

= Chapter 1 =

Introduction

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The research reported in this volume analyzes the nexus between criminal-justice policies and labor markets, from the perspective of released prisoners. Like other formative social institutions, the labor market is integral to the successful reentry and reintegration of released prisoners into their families and communities. Their path away from crime and future prison spells, what criminologists call desistance, depends critically on employment, specifically finding and holding a good job (Sampson and Laub 1993; Hagan 1993; Uggen 2000; Bushway and Reuter 2002). By contrast, the probability of recidivism—cycling out of prison and back in—varies inversely with an individual’s labor-market opportunities, measured by both employment and real wage rates.

Using varied data sources and empirical methodologies, the chapters in this book examine how released prisoners fare in the labor market, with a focus on how their prison experience affects their labor-market opportunities. Where appropriate, the contributors specify and test the hypothesis that a criminal-justice record in general and a prison spell in particular reinforce the steepening barriers to employment for those on the socioeconomic margins. Although the results may not be definitive (for reasons discussed presently), they suggest that the greater use of incarceration may confine less-educated individuals to the “secondary” labor market, characterized by erratic employment and low earnings. Given the link between employment and crime, the evidence further implies that ex-offenders face significant risks of recidivism and hence future prison spells, notably when they are released into relatively weak labor markets. In other words, poor labor-market prospects make ex-prisoners more likely to fall into a vicious cycle, a revolving door of prison release, crime, and reincarceration.

This labor-market perspective does not discount the public safety benefits of a vast prison system, but instead warrants a fuller accounting of its costs and net returns. Standard benefit-cost analyses focus on the benefits side of the equation—the reductions in crime rates because of the incapacitation and deterrent effects of tougher criminal sanctions (Levitt 1996, 2004; Nagin 1998; Spelman 2000). They measure the costs simply in terms of the fiscal expenditures on building and operating more prisons as opposed to other public goods. If mass incarceration causes significant unintended individual and social costs, then this standard accounting is biased in favor of imprisonment and against alternative sanctions.

To elaborate our point, we consider the most relevant counterfactual example, in which a nonviolent drug offender is sentenced to prison rather than a nonincarcerative sanction such as a drug court.¹ If the individual faces a greater likelihood of recidivism because of his prison experience, then the total social costs of the “get tough” policy should include the additional harm from his post-release criminal activity and the administrative costs related to his subsequent arrest, prosecution, and imprisonment. A growing scholarly literature on mass incarceration has identified a number of other potential unintended social costs, ranging from the disruptions to and burdens on families, especially children, the erosion of neighborhood social capital and consequent weaker informal social control mechanisms, and political alienation and distrust of public authority (Pattillo, Weiman, and Western 2004; Hagan and Dinovitzer 1999; Rose and Clear 1998; Clear, Rose, and Ryder 2001; Mauer and Chesney-Lind 2002; Huo and Tyler 2002; and Travis and Waul 2004).

Labor-Market Barriers to Reentry

Before reviewing the individual chapters, we first specify and elaborate the central hypothesis explored by each and then discuss limitations in their scope and methodology. The more narrow version of the hypothesis considers whether a felony conviction and prison term further diminish the prospects of more marginal offenders in formal, legal labor markets. That is, when compared to comparable individuals without a felony conviction and prison term, will ex-offenders realize lower employment and hourly earning rates?

A criminal-justice record can influence an individual’s future labor-market experiences through distinct causal mechanisms whose cumulative impact will likely depend on the stage in an individual’s life course when he first has a conviction.² A felony conviction with or without a prison term tarnishes marginal offenders with a social stigma, likened by Daniel S. Nagin (1998) to a scarlet letter. The stigma effect is clearest

when it is inscribed by formal regulations and legal precedents (Holzer, Raphael, and Stoll 2004). State laws prohibit the employment of convicted felons in occupations ranging from child- and dependent-care service providers to barbers and hairdressers. Some states also cut off their access to public employment, which has been an important source of work for inner-city minorities. Even in the absence of explicit legislation, many employers are wary of employing convicted felons because of the mounting case law delineating their liability risks for “negligent hiring.”³

The stigma effect can also be more subtle, triggering serious doubts if not fears on the part of those making the hiring decision. Employers may not trust convicted felons to handle valuable merchandise and cash, and may doubt whether they possess the requisite “soft” skills to interact effectively with customers (Holzer 1996; Moss and Tilly 2001). They may also worry about ex-felons’ future entanglements with other employees, as well as with criminal-justice authorities that can disrupt the work environment and work routines. Survey evidence suggests that of several stigmatized groups—including welfare recipients, GED earners, and those with spotty work histories—managers are most reluctant to hire ex-offenders (Holzer, Raphael, and Stoll 2004). Also, as shown by Devah Pager (2003; see also chapter 5, this volume), the stigma of a criminal record is powerfully mediated and compounded by racial stereotypes.

Not only can a prison record tarnish a person’s image, but a prison experience itself can also seriously erode an individual’s human and social capital. If an inmate’s human capital depreciates in prison, then upon release they will be a less productive and hence valuable worker. At the very least a prison spell will create a lengthy gap in a person’s employment record and so raise questions about his reliability, an especially important job characteristic in more lean manufacturing and distribution systems. Regardless of formal training, valuable work skills are often learned, honed, and maintained through on the job experience. While in prison, individuals not only will experience the loss of their accumulated skills but also will fail to keep pace with peers who stayed straight and worked continuously during this time.

Prison life, especially in the era of mass incarceration, is unlikely to remedy the problem—if anything, it may compound it. Prison education and vocational programs, although at best an imperfect substitute, have in any case been significantly scaled back because of skepticism over their value and also because of budget constraints in the face of rapidly expanding prison populations.⁴ At the same time prison is the quintessential “school of hard knocks,” where survival demands the acquisition of skills and behaviors not well suited to the workplace. They are also breeding grounds for serious infectious diseases (notably

HIV/AIDs, tuberculosis, and hepatitis B and C) and psychological disorders such as drug addictions and depression (Petersilia 2003, 48–51; Hammett, Roberts, and Kennedy 2001).

Robert J. Sampson and John H. Laub (1993; 2005) have persuasively argued that the impacts of a criminal conviction and incarceration depend critically on an individual's stage in his life course. They can be especially noxious at the critical life-course transition from late adolescence through early adulthood. During this formative period most individuals are completing their educational training and beginning a period of experimentation and learning in the labor market. While searching for work and on the job, they acquire valuable, often intangible, general, and firm-specific skills. What's more, they forge social contacts, often the weak ties, that prove critical to future job mobility (Granovetter 1973). This perspective implies that by their late twenties most individuals will have acquired the complement of human and social capital to land an "adult" job and realize large, positive earnings from continued work experience.

A criminal and prison record during this pivotal moment will certainly delay if not completely derail this maturation process. Thus, instead of eventually settling down into a "good" job, released prisoners may find that they lack the requisite connections to get a foot in the door and, even if they do get a job, the skills to hold down such a position. Confined to the "spot" secondary market, they will tend to churn in and out of a series of dead-end jobs. Although in the short run these positions may yield higher earnings than a "regular" job, they afford limited opportunities for steady long-term employment, human-capital accumulation, and career advancement, and benefits.⁵ With this employment track record, they may also find it more difficult to form other enduring attachments such as marriage, which has been shown to be another essential ingredient in desistance from crime (Laub, Nagin, and Sampson 1998; Sampson, Laub, and Wimer 2006).

Our arguments so far have focused on individual-level effects and so ignore a critical feature of mass incarceration, its disproportionate social and spatial incidence among less-educated minority men. The flip side of their exodus to prison is their eventual return, most often to their original or sending communities (Petersilia 2003, 22; Travis 2005, 31–34). Because of this "iron law" of imprisonment—that "they all come back"—we must be cautious in drawing any simple conclusion about the direct impacts of mass incarceration on local labor markets. The removal of large numbers of individuals to prisons may reduce local labor supplies and so benefit those who remain behind. These effects, however, are likely to be transitory, quickly offset by the return flow of released prisoners.

A more systematic approach must also consider the cumulative impact of “coercive mobility,” measured as the stock of all individuals who have ever directly experienced prison (Rose and Clear 1998; Clear, Rose, and Ryder 2001). According to estimates by Becky Pettit and Bruce Western (2004), among others, nearly one-third of less-educated African American men have been imprisoned by their mid-thirties. Given this extraordinary social-spatial concentration, we must heed the lessons of urban geography and consider the possible neighborhood effects of mass incarceration on inner-city communities. Although intrinsically difficult to identify and measure empirically, these externalities may nonetheless exert powerful influences, reinforcing, weakening, or even transcending the individual-level ones.

Elaborating his basic metaphor of stigma, the scarlet letter, Nagin (1998) illustrates one unintended consequence of mass incarceration. He observes that a prison record only derives its potency as a social stigma and so deterrent when it is the exception and so brands the bearer as an outlier in the community. Over time, as the prevalence of a prison record steadily rises, it may eventually cross a critical threshold whereby the scarlet letter becomes more the norm and perhaps even a status symbol. Even more perverse, the evidence suggests, the policies of mass incarceration have strengthened, rather than weakened, the size and sway of criminal gangs both within and outside prison walls.⁶

The cumulative toll of incarceration may in turn disrupt the local low-skilled labor market as a whole, not just for released prisoners. Thus, Nagin’s metaphor of the scarlet letter, while illuminating, is ultimately misleading. After all, with some exceptions, released prisoners do not bear a visible sign of their status. Without relatively inexpensive and reliable information on criminal-justice records, an employer must form a judgment about the applicant and will typically depend on external cues to do so—superficial characteristics that they associate with ex-convicts. In the context of mass incarceration, this form of statistical discrimination may result in *de facto* racial profiling, in which employers simply assume that less-educated African American and Hispanic men have a prison record. They will tend to treat all individuals in these demographic groups the same, regardless of their actual criminal-prison background (Holzer, Raphael and Stoll 2006; see also Pager 2003 and chapter 5, this volume).

This perspective has one clear implication about employers who draw on the low-skilled labor market in areas with concentrated populations of released prisoners. All other things being equal, they will more likely conduct criminal background checks, especially in the current era of new low-cost Internet search companies (Holzer, Raphael, and Stoll 2004; Holzer, Raphael, and Stoll, chapter 4, this volume; Bush-

way et al., chapter 6, this volume). This perspective identifies a potential and possibly significant recruitment cost facing these employers, especially if they are wary of or prohibited from hiring released prisoners and concerned about the quality of the background information. Thus, inner-city employers may actually face diminished labor supplies and a less hospitable business climate, factors that can reinforce a decision to relocate to suburban or rural areas and so diminish the total quantity and quality of jobs available in these neighborhoods.

Scope and Method

Although the authors address general issues about the prison and post-release experiences of ex-offenders, the decision was made that their empirical analyses should focus on men; thus, they ignore the most rapidly growing segment of the prison population, women. This decision was made on strictly pragmatic grounds. Despite the increasing number of women in the prison population, over 90 percent of all inmates are men, and so the vast majority of the annual flows into and out of prisons are men. This restriction yielded samples large enough to be finely parsed by demographic, socioeconomic, and spatial-temporal conditions and so best served our primary goal of sharpening the empirical estimates of prison effects.⁷

A second related research objective illustrates the qualitative importance of these differences in numbers. In chapter 2 we document the disproportionate arrest and incarceration rates of young African American men and women for drug crimes, especially more marginal nonviolent offenders. For black men, these levels have reached a point where prison has become a modal experience with potentially dire social consequences for the entire population, not just those who are arrested, convicted, and incarcerated.⁸ By focusing on men these studies also shed empirical light on these elusive agglomeration, or neighborhood, effects, which have become a distinguishing feature of this new era of mass incarceration.

Methodologically, the concentration of incarceration among young, less-educated, inner-city minority men seriously confounds the empirical problem of identifying and measuring the effect of prison, as opposed to other causal factors, on ex-offenders' employment and earnings. Because of their low levels of human and social capital to start with, these individuals face dimmer labor-market prospects in the "new economy" anyway and so are at greater risk for criminal behavior and a prison spell. We cannot readily infer a prison effect from their spotty post-release work history, which may simply mirror their pre-prison labor-market experiences and so reflect underlying personal

characteristics that led them into prison to begin with. In other words, the correlation between a prison spell and poor labor-market outcomes may be spurious.

Agglomeration effects from mass incarceration compound these selection biases. An obvious example of the former is racial profiling, whereby employers impute to all cohort members, not just ex-offenders, the same social stigma and so treat them the same. Consequently, even if there is a strong negative prison effect, researchers will observe negligible differences in the labor-market outcomes of those who do and do not experience a prison spell and incorrectly reject the prison-effect hypothesis.

To minimize these potential biases, the statistical analyses reported in Part III rely on rich longitudinal data sets based on state administrative records and careful research designs. The data sets include individual-level variables that can be used to control for the observed and unobserved personal characteristics that independently influence post-release employment outcomes such as educational attainment, marital status, and psychological propensities and individuals' prior labor-market experiences. To control for turning-point trajectories, they also include individuals incarcerated at the very end of the period, whose earlier labor-market outcomes are presumed to be untarnished by a prison record.⁹ The data sets also exploit independent or exogenous conditions, such as local unemployment conditions, that differentially influence the labor-market experiences of ex-offenders and so help to identify the impact of a prison spell.

Needless to say, these solutions are not ideal, and cannot be expected to yield definitive results. Indeed, as explained in the methodological appendix to this chapter, the statistical analyses reported in Part III understate the full force of the prison effect. Because their sampling frame is limited to individuals who were incarcerated at some point during the 1990s, their analyses address how, not whether, a prison experience influences labor-market outcomes. Still, we believe that they furnish the most comprehensive and reliable statistical evidence to date on this critical aspect of the policy question. Our confidence is bolstered by the preponderance of indicators pointing in the same direction, but derived from different kinds of data sources—administrative records from diverse states, individual-level surveys, and audit studies—and empirical methods ranging from panel regression analysis to more narrative accounts. At the same time, we recognize that the findings published in this volume represent not the last word but an important first step on the topic and hope that our efforts will inspire replication and elaboration in a systematic attempt to determine the full costs as well as benefits of mass incarceration.

Plan of Book

The book is divided into three parts. The contributions to Part I set the stage conceptually and concretely for the subsequent statistical analyses of labor-market conditions facing ex-offenders and their labor-market outcomes. Part II focuses on the demand side of the market and specifies concretely the additional barriers confronting released prisoners, compared to other marginal workers, in their search for employment. The chapters in Part III all use administrative data to analyze individual-level outcomes—employment, earnings, and future imprisonment of ex-offenders in the wake of their release. The final chapter synthesizes the contributions to this volume from the perspective of evolving labor-market institutions in post-industrial America.

The recent run-up in incarceration rates is often conceived as a “social experiment,” one combining more aggressive policing and prosecutions, the greater use of mandatory prison sentences and of stiffer prison sentences, and harsher prison and parole conditions.¹⁰ In addition to documenting these policy shifts and their immediate impacts, chapter 2 draws out the implications of this “social experiment” metaphor. We present a variety of quantitative evidence to justify our views stated above, that mass incarceration is a novel policy regime and that it evolved in response to shifting labor-market conditions. Our analysis further points to the pivotal role of the crack cocaine epidemic and the War on Drugs, dating from the mid-1980s, which was seen to justify tougher penal sanctions against a wider range of drug crimes, not just more serious (that is, violent) drug offenders, than the previous practice. We then specify conceptually the critical question addressed by subsequent chapters: What are the labor-market impacts of incarcerating more marginal offenders.

In chapter 3, Christy A. Visher and Vera Kachnowski present data gathered from interviews with a representative sample of recently released male prisoners in Chicago about their employment experiences before, during, and after prison. The results reported here are part the larger study of men being released from state correctional institutions, launched by the Urban Institute in 2000.¹¹ The research here examines pre-prison, in-prison, and after-prison employment histories and the ways these vary with individual, family, and other important factors. One novel feature of this research is that the men are interviewed about their employment histories within the three-month period after their release from prison and again four to eight months after being on the outside.

Their results show that the employment rates of these men remained relatively low in the shorter and longer periods after prison, though these rates did rise somewhat between the first and second interviews.

The employment rates remained low despite the fact that ex-prisoners felt that employment was a key to their successful reintegration, were motivated to find work, and thought that once they got a job they would manage to keep it. A variety of factors seem to be correlated with whether ex-offenders find work after prison, including employment before prison, participation in a job training program during prison, strong family support, and avoidance of drugs after release.

The chapters in Part II consider the impacts of a criminal record on the employability of ex-offenders from the perspective of potential employers—the demand side of the labor market. Because most ex-offenders have limited formal education and job experience, Part II focuses on the lower end of the market, entry-level jobs requiring at most a high school degree or its equivalent. Employing distinct empirical methodologies, Harry Holzer, Steven Raphael, and Michael Stoll and Pager (chapters 4 and 5, respectively) examine employers' responses to a similar kind of question. Holzer, Raphael, and Stoll surveyed human resources managers in the Los Angeles labor market, asking them whether the firm would be willing to hire an individual with a criminal record. In Pager's more experimental audit study, managers in the Milwaukee area were confronted with the decision of whether to hire an individual with a criminal record or an otherwise equivalent job applicant and so had to decide the matter in context. The concluding chapter in Part II, by Shawn Bushway, Shauna Briggs, Mischelle Van Brakle, Faye Taxman, and Meridith Thanner, examines an increasingly important ingredient in firms' hiring decisions: the supply and quality of information about job applicants' criminal backgrounds.

A key variable in these analyses is what it means to have a criminal record. A criminal background search could lead to an entry if an applicant has been arrested but acquitted. Likewise, an offender with a criminal conviction need not have served time in prison and so will have no prison record. The nature of the record matters not only because of our concern over the unintended consequences of mass incarceration but also because it bears on the causal questions of how a criminal record affects an individual's labor-market opportunities, in this case, the perceptions and behaviors of employers. A felony conviction alone or even frequent arrests without prosecution and conviction may be sufficient to trigger a negative signal. A prison spell would reinforce the stigma, but also add the negative impacts on the applicant's human and social capital, such as his "soft" skills and personal references.

Holzer, Raphael, and Stoll's study considers the more general impacts of a criminal record, rather than of a prison spell per se. Based on a representative sample of employers, they delineate firms' preferences for hiring ex-offenders, as measured by managers' responses to the counterfactual question. Their focus on firms' more stated rather

than actual demand for labor is an obvious limitation of the survey methodology. To test the consistency and hence reliability of these variables, the authors included additional relevant questions, for example, about firms' actual hiring of ex-offenders over the previous year and the frequency with which they conduct criminal background checks. With these and additional survey data, the authors carefully depict the types of firms—by sector, size, job characteristics, and so forth—that are more and less likely to employ ex-offenders and use associated screening practices. Given the timing of the data collection, the authors also shed some light on the impact of the “high-pressure economy” of the late 1990s and post-9/11 security concerns on the demands for these disadvantaged workers.

Pager's more experimental research design enables her to draw more causal inferences about the impacts of a drug conviction and prison spell, albeit over a more restricted set of firms and questions. The nature of her study limits the outcomes to a dichotomous variable—whether after submitting the job application the tester receives a callback. By contrasting the experiences of white and black pairs of testers and of testers who did and did not speak to the personnel manager at this initial stage, she adds the dimensions of race and personal contact to see how they individually and jointly influenced the demand for the released prisoners. The study is also confined to firms that were currently advertising for workers and in public sources. Because of deteriorating macroeconomic conditions, fewer firms were likely to have vacancies and so to search actively for new hires. Still, she reasons, firms that advertised in public venues would be less likely to discriminate, whether on the basis of race or criminal record. Consequently, finding lower callback rates among testers with a criminal record would be powerful evidence in support of the existence of an incarceration effect.

Both chapters show employers' reluctance to hire individuals with a criminal record. At the same time, they also identify a number of mediating factors that can reinforce or moderate this negative criminal background effect. Most striking, but not too surprising, is the powerful influence of race on potential employers, who seem to assume that black applicants are “guilty until proven innocent.” Additionally, both studies point out some flexibility in managers' judgments, evidenced by their differential treatment of what we term the “marginal” versus the more hardened offender.¹²

In both instances, information about an individual's criminal background may be decisive in allowing him to get a foot in a company's door. Interestingly, despite reaching similar conclusions about the force of racial profiling or stereotyping, Holzer, Raphael, and Stoll and Pager express divergent views on the policy question of making criminal-justice records more or less accessible. Before resolving this

important policy issue, Shawn Bushway and his colleagues, in chapter 6, insist that we must first assess the sources and quality of the information disseminated. Their contribution is especially timely, as they clearly depict a “revolution” in criminal-record reporting driven by both legal and technological innovations. On the legal side are court rulings on the question of access and also the increasing prevalence of public and private sanctions against hiring ex-offenders. At the same time the IT revolution has greatly accelerated the public-sector goal of creating centralized, comprehensive criminal-justice databases, while it has also reduced the entry cost for competitive private vendors.

Chapter 6 describes these two trends and then addresses the question of the quality, not simply the quantity, of the information supplied. Through qualitative analysis, Bushway and his colleagues consider the various public and private measures to produce more comprehensive, reliable reporting on criminal backgrounds, such as the FBI’s National Crime Information Center, and the various measures of self- and public regulation of private-sector firms. Their account underscores the inherent problems in achieving this goal because of the highly decentralized U.S. criminal-justice system with its multiple and often incompatible information systems. They also question the effectiveness of government regulation in this case, as compared to the credit-history reporting industry, because of the proliferation of smaller firms, which are more difficult to monitor. They underscore these concerns through a simple empirical exercise comparing the information contained in the FBI database and provided by a larger private vendor for a sample of parolees in northern Virginia.

The study by Bushway and his colleagues raises some disturbing questions about the consequences of sketchy criminal-justice information, especially on young, less-educated black males, who are most vulnerable to profiling or stereotyping. And, they ask, are there any incentives for suppliers or demanders of this information to remedy the problems? For example, do employers purchase this information, regardless of its quality, merely to shield themselves from negligent-hiring lawsuits? Do county and state officials have the incentives and resources to digitize their records in compatible formats for integration into a national database? Answers to these questions, while important, still will not resolve the more pressing policy issue raised by Holzer, Raphael, and Stoll and Pager on how to deploy this information effectively, not only to bolster public safety but also to protect the rights of ex-offenders from the “collateral consequences” of their transgressions—possibly more enduring than incarceration’s direct consequences.

Part III focuses on the life outcomes of ex-offenders upon their release from prison. The research in these chapters is unique in its use of

large administrative data sets from departments of corrections in four states: California, Florida, Ohio, and Washington. With the exception of the California study, the authors have also collected administrative data on employment from the states' unemployment-insurance (UI) agencies, which they have linked to the corrections data.¹³ Given the rarity of this type of administrative-data analysis, the simple state-by-state comparison of descriptive statistics is itself of considerable interest.

With this goal in mind the authors have derived comparable measures that they use to directly compare the sample populations to a single reference point, the Washington case.¹⁴ Some of the baseline differences are striking. For example, African Americans make up 27 percent of the prison population in Washington, 46 percent in Florida, and 56 percent in Ohio. Employment outcomes prior to admission also differ greatly. In the year prior to admission prisoners in the Washington sample had a 50 percent chance of working, while those in the Florida and Ohio samples only had a 32 percent and 35 percent likelihood, respectively.

These chapters all analyze whether the incarceration experience can harm prisoners' post-release employment outcomes. They also consider how their employment prospects and recidivism rates are mediated by participation in prison programs and by post-release labor-market conditions. There is a substantial literature (see also chapters 4 and 5 in this volume) that clearly demonstrates that many employers are unwilling to hire ex-offenders. Becky Pettit and Christopher Lyons (chapter 7) are particularly concerned with the possibility that the incarceration experience will lead to declines in employment outcomes. They find a sizable positive (30 percent) increase in employment rates in the immediate post-release period relative to the year prior to incarceration, yet this spike in employment turns out to be transitory. It disappears entirely after one and a half years and actually becomes negative after three years. This unexpected result is partially corroborated by John H. Tyler and Jeffrey R. Kling (chapter 8) for Florida and William J. Sabol (chapter 9) for Ohio. Sabol's analysis implies the exact same time line and trajectory of employment rates: an immediate post-release spike that diminishes after 1.5 years. Analyzing earnings, not employment, Tyler and Kling show a sharp post-release increase in average levels that is most likely due to higher employment rather than to higher wage rates. They do not, however, find comparable evidence of a rapid decline in employment.

Pettit and Lyons and Sabol conjecture, reasonably, that the increase in employment is due to post-release supervision—the pressure parole officers exert on ex-prisoners to find and keep a job. In the Florida case, however, Tyler and Kling do not find a significant supervision effect.

We hope that future work can be directed at better understanding the cause of the post-release employment increase and at investigating whether there are ways to leverage this possibly short-term surge in employment for better long-term outcomes.

Pettit and Lyons also investigate the impact of incarceration on the wages of released prisoners. For about four years after their release, they earn slightly lower wages than they did in the pre-incarceration period. So although employment rates increase, wages do not. Their relatively flat wage trajectories are especially troubling when viewed in light of the more typical positive age-earnings profile of individuals in these formative years. An important question, which these data unfortunately cannot answer, is whether the declining employment outcomes of released prisoners are directly linked to their low wages and poor long-term labor-market prospects or to their criminal justice record.

Finally, Pettit and Lyons and the other authors show that there is no common incarceration effect on individuals. The negative impact of incarceration on wages, they predict, will be particularly large for inmates who had relatively better job opportunities prior to prison, a group that includes whites, high-status offenders, and less serious offenders. Their analysis largely confirms this prediction, and also shows a larger post-release employment spike for higher-risk than for more marginal offenders. These results do not have any clear policy implications, but do place the evidence of a modest race effect on the sentencing decisions, which is harsher for blacks, in a new context. Perhaps judges are aware that some groups experience greater extralegal sanctions from the community, and therefore need to be treated less harshly by the system itself.

Chapter 8 focuses on the impact of prison programming, specifically GED (general equivalency diploma) training and certification, on ex-inmates' employment outcomes. The analysis has a particularly strong quasi-experimental research design that relies on the panel structure of the data to control for selection bias, based on unobserved differences between individuals. As a purely methodological point, they show that the results of these models are not substantively different from those of models with extensive controls, including prior criminal history, education levels, and employment. As a guide to research, the result suggests that rich cross-sectional data sets may be just as effective as panel data sets in addressing selection problems.

Tyler and Kling report no significant overall effect of a GED on post-release earnings. Minority males, however, constitute an important exception, although the benefits of a GED on their wages, as on employment, appear to be transitory. They also test whether the benefit of a GED is merely a certification effect or due to learning while in prison.

Because of their careful research design and rich administrative data, their study represents the strongest evidence to date on the topic and yields similar results to a comparable study of Project Prep by William G. Saylor and Gerald G. Gaes (1999). William J. Sabol, in chapter 9, also looks at whether receiving a GED in prison improves employment outcomes and finds no effect for any group. This result is especially surprising, because Sabol's data set has a more limited set of controls and so cannot fully adjust for the expected positive bias in comparing self-selected participants to nonparticipants.

Sabol's main focus is the effect of local unemployment rates on employment and recidivism. Research in labor economics implies that local unemployment rates are correlated with an individual's chances of finding a job, especially for those such as released prisoners who are at the tail end of the job queue (Neumark 2002; Offner and Holzer 2002). Sabol finds a rather large labor-market effect, which is consistent with Holzer, Raphael, and Stoll's (2004) results that released prisoners are the last hired and the first fired, even in tight labor markets. The labor-market effect found by Sabol is nearly twice as large as results derived by Paul Offner and Holzer (2002) for less-educated black men.

Raphael and Weiman also examine the local labor-market-recidivism link, but in a context where parole revocations were common, accounting for 70 percent of new prison admissions. Although the average effect of the unemployment rate on the likelihood of recidivism is small, it increases with time. This result suggests that employment conditions are more important for lower-risk, less criminally prone offenders, who are more likely to survive the first few months of release without returning to prison. Raphael and Weiman test this hypothesis by contrasting the unemployment-recidivism connection for more and less serious offenders, where degree is measured by the number of prior prison spells. Since the local labor-market effect is the product of (1) the impact of local unemployment rates on the probability that a parolee finds a job and (2) the effect of having a job on the likelihood of violating parole, the authors use Sabol's estimate of (1) to derive an estimate of (2). For more marginal offenders, they show that this latter effect can be significant, a finding that has important policy implications.

Taken together, the results in this section present an intriguing picture of the employment prospects of ex-inmates. All four chapters show that pre-prison employment, prior criminal history, and pre-prison education levels are extremely important predictors of post-prison employment and recidivism rates. These differences dwarf even the most optimistic expectations about the potential impact of prison employment programs. Despite this sobering reality check, it is also true that the authors of all the chapters find large increases in immediate post-

release employment rates, even for inmates with very low levels of pre-prison employment. These outcomes may partly depend on the local unemployment rates in the areas to which the inmates return. Local conditions, Sabol and Raphael and Weiman find, can significantly affect the prospects of released prisoners' finding a job and going straight.

This research does paint a rather dim picture of the prospects for more serious offenders with low levels of education and prior employment. All of the chapters suggest that this group will have very poor post-release employment outcomes, even if they participate in prison programs. Still, according to Tyler and Kling, prison programming may yield some benefit, albeit transitory, to minorities and so help the most disadvantaged group of inmates. Although supporters of GED programs might be discouraged by the relatively weak evidence for their efficacy in this volume, even weak evidence in favor of an effect is somewhat surprising, given the extremely poor employment prospects and criminal involvement of this group of ex-inmates.

In the final chapter, Bruce Western considers the contributions to this volume in terms of an emerging "institutional" perspective on mass incarceration. By this he means more than the mere fact that prisons are institutions with their distinct organizational charts, cultures, and norms. In the era of mass incarceration, he argues, prison has become institutionalized—the norm—in the lives of disadvantaged individuals, especially young, less-educated minority men living in inner cities. Harsh criminal-justice policies mean that they are more likely than other groups to wind up in prison and to bear its scars, which in the labor-market context can skew their employment opportunities and risk of recidivism.

In elaborating this perspective, Western highlights the volume's main themes. First, the policies of mass incarceration must be seen in the context of the political economy of inequality in the United States, especially inequality that occurs along racial-ethnic lines. Second, they can reinforce these inequalities by confining released prisoners to the erratic low-paying jobs of the secondary labor market, thereby increasing these young men's risk of recidivism. And finally, carefully designed programs, informed and honed by ongoing research, can weaken the revolving-door effect, but current investments in training and reentry programs are too limited in scale and scope to have much impact.

The contributions to this volume focus on the impacts of policy but do not venture any explicit policy prescriptions to deal with the connection between socioeconomic disadvantage and crime and the dim employment prospects of released prisoners. Their analyses do, however, bear on vital policy issues that come up at virtually every stage in the criminal-justice system and every level of government. Chapters 2, and

9 through 11 deliver the clear message that macroeconomic and employment policies can have a significant impact in preventing crime and incarceration to begin with and in improving the employment prospects of those marginal offenders reentering their communities. The analyses in Part 3 raise familiar doubts about the effectiveness of rehabilitation programs such as GED preparation, although they do note the potential benefits of employment experience, whether obtained prior to or during the prison spell. And finally the contributions to part II identify the potential benefits but also the pitfalls of recent policy innovations centralizing and disseminating criminal-justice records.

Chapters 2 and 11 deviate from this course and explicitly broach the issue of the policy alternatives to mass incarceration. Chapter 2, focusing on the War on Drugs, concludes with an assessment of drug courts, an alternative to incarceration, especially for more marginal offenders. Consistent with our perspective, we ask whether drug courts constitute an effective option to reduce recidivism and so enhance public safety, at a lower actual cost to governments and social cost to offenders and their communities. Western, in chapter 11, goes even further, maintaining that comprehensive investments in training and reentry programs, while symbolically important, are not likely to make more than a marginal difference in the lives of released prisoners because of these men's myriad deficits and the harsh environment that they face. In the end, he restates the viewpoint that motivated the research in this volume, that a more effective and humane policy would strive to reverse the isolation and alienation of those on the socioeconomic margins and in this way reduce their likelihood of winding up in prison in the first place.

Appendix: Defining and Estimating Prison Effects

Analyzing the impacts of mass incarceration on individual labor-market outcomes poses a number of thorny conceptual and methodological problems. At the most fundamental level, the notion of a "prison effect" has several connotations, corresponding to the range of policy innovations that have contributed to the dramatic run-up in incarceration rates since the late 1970s. We can assess the impacts of the following on individuals who have committed a felony offense:

1. More aggressive policing, which increases the likelihood of an arrest
2. Mandatory minimum sentencing policies, which increase the subsequent risk of a prison commitment

3. Steeper minimum sentences and truth-in-sentencing policies, which increase the length of a prison stay

(See, for example, Grogger 1995; Western 2002; and Kling 2006.)

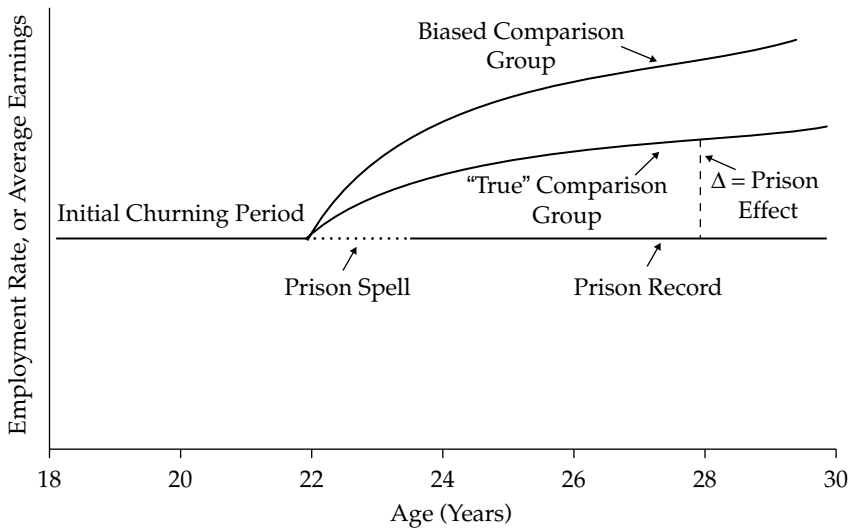
We consider Western (2002) and Kling (2006), because they narrowly focus on the prison dimension of mass incarceration. Put simply, Western asks *whether*, while Kling asks *how*, a prison spell affects an individual's post-release earnings and employment trajectories. In turn, they analyze different data sets with different sampling frames. From the National Longitudinal Survey of Youth (NLSY) 1979, Western (2002) selects a subsample of individuals who either experienced an incarceration (prison or jail) spell by 1998 or reported other contact with criminal-justice authorities (ranging from police stops to a conviction and probation) in a 1980 supplement. Because he assumes that latter group were "at risk" for but eluded incarceration, he can interpret the difference between their labor-market trajectories as evidence of a prison effect—the stigma of an arrest, conviction, and a prison sentence plus any negative consequences of the prison experience itself. Because the NLSY 1979 survey data contain no detailed information on life behind bars for those who experienced prison, Western's analysis cannot resolve the prison effect into its constituent elements—that is, test the distinct causal theories discussed in the main text.

Kling (2006), as in the research reported in Part III of this volume, fills this empirical gap by estimating the labor-market effects of different prison experiences, in this case shorter versus longer prison spells.¹⁵ It therefore draws on an alternative data source, the comprehensive records of the Florida Corrections Department, which document the prison spells of all individuals incarcerated during the sample period. Kling (2006) can only assesses whether more or less prison time, not imprisonment per se, adversely affected the post-release outcomes of ex-offenders, as well as any offsetting impacts from participation in and completion of rehabilitation or training programs available only to those serving longer terms.

We illustrate the basic hypothesis of a prison effect and the methodological problems in estimating it through a simple diagram based on Western (2006). The graphs in figure 1A.1 delineate the age-employment or age-earnings profiles of those with and without a prison record during their formative years, from late adolescence to early adulthood.

Their profiles are initially flat, reflecting the churning into and out of jobs, characteristic of new entrants.¹⁶ Over time, individuals without a prison record are expected to accumulate labor-market and on-the-job experience and find a suitable job match. Once they land a "good" job, their employment rates and earnings will steadily increase (as shown by the curve labeled "True Comparison Group"). An incarceration

Figure 1A.1 Hypothetical Employment-Earnings Profiles of “At-Risk” Young Men With and Without a Prison Record



Source: Authors' compilation.

tion spell (noted by the dotted line) interrupts this vital life-course transition, and confines released prisoners to the secondary labor market. We depict their diminished labor-market prospects by a flat age-employment profile, at levels comparable to their pre-prison experience (as shown by the line marked “Prison Record”).¹⁷

Conceptually, we gauge the prison effect by the widening gap between the employment-earnings profiles of the two groups as they age (denoted by Δ for age twenty-eight years), that is by quantitative difference in their slopes. Methodologically, an accurate or consistent estimate of this parameter depends critically on whether the two groups are truly comparable and so experience divergent labor-market outcomes only because of cumulative impact of an arrest, conviction, and prison commitment. If not, other factors may systematically influence their criminal and labor-market outcomes and so potentially skew or bias the parameter estimate. For example, instead of by mere chance, some in the comparison group may have avoided a prison spell because of personal characteristics or locational advantages that also made them more employable. The failure to incorporate this factor into the empirical model would overstate the influence of a prison record on the labor-market trajectories of the comparison group.

Graphically, we depict this problem of selection bias by adding a second, steeper, employment-earnings profile to figure 1A.1 (labeled “Biased Comparison Group”). In this case the comparison group is too broad. It includes individuals who systematically differ from released prisoners along a number of dimensions that significantly bolster their labor-market prospects but also lower their incarceration risks. Failure to identify and control for these factors, then, will result in a spurious negative correlation between labor-market outcomes and a prison record—in other words, an overestimation of the prison effect.

Avoiding these methodological pitfalls hinges critically on the proper specification of the empirical model and ultimately on the quality of the data set. It must include sufficient background information to control for the influence of confounding factors that affect both labor-market and incarceration outcomes. With the rich information in the NLSY 1979 survey data, Western can account for individual differences along a number of relevant dimensions such as human capital, psychological dispositions, and social attachments. Moreover, the data set tracks individuals’ prior labor-market histories, which can serve as a benchmark to gauge the influence of relevant unobserved factors that do not vary over time (that is, “fixed effects”). Still, these data and methods are not ideal and so cannot detect all of the relevant factors that specify the comparison group, especially if they cumulate over time. One example is the prevalence of incarceration itself, which may systematically affect the likelihood of an arrest leading to prison and of a contact leading to a good job.¹⁸

The empirical analysis in Kling (2006) and in Part III of this volume must resolve similar methodological pitfalls. As in the previous example, a strong positive correlation between the length of a prison term and the prison effect on post-release outcomes may be spurious. In states with mandatory guidelines and truth-in-sentencing laws, for example, more serious offenders committing more serious crimes will serve longer terms. Thus, their criminal record, rather than the length of the prison spell per se, may directly limit their post-release labor-market prospects.¹⁹ Moreover, the evidence of such crimes and longer sentences may be symptomatic of underlying personal characteristics that would likewise disqualify individuals for good jobs (such as impulsivity or a drug addiction).

To minimize these selection biases, Kling (2006) constructs a rich data set that links prison, arrest, and employment administrative records. Consequently, the empirical model can control for direct evidence on inmates’ criminal history, including the most recent offense, but also unobserved factors that are reflected in their pre-incarceration employment history. Although Kling finds no significant long-term impacts of lengthier prison spells, we must still be cautious in interpreting

this result for several reasons. The data sources, for example, may not contain pertinent information about the ex-felons' pre-prison labor-market experiences such as employment and earnings off of the books or the influence of an even earlier prison spell. Moreover, the very question under investigation may be relevant to only select prison populations, whose greater incarceration rates depended more on increasing the length of the prison term as opposed to increasing the rate of arrest or of a prison commitment following an arrest (Blumstein and Beck 1999, 34–36; Western 2006, 45).

Notes

1. We elaborate and concretize this example in the conclusion to chapter 2, where we present actual estimates of the net social benefits from assigning drug offenders to drug courts rather than confining them to jail or prison. These net benefits are just the flip side of the net social costs of “business as usual,” the prison option. Throughout we use the pronouns “him” or “his,” because the research in this volume focuses on male prisoners.
2. This discussion draws on and elaborates the theoretical analysis of prison effects and methodological issues in estimating them found in Bruce Western, Jeffrey R. Kling, and David F. Weiman (2001).
3. Under the principle of negligent hiring, employers can be held liable for an employee's criminal actions if they “know, or should have known” of the employee's past criminal behavior (Bushway 1996; Glynn 1988). Employers have lost 72 percent of negligent-hiring cases and have had to pay an average settlement cost of more than \$1.6 million per case for “punitive damages as well as [plaintiffs'] . . . loss, pain, and suffering” (Bushway 1996; Connerley, Arvey, and Bernardy 2001). The high probability of losing coupled with hefty settlement awards suggest that fear of litigation may substantially deter employers from hiring applicants with criminal-history records. We have found no data on the number of negligent-hiring suits, but anecdotal evidence indicates that they are on the rise.
4. Indeed, the most recent Bureau of Justice Statistics evidence shows that state and federal prisons are operating near or above their optimal capacity (Harrison and Beck 2005, 7). According to Caroline Wolf Harlow (2003, 4), participation rates of state prisoners in all educational programs except vocational training declined between 1991 and 1997. Tyler and Kling and William Sabol (chapters 8 and 9, respectively, this volume) evaluate the post-release employment benefits of prison GED programs.
5. See Nagin and Waldfogel (1998); and Western (2002). Figure 1A.1 in the appendix graphically illustrates this more dynamic version of the “prison-effect” hypothesis and the methodological conundrums in accurately estimating its magnitude.
6. See John Hagedorn and Perry Macon (1988, 84–86, 158–62) and Hagedorn (1998, 399–400); and Sudhir Alladi Venkatesh and Steven D. Levitt (2000). Prisons are ideal venues for recruiting new gang members and for gang leaders to meet to coordinate their geographically dispersed operations. In

the Chicago case studied by Venkatesh and Levitt, the authors imply that a prison experience was a necessary credential for upward mobility in the gang hierarchy, much like a college degree for middle-class youths.

7. Paige M. Harrison and Allen J. Beck (2005, 4–5, 8) provide 2004 estimates of the prison population and incarceration rates by demographic group. By way of illustration we contrast the annual flows of women entering and exiting prisons in California and Washington, the sample states with the largest and smallest prison populations. In 1998 there were approximately 13,000 female and 120,000 male prisoners in California, and in Washington there were 800 women and 5,500 men (see the Bureau of Justice Statistics data at www.ojp.usdoj.gov/bjs/dtdata.htm#corrections). An empirical study of female prisoners with a comparable level of detail, therefore, must consolidate release cohorts over several years (see, for example, Cho and Landonde 2005).
8. By 2004 the incarceration rate for black men had reached 3.2 percent of the total population, as opposed to only .2 percent of black women. Consequently, nearly one-third of black men would likely experience imprisonment in their lifetimes, as compared to only 5.6 percent of black women, approximately the same rate as for white men (Bonczar 2003, 1).
9. Individuals incarcerated at the end of the sample period constitute a kind of control group, whose earlier labor-market experiences may illustrate the “counterfactual” case: the age-earnings profiles of released prisoners in their late twenties who had not been incarcerated in their mid-twenties. Of course, the individuals in the comparison group may have experienced an earlier prison spell, before the data set was constructed, which would skew their 1990s labor-market record.
10. The sentencing component of this mix is itself a bundle of reforms, including the abolition of discretionary parole release; determinate sentencing commissions and guidelines; mandatory minimum sentences, including, in the extreme, “three-strikes” sentences for repeat offenders; and “truth-in-sentencing” policies. In chapter 2 we describe each policy type and specify when and where the most notable examples were enacted.
11. For an overview of the Urban Institute research, see “Understanding the Challenges of Prisoner Reentry,” at www.urban.org/url.cfm?ID=411289.
12. The notion of a marginal offender is defined in economic and criminological terms in the third section of chapter 2.
13. The UI data in Florida and Ohio only provide information on individuals’ quarterly earnings from covered jobs, whereas the Washington data also include reported hours from which average wages can be derived (equal to quarterly earnings divided by quarterly hours). The UI records serve as a rough indicator of employment opportunities, but only in the formal sector. Missing is any evidence of employment and earnings from informal work such as day labor and of course any illegal activity, which may be found in self-reported survey data (see for example Uggen 2000; Western 2002; and Kornfeld and Bloom 1999).
14. In terms of criminal-justice policies, Washington state falls on the more lenient end of the spectrum. Its overall prison incarceration rate in 2005 was only 263 inmates per 100,000 people, as compared to the national aver-

age of 488 and a rate of 392 in Ohio, 456 in California, and 492 in Florida (Harrison and Beck 2005). Furthermore, in Washington, maximum prison sentences and time served are shorter on average than in the other states, even though the inmates serve a greater fraction of the maximum sentence (Ditton and Wilson 1999).

15. The chapter by Tyler and Kling analyzes a similar question, whether prisoners who participated in and completed a GED training program had different post-release outcomes. Chapters by Pettit and Lyons, Sabol, and Raphael and Weiman, by contrast, assess the impact of a comparable prison experience on ex-offenders who differ by race, education, or destination upon release.
16. Our analysis in figure 1A.1 depicts the aggregate, or average, trajectory of individuals in this cohort, that is, it “averages” over the more volatile experiences of individuals.
17. Although they examine a different issue, all of the studies in Part III show a sharp but transitory increase in post-release employment rates and total earnings. Our case, therefore, illustrates the long-term trajectories, after employment rates fall back to their pre-prison levels.
18. If quantitatively important, this factor would add another component to the estimated prison effect, the agglomeration effect (also call the concentration, or neighborhood, effect) of mass incarceration taken as a complex of policies.
19. As shown by Holzer, Raphael, and Stoll and Pager (chapters 4 and 5, respectively), employers’ likelihood of hiring an ex-offender is contingent on the offense that landed them in prison in the first place. They may be more likely to overlook a first-time nonviolent drug offense than an assault.

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