our review.

### D. Randomization

The discussion below concerns the implications of the absence of randomization in observational studies of the effect of imprisonment. It addresses two distinct topics: the minimum set of characteristics that must be taken into account to obtain a credible estimate of the effect of imprisonment and the possible impact of unaccounted-for factors on the treatment effect estimate. These topics are discussed in turn, but before turning to them, we discuss the role of randomization in experiments.

Randomization's salient status in experimental design stems from its capacity to assure that subjects assigned to treatment and control groups systematically differ in only one way—their treatment status. More specifically, randomization ensures that in expectation there are no differences in any characteristic whether measured or not, or whether thought to be relevant or not, between treatment and control. In turn, this ensures that in expectation the difference in outcome between treatment and control measures the effect of treatment. Stated differently, in expectation the outcome for the controls provides the counterfactual for the treated—that is, what would have happened to them, on average, had they been in the control condition and vice versa. By contrast, in analyses of observational data, there is no such guarantee that the treated and control group differ only in their treatment status. As a result, other systematic differences may bias the treatment-effect estimate obtained from a simple comparison of outcomes for the treated and controls. For example, suppose the experience of imprisonment has no effect on recidivism. We would still likely observe higher reoffending among the imprisoned compared to the nonimprisoned because persons with more lengthy prior records are more likely to be imprisoned and are also more likely to return to crime.

The primary objective of most statistical methods for drawing causal inferences from observational data is to somehow take account of other variables that might bias the desired treatment-effect estimate. Thus,

in observational studies, characteristics of individuals and their circumstances are generally taken into account to avoid bias. In contrast, in experimental studies blocking or stratifying by characteristics of study participants is not done to avoid bias; that is the job of randomization. Instead, it is done to reduce variance or to learn how treatment effects vary across selected characteristics of study participants (e.g., sex).

Suppose we had an observational data set that measured the post-sanction offending of a sample of individuals who had received and not received a custodial sanction. Per the prior discussion of specification of treatment and control conditions, decisions would have to be made on how to account for differences in sentence length among the imprisoned perhaps by limiting the analysis to individuals receiving sentences within some specified interval, for example, less than 2 years. Similarly, design decisions would have to be made about whether to distinguish among different types of noncustodial sanctions. Having made these design decisions, the next crucial question that needs to be addressed is: what is a minimum set of characteristics of the individual that must be taken into account to assess the effect of imprisonment on subsequent offending? The answer to this question requires careful consideration of factors that might affect both the sentencing decision and the reoffending rate independent of the actual sentenced received. Any such variable may lead to bias in the treatment-effect estimate of imprisonment. Stated in such general terms, the list of potentially confounding variables is endless, so we reframe the question as: what variables do we know are strongly related to one or both outcomes, sentence and/or offending, based on prior research or on institutional realities? These variables should constitute the minimum necessary set of control variables. In our judgment, two case characteristic variables—prior record and conviction offense type—and three demographic variables—age, race, and sex—definitely should be included on this list.

The criminal statutes of all countries in Western Europe and North America prescribe criminal penalties according to the conviction offense type. More serious crimes are more likely to result in imprisonment and more lengthy sentences. According to the Langan and Levin (2002) analysis, recidivism rates also varied substantially by conviction offense type, a finding that is also borne out by other studies (Beck and Shipley 1989; Sabol et al. 2000; Sentencing Guidelines Commission 2005*a*, 2005*b*; Kentucky Department of Corrections n.d.).

Three-year arrest recidivism rates were highest for robbers (70.2 percent), burglars (74.0 percent), motor vehicle thieves (78.8 percent), and selling or possessing stolen property (77.4 percent) and lowest for homicide (40.7 percent), rape (48.0 percent), and other sexual assault (41.4 percent). One contributing factor to these differences across conviction offense type is likely age. The recidivism rates are lowest for conviction offense types that are more likely to result in lengthy sentences. Consequently, individuals convicted of these offences are likely on average to be older upon release. An extended discussion of the importance of controlling for age follows below.

Prior record of convictions is also generally relevant to the sentencing decision and is perhaps the best predictor of reoffending rate. An enormous literature shows that prior record—whether measured by arrest, conviction, or prior imprisonment—is a sturdy predictor of subsequent offending (Bureau of Justice Statistics 1988; Beck and Shipley 1989; Loeber and Le Blanc 1990; Nagin and Paternoster 1991; Gendreau, Little, and Goggin 1996; Langan and Levin 2002; Florida Department of Corrections 2003). The strength of this relationship is illustrated by the Langan and Levin (2002) analysis. They find that 3-year arrest recidivism rates increase from 40.6 percent for individuals with one prior arrest to 82.1 percent for individuals with 16 or more prior arrests. Data from the Cambridge Study of Delinquent Development (Farrington et al. 2006) also nicely illustrate this relationship with a general population sample rather than a prison sample. The study is based on a sample of about 400 males from a working-class neighborhood in London born around 1954. Conviction data are available from ages 10 to 50. Individuals with no convictions up to age 16 averaged 0.6 convictions from ages 17 to 50, whereas as those with one, two, or three convictions as juveniles, respectively, had an average of 3.8, 4.8, and 5.2 convictions over this age range.[3]

Turning now to the demographic variables, vast literatures document large differences in offending rates by age, sex, and race (Hindelang 1981; Bureau of Justice Statistics 1988; Blumstein 1995; Steffensmeier and Allan 1996; Hawkins et al. 2000; Sampson, Morenoff, and Raudenbush 2005; Haynie and Armstrong 2006; Fox and Zawitz 2007). There is also a smaller literature documenting differences in recidivism by demographic group (Beck and Shipley 1989; Gendreau, Little, and

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[3] We thank David Farrington for making these data available to us.

Goggin 1996; Sabol et al. 2000; Florida Department of Corrections 2003; Cannon and Wilson 2005; Sentencing Guidelines Commission 2005*a*, 2005*b*; Kentucky Department of Corrections n.d.). The differences are smaller than for the offending rate in the general population, but demographic differences still persist. For example, the Florida Department of Corrections (2003) found that black males were 27.1 percent more likely to reoffend than their white male counterparts. This report also found that for males each 1-year increase in age lowered the probability of reoffending by 3.2 percent. Similarly, Sabol et al. (2000) found that males, blacks, and young offenders have higher rates of return to federal prisons than females, whites, and older offenders (16.2 percent vs. 11.6 percent, 24.4 vs. 13.4 percent, and 13.9 percent vs. 11.0 percent, respectively).

For our purposes here, we give special attention to the importance and manner in which age is accounted for in the analysis. Cochran (1965) used the adjective "disturbing" to label variables that might confound the treatment-effect estimate in observational data. The simplest and most direct way to statistically account for "disturbing" variables is by matching each treated unit with a control unit that is identical on all disturbing variables thought to be relevant. Although clean, rigorous, and straightforward, this approach quickly runs afoul of the "curse of dimensionality," namely, there are too many variables upon which it would be desirable to match to identify suitable matching controls. Other methods must be used to account for disturbing variables. Two of the most widely used are regression-based methods and propensity score matching. It is important, however, to recognize that the surest way of accounting for a variable that may somehow be biasing results is by exact matching. It is for this reason that we judge it important to match imprisoned and nonimprisoned individuals by age and thereafter to use other methods such as regression or propensity score matching to account for other potentially confounding variables. We give age this special status for control because offending rates are highly age dependent and because the postsanction outcome variable, offending rate or recidivism probability, necessarily must be measured over age. Therefore, we regarded it as very important in analyses of observational data to compare the postsanction offending rate of an imprisoned individual with that of one or more nonimprisoned individuals who are the same age.

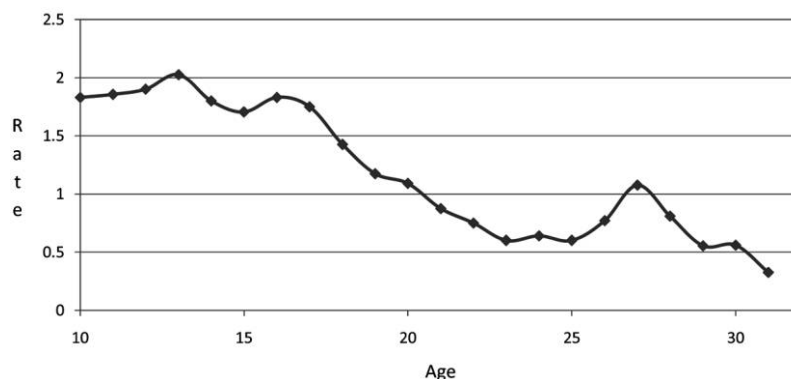Data from the Cambridge Study of Delinquent Development (Far-

F IG . 2.—Three-year smoothed reoffending rate (Cambridge Study of Delinquent Development).

rington et al. 2006) illustrate the sensitivity of reoffending rate to age. The demonstration is based on the conviction counts of each study participant from age 10, the youngest age of criminal responsibility in England, to age 32. For individuals convicted at each age their average rate of offending was computed for the three subsequent years. For example, for all individuals convicted at age 16, their average conviction rate from ages 17 to 19 was computed. To smooth out year-to-year fluctuations in reoffending rate at each age, a smoothed reoffending rate was computed. At each age, it equals 50 percent of the reoffending average for that age plus 50 percent of the rate for the immediately prior age. The smoothed series is shown in figure 2. From the outset, the reoffending rate starts high, reflecting the high offending rate of a small group of early starters. It then makes a brief decline, which is followed by a new rise that peaks at age 17. Thereafter, the rate declines precipitously until age 23, whereupon it moves erratically.

Figure 3 shows a companion curve based on the conviction records of all individuals convicted in the Dutch courts in 1997. The Dutch data follow a different pattern. The smoothed reoffending rate rises erratically until about age 30, whereupon it declines rapidly.

Despite the differences between the Dutch and English data, both make clear that reoffending rates are very age dependent. Consequently, age must be very exactly controlled for to avoid age-related biases in postsanction comparisons of offending rates of individuals receiving different sanction types. Small imbalances in the age distri-
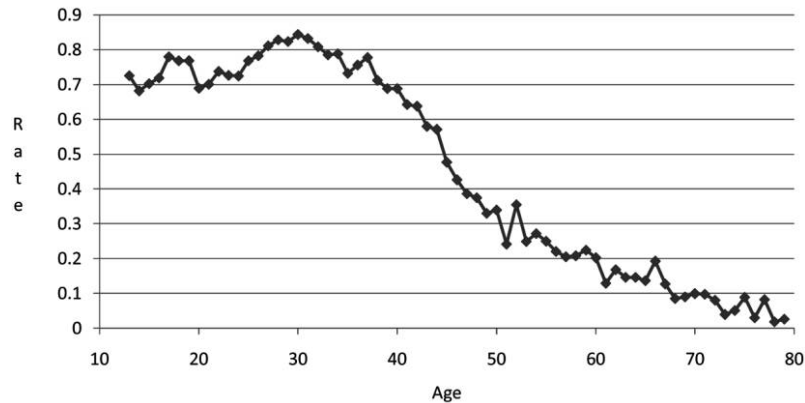
FIG. 3.—Three-year smoothed reoffending rate (Netherlands)

bution between comparison groups could contaminate the sanction effect difference with large age effects. For instance, in the Cambridge data the smoothed reoffending rate declines by an average of 18 percent per year between 17 and 23. Changes of this magnitude may well be larger than the effect, whether positive or negative, of the experience of imprisonment on the reoffending rate. Further, because the chances of incarceration increase with prior record and because a prior record takes time to accumulate and individuals necessarily age while incarcerated, there are likely to be age differences between the incarcerated and nonincarcerated convictees. For these reasons, we believe that exact matching on age is very important in studies of the effect of imprisonment on recidivism.[4]

## E. Making Judgments about the Veracity of Findings

Suppose a study takes into account the minimum set of potentially confounding variables described above. How confident can one be of the resulting imprisonment effect estimate? There is, of course, no definitive answer to this question, but an econometric perspective provides a useful vantage point for considering an answer. In modeling

[4] One might think that a demonstration that the average age of treated and controls are the same is sufficient to rule out age as potential source of bias in the treatment effect estimate. This is not correct. If there is a nonlinear relationship between age and offending, as there clearly is, difference in higher order moments (e.g., variance) between the treated and the controls may still induce bias.

choice, in this case the judge's sentencing decision between a custodial versus noncustodial sanction, econometricians draw a strong distinction between information available to and used by the decision maker and information available to the statistical analyst (see Heckman 1990; Manski 1995). The decision makers are assumed to base their decision on a larger set of information than is available to the analyst. Failure to account for this disjuncture in the information set of the decision maker and analyst can lead to serious bias. For example, suppose sentencing decisions were influenced by whether the individual is a drug or alcohol addict, but that data on drug or alcohol addiction status are unavailable to the analyst. Because of the strong positive correlation of drug or alcohol addiction and crime (Anglin and Hser 1987; Anglin and Speckart 1988; Harrison and Gfroerer 1992; Fergusson, Lynskey, and Horwood 1996; Ge, Donnellan, and Wenk 2001; Gjeruldsen, Myrvang, and Opjordsmoen 2004; Dorsey, Zawitz, and Middleton 2007), the lack of statistical control for addiction will bias the treatment-effect estimate unless other controls used by the analyst somehow account for the absence of direct control for addiction.

From the econometric perspective, the question concerning our confidence in the imprisonment treatment-effect estimate is reframed more precisely as: how much information is left out of the minimal set of control variables that affect judges' sentencing decisions and also likely influence recidivism? The answer to this question depends in part upon the time and jurisdiction that are the source of the data under analysis. An individual's sentence in some jurisdictions is largely determined by the types of offenses for which the person is convicted and by his or her prior record. For data from jurisdictions such as these, whether in the United States or elsewhere, our minimal set is probably sufficient for making credible, albeit not conclusive, inferences about imprisonment effects.

For data from times and jurisdictions where judges have more sentencing discretion, research on clinical versus actuarial prediction of violence provides a useful perspective for judging the adequacy of our minimal set of controls. Clinical risk assessment refers to the unstructured judgments of trained mental health practitioners based on their theoretical orientation and clinical experience. Actuarial assessments are based on structured assessment instruments for collecting and analyzing data and ultimately making a prediction (Monahan 2006, 2008). Research has overwhelmingly demonstrated the superiority of actuarial

methods (Grove and Meehl 1996; Swets, Dawes, and Monahan 2000; Ægisdóttir et al. 2006). This conclusion suggests that subtle characteristics of the individuals or their circumstances (e.g., demeanor) that may affect a judge's sentence decision and are unlikely to be recorded in the data are also unlikely to bias results because they are not very predictive of behavioral outcomes.

Thus, the adequacy of the minimal set of control variables in circumstances where judges have discretion would seem to turn on the overlap between the minimal set and the information in actuarial instruments. Monahan (2006, 2008) provides a valuable summary of the four categories of variables that are included in modern actuarial assessment instruments. One of Monahan's categories is labeled "what the individual 'is'" as measured by age, sex, race, and personality. A second category describes what the person "has" as measured by major mental disorders, personality disorders, and substance use disorders. The third category describes what the person "has done" as measured by prior crime and violence. The fourth category describes what has been "done to" the person as measured by pathological family environment and victimization.

Note that there are many important points of overlap between Monahan's list and our minimal list. Age, sex, race, conviction offense, and prior record are important components of what the individual "is" and "has done." There is also some degree of correspondence between what the person "has" and the offenses for which the person is convicted. For example, there are important points of similarity between criminal behavior and mental health diagnoses like conduct disorder and antisocial personality. However, this correspondence should not be overstated because criminal infractions and mental health diagnoses are not the same things. Depression and schizophrenia, for instance, have no symptoms that can be described as criminal behaviors. More generally, Monahan's list includes many items that might well appear in a presentence report or come up in a sentencing hearing. Thus, the ultimate judgment on whether the minimal list of controls achieves a threshold of credibility must necessarily be judged on a case-by-case basis. The determination should turn upon knowledge of the sentencing practices that generated the data underlying the study, such as how much discretion judges have, to what degrees sentencing is routinized by custom or bureaucratic procedure, and what information is available in presentence reports. Alas, in this review, we do not have the infor-

mation to make such a determination for each of the studies that we consider.

### III. Review of the Evidence

We separately review studies of the effect of custodial versus noncustodial sanctions and studies of the effect of sentence length on reoffending. The review of the evidence on custodial versus noncustodial sanctions is organized around four categories of studies based on their methodology: experimental and quasi-experimental studies, matching studies based on observational data, regression studies based on observational data, and a small group of studies using other methods.

The literature on the effects of imprisonment varies on many dimensions that are relevant to an assessment of its scientific quality and to its substantive interpretation. These include issues such as methodology used, vintage of the study, characteristics of the offender population, and type of custodial and noncustodial sanction examined. We organize our review along the dimension of the type of methodology used because the question being asked—what is the effect of imprisonment on reoffending?—is first and foremost a question that must be answered statistically. An assessment of the quality of the evidence, therefore, must focus on the rigor of statistical analysis. Because each class of methodology poses specific challenges to its application, we chose to use methodology as the primary organizing device for categorizing studies.

We also chose not to use meta-analysis as a device for summarizing the evidence. This decision was made due to a concern that, at this stage in the research, the synthesis of evidence through this statistical method would obscure important subtleties related to large differences in quality across studies, the types of sanction options being examined, and the characteristics of the offender population. Nonetheless, especially as studies on the impact of imprisonment on reoffending become more plentiful and of a higher quality, the application of meta-analysis to the extant body of evidence would be useful.

A prior review by Villettaz, Killias, and Zoder (2006) of the effects of incarceration on reoffending, which concluded that there was no systematic evidence for either a criminogenic or preventive effect, provided an invaluable starting point for identifying the studies included in this review. We are greatly indebted to these authors for their efforts

in tracking down studies. The list of studies from Villettaz, Killias, and Zoder was supplemented by another review of the literature by Gendreau, Goggin, and Cullen (1999) and by our own efforts to identify relevant studies mostly of more recent vintage. (Gendreau, Goggin, and Cullen concluded that the evidence pointed to a criminogenic effect of the prison experience.) Although there are undoubtedly studies that we have not identified, we are confident that we have been successful in identifying all experimental studies and all studies based on observational data that account for our minimum list of control variables. We also expand on Villettaz, Killias, and Zoder (2006) by reviewing evidence on the relationship between time incarcerated and reoffending rate.

## A. Experimental and Quasi-Experimental Studies

Only five studies randomly assigned custodial versus noncustodial sentences. To this group, we add one Netherlands-based quasi-experimental study that involved a royal pardon in celebration of the wedding of the Princess (now Queen) Beatrix. Prison sentences of 14 days or less were suspended for crimes committed prior to January 1, 1966. Persons sentenced to prison for 14 days or less for crimes committed after that date did not benefit from the pardon. The contrast between those benefiting and not benefiting from the royal pardon formed the basis for inferring the imprisonment effect.

Table 1 summarizes these five studies in terms of the characteristics of the population studied, the nature of the custodial and noncustodial sanctions that were randomized between treatment and control, and the outcome of the experiment. Three dimensions of outcome are summarized. The first is whether the outcome is measured by recidivism rate or offending rate. The former measure inherently has less statistical power for inferring a statistically significant treatment effect. The second is whether the point estimate of the treatment effect, the difference in outcome between the custodial and noncustodial groups, points to a lower or higher rate of recidivism/reoffending rate for the custodial group. The third is whether this difference is statistically significant at the .05 level or lower.

Consider first the outcomes of the studies. Among the experimental studies, all but Bergman (1976) measure outcomes using both offending and recidivism rates. The quasi-experimental study by Van der Werff (1979) is based on recidivism rates. All five studies report at least

one point estimate suggesting a criminogenic effect of incarceration. While most are not statistically significant, three studies report at least one significant criminogenic point estimate. Three of five studies report at least one point estimate suggesting a preventive effect of a custodial sentence, but only in Barton and Butts (1990) is the preventive effect estimate statistically significant. We note, however, that this significant preventive effect may reflect an incapacitation rather than a behavioral response. Barton and Butts report data indicating that those assigned to secure confinement were actually incarcerated for nearly two-thirds of the 1-year follow-up period.

Taken as a whole, it is our judgment that the experimental studies point more toward a criminogenic rather than preventive effect of custodial sanctions. The evidence for this conclusion, however, is weak because it is based on only a small number of studies, and many of the point estimates are not statistically significant.

Several features of the populations used in these studies also limit their usefulness for understanding the effects of imprisonment on reoffending in the context of contemporary trends in the use of imprisonment. Two of the five studies involve juveniles, and all but one, Killias, Aebi, and Ribeaud (2000), use data that are more than 20 years old, with two based on data from more than 40 years ago. Among the four studies involving adults, only Bergman (1976) is based on populations that can be characterized as serious adult offenders. The subjects of the Killias, Aebi, and Ribeaud (2000) and the Van der Werff (1979) studies both involved populations of individuals who, if imprisoned, would have received sentences of 14 days or less. Thus, their offenses were unlikely to be very serious.

### B. Matching Studies

Eleven studies are categorized as matching studies. Studies were designated as matching based if they relied principally upon either variable-by-variable matching or propensity score matching to account for potentially confounding variables.

In principle, variable-by-variable matching is the foolproof way of controlling for potentially confounding variables. In practice, the capacity to control for confounders via "by-variable" matching is limited by the tyranny of dimensionality. The availability of matches on multiple dimensions rapidly moves to the null set as more dimensions of matching are added. The tyranny of dimensionality is further exacer-

TABLE 1

Experimental and Quasi-Experimental Studies of Custodial versus Noncustodial Sanctions

| | | Treatment Description | | Outcome | | |
|---|---|---|---|---|---|---|
| Authors | Population Characteristics | Custodial | Noncustodial | Offending/ Recidivism Rate | Sign of Custodial Effect | Statistical Significance of Effect (5%) |
| Barton and Butts (1990) | Male juveniles from Wayne County, MI, Juvenile Court, serious violent offenders excluded ($N = 511$) | Training school (avg. = 12.8 months) | Intensive supervision (avg. = 5.6 months) | Both | Official records: 2 1+ Self-report: 5+ | Official records: 2 of 2− sig. 1 of 1+ sig. Self-report: 1 of 5+ sig. |
| Bergman (1976) | Mostly male second felony offenders, Oakland County, MI ($N = 109$) | Incarceration | Extensive probation | Recidivism | 1+ | 1 of 1+ sig. |
| Killias, Aebi, and Ribeaud (2000) | Mostly male adults from Switzerland sentenced to prison terms of up to 14 days; only 22% convicted of a criminal code offense ($N = 123$) | Up to 14 days of jail | Community work: 8 hours = day in jail | Both | 3+ | 0 of 3+ sig. |

| Schneider (1986) | Mostly male juveniles in Boise, ID ($N = 181$) | 8 days + 9 months probation | Restitution/community service + 9 months probation | Both | 2+ | 0 of 2+ sig. |
|---|---|---|---|---|---|---|
| Van der Werff (1979; quasi-experimental) | Individuals with sentences of 14 days or less | 14 days or less | Suspended sentence | Recidivism | Traffic offenders: No difference  Property offenders: 1−  Violent offenders: 1+ | Property offenders: 0 of 1− sig.  Violent offenders: 1 of 1+ sig. |

NOTE.—Avg. = average; + = criminogenic effect; − = preventive effect; sig. = significant.

bated for matches on continuous variables. Matching on continuous variables such as age or income necessarily requires converting the variable into range categories. More fine-grained ranges provide for better control but also make it more difficult to identify suitable matches.

Table 2 summarizes the variables that were matched in the studies based on by-variable matching. To varying degrees, these studies account for our minimum set of control variables—age, race, sex, prior record, and current offense. Kraus (1974) matches on the largest number of variables (seven) that, with the possible exception of race, covers all of the minimum set. Even more impressive is how fine grained the matching was. In most instances, for example, individuals were matched on age within a year of their birth. Savolainen et al. (2002) also had quite detailed matching criteria that, for example, distinguished among seven charge types and five felony severity levels. However, they did not match on race, and at older ages matching seemed to be quite coarse. Smith and Akers (1993) also matched over all dimensions of the minimum set except race, but their matching variable for prior record, probation violations, was crude compared to Savolainen et al. (2002) and Kraus (1974). Muiluvuori (2001) matches on all dimensions of the minimum set, but matching on age is only within 5 years, and prior record is measured only by number of prior imprisonments. Finally, Petersilia and Turner with Peterson (1986) directly match only on gender and conviction offense. The remaining variables are accounted for via a three-level summary score and regression.

The results of all matching-based studies are summarized in table 3. For those studies based on "by-variable" matching, the most consistent set of findings is reported in the Kraus study that, in our judgment, also did the best job of matching. For both first offender juveniles and recidivist juveniles, the results point overwhelmingly toward a criminogenic effect of custodial sanctions. A majority of the point estimates in the other "by-variable" matching studies also point to a criminogenic effect, but only in the Savolainen et al. (2002) study are they consistently statistically significant.

Three studies are based on propensity score matching. In this context, the propensity score is the conditional probability of receiving a custodial sanction rather than a noncustodial sanction given observed covariates (Rosenbaum and Rubin 1983). In the propensity score–based studies reviewed here, the propensity score was created by a logit re-

## TABLE 2
### "By-Variable" Matching Studies: Variables Matched Upon

| Authors | Variables Matched from Minimum Set | Exact Matching or Age within 1 Year | Variables beyond Minimum | Comment |
|---|---|---|---|---|
| Kraus (1974) | All except possibly race* | Almost always yes | Age at first arrest | Generally succeeded in exact matching on key variables |
| Kraus (1978) | All except race | Yes | Number of current charges | Succeeded in matching |
| Kraus (1981) | All except possibly race* | No | None | Succeeded in exact matching on key variables |
| Muiluvuori (2001) | All† | No | None | Matching on age only within 5 years |
| Petersilia, Turner, with Peterson (1986) | Gender and conviction offense | No | Drug addiction, year of sentencing, county of conviction, three-level summary score | Prior record accounted for in a three-level summary score; age and race accounted for by regression |
| Savolainen et al. (2002) | All but race | No | Place of prosecution | |
| Scarpitti and Stephenson (1968) | All except current offense | All 16–17 years old | Socioeconomic status index (family income, education, and occupation of family breadwinner) | Matched on delinquency history index (age first known to court, number of delinquent offenses, types of prior offenses) |
| Smith and Akers (1993) | All except race | Not explicitly stated | None | Prior record was measured by probation violations not prior felonies and by sentencing cell |

* Data are from the early 1960s, a time when Australia was almost entirely white except for Aboriginals. Not clear whether this racial minority was included in the study.
† Race was not explicitly matched upon, but the location of the study, Finland, is nearly all white.

TABLE 3

Matching Studies: Findings on Custodial Sanction Effect

| Authors | Offending/ Recidivism Rate | Method of Matching | Treatment Description | | Sign of Custodial Effect | Statistical Significance of Effect (5%) |
|---|---|---|---|---|---|---|
| | | | Custodial | Noncustodial | | |
| Apel and Sweeten (2008) | Both | Propensity score | Incarcerated in an adult or juvenile facility (avg. = 4.1 months) | Convicted but not incarcerated | 27+ 18− | 3*/7† of 27+ sig. 1*/0† of 18− sig. |
| Kraus (1974) | Both | By variable | Detention | Probation | First offenders: 8+ 2− Recidivists: 19+ 2− | First offenders: 3 of 8+ sig. 1 of 2− sig. Recidivists: 19 of 19 + sig. 2 of 2− sig. |
| Kraus (1978) | Both | By variable | Remand in custody followed by probation | Remand at home followed by probation | 5+ 2− | 1 of 5+ sig. 0 of 2− sig. |
| Kraus (1981) | Both | By variable | Residential school for truants | Probation | 3+ 2− | 0 of 3+ sig. 2 of 2− sig. |
| Loughran et al. (2008) | Offending | Propensity score | Placement in custodial sanction (avg. = 11 months) | Probation | 1+ | 0 of 1+ sig. |

| | | | | | | |
|---|---|---|---|---|---|---|
| Muiluvuori (2001) | Both | By variable | Prison (avg. = 7.8 months) | Community service (avg. = 3 months) | 33+ 2− | No significance tests |
| Nieuwbeerta, Nagin, and Blokland (forthcoming) | Offending | Propensity score | Prison (less than 1 year) | Convicted but not imprisoned | 11+ 1− | 11 of 11+ sig. 0 of 1− sig. |
| Petersilia, Turner, with Peterson (1986) | Recidivism | By variable | Incarceration (avg. = 12.5 months) | Probation | 12+ | 4 of 12+ sig. |
| Savolainen et al. (2002) | Both | By variable | Incarceration | Various types of supervision and counseling | 10+ 8− | 9 of 10+ sig. 2 of 8− sig. |
| Scarpitti and Stephenson (1968) | Recidivism | By variable (specifically three indices) | Residential group (avg. = 9 months) Reformatory school (avg. = 4 months) | Probation Nonresidential group center (avg. = 4 months) | 3+ 1− | 2 of 3+ sig. 0 of 1− sig. |
| Smith and Akers (1993) | Recidivism | By variable | Prison | Community control | 9+ 3− | 0 of 9+ sig. 0 of 3− sig. |
| Weisburd, Waring, with Chayet (2001)[a] | Both (offending rate is ordinal in nature) | Propensity score | Prison | Convicted but not imprisoned | 10+ 5− | 0 of 10+ sig. 0 of 5− sig. |

NOTE.—Avg. = average; + = criminogenic effect; − = preventive effect; * = results for nearest neighbor matching; † = results for kernel matching; sig. = significant.

[a] Weisburd, Waring, and Chayet (1995) findings are included in this piece.

TABLE 4

Propensity Score–Based Matching Studies: Variables in Score

| Authors | Variables from Minimum Set | Exact Matching or Age within 1 Year | Variables beyond Minimum |
|---|---|---|---|
| Apel and Sweeten (2008) | All | No | Many measures of family and educational background and miscellaneous risk factors such as substance abuse |
| Loughran et al. (2008) | All but current offense[a] | Not explicitly stated | Many measures of mental health, substance abuse, family, education, employment background, punishment costs, social capital, risk/need scores, involvement in the community, impulse control, gang membership, and social support |
| Nieuwbeerta, Nagin, and Blokland (forthcoming) | All | Yes | Married, children, early conviction, trajectory group, and very detailed measurement of prior record and current offense severity |
| Weisburd, Waring, with Chayet (2001)[b] | All | No | Geographic location, education, socioeconomic status, cooperation with prosecution, remorse for crime, and detailed measurement of current offense severity |

[a] All participants were serious offenders, with most being guilty of felony offenses.
[b] Weisburd, Waring, and Chayet (1995) findings are included in this piece.

gression in which the dependent variable was receiving a custodial sanction or not and the regressors were the types of variables listed in table 4. Rosenbaum and Rubin (1983) demonstrate that if the propensity score is correctly specified, matching treated individuals with controls who have the same propensity score will balance all covariates included in the propensity score. This proof implies, for example, that if two individuals, one treated and one control, have the same propensity score, say a .2 chance of secure confinement, the covariates in the score will be of no further use in predicting which of these two individuals was actually incarcerated. Thus, by matching on a single metric, the propensity score, there will be no systematic tendency for the observed covariates determining that score to be different between the

treated and the controls. A nontechnical survey of methods and results about propensity scores is given by Joffe and Rosenbaum (1999).

The principal advantage of propensity score matching compared to variable-by-variable matching is that it averts the curse of dimensionality because there is generally no practical limit to the number of covariates that can be included in the propensity score and thereby can be balanced between those receiving and not receiving secure confinement. Table 4 summarizes the variables included in the propensity score–based analyses. All include the minimum control set in the model plus many other variables.

The results of the propensity score–based analyses, which are reported in table 3, are mixed. Nieuwbeerta, Nagin, and Blokland (forthcoming) find consistent evidence of a statistically significant criminogenic effect of (first-time) incarceration in a Netherlands-based sample. In a study of white-collar criminals in the U.S. federal courts, Weisburd, Waring, and Chayet (1995) find no statistically significant evidence of an effect, but all point estimates point in a criminogenic direction. We note, per the discussion in Section II, that the lack of significance may be attributable to their relying solely upon a recidivism outcome measure. Finally, Apel and Sweeten (2008) report both positive and negative point estimates, with most nonsignificant.

Overall across both types of matching studies, the evidence points to a criminogenic effect of the experience of incarceration. Although the preponderance of criminogenic point estimates is not statistically significant, there is more evidence for statistically significant effects than in the experimental studies.

The type and vintage of the data used in the matching-based studies are quite varied. The most recent data are used in the Apel and Sweeten study, which is based on the 1997 National Longitudinal Study of Youth, and the oldest are used in the Kraus study, which is based on data from the early 1960s. Six studies examine incarceration effects in adulthood only (i.e., 18 years or older), one study's exclusive focus is juveniles (Kraus), and one study examines effects in juvenile and adult age periods (Apel and Sweeten). Also, as would be expected, the treatment conditions for custodial and noncustodial sanctions are less precisely defined than in the experimental studies. Variation in the length of incarceration was generally not reported, and in more than half of the studies noncustodial sanctions combined all possible noncustodial sanctions from probation to community service to fine.

*C. Regression-Based Studies*

Regression-based studies form the largest body of evidence on the effect of custodial sanctions. The regression-based studies use various forms of multivariate regression analysis, such as logit regression or ordinary least squares regression, to account for potentially confounding variables. We identified 31 regression-based studies that included a dichotomous indicator variable distinguishing a custodial versus noncustodial sentence in the model. Table 5 summarizes the key features of these studies.

The studies examine a great variety of custodial and noncustodial sanction types. Some studies compare prison to different types of probation. Others combined prison with other forms of sanction (e.g., fines) or treatment (e.g., alcohol counseling) in making the comparison. Studies of juvenile populations focus on the effects of varying forms of institutional placement, such as shock incarceration or boot camps. The populations that are studied are also quite varied—there are studies of U.S. and non-U.S. populations, of juvenile and adult populations, and of different types of offenders such as drunk drivers or nonviolent offenders. The most commonly used outcome measure is recidivism.

In 22 studies, a majority of the custodial effect point estimates are positive, in seven a majority of the estimates are negative, and in two studies the estimates are equally split. Seventeen studies report at least one positive estimate that is statistically significant, and seven report at least one negative point estimate that is statistically significant. Thus, as in the matching studies, the predominant finding in the regression-based studies is an association suggesting a criminogenic effect of custodial sanctions. Although the variety of these studies makes it impossible to examine formally whether this pattern depends upon the type of custodial and noncustodial sanction examined or on the population studied, informally we could detect no such pattern.

Also reported in table 5 are the control variables utilized in these studies. Only 16 studies control for age, race, sex, prior record, and conviction offense type either by including the variable in the regression or limiting the analysis to specific types of people on one of these dimensions. A majority of the point estimates are positive in 13 of the studies. In the remaining three, a majority are negative. Twelve report at least one significant positive effect, and three report at least one significant negative effect. Thus, even for the subset of studies accounting for our minimum set of variables, the preponderance of the

point estimates point to a criminogenic effect. Also summarized in the table are other variables controlled for beyond the minimum set. The addition of more variables does not seem to alter the conclusion that the imposition of custodial sanctions is generally associated with higher recidivism.

Our principal concern with the regression-based studies is the manner in which they account for age. Inclusion of age as a regressor is not equivalent to matching on age. Precise modeling of the underlying relationship between offending rate and age is imperative to avert bias. Otherwise small imbalances in the average age between those receiving and not receiving custodial sanctions could reflect model imperfections in accounting for age rather than the actual effect of custodial sanctions on recidivism. We return to this issue when we discuss the evidence on the relationship between time served in prison and reoffending. Even in the studies that control for the minimum set of control variables, to some degree, measures of prior record were often crude.

### D. Other Studies

Below are brief descriptions of seven studies that do not neatly fit into the experimental, matching, or regression study categories. The studies are notable either because of the methodologies used or because of their findings—or both.

1. *Drago, Galbiati, and Vertova (2009).*    In May of 2006, Italy's Collective Clemency 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 5 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 1 and 36 months. This analysis examines whether the natural experiment created by the early release of these individuals had an effect on the reoffending rate. One key finding was that each month of residual sentence was associated with a 1.24 percent reduction in the propensity to recommit crime. The second key finding was that individuals who spent more time in prison for their clemency offense were less responsive to the incentive not to reoffend created by their having to serve out the residual sentence for that offense. This suggests that time served in prison reduces responsiveness to future punishment.

2. *Helland and Tabarrok (2007).*    This study examines whether Cal-

TABLE 5

Regression Studies: Findings on Custodial Sanction Effect

| Authors | Offending/ Re-cidivism Rate | Controlled Directly for Age, Sex, Race, Prior Record, and Conviction Offense (Yes/No) | Other Control Variables | Treatment Description | | Sign of Custodial Effect | Statistical Signifi-cance of Effect (5%) |
|---|---|---|---|---|---|---|---|
| | | | | Custodial | Noncustodial | | |
| Annan, Martin, and Forst (1986); Martin, Annan, and Forst (1993) | Recidivism | All but race | Blood alcohol content, counsel, judge with reputation for jail sentence | Jail (avg. = 2 days) | Fine | 1+ | 0 of 1+ sig. |
| Bondeson (1994) | Recidivism | No | Risk score of 36 variables from six in-dices: (1) home and conditions of up-bringing, (2) personal attributes and scholastic achievement, (3) maladjusted behavior and Child Welfare Board ac-tion, (4) abuse of alcohol and drugs, (5) occupational status, housing and ac-commodation, (6) sentences for earlier criminality[a] | Probation with institu-tional treatment | Conditional sentence Probation | 2+ | 0 of 2+ sig. |
| Bonta, Wallace-Capretta, and Rooney (2000); same findings in Bonta, Wallace-Capretta, and Rooney (1999) | Recidivism | All but age, race, prior record, and current offense[b] | Controlled for risk through LSI-R score (includes criminal history, accom-modation, emotional health, financial problems, attitudes/orientations, educa-tion/employment, companions, sub-stance abuse, leisure/recreation, family relationships)[c] | Prison | Treated probation and EM combined | Low risk: 1− High risk: 1+ | Low risk: No signif-icance test High risk: No signif-icance test |
| Brennan and Mednick (1994) | Recidivism | All but race[d] | Time in prison | Prison | Fines Probation | 10+ | 1 of 10+ sig. |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Clarke and Harrison (1992) | Recidivism | Yes | Time served | Regular parolee (avg. = 20 months) Max out parolee (avg. = 28 months) Paroled and terminated (avg. = 7 months) Split probation (up to 6 months) | Regular probation | 17+ 3− | 15 of 17+ sig. 0 of 3− sig. |
| De Jong (1997) | Recidivism | Yes | Days sentenced, positive drug tests, education, employment, marital status | Jail | No jail | 2+ | 1 of 2+ sig. |
| De Young (1997) | Offending | Yes (race was controlled with an aggregate measure: % black in ZIP code) | Class of driver's license, prior crashes, ZIP code crash average, moving violation average, average travel time to work, % unemployed, % receiving public assistance, % renting residence, median family income | Treatment program, jail and license restriction Treatment program and jail Jail and license suspension Jail only | Treatment program and license restriction Treatment program | 8+ | 4 of 8+ sig.[e] |
| Flowers, Carr, and Ruback (1991) | Offending | All but prior record | Place of conviction, risk score (including substance use, employment, address change, prior record, current offense), need scores (including substance use, health, mental ability, employment, education, and marital, emotional, financial, familial ability) | SAI graduates (boot camp) (avg. = 3 months, then probation) Incarceration with no priors under 6 months Incarceration with no priors 6–36 months Incarceration with priors under 6 months Incarceration with priors 6–36 months | Regular probation Intensive probation supervision | 8+ 2− | 6 of 8+ sig.[f] 0 of 2− sig.[f] |
| Gordan and Glaser (1991) | Recidivism | Yes | Education, employment, drug problems | Probation + jail Probation + jail + financial penalty | Probation only Probation + financial penalty | 5+ 1− | 5 of 5+ sig. 0 of 1− sig. |

TABLE 5 (*Continued*)

| Authors | Offending/ Re-cidivism Rate | Controlled Directly for Age, Sex, Race, Prior Record, and Conviction Offense (Yes/No) | Other Control Variables | Treatment Description | | Sign of Custodial Effect | Statistical Signifi-cance of Effect (5%) |
|---|---|---|---|---|---|---|---|
| | | | | Custodial | Noncustodial | | |
| Gottfredson and Barton (1993) | Offending | All but sex, race, current offense[g] | Seriousness ranking for most serious alleged crime and place of residence | Youth facility | Noninstitutionalized (after facility closed) | Official: 8− Self-report: 5+ 5− | Official:[h] 4 of 8− sig. Self-report:[h] 0 of 5+ sig. 1 of 5− sig. |
| Gottfredson (1998) | Both | No | Multiple risk measures that include age, judge rating of arrest record, race, substance abuse, prior record, serious-ness of offense, type of offense, and sex, time served, time in community, selection for confinement (age, type of offense, seriousness, mitigating or ag-gravating factors, counsel and proba-tion recommendation, judge's impor-tance of rehabilitation or retribution)[i] | Split sentence Youth facility Jail Prison (avg. = 44 months for people in jail or prison) | Probation | 5+ 7− | 1 of 5+ sig. 1 of 7− sig. |
| Jolin and Stipak (1992) | Recidivism | All but race and prior record | Risk assessment score, employment, marital status, substance abuse, time at risk | Work release | Intensive outpatient drug program (avg. = 7 months) ESP (EM and home confinement) | 1+ 1− | 0 of 1+ sig. 0 of 1− sig. |
| Jones and Ross (1997a) | Recidivism | Yes | 14 client assessment items (including age at first conviction, attitude, fi-nances, associates, substance use, mari-tal status, prior record), follow-up pe-riod, county | Boot camp (IMPACT) (avg. = 3–4 months, then released on su-pervised probation) | EM/house arrest (avg. = 3 months, then released as high-risk case on regular probation) | 5− | 5 of 5− sig. |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Jones and Ross (1997b) | Recidivism | Yes | Education, employment, address change, county, follow-up period, marital status | Boot camp (IMPACT) (avg. = 3–4 months) | Probation | 4+ 1− | 2 of 4+ sig. 0 of 1− sig. |
| Jones (1991) | Recidivism | All but prior record and current offense[j] | Time at risk, drug abuse | Prison | Probation | 1+ | 0 of 1+ sig. |
| Lloyd, Mair, and Hough (1994) | Recidivism | All but race | None | Prison | Community service Probation 4A/4B Regular probation | 3− | 1 of 3− sig. |
| MacKenzie (1991) | Recidivism | All but race and current offense[k] | Intensity of supervision[l] | Shock incarceration (avg. = 3–4 months) | Probation | 2+ | 0 of 2+ sig. |
| MacKenzie et al. (1995) | Recidivism | Yes | Intensity of supervision and time | Boot camp (separately for each state—GA, LA, SC, and TX) | Probation (separately for each state) | 5+ 14− | 3 of 5+ sig. 3 of 14− sig. |
| MacKenzie and Shaw (1993) | Recidivism | All but sex,[m] race, current offense[n] | . . .[o] | Shock incarceration | Probation | 3+ 2− | 2 of 3+ sig. 1 of 2− sig. |
| Nirel et al. (1997) | Recidivism | All but sex, race, and current offense[p] | Propensity score (PS) determined with stepwise regression with marital status, born in Israel, religion, sex, age, type of offense, sentencing agent, criminal history as covariates; however, the regression only led to the inclusion of marital status, type of offense, prior record, and sentencing agent in the PS | Prison (up to 6 months) | Service work | 1+ | 1 of 1+ sig.[q] |
| Nunes et al. (2007) | Recidivism | All but race, prior record, and age | RRASOR score (age at release, prior sex offenses, victim gender, and victim-offender relationship) | Incarceration (avg. = 21.24 months) | Community sanction | 2+ | 0 of 2+ sig. |
| Parisi (1981) | Recidivism | No, but all in risk score | Risk score (sex, race, age, marital status, education, prior record, current offense, and term of probation), time under supervision | Split sentence | Probation | 5+ 6− | 0 of 5+ sig. 0 of 6− sig. |

TABLE 5 (*Continued*)

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| Authors | Offending/ Re-cidivism Rate | Controlled Directly for Age, Sex, Race, Prior Record, and Conviction Offense (Yes/No) | Other Control Variables | Treatment Description | | Sign of Custodial Effect | Statistical Signifi-cance of Effect (5%) |
|---|---|---|---|---|---|---|---|
| | | | | Custodial | Noncustodial | | |
| Roeger (1994) | Recidivism | Yes | Marital status, education, employment | Parolees (after serving between 1 month and 2 years) Prisoners released sentence served | Probation Community service | 4+ | 0 of 4+ sig. |
| Spohn and Holleran (2002) | Recidivism | Yes | Employment and predicted probability of imprisonment in 1993 (seriousness of offense, statutory classification of the offense, number of convictions, offender's prior criminal record, whether the offender was on probation at the time of arrest, whether a gun was used, type of disposition, pretrial status, race, age, gender, employment, type of attorney) | Prison | Probation | 4+ | 4 of 4+ sig. |
| Tashima and Marelich (1989) | Offending | Yes[r] | Prior accidents, license class, ZIP code averages of single, separated, education, urban, carpool, own car, income | Jail only First offender program + jail | License suspension License restriction only First offender program + license restriction SB38 program + license restriction | 14+ 2− | 12 of 14+ sig.[s] 0 of 2− sig.[s] |

| Study | Outcome | Controls | Control variables | Harsher condition | Lenient condition | Direction | Significance |
|---|---|---|---|---|---|---|---|
| Taxman and Piquero (1998) | Recidivism | All but race | Numerous punishment (license restriction, fine, probation) and rehabilitative (abstinence, AA, alcohol education) measures | Jail | No jail | Full sample: 1+ First offenders only: 1− | Full sample: 0 of 1+ sig. First offenders only: 0 of 1− sig. |
| Ulmer (2001) | Recidivism | Yes | Education, marital status, drug treatment ordered, time since release/ sentencing | Incarceration Work release | Probation House arrest | 3+ 1− | 0 of 3+ sig. 0 of 1− sig. |
| Van der Werff (1981) | Recidivism | All but race | None | Completely and partly unconditional custodial sanctions | Fines and all completely conditional custodial sentences | 4+ | 3 of 4+ sig. |
| Weisburd, Waring, with Chayet (2001), separate analysis than presented in matched studies table | Recidivism | Yes | Marital status, home ownership, parenthood, community reputation, class position, education, substance abuse, education, informal sanctions (i.e., family suffering, loss of job, etc.) | Prison | Convicted but not imprisoned | 1+ | 0 of 1+ sig. |
| Wheeler and Hissong (1988) | Recidivism | Yes | None | Jail | Fine Probation | 2+ | 1 of 2+ sig. |
| Wiebush (1993) | Both | None[t] | | Department of Youth Services placement | Intensive supervision unit (minimum = 6 months) | 2+ 2− | 0 of 2+ sig. 1 of 2− sig. |
| Wooldredge (1988) | Recidivism | Yes | GPA, county, parental marital status, learning disabilities, characterized as behavioral problems | 1 month detention 1 year probation + 1 month detention 1 year probation + more than 1 month detention 2 years probation + 1 month detention 2 years probation + more than 1 month detention | Case dismissed 1 year probation 2 years probation | 3+ 6− | 1 of 3+ sig. 3 of 6− sig. |

## TABLE 5 (*Continued*)

| Authors | Offending/ Re-cidivism Rate | Controlled Directly for Age, Sex, Race, Prior Record, and Conviction Offense (Yes/No) | Other Control Variables | Treatment Description | | Sign of Custodial Effect | Statistical Signifi-cance of Effect (5%) |
|---|---|---|---|---|---|---|---|
| | | | | Custodial | Noncustodial | | |
| Wright and Mays (1998) | Recidivism | Yes | None | Boot camp graduates Prisoners | Probationers | 2+ | 1 of 2+ sig. |

NOTE.—Avg. = average; + = criminogenic effect; − = preventive effect; sig. = significant. EM = electronic monitoring.
[a] Risk score did not include race, age, current offense.
[b] Only included nonviolent, nonsexual offenders.
[c] Nonsignificant differences between groups on age, marital status, education, substance abuse, mental health, employment, finances, marital status.
[d] Race was not explicitly controlled but the location of the study, Denmark, is nearly all white. Only examined property offenders.
[e] Only report the first DUI offender results.
[f] Only report the 36-month follow-up.
[g] Only controlled for significant differences between groups (nonsignificant differences for sex, race, current offense).
[h] Only report longest follow-up, 2.5 years.
[i] Above variables were most related to recidivism; thus, there were other variables such as race that were not included in the risk score that did not predict recidivism.
[j] Most measures of prior record and current offense were not significantly related to rearrest, thus not included as controls.
[k] Nonsignificant differences on race, current crime type, and adult criminal history.
[l] Nonsignificant differences on education.
[m] Sex was not explicitly controlled; however, the number of women in the sample is small.
[n] Nonsignificant differences for race and type of current offense.
[o] Nonsignificant differences for education and age at first arrest.
[p] Sex, age, current offense are included in propensity score.
[q] Only report findings of regression analysis.
[r] Race is percent nonwhite in ZIP code and only report for first-time DUI offenders.
[s] Only report longest follow-up (2 years).
[t] Nonsignificant differences on age, race, sex, adjudicated complaints, offense history score, nature of current offense, prior probation, and risk score.

ifornia's "Three Strikes" law deters offending among individuals previously convicted of strike-eligible offenses. The future offending of individuals convicted of two strikable offenses was compared with that of individuals who had been convicted of only one strikable offense but who, in addition, had been tried for a second strikable offense but were ultimately convicted of a non-strike-eligible 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 strikable offenses. The authors attribute this reduction to the greatly enhanced sentence that would have accompanied conviction on a third strikable offense.

3. *Bhati and Piquero (2008).*    This analysis is based on the arrest histories of a very large sample of individuals released from U.S. prisons in 1994. The analysis uses an "information-theoretic" hazard model that projects a post-1994 offending trajectory based on the pre-1994 trajectory. This projected trend is then compared with the post-1994 actual offending to infer the imprisonment effect. Based on this contrast, it is concluded that the experience of imprisonment is associated with a very large reduction in future offending by a combination of deterrence and incapacitation.

4. *Wimer, Sampson, and Laub (2008).*    This analysis is based on the data first assembled by Eleanor and Sheldon Glueck and subsequently updated by Robert Sampson and John Laub (Sampson and Laub 1993; Laub and Sampson 2003). The study is notable as much for the statistical methods used as for its results. It finds that periods of imprisonment are associated with subsequently higher rates of criminality as measured by arrest. However, when they apply methods recently developed by Robins and colleagues (Robins 1999; Hernán, Brumback, and Robins 2000; Robins, Hernán, and Brumback 2000) for causal inference with nonexperimental data, they find that the evidence of a seeming criminogenic effect of imprisonment is quite fragile.

5. *Manski and Nagin (1998).*    This study, based on data from the Utah juvenile justice system, examines the effect on recidivism of placement in a secure residential facility. The study is based on a methodology that does not produce a point estimate of the effect of residential placement. Instead the methodology results in a bound on the residential placement treatment effect. The argument advanced for this approach is that point estimates based on observational data require

the use of models that make strong, often unverifiable, assumptions about treatment selection and outcomes. As a result, there are often strong disagreements about modeling assumptions. The bounding approach, it is argued, is based upon models that make fewer assumptions. The models and the attendant findings thus are less subject to dispute about the validity of underlying assumptions. The cost of the bounding approach is that the analysis does not provide a specific estimate of the treatment effect. For this application, the bounds do not definitely sign the treatment effect of secure confinement as either criminogenic or preventive. The analysis does suggest, however, that the likely sign of the effect depends upon the sentencing rule that is assumed to be used by juvenile court judges. If judges sentence juveniles to the treatment mode that will most likely reduce their recidivism, the evidence points toward a criminogenic effect of secure confinement. However, if judges employ a skimming model whereby they assign the highest rate offenders to secure confinement, the results point toward a preventive effect.

6. *Berube and Green (2007) and Green and Winik (2008).*   We combine these two studies because they both use the same methodological innovation. The validity of results based on nonexperimental data depends crucially on the adequacy of controls for potentially confounding variables like prior record. These two studies attempt to circumvent this vulnerability by taking advantage of a long-standing institutional feature of the U.S. federal court system—the random assignment of cases to judges. If there are differences in harshness across judges, such variation creates a natural experiment whereby otherwise comparable cases will be sentenced differently. Both studies report evidence of such variation in judicial harshness. Econometricians refer to variables that result in random assignment of treatment, in this case judge assignment, as instruments. Neither study finds statistically significant evidence that imprisonment affects recidivism. Point estimates of the treatment effect are equally divided between positive and negative values.

7. *Analysis.*   Although the analytical approaches used across these studies are quite different, a shared characteristic of each study is that each gives close attention to constructing a counterfactual. Drago, Galbiati, and Vertova's (2009) and Helland and Tabarrok's (2007) approaches to constructing the counterfactual are the least technical and most transparent. Drago, Galbiati, and Vertova take advantage of the

fact that the Collective Clemency Bill effectively randomly assigned a residual sentence length to prisoners given early release. Thus, comparisons of the recidivism rates across individuals with different residual sentence lengths provided the basis for estimating the "dose-response" curve between recidivism and sentence length. Helland and Tabarrok (2007) used the behavior of individuals with two convictions, one strikable but the other not strikable, to create the counterfactual for individuals with convictions for two strikable offenses. The other studies used more technically elaborate approaches to create the counterfactual. Berube and Green (2007) and Green and Winik (2008) exploit the random assignment of cases in the U.S. federal court system to construct an instrumental variable that provides the statistical basis for creating the counterfactual. Bhati and Piquero (2008) attempt to extrapolate the counterfactual from the individual's offending history prior to release in 1994. The approach used in Wimer, Sampson, and Laub (2008) to create a counterfactual cannot be described in a few sentences but involves inverse probability weighting based on time-dependent propensity scores. Finally, Manski and Nagin (1998) create bounds on the counterfactual over successively stronger assumptions about the treatment selection process.

What do we make of the substantive significance of these findings? We have concerns about the validity of Bhati and Piquero's (2008) approach to constructing the counterfactual. The validity of this approach rests upon the projection of the preincarceration offending trajectory providing a valid counterfactual of the postincarceration trajectory absent the experience of imprisonment. This counterfactual would seem to be biased toward showing a preventive effect because the pre-release offending trajectory necessarily requires criminal activity—otherwise the individual would not have been incarcerated. Thus, even if imprisonment had no effect on criminal behavior, a reduction in offending relative to the prerelease trajectory would be expected based solely upon regression to the mean. Also, as Bhati and Piquero recognize, their estimate includes an incapacitation effect. Although we admire the ingenuity of the Berube and Green (2007) and Green and Winik (2008) instrument, one important design feature of both studies is troubling: each retains in the estimation sample individuals who were acquitted or whose cases were otherwise dismissed post–random assignment to the judge. Thus, these individuals also contribute to the statistical construction of the counterfactual for imprisonment. In our

judgment, these individuals should not have been included in the analysis. Further, due to the short follow-up period in the Green and Winik (2008) study, 3 years postassignment to a judge, a criminogenic effect of imprisonment may have been suppressed by an incapacitation effect. While we have no specific methodological criticism of the Manski and Nagin study (an admittedly biased assessment!), its focus on juveniles and its avowedly methodological emphasis makes its substantive findings of lesser interest for our purposes here. Likewise, we have no criticism of the methodology used in the Wimer, Sampson, and Laub study, but the fragility of findings forecloses substantive conclusions.

   This leaves us with the Drago, Galbiati, and Vertova (2009) and Helland and Tabarrok (2007) studies. These two studies, we believe, have important substantive implications because they provide a perspective on why it might be that the experience of punishment may be both criminogenic and a deterrent. As discussed in Section I, there are many mechanisms by which the experience of punishment might be criminogenic. Similarly, there are many mechanisms by which the experience may have an ameliorative effect on criminality. Among the most compelling arguments for a preventive effect is one that involves expectations about punishment for future offending—namely, that future offending will be sanctioned even more harshly than prior offending. The Drago, Galbiati, and Vertova (2009) and Helland and Tabarrok (2007) findings are supportive of this expectations-based mechanism but in a specific sort of way. Most sanctioning systems punish repeat offenders more harshly, yet when Helland and Tabarrok replicated their analysis in states without three-strike laws, they found no differences in the recidivism of offenders with one and two strikable offenses by California standards. This suggests that expectations-based deterrence among repeat offenders requires a very tangible, "in your face" disincentive to reoffending that is hard to neglect. In the case of the Drago, Galbiati, and Vertova study, the disincentive was the residual sentence to be added to the penalty for any new offense. In the case of the California three-strikes sentencing regime, it was the threat of imprisonment for life for a third strike. Other evidence on the importance of tangible incentives to affect the behavior of active offenders is from an experiment conducted by Weisburd, Einat, and Kowalski (2008) that found that the imminent threat of incarceration was an effective incentive for getting convicted offenders to make payment on delinquent fines.

The second notable finding from the Drago, Galbiati, and Vertova study was that the deterrent effect of residual sentences diminished with length of incarceration for the offense that received clemency. This finding is consistent with the finding of much of the literature that points to a criminogenic effect of the experience of imprisonment. As emphasized in Section I, criminogenic and preventive effects are not mutually exclusive; they both can be operating in unison with the net effect either in a criminogenic or preventive direction depending on the summation of the combined criminogenic and preventive influences. The Drago, Galbiati, and Vertova findings suggest that the criminogenic influences may mount with time in prison.

*E. Length of Imprisonment and Reoffending Rate*

Most studies of the effect of custodial sanctions on reoffending do not examine the relationship between length of confinement and reoffending. Understanding this relationship is vital for both public policy and science. Custodial sanctions are very expensive. For adults the average cost of imprisonment is $30,000 per person-year or more, and for juveniles it can exceed $100,000 (Nagin et al. 2006). Thus, for public policy purposes, knowledge of the effect of length of confinement on reoffending rate is of manifest importance. It is also of scientific importance for testing many of the theories of a criminogenic effect of imprisonment that predict that a longer spell of incarceration will result in a higher reoffending rate.

In the parlance of medical research, the relationship between length of confinement and reoffending rate is an example of a dose-response function. We know of only two experimental studies of this dose-response relationship. They are summarized in table 6. The study conducted by Deschenes, Turner, and Petersilia (1995) compares incarceration to intensive community supervision (ICS). Because treatment assignment occurred after the eligible convictees had been incarcerated, assignment to ICS shortened the period in prison rather than averted the experience of imprisonment. For this reason, we describe Deschenes, Turner, and Petersilia as an investigation into how incarceration time dosage affects reoffending. After 2 years, treatment effect estimates of more lengthy confinement are about equally split between criminogenic and preventive effects. However, only one of 17 estimates is statistically significant. The second study was conducted by Berecochea and Jaman (1981). In this study, the custodial term of a random

TABLE 6

Experimental Studies of Time Served

| | | Length Description | | Outcome | | |
|---|---|---|---|---|---|---|
| Authors | Population Characteristics | Longer | Shorter | Offending/Recidivism Rate | Sign of Length Effect | Statistical Significance of Effect (5%) |
| Berecochea and Jaman (1981) | All male Californian felon inmates who received a parole date between March and August 1970 set greater than 6 months from the time of the hearing | Custodial term not reduced by 6 months | Custodial term reduced by 6 months | Recidivism | 1 year follow-up: 1− 2 year follow-up: 1− | 1 year follow-up: 0 of 1− sig. 2 year follow-up: 0 of 1− sig. |
| Deschenes, Turner, and Petersilia (1995) | Offenders in Minnesota who had been recently convicted and committed to the state prison system for 27 months or less or offenders who had violated the terms of their probation and had been committed to the Commissioner of Corrections | Prison (avg. = 228 days) | Released on intensive community supervision (avg. = 124 days) | Both | 10+ 7−[a] | 1 of 10+ sig. 0 of 7− sig. |

NOTE.—Avg. = average; + = criminogenic effect; − = preventive effect; sig. = significant.
[a] Only report 24-month follow-up to ensure prison group has sufficient amount of time at risk.

sample of male felony inmates in California prisons was reduced by 6 months. Recidivism rates at 1 and 2 years were higher for these individuals compared to a control group that did not benefit from the reduction. The seeming preventive effect of experiencing a longer prison term, however, was not statistically significant.[5]

We identified 17 nonexperimental studies that report evidence on the dose-response relationship. Three were matching-based studies, and 14 were regression-based studies. The three matching-based studies are summarized in table 7. Two studies are more than 25 years old. Jaman, Dickover, and Bennett (1972) compare the recidivism of burglars who served 25 months or longer with those serving 24 months or less. This study controls for all the variables in our minimum set. By limiting the analysis to male burglars, gender and conviction offense are accounted for. By-variable matching was used to account for the remaining variables in the minimum set plus several others. Jaman, Dickover, and Bennett found that recidivism was higher for the longer-time-served group at 6, 12, and 24 months following release. However, the difference was only statistically significant at 24 months. Kraus (1981) studied juveniles. All the variables in the minimum set except possibly for race were accounted for. The dosage contrasts are summarized in table 7. For two age groups, longer confinement was associated with higher recidivism; for one age group, it was associated with lower recidivism. None of the differences were statistically significant. Loughran et al. (2008) is based on a contemporary longitudinal data set tracking serious juvenile offenders. It uses a propensity score matching–based methodology that balances on a very lengthy set of variables, including those in the minimum set. Their point estimate of the effect of custodial versus noncustodial placement is again in the criminogenic direction but falls short of significance. They find no evidence of length of confinement affecting reoffending rate either in a criminogenic or preventive direction.

Table 8 summarizes the 14 regression-based studies relating time served to reoffending. We note that the aim of most of these studies was some purpose other than studying the dose-response relationship. Time served was included only as a control variable usually among many others. The results of these studies are quite varied. In terms of

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[5] Philip Cook of Duke University reports that in his own analyses of the results he finds that the preventive effects are statistically significant for a two-tailed test at the .10 level (personal communication).

TABLE 7

Matched Studies of Time Served

| Authors | Offending/Recidivism Rate | Matched by Variable on Age, Sex, Race, Current Offense, and Prior Record (Yes/No) | Variables beyond Minimum Set | Comment on Matching | Length Description | | Sign of Length Effect | Statistical Significance of Effect (5%) |
|---|---|---|---|---|---|---|---|---|
| | | | | | Longer | Shorter | | |
| Jaman, Dickover, and Bennett (1972) | Recidivism | Yes[a] | Substance abuse, parole region of release, type of parole unit, base expectancy score (prior record, drug use, current offense, family criminality, employment, and living situation) | Found significant differences between groups on the following, which were also related to parole outcome: aliases, institutional academic rating, and home at final AA appearance | 25 or more months prior to parole release | Less than 24 months prior to parole release | At 6-month follow-up: 1+ At 12-month follow-up: 1+ At 24-month follow-up: 1+ | At 6-month follow-up: 0 of 1+ sig. At 12-month follow-up: 0 of 1+ sig. At 24-month follow-up: 1 of 1+ sig. |
| Kraus (1981) | Recidivism | All except possibly race* | None | Succeeded in matching on key variables | For ages 9–12: 10–19 months For age 13: 9–16 months For ages 14–15: 8–20 months | For ages 9–12: up to 9 months For age 13: up to 8 months For ages 14–15: up to 7 months | For ages 9–12: 2+ For age 13: 2+ For ages 14–15: 1− | For ages 9–12: 0 of 2+ sig. For age 13: 0 of 2+ sig. For ages 14–15: 0 of 1− sig. |

| Loughran et al. (2008) | Offending | No; however, matched with propensity score | Mental health, substance abuse, family, education, employment background, punishment costs, social capital, risk/need scores, involvement in the community, impulse control, gang membership, and social support | All of the key variables except current offense were included in the propensity score[b] | 6–10 months 10–13 months >13 months | 0–6 months | 1+ 2− | 0 of 1+ sig.[c] 0 of 2− sig.[c] |

NOTE.—+ = criminogenic effect; − = preventive effect; sig. = significant.
* Data are from the early 1960s, a time when Australia was almost entirely white except for Aboriginals. Not clear whether this racial minority was included in the study.
[a] Age not within 1 year and only report for burglary.
[b] All participants were serious offenders with most being guilty of felony offenses.
[c] Compared each time frame with 0–6 months.

TABLE 8

Regression Studies of Time Served

| Authors | Offending/Recidivism Rate | Controlled Directly for Age, Sex, Race, Prior Record, and Conviction Offense (Yes/No) | Other Control Variables | Length Description | | Sign of Length Effect | Statistical Significance of Effect (5%) |
| | | | | Longer | Shorter | | |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Clarke and Harrison (1992) | Recidivism | Yes | Time served | Continuous variable of time served before release | | 4+<br>1− | 4 of 4+ sig.<br>0 of 1− sig. |
| De Jong (1997) | Recidivism | Yes | Days sentenced, urine positive for certain drugs, education, employment, marital status | Continuous variable of days sentenced | | 1+<br>1− | 0 of 1+ sig.<br>1 of 1− sig. |
| Deschenes, Owen, and Crow (2006) | Both | Yes | None | Continuous variables of sentence length and time served | | 6− | 6 of 6− sig. |
| Finn and Muirhead-Steves (2002) | Recidivism | Yes | Substance abuse, EM, education, total success score (prior record, drug use, current offense, employment, education), EM × crime type interactions | Continuous variable of time served in years | | 2− | 0 of 2− sig. |
| Flowers, Carr, and Ruback (1991) | Offending | All but prior record | Place of conviction, risk score (including substance use, employment, address change, prior record, current offense), need scores (including substance use, health, mental ability, employment, education, and marital, emotional, financial, familial ability) | Incarceration with no priors 6–36 months Incarceration with priors 6–36 months | Incarceration with no priors under 6 months Incarceration with priors under 6–36 months | 1+<br>1− | 0 of 1+ sig.[a]<br>0 of 1− sig.[a] |

| | | | | | | |
|---|---|---|---|---|---|---|
| Gainey, Payne, and O'Toole (2000) | Recidivism | Yes | Days on EM, sentence length, marital status, employment, number of people in home, interaction of EM days × marital status, and interaction of jail days × priors, time at risk | Continuous variable of days in jail | 1+ 1− | 0 of 1+ sig. 0 of 1− sig. |
| Gottfredson (1998) | Offending | No | Risk measure that includes age, judge rating of arrest record, race, substance abuse, prior record, seriousness of offense, and type of offense, time served, time in community, selection for confinement (age, type of offense, seriousness, mitigating or aggravating factors, counsel/probation recommendation, judge's importance of rehabilitation or retribution)[b] | Continuous variable of months served in custodial sanction (youth facility, jail, or prison) | 1+ 2− | 0 of 1+ sig. 0 of 2− sig. |
| Harer (1994) | Both | All but current offense | Salient Factor Score, substance abuse, CJS supervision, prison misconduct, education, employment, type of release, living situation, ZIP population, poverty, unemployed | Continuous variable of prison term served in months | 1+ 1− | 0 of 1+ sig. 0 of 1− sig. |
| Holland, Pointon, and Ross (2007) | Recidivism | Yes | None | Continuous variable of time served in prison | 0; the exponent was 0.00 with Exp(B) = 1.00 | Not significant, no effect |
| Kim et al. (1993) | Recidivism | Yes | Employment, income, marital status, supervised release, number of sworn officers/1,000 population, probability of conviction for arrest for drugs, proportion of drug arrests to total arrests, population, average earnings | Continuous variable for number of days served in prior incarceration | 1− | 0 of 1− sig. |

TABLE 8 (*Continued*)

| Authors | Offending/Re-cidivism Rate | Controlled Directly for Age, Sex, Race, Prior Record, and Conviction Offense (Yes/No) | Other Control Variables | Length Description | | Sign of Length Effect | Statistical Signifi-cance of Effect (5%) |
| | | | | Longer | Shorter | | |
|---|---|---|---|---|---|---|---|
| Maguire, Flana-gan, and Thornberry (1988) | Recidivism | Yes | Education, marital status, military ser-vice, employment, substance use, prison rule infraction rate | Continuous variable of number of years incarcerated | | 1− | 1 of 1− sig. |
| Nunes et al. (2007) | Recidivism | All but race, prior rec-ord, and age | RRASOR score (age at release, prior sex offenses, victim gender, and victim-offender relationship) | Continuous variable of number of months incarcerated | | 2 instances of a 1.00 odds ratio | Not significant, no effect |
| Orsagh and Chen (1988) | Recidivism | Yes | Marital status, involved in prerelease rehabilitation program, unemployment rate in county of release, involved in work release program | Continuous variable of length of continuous incarceration up to the release date in natural logs | | 1+<br>1− | 1 of 1+ sig.[c]<br>0 of 1− sig.[c] |
| Petersilia, Turner, with Peterson (1986)[d] | Recidivism | All but current offense | Substance abuse, weapon use, injury to victim, relationship to victim, marital status education, living situation, county of conviction | Continuous variable of time served in months | | 12− | 5 of 12− sig. |
| Wooldredge (1988) | Recidivism | Yes | GPA, county, parental marital status, learning disabilities, characterized as behavioral problems | More than 1 month detention | 1 month detention | 3+ | 2 of 3+ sig. |

NOTE.—+ = criminogenic effect; − = preventive effect; sig. = significant. EM = electronic monitoring; DV = dependent variable.
[a] Only report 36-month follow-up.
[b] Above variables were most related to recidivism and included in the risk and selection score; however, there were other variables such as race and sex that were not included in the risk score and selection score because they did not significantly predict recidivism.
[c] Only reported logit analysis since DV is dichotomous.
[d] Unlike earlier in their study, they did not use matching when examining the impact of sentence length.
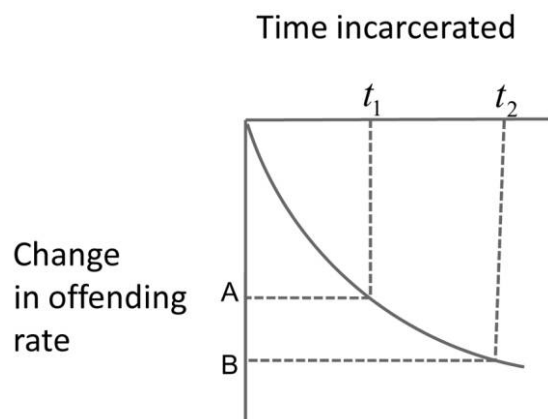
## Time incarcerated



FIG. 4.—Dose-response relationship between time incarcerated and change in offending rate.

counts of point estimates, seven report equal numbers of positive and negative point estimates, five report a majority of negative estimates, and two report a majority of positive estimates. In half of the studies, none of the point estimates, whether positive or negative, are significant, four studies report at least one significant negative estimate, and three studies report at least one significant positive estimate.

What conclusions do we draw from these studies of the dose-response relationship between time served and reoffending? The one experimental study is suggestive of a preventive effect, but that effect may be attributable to incapacitation. Two of the matching studies point weakly to a criminogenic type dose-response relationship, but both are extremely dated. The Loughran et al. (2008) study suggests a possible criminogenic effect of placement but finds no linkage between time served and reoffending. We draw no conclusions from the results of the regression studies. Not only are results extremely varied, but more importantly all of the studies suffer from a fundamental analytical flaw. This flaw relates to the potential sensitivity of regression-based studies to specification errors in the model of the relationship of age and offending rate.

Figure 4 depicts a hypothetical dose-response relationship. It assumes a preventive effect, but the points we wish to make apply equally to a criminogenic dose-response relationship or to a curve that has
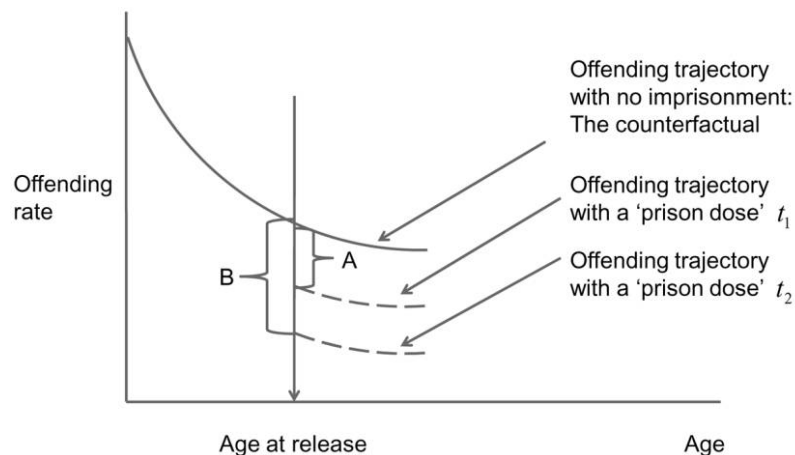
Offending trajectory
with no imprisonment:
The counterfactual

Offending trajectory
with a 'prison dose'  $t_1$

Offending trajectory
with a 'prison dose'  $t_2$

Offending
rate

B

A

Age at release                                    Age

FIG. 5.—Rate of offending for different doses of "prison"

both criminogenic and preventive regions. The horizontal axis measures the dose—namely, time served in prison. The vertical axis measures the reduction in the offending rate. The curve measures the size of the reduction corresponding to each prison dosage. For a time served of $t_1$, the predicted reduction is $A$. For a longer time served $t_2$, the reduction $B$ is even larger. These reductions are measured relative to an implicit counterfactual sentence of zero time served.

The change in offending rate is not observed or observable. It must be inferred from the actual offending rates of individuals who spent varying amounts of time in prison. Figure 5 depicts the inference problem. The vertical axis measures the offending rate, and the horizontal axis measures age. The vertical arrow in the graph denotes the age of release from prison. The curve to the right of the release age denotes the pretreatment age–offending rate relationship. It assumes a declining relationship, but a rising relationship could just as well have been assumed for the points we wish to make. To the right of the release arrow are the offending trajectories associated with prison dosages $t_1$ and $t_2$. Also, to the right of the release arrow is a solid trajectory denoting the counterfactual offending trajectory for a prison dosage of no time served. The $t_1$ dosage trajectory is $A$ offending rate units below this counterfactual, and the $t_2$ dosage trajectory is $B$ units below the counterfactual.

The objective of a statistical analysis is to infer these differences in offending rate from data on offending rates of individuals who spent varying amounts of time in prison. If the statistical analysis was conducted based on data from an experiment in which sentences of 0 and of $t_1$ and $t_2$ were randomized across subjects, then estimates of $A$ and $B$ could be obtained directly from the difference in postrelease offending between those receiving no prison sentence and those receiving sentences of $t_1$ and $t_2$, respectively. This straightforward approach works because, due to randomization, all potential confounders are balanced across treatment conditions. Most critical among these potential confounders is age. Because offending rate is changing with age, imbalances in age across dosage levels could seriously bias results. For example, if the average age of individuals receiving dosage $t_1$ was less than the average for the zero-dose individuals, the preventive effect, $A$, would be understated. Indeed, if the age imbalance were large enough, the effect estimate could be in the criminogenic direction.

Now consider the problem of using regression to uncover the dose-response relationship with nonexperimental data. In these data, individuals receiving prison sentences will have been released from prison at varying ages. Thus, the pre- and posttreatment offending histories of individuals will vary in length and by age. The pretreatment age-offending rate trajectory must somehow be statistically reconstructed from data in which there is no set pretreatment age. Similarly, the postrelease offending trajectories must be reconstructed for varying amounts of time served. This challenge has been the subject of much serious statistical work (see Imbens 2000; Lu et al. 2001; Zanutto, Lu, and Hornik 2005). Suffice it to say that solutions require far more than simply adding time and age to the list of regressors in a regression model.

### IV. Implications for Policy and Future Research

A remarkable fact is that despite the widespread use of imprisonment across democratic nations and the enormous expansion of the prison system in the United States, rigorous investigations of the effect of incarceration on reoffending are in short supply. Among criminologists, there is no lack of commentary about prisons, with most scholars decrying their overuse generally and their disproportionate use with minority groups. But critical commentary is not equivalent to demar-

cating scientifically the comparative impact on future criminal involvement of custodial as opposed to noncustodial sanctions. A new generation of investigations is needed.

This is not to say that existing research does not allow for a provisional conclusion about the likely impact of incarceration. Thus, a key finding of our review is that the great majority of studies point to a null or criminogenic effect of the prison experience on subsequent offending. This reading of the evidence should, at least, caution against wild claims—at times found in "get tough" rhetoric voiced in recent decades—that prisons have special powers to scare offenders straight.

Still, it is equally important to note that several important exceptions to this overall pattern of results were found and are discussed below. Although in most studies the majority of point estimates imply criminogenic effects, estimated effects are not always statistically significant by conventional standards. More importantly, in many studies important variables like prior record or age are not adequately accounted for. Moreover, studies are often dated or involve juvenile populations. Thus, as imprisonment is used in contemporary democratic societies, the scientific jury is still out on its effect on reoffending. Even so, serious effort should be committed to studying the implications for public policy and social science of a possible criminogenic effect of imprisonment. In this section, we outline a conceptual framework and research agenda designed to explore these implications.

## A. Imprisonment and Crime Rates

At the outset of this essay, a distinction was drawn between general and specific deterrence. We now reiterate that distinction with the observation that there is no inherent contradiction between the conclusions that the threat of punishment has a substantial deterrent effect in the population writ large, often called general deterrence, and the conclusion that the experience of punishment increases criminality. We do not assert that either of these conclusions is known with certainty, but we do assert that there is sufficient evidence for both and that they should be taken seriously. Accordingly, we need a theory and modeling structure that accounts for these contending effects of sanctions on crime rate.

Figure 6 is a schematic of such a modeling structure. We do not formalize the relationships depicted in the figure.[6] That must be left

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[6] We thank Jonathan Caulkins for suggesting the basic form of this schematic.
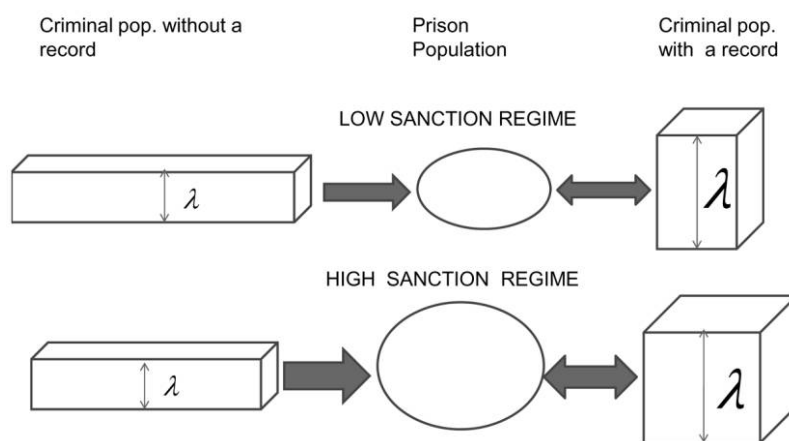
FIG. 6.—Crime and low versus high sanction regimes

for future research. Rather, the purpose of the figure is to provide a conceptual structure for commenting on the types of useful questions for policy and science that need to be addressed. The figure delineates three populations: the population of offenders without records of imprisonment, the population of offenders with imprisonment records, and the prison population. It compares the sizes and flow rates among these populations under regimes of low and high prison sanction severity. The prison population is designated by an ellipse, and its size is represented by the surface area of the ellipse. Offending populations are represented as three-dimensional rectangles. Their size is represented by the two-dimensional surface area of the rectangle.

Under a regime of high sanction severity, we anticipate that due to general deterrence the size of the offending population without criminal records will be smaller than that under a regime of low sanction severity. We also anticipate that the high sanction severity regime will result in a larger prison population that, in turn, results in a larger population of offenders with a criminal record.[7] The arrows connecting

[7] If the general deterrent effect is strong enough, a regime of high severity could actually reduce prison population. However, even the maximum elasticities of crime rates with respect to the imprisonment rate reported in Donohue (2007) are unlikely to be sufficient to result in falling prison populations. The model also anticipates that prisoners will eventually be released, which is the case for the vast majority of the prison population.

the populations represent flows between the populations. The arrow connecting the population of offenders without record to the prison population is one-headed to the right because exit from prison to the population without a record is impossible by definition. The arrow connecting the prison population to the population of criminals with a record is two-headed because individuals with a record can return to prison. The larger arrow sizes under the high severity regime reflect our expectation that flows out of the criminal population without a record into the prison population and eventually into the criminal populations with records will be higher than in the low severity regime.

The schematic depicts the population of offenders with and without records in three dimensions. The third dimension reflects their rate of offending, λ. Based on the findings of our review, the rate of offending for the population with a record is predicted to be higher than that of the offending population without a record.

Under which sentencing regime would crime rates be higher? This question has no definite answer. Compared to the high severity regime, the low severity regime would have more crime from the population of offenders without records due to the relatively larger size of this population. It would also have less crime averted by incapacitation because prison populations are smaller in the low severity regime. On the other hand, under the low severity regime, there would be comparatively less crime from the criminal population with records due to its smaller size. Reduction in the size of the population with records results in a proportionately larger number of crimes averted than reductions in the population without records because the former population offends at a higher rate than the latter population. Thus, the net difference in crime between the low and high regimes depends upon the size of the crime reduction from deterrence and incapacitation in the high versus low regime compared to the increase in crime from the criminogenic effect of prison in the high versus low regime.

### B. A Research Program on the Effects of Imprisonment on Reoffending

Developing the empirical and analytic capacity to do this accounting of competing effects will require more research on general deterrence, incapacitation, and the effect of imprisonment on reoffending. Because general deterrence and incapacitation are not the focus of this essay, we refer readers to Nagin (1998), Doob and Webster (2003), and Donohue (2007) for extended discussions of important gaps in knowledge

about these topics. Gaining a better fix on the effect of imprisonment on reoffending will require a research program that uses more policy-relevant data to ask more policy-relevant questions, measures the dose-response relationship between length of imprisonment and reoffending rate and between number of times imprisoned and reoffending rate, examines how the experience of punishment affects perceptions of sanction risk, tests the mechanisms by which the experience of imprisonment may be criminogenic, and unpacks the effect of different noncustodial sanctions. These components are discussed in turn.

1. *Using More Policy-Relevant Data to Ask More Policy-Relevant Questions.*    If the experience of imprisonment is criminogenic at least for some sizable segment of those imprisoned, the natural question from a public policy perspective is: what would be the impact on crime rates of incrementally cutting back on the use of the prison sanction? Part of the reason existing research falls well short of being useful for answering a question such as this is that most research is not relevant to predicting effects in the context of contemporary sanction policy. No matter how well done, studies of custodial sanctions on Australian youth based on data from the early 1960s or of the effect of a royal pardon for sentences of 2 weeks or less tell us little about the impact of consequential changes in contemporary sanctioning policy.

The challenge of laying out a policy-relevant research agenda is further complicated by large differences in the sanctioning policies and prison conditions across countries. We thus outline our recommendations for a workable, policy-relevant research program for one country with the hope that it will serve as a blueprint for other countries. We focus in particular on the United States not only because it has the highest imprisonment rate of all North American and Western European countries but also because the sources of the increase have been best documented.

Let us return to the question of how the crime rate would be affected by incrementally cutting back on the use of the prison sanction. The answer to this question requires a specification of the way in which the use of prison sanctions would be curtailed. We use the term "marginal" prisoners to label individuals whose terms of imprisonment would be affected by a specified change in sanction policy on the "margin."

How, then, might the marginal prisoner be conceived of in the United States? There is no easy answer to this question. One conception that has desirable social welfare virtues is to define the margin as

those prisoners whose reoffending is most exacerbated by the experience of imprisonment (Manski and Nagin 1998). From a practical point of view this conception has two serious drawbacks. First, it begs the question because it provides no guidance on what type of individual that might be. Only the research itself can answer the question. Second, it is a conception whose answer might mightily offend the principles of equity and justice. Those most adversely affected might include individuals who have committed the most serious crimes or who have come from more privileged backgrounds. Sentencing such individuals to a noncustodial sanction or to shorter prison terms would be ethically offensive to many. Another vantage point that might provide a useful perspective for defining the marginal prisoner is recent history. In the United States, combined federal and state imprisonment rates have been rising more or less steadily for nearly 4 decades from a rate of about 100 per 100,000 population in the early 1970s to about 500 per 100,000 population at the present time. A reversal of selected policies that contributed to this massive increase is still another approach to defining the margin. This is the approach that we take here.

There are two basic mechanisms for reducing reliance on sanctions involving incarceration—one affects the extensive margin of use of imprisonment by reducing the fraction of the convicted who are incarcerated. The other affects the intensive margin by reducing the sentence length of those who are incarcerated. Still another relevant margin, at least in the American context, involves parole violators. In the United States, changes in the policies regarding conditions for parole revocation could also have a material impact on prison populations (Blumstein and Beck 2005).

Blumstein and Beck (1999) and Raphael and Stoll (2007) have closely scrutinized the primary sources of the increases in imprisonment since the 1980s. Since the mid-1980s, there has been a dramatic increase in the imprisonment of drug offenders whether measured on a per capita basis or as a percentage of the prison population. Escalation of the imprisonment of drug offenders has been particularly marked in the federal prison system; more than half of the federal prisoners were committed for drug offenses. There has been much criticism of the sanctioning policies that are the source of the dramatic increase in the incarceration of drug offenders. The criticisms focus on their disproportionate impacts on African Americans (Tonry 1995; Human Rights Watch 2008) and their ineffectiveness in curtailing illicit drug use (Ry-

dell, Caulkins, and Everingham 1996; Caulkins et al. 1999). Thus, we recommend that priority attention be given to analyzing the effect of imprisonment on reoffending among drug offenders.

Blumstein and Beck (1999) also decompose the source of the increase in the imprisonment rate between the extensive margin, the probability of commitment given arrest, and the intensive margin, time served. The relative contribution of these two components varies by crime type. Aside from drugs, the only nonviolent crime that they examine is burglary. According to Blumstein and Beck, between 1980 and 1996 per capita rates of incarceration for burglary nearly doubled. The greatest part of that increase was attributable to increased time served. We thus recommend that priority attention be given to examining the relationship between time served and reoffending rate, for burglary in particular, because it is not a violent crime, and also other crimes even if they involve some element of violence. This brings us to the second element of the proposed research agenda.

2. *Measuring the Dose-Response Relationship between Length of Imprisonment and Reoffending Rate and between Number of Times Imprisoned and Reoffending Rate.*   As discussed in Section III, there is little convincing evidence on the dose-response relationship between time spent in confinement and reoffending rate. Because imprisonment is so costly, $30,000 per person-year or more, knowledge of this relationship has important implications not only for public safety but also for state, local, and federal budgets. Another dimension of the dose-response function is the relationship between offending rate and the number of prior spells of incarceration. Many of the theories of how the prison experience might affect reoffending, particularly those involving stigma, suggest that effects will decline with number of spells, yet to our knowledge no research has been conducted on this dimension of the dose-response function.

The primary statistical hurdle to estimating both of these dimensions of the dose-response relationship is that neither the length of incarceration nor the number of spells of incarceration is randomly assigned. Short of a randomized experiment, which is not likely feasible for the sorts of marginal populations that we recommend be studied, the analysis will have to be based on nonexperimental, observational data. What characteristics should these data have, and what methods should be used to analyze them to circumvent inference problems?

Concerning the characteristics of the data, we return to Monahan's

(2008) categories of variables in modern actuarial-based risk assessment instruments: what the person "is" as measured by age, sex, race, and personality; what the person "has" as measured by major mental, personality, or substance use disorders; what the person "has done" as measured by prior crime and violence; and what has been "done to" the person as measured by pathological family environment and victimization. It is important that the data have as many measurements on each of these dimensions as is practically possible. More complete measurements will set the stage for a more convincing set of statistical adjustments to account for the nonrandom assignment of sentences. In this regard, it is particularly important to have extensive measurements of what the individual has "done" as measured by prior record and characteristics of the conviction offense. Because these factors represent the primary determinants of sentence type and length, it is vital that they be measured on as many dimensions as possible. Also, for analyses of drug offenders, data on what the individual "has" as measured by the severity of their substance abuse are very important.

What methods should be used to analyze these data? Because it is unlikely that randomized experiments can be conducted in this setting, researchers should be on the lookout for natural experiments such as the random assignment of judges in the U.S. federal courts that Berube and Green (2007) and Green and Winik (2008) exploit or the variations in time served created by the Collective Clemency Act that are exploited by Drago, Galbiati, and Vertova (2009). Determinate sentencing grids, for example, may provide a quasi-experiment for constructing the dose-response relationship between sentence length and reoffending rate. The reoffending rates of individuals with sentencing scores that place them just above and below breakpoints on the sentencing grid could be compared. Finally, it is imperative that close attention be given to balancing on age and more generally to the statistical construction of the counterfactual. We recommend the use of methods such as those used in Loughran et al. (2008).

3. *Understanding How the Experience of Punishment Affects Perceptions of Sanction Risk.*    Balanced against the many studies finding evidence of a criminogenic effect of imprisonment are two important studies, Drago, Galbiati, and Vertova (2009) and Helland and Tabarrok (2007), that find convincing evidence of a deterrent effect. These studies are distinctive not only because of their findings but also because of their framing of the research question. In all other studies, the research

question is framed in terms of the effect of custodial versus noncustodial sanctions on reoffending. By contrast, the research question in Drago, Galbiati, and Vertova and in Helland and Tabarrok is framed in classic deterrence terms: does the threat of more punishment deter more crime?

Helland and Tabarrok examine whether the draconian consequences of a third strike in California deter reoffending, and Drago, Galbiati, and Vertova examine whether the threat of having to serve out the residual sentence on the clemency offense deters future offending. In both studies, the authors attempt to hold prior prison experience constant. In the Helland and Tabarrok study, the third-strike deterrent effect is estimated by comparing the behavior of individuals with two convictions, one strikable but the other not strikable, with individuals that have convictions for two strikable offenses. Thus, their analysis strategy holds constant the number of convictions and presumably also spells of imprisonment. By design, therefore, the analysis strategy provides no information on the effect of the prison experience itself on reoffending. By contrast, the Drago, Galbiati, and Vertova study is informative on whether the length of incarceration for the clemency offense affects recidivism. They find an interaction of the deterrent effect of the residual sentence and time served for the clemency offense: the longer the time served, the less deterrent effect. This finding aligns with our conclusion that the empirical evidence points to an overall criminogenic effect.

These studies were discussed under the heading "understanding how the experience of punishment affects perceptions of sanction risk" because, if indeed their results are a reflection of deterrence, prior contacts with the criminal justice system must have affected perceptions of future punishment. Nagin (1998, p. 1) commented that "knowledge about the relationship between sanction risk perceptions and policy is virtually nonexistent; such knowledge would be invaluable in designing effective crime-deterrent policies." He went on to recommend that research on the link between sanction policy and perceptions be a priority research topic. A decade later we re-endorse this recommendation.

The Drago, Galbiati, and Vertova and Helland and Tabarrok studies suggest that there is a link between policy and perception, at least in some circumstances. Both studies, however, focus on circumstances in which the threat of heightened punishment was likely and unusually

tangible to a population whose prior offending had already demonstrated their lack of responsiveness to sanction threats. In the Helland and Tabarrok study, that tangibility was probably the draconian consequences of the third strike. Helland and Tabarrok concluded that the crime-reduction benefits of this draconian threat likely fell far short of the cost of a lifetime prison term. By contrast, Drago, Galbiati, and Vertova's study examines a policy that would be much more likely to pass a cost-benefit test. The saliency likely stemmed from the offender's certain knowledge of the residual sentence premium for future offending.

These are, of course, merely speculations for why the policy-to-perceptions link was closed in the Drago, Galbiati, and Vertova and Helland and Tabarrok studies. What is required is a systematic research program into questions previously elaborated upon in Nagin (1998). How do would-be offenders combine prior experience with the criminal justice system and new information on penalties? How long does it typically take for persons to become aware of new sanctioning regimes? How do they become aware of changes in penalties, and what information sources do they use in updating their impressions? How do novices form impressions of sanction risks? These questions speak to the broader issue of whether sanction risk impressions are easily manipulable.

4. *Analyzing the Mechanisms by Which the Experience of Imprisonment May Be Criminogenic.* Relatively few studies seek to peer into the "black box" of imprisonment to understand why this experience might increase crime. One critical line of inquiry is to assess whether incarceration affects a person's perceptions of the costs of crime. Other potential mechanisms have been proposed. These include social learning, identity transformation, attenuated social bonds, and strains induced by being in prison and adapting to community reentry. This research enterprise also should consult the scholarship on the predictors of recidivism (e.g., Gendreau, Little, and Goggin 1996) to see if these are exacerbated by imprisonment. Finally, scholars may wish to pay particular attention to "dynamic" as opposed to "static" risk factors (Andrews and Bonta 2006). Dynamic risk factors are sources of reoffending that are mutable (e.g., antisocial attitudes). Static risk factors are "in the past" and thus cannot be altered (e.g., criminal history). Dynamic risk factors are salient because they can be targeted for change by interventions. Accordingly, they may be of particular rele-

vance in policy discussions on how best to minimize the criminogenic effect of imprisonment.

5. *Unpacking the Effect of Different Noncustodial Sanctions.*    The effect of custodial sanctions on reoffending depends not only on the length and conditions of the custodial sanction but also on the type, term, and conditions of the noncustodial sanction that is the alternative. The variety of noncustodial sanctions available to policy makers is at least as varied as for custodial sanctions. Noncustodial sanctions include fines, community service, mandatory treatment, and probation of varying intensity. As demonstrated by the Deschenes, Turner, and Petersilia (1995) experiment in which some individuals randomized into intensive supervision probation opted instead to serve their prison term, a noncustodial sanction may be deemed more onerous than a custodial sanction (see also Wood and May 2003). Further, a noncustodial sanction can include the threat of incarceration for nonconformance with the conditions of the sentence. Thus, there is no inherent contradiction in the conclusion that a given custodial sanction is criminogenic compared with one type of noncustodial sanction (say a specific type of intensive supervision probation) but has a preventative effect compared to another type of noncustodial sanction (say a specified fine amount).

Accordingly, we offer two related recommendations. First, analyses of nonexperimental data should pay closer attention to specifying the type and conditions of the noncustodial sanction that is being compared to a custodial sanction. Second, data should be assembled that will allow the comparison of custodial sanctions to more specific types of noncustodial sanctions.

We close with the observation we made at the outset: it is remarkable that so many democratic societies, most especially the United States, incarcerate so many people without good estimates of the effects of this very expensive sanction on macro-level and individual-level crime rates. It is our hope that this essay in some small way will help to correct this regrettable state of affairs.

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