


## The Growth of Incarceration in the United States: Exploring Causes and Consequences

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## Appendix C

### Incarceration in the United States: A Research Agenda

**T**his report on the causes and consequences of high rates of incarceration identifies a variety of areas in which research is notably missing or critically inconclusive. The report outlines a research agenda in three parts—(1) on the experience of being incarcerated and its effects, (2) on alternative sentence policies, and (3) on the impact of incarceration on communities. This appendix elaborates on this research agenda, describing several of the key research priorities in greater detail.

Much of the research on the consequences of incarceration is directed at what statisticians call the identification of causal effects—isolating independent changes in incarceration and studying how outcomes vary as a result. Although researchers have focused on the challenge of identification, our review suggests that many of the main research priorities for the field are of a more fundamental kind, of data and measurement and of conceptualization.

We describe four main research priorities that follow from our detailed review of the causes and consequences of incarceration: (1) research on the effects of incarceration should specify more precisely the treatment whose effect is being estimated; (2) research should examine the heterogeneity of incarceration effects across individuals and operating at different levels; (3) causal questions are only a subset of significant research questions, and researchers should also study the whole structure of conditions of disadvantage that are correlated with incarceration; and (4) significant data limitations currently restrict the full development of the research program on incarceration, and new data are needed on conditions of confinement,

on sentencing, and on the course people follow as they move through the criminal justice system.

### SPECIFYING THE TREATMENT

A number of chapters of this report note in different ways that research on “the effects of incarceration” is sometimes poorly specified from policy and scientific perspectives. Research on causal effects typically contrasts a group receiving a “treatment” with a comparison or “control” group. Average differences for some outcome of interest between the treatment and control groups provide an estimate of the causal effect of the treatment, provided the two groups are identical in expectation in all relevant respects except for their assignment to the treatment or control group (Morgan and Winship, 2007, review methods and concepts). Scientific debates often surround whether treatment and control groups are identical in all relevant respects and what those respects might be.

In research on the effects of incarceration, the content of the treatment (“incarceration”) and the content of the control (“not incarceration”) often are not precisely specified and may not be informative for policy (Nagin et al., 2009). The effects we associate with incarceration and review in this report—from specific deterrence, through behavioral reform, to diminished job skills and psychological trauma—are linked not only to the deprivation of liberty but also to the conditions of penal confinement. The effects of incarceration may vary greatly depending on conditions of overcrowding, the quality of health and treatment services, order and safety in specific facilities, exposure to administrative segregation, and so on. This report, particularly in Chapter 6, shows that the conditions of penal confinement vary enormously across jurisdictions, as well as across facilities within jurisdictions. Still, there is very little research in any domain—on recidivism, health, employment, families—that measures the actual content of the “treatment” of incarceration and then relates it to the large number of post-incarceration outcomes of scientific and policy interest.

Just as the conditions of confinement typically remain unspecified, the conditions faced by the comparison group remain undescribed in most research. Comparison groups may, for example, serve less time, be under community supervision, participate in diversion, or just be free and unsupervised in the community. Each of these control conditions implies a different kind of contrast with the treatment condition of incarceration.

In studying the effects of incarceration, future research should attempt to specify the treatment and control conditions more systematically. Researchers could usefully study the effects of specific conditions of incarceration in contrast to some specific community alternative. The contrast in outcomes from conditions of confinement and a clearly defined community

alternative would serve as a test for theories describing how incarceration influences later outcomes. A contrast between well-defined conditions of confinement and a well-defined community alternative also would have the advantage of enabling clear interpretation of causal estimates. Such estimates can provide precise guidance to policy makers.

If incarcerated study subjects are drawn from a range of facilities, then the research design should ideally describe the variety of institutional conditions from which the treatment group is drawn. If study subjects are drawn from a variety of security levels, for example, the treatment could be interpreted as an average over those conditions. The same point applies to the control conditions. The design should ideally specify the range of conditions obtaining for the control group. Often in program evaluation, control group subjects may seek programs or treatment outside the experiment. This kind of extra-experimental program participation should be reflected in the interpretation of treatment effects. Well-designed evaluations will assign subjects to alternative, lower-dose program conditions. In these cases, the contrast that defines the treatment effect is clearly specified.

For example, the evaluation of the Center for Employment Opportunity's (CEO) transitional jobs programs for New York parolees assigned control group subjects to a jobs resource room. In the control condition, parolees searched online jobs databases with program staff, whereas members of the treatment group worked in subsidized employment (Redcross et al., 2009). In the CEO evaluation, control group subjects were clearly occupied in alternative program activities, although even in this case the control group may have been contaminated by extracurricular program participation outside of the experiment. If participation in these extra-experimental programs has effects, this will alter the baseline against which the tested program is assessed. Improvements in baseline measurement of treatments and controls may be achieved with surveys of all experimental subjects before assignment to program conditions (see, e.g., Brock et al., 1997).

Research on deterrence and incapacitation also indicates the importance of specifying the treatment precisely. The strongest evidence on the effects of incarceration on crime concern the small deterrent effect of long sentences. As indicated in Chapter 5, research on the "total effect" of incarceration on crime addresses a question that is poorly specified from a scientific and policy viewpoint. Changes in the incarceration rate can be obtained with a wide variety of policies affecting, say, the initial decision to incarcerate, the length of sentences, and parole release. Most important, different people are being incarcerated under each policy alternative. Thus each policy change, even if yielding an identical change in the rate of incarceration, has different implications for deterrence and incapacitation. Moreover, policy makers do not control the incarceration rate itself but instead control policies defining the use of incarceration in contrast to

noncustodial alternatives. Future research should thus study the effects of specific sentencing policies. Policies defining sentence length and the certainty of incarceration given an arrest emerged as strong research priorities in our review of the work on deterrence and incapacitation.

Analysis of a habitual offender enhancement in the Netherlands by Vollaard (2012) offers a good example of analysis of a specific policy measure, with a clearly defined comparison group. The habitual offender enhancement was introduced at different times in different localities, and offending rates were compared before and after its adoption. Three things stand out in the Vollaard study: (1) the analysis estimates the effect of a discrete change in sentencing policy; (2) the analysis examines how the policy effect evolves over time (it declines); and (3) the analysis offers a detailed account of the policy's implementation, describing how the courts applied it and the kinds of offenses being prosecuted (mostly those committed by indigent heroin addicts). By focusing on a specific policy intervention and describing its implementation, Vollaard (2012) offers a sharp definition of the treatment effect as it actually came to operate at the research site.

### THE HETEROGENEITY OF INCARCERATION EFFECTS

The “effect of incarceration” is a coarsely defined quantity because the conditions of incarceration vary greatly, and the policies that yield marginal increments in incarceration also show significant variation. Our review of the research indicated further that evidence for the effects of incarceration may be weak because the effects are likely to vary significantly across individuals, social contexts, and units of analysis.

Studying the heterogeneity of the effects of incarceration may be one way of addressing the disparity in results reported in the research literature and the larger debate over incarceration's positive and negative effects. Incarceration may successfully deter crime, but perhaps only in a subset of the population. Similarly, incarceration may diminish human capital and reduce employment opportunities, but only for certain kinds of people.

Evidence for the heterogeneity of incarceration effects is abundant. For example, experimental audit studies of employers suggest that the stigma of a criminal record is greater for an African American job seeker with a criminal record than for a white job seeker with a criminal record (Pager, 2007; Pager and Quillian, 2005). Another line of research suggests that high-status respondents are more likely than low-status respondents to be deterred by public punishments (Nagin, 1998, 2013). In both cases, the effects of criminal stigma vary in different ways across population. In ethnographic research, Edin and colleagues (2006) find that incarceration is generally destabilizing for poor families with children, although for some, incarceration of a violent spouse can help restore order to the household.

In some cases, the authors find, incarceration offers a period of reflection in which people can decide to desist permanently from crime.

In these examples, the effect of incarceration appears to vary with different individual-level characteristics and dispositions. Other research suggests that incarceration may also vary across social contexts. For example, the negative effects of incarceration on child well-being may depend on whether fathers are resident in the household. Where incarceration is associated with dissolution of the household, its negative economic effects for children are greater (Geller et al., 2012). In general, the negative effects of incarceration may be greater where those going to prison are embedded in the prosocial roles of worker and resident father.

These examples do not suggest any systematic account of the heterogeneity of the effects of incarceration, but they do suggest that incarceration may vary greatly in its effects. Currently, there is little understanding of whether this variation is systematic, perhaps unfolding in similar ways in different domains.

In contrast with the usual ideas about the heterogeneity of causal effects, incarceration may have different effects for different social units. The effects of incarceration on individuals, for example, may be quite different from the effects on families or neighborhoods. The idea that incarceration has aggregate-level effects, beyond the individuals incarcerated, turns on external effects whereby those who have not been incarcerated are somehow affected by the incarceration of others. It is easy to think about these external effects in the case of families. The incarceration of a husband for domestic violence may make a family safer through the husband's incapacitation. At the community level, Clear (2007) argues that the population turnover associated with incarceration may be criminogenic for the wider neighborhood, as the informal social ties that would otherwise sustain public safety are undermined. In labor markets or marriage markets with high incarceration rates, employers and prospective spouses may assume that potential employees or spouses have previously been incarcerated even when they have not, with the effect of reducing overall rates of employment or marriage. As discussed in Chapter 10, compelling empirical tests are difficult to design, but these kinds of equilibrium effects are seldom studied and go beyond a simple summation of the individual-level effects of incarceration.

Some outcomes gain their social significance through their prevalence in a group or community. For example, the legitimacy of criminal justice authorities and the level of public health are viewed as aggregate-level phenomena because they describe environments or social contexts that may themselves have individual-level effects.

In the context of a steep socioeconomic disparity in incarceration, the effects of high rates of incarceration on institutional legitimacy appear to

be a strong research priority. The legitimacy of criminal justice authority is not just the sum of individual beliefs about prison and police. Institutional legitimacy suggests a set of beliefs and values that are shared within a community. The causal force of legitimacy depends on the prevalence of shared beliefs about the propriety of criminal justice authorities. To the extent that community residents feel that criminal justice authorities are legitimate, they may feel more compelled to comply with directions, actively assist in investigations, and desist from crime. Legitimacy produces these behavioral responses not only because of individual beliefs but also because individuals act out social expectations accompanying the role of being a community member. Although there is a large literature on criminal justice legitimacy, particularly police legitimacy, the level of community-wide support for criminal justice authorities often is inferred from individual-level opinions and attitudes (Unnever, 2013). In a context of high incarceration rates that are spatially concentrated, the effects of incarceration on institutional legitimacy may be more complex than the simple summation of individual beliefs.

Like legitimacy, public health has collective significance, providing a social context for individual effects. Given clear evidence of the high rates of infectious disease in the incarcerated population and the individual health effects of incarceration, understanding the effects of incarceration on public health in the aggregate is a key research priority. An example of this type of research is provided by Johnson and Raphael (2009), who examine the impact of incarceration on racial disparities in AIDS infection. Their analysis predicts the age- and race-specific rate of AIDS in each state as a function of the incarceration rate. The key conceptual contribution of this research design involves predicting the prevalence of AIDS among women from the incarceration rate of men. The authors find that nearly all the black-white disparity in AIDS among women is related to the racial disparity in incarceration among men. Although only a first contribution to a larger research program on the public health effects of high rates of incarceration, this analysis underlines the importance of studying aggregate-level effects whereby those beyond the penal system are nevertheless affected by it.

## BEYOND CAUSALITY

Much of the discussion of future priorities for research on high rates of incarceration has focused on questions about causal effects. Research showing the close correlation among incarceration, crime, race, poverty, addiction, mental illness, family instability, neighborhood poverty, and residential segregation is noted throughout this report. The correlation of incarceration with an array of other measures of social and economic

marginality has been observed at the individual level, across families, at the level of neighborhoods and states, and over time.

These correlations are so dense, with all factors apparently being endogenous, that it is difficult or impossible to draw causal linkages among them. What may be more significant here is simply the fact of high intercorrelation. The various correlates of disadvantage cluster in a complex or syndrome that should be studied in its own right. The research priority may shift from assigning causal priority to describing how this complex has arisen and changed over time. In that process, incarceration plays a key role. Something in the nature of the relationship among the state and society, race relations, and social inequality has been transformed by the substantial growth in prison and jail populations. Whatever its effects, life in poor, high-crime communities now is also characterized by very high levels of criminal justice supervision in addition to well-documented social problems of unemployment, housing insecurity, nonmarital births, family complexity, high school dropouts, and so on.

Under conditions of high incarceration rates, the structure of correlation among incarceration, street crime, and social and economic disadvantage emerges as an important social fact. Sampson (2012) makes similar arguments about the persistence of segregation and poverty in Chicago neighborhoods. European students of “social exclusion” and “multiple disadvantage” also emphasize the highly correlated character of the many dimensions of social inequality (see Duncan and Corner, 2012; Papadopoulos and Tsakloglou, 2006).

At least three kinds of research questions emerge from this perspective. First, at a purely descriptive level, what kinds of social conditions are most closely correlated with incarceration, and how does the structure of these correlations vary across time and space? Answering this question would help in identifying and describing the cluster of social conditions in which prison time is now commonplace.

Second, how can variation in the pattern of correlations be described in a way that would be useful for analysis? Research in other fields has viewed this as a problem of scale construction, in which a variety of factors are combined to measure an underlying construct. However, this approach does not quite capture the idea that it is not a score on a scale but the strength of association of incarceration with other variables that may be consequential for social science and for policy. Motivation for examining the pattern of correlation—rather than trying to isolate the effects of individual factors—might derive from both a high level of interaction operating with incarceration and its correlates and a high level of feedback or endogeneity operating among the factors. In this context, efforts to assess individual causal effects will result in misspecification. Studying the cluster



of conditions and variations in the cluster across time and space emerges as an important research priority.

Third, what social dynamics are associated with those times and places in which incarceration is closely correlated with a variety of other markers of social and economic disadvantage? Research on urban ecology and inequality and on social exclusion in poor European communities argues that each factor, in a setting of clustered disadvantages, may reduce opportunity and social mobility only a little, but a whole cluster of disadvantages may have a much larger impact. Thus, the reproduction of social inequality and persistent poverty results not only from historic levels of poverty but also from the myriad social conditions with which poverty is correlated. Such contexts of strongly correlated social and economic disadvantage are characterized by “hysteresis” in which prevailing social conditions become self-sustaining.

## NEW DATA COLLECTION

The research agenda described here indicates the importance of significant new data collection. First, new research will need data on the conditions of confinement. Second, new research will need longitudinal data that include observations before and after incarceration. Third, new research will require better measurement of sentencing policy at the city, state, and national levels.

### Conditions of Confinement

In his classic ethnography, Sykes (1958) precisely detailed nearly every aspect of the conditions of confinement in a maximum security prison in New Jersey, documenting everything from how the physical space of the prison was laid out, to the rigid schedule inmates kept, to how men dealt with the myriad deprivations of prison life, to the infractions that would get them put in the hole. In so doing, Sykes provided a compelling portrait of how even within the same prison, the conditions of confinement could vary dramatically, with often important implications for prisoners not only during their confinement but also after their release.

Unfortunately, existing data do not provide even the most basic information regarding the conditions of confinement faced by prisoners. Existing data do not, for instance, make it possible to differentiate prison incarcerations from jail incarcerations. In a similar vein, the data provide little to no insight into the level of overcrowding in facilities, the programming available (ranging from educational, to vocational, to anger management, to drug treatment), or any other characteristics of the institutions. To illustrate this point, consider two of the best longitudinal data sets available

for exploring the consequences of incarceration and two studies using some of the best data collected within prisons.

To start with the within-prison data, Haney (2003) and Lerman (2009b) both use data on prisons in California to show how sensory deprivation and security level shape mental health and criminal propensities. These are both compelling studies, to be sure, but the fact that they are so exemplary in this field suggests just how limited the available data are.

Turning to existing longitudinal data, both the National Longitudinal Survey of Youth 1979 (NLSY79) and the Fragile Families and Child Wellbeing Study (FFCW) have been used for some of the most highly cited studies on the consequences of incarceration (e.g., Lopoo and Western, 2005; Western, 2002; Wildeman, 2010). Yet neither of these sources includes a single question on the conditions of confinement, making it impossible to tell what component of the incarceration experience is driving any effects or, on an even more basic level, whether these effects are driven by prison or jail incarceration. For example, it is nearly impossible to analyze variation in incarceration outcomes by type of criminal conviction (beyond broad violent versus nonviolent distinctions available in only a few data sets), security level of the confinement institution, or reentry services utilized after release. Such variability is of tremendous theoretical and practical importance, but rigorous analysis of these contextual factors currently remains beyond the reach of social scientists.

Because information is lacking on the conditions of confinement, therefore, it remains impossible to know how the conditions of confinement could be varied to minimize the consequences of incarceration (and reduce recidivism rates), with or without sizable decreases in incarceration.

### Longitudinal Data

The preceding sections have cited a series of methods that have been underused in the study of the consequences of incarceration. But what are the data demands for these methods? The data demands for many of these methods—especially those that require some source of exogenous variation in incarceration—are quite steep. With existing longitudinal data, one needs to design a clever experiment or rely on a natural experiment.

But what are the data demands for some of the methods that rely on longitudinal data? To consider one of the simpler—and more often utilized—methods, running a fixed effects model requires that the data include measures both before and after the incarceration experience. With an event such as incarceration, where the effects of current and prior incarceration likely differ and are both of interest, this requires a minimum of three data points—although many more data points would be better because the effects of ever having been incarcerated might change over time. To again

consider the same two excellent longitudinal data sets, what measures of incarceration are available in the NLSY79 and the FFCW? Twenty-four waves of the NLSY79 are currently available, all of which include information on current incarceration. Because of the large number of waves of data, analysts using these data can also construct a measure of prior incarceration, although this measure likely captures only prior prison incarcerations, not jail incarcerations.

The FFCW data, which are in many ways the broadly representative data set that includes the second-best measures of incarceration, illustrates just how badly needed are repeated measures of incarceration. As of this writing, the FFCW had five waves of data (at the child's birth and around ages 1, 3, 5, and 9), with an additional wave of data (around age 15) currently in the field. So what incarceration measures do these data include? At birth and age 1, the measures of paternal incarceration are very limited, with only fathers currently incarcerated being counted with confidence as having been incarcerated since the child's birth. Between ages 1 and 3, the measures of paternal incarceration improve markedly, with some information not only on whether the father is currently incarcerated but also on whether he was incarcerated since the last interview, which makes it possible to easily run a fixed effects model (or other fairly rigorous models). The measures of paternal incarceration available at age 5 are the strongest and enable use of a range of modeling strategies. Yet by age 9, the vast majority of fathers currently incarcerated were not followed into the prison, leading to much lost information on them. So the second-best data set traditionally used to consider this topic has five waves of data, one of which contains excellent incarceration data (age 5), three of which contain incarceration data that are good but not great (ages 1, 3, and 9), and one of which contains essentially no incarceration data. That this is the data set with the second-best incarceration data suggests how badly more data are needed.

Although the NLSY79 likely provides a much better measure of prison incarceration than jail incarceration, it provides no information on incarcerations occurring between waves and can be used only to consider a small range of outcomes for adult men (labor market outcomes, marriage and divorce, and health). Still, the NLSY79 provides an ideal model for how to measure incarceration consistently over a long survey. To grasp the consequences of incarceration for individuals and society more fully, future data must contain more complete, repeated measures not only of incarceration but also of crime, arrest, conviction, probation, and parole.

### Sentencing Policy

Finally, more information on specific sentencing policies and practices at the national, state, and jurisdictional levels are needed to understand the

role of policy in shaping incarceration rates, recidivism risks, and inequality in both. No reliable national database tracking the sanction regime of each of the 50 states and the federal government is available (National Research Council, 2012). By sanction regime is meant the sanctions that are legally available for the punishment of various crimes, as well as measurements of the actual administration of the legally available options (e.g., sentence length, time served). Without such data, it is impossible to make systematic cross-state comparisons of sentencing practices and their potential outcomes (e.g., effects on crime rates). The sanction regime also extends beyond the penal code prescribing the duration of sentences.

Case studies indicate the many dimensions of criminal sanction. For example, some jurisdictions have adopted policies and enforcement measures that restrict the movement of formerly incarcerated men and women, limiting their access to public spaces (e.g., Beckett and Herbert, 2011) and creating novel risks for rearrest. This example also illustrates how sentencing policy shapes not only the kind of punishment received but also who receives it. If sanctions are attached to presence in certain urban areas, or similarly if sentence enhancements are associated with urban density (close to a school zone, for example), then minority populations who are predominantly urban residents may be at great risk of sanction. If sentence enhancements are added to third-time felonies, then longer sentences will be served by older people with relatively long criminal histories.

Chapter 3 addresses the potential slippage between penal policies and their implementation in the courts. While analysis of the implementation of specific sentencing policies is a key supplement to understanding policy effects, opportunities for analysis across jurisdictions and over time would nevertheless be important for extending understanding of the crime and other social effects of the precise levers driving variation in incarceration rates.