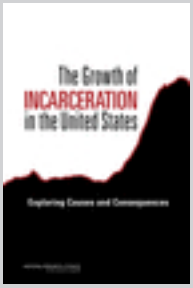

The Growth of Incarceration in the United States: Exploring Causes and Consequences

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Consequences for Employment and Earnings

This chapter reviews the labor market literature to examine the extent to which the experience of serving time in prison affects subsequent labor market outcomes. In the best of worlds, those who were incarcerated would serve their time and receive treatment if necessary, and upon release would be able to return to work or find meaningful employment. For many ex-prisoners, however, labor market prospects after prison are bleak. Several studies of ex-prisoner populations report that roughly half remain jobless up to a year after their release. For example, a longitudinal study of 740 males exiting prisons in Illinois, Ohio, and Texas, conducted by the Urban Institute (Visher et al., 2010), found that only 45 percent were formally employed 8 months after release (65 percent had been employed at some point since release). Similarly, a study of 46,000 Ohio ex-prisoners released in 1999 and 2000 found that 42.5 percent remained unemployed 1 year after release (Sabol, 2007, based on linked unemployment insurance data). Other small-scale studies have found even lower rates of employment following release (Petersilia, 2003; Festen and Fischer, 2002; Nelson et al., 1999).

For the most part, available employment research does not examine the effects of rising rates of incarceration. Therefore, we cannot directly address how the employment outcomes of ex-prisoners may have changed as the experience of incarceration became more widespread. In addition, the effect of incarceration, as measured in most studies, may reflect the effect of a conviction with or without incarceration (see below for exceptions). Nonetheless, we believe the findings discussed here suggest an increasing labor market impact of incarceration, at least in terms of the numbers

affected, and notably concentrated among certain populations whose overrepresentation among the incarcerated is discussed elsewhere in this report.

Because the ex-prisoner population is skewed toward prime-age men, much of the literature salient to the discussion in this chapter focuses on men; only a few studies focus on women, as noted below. The chapter begins with a discussion of the possible mechanisms through which incarceration reduces wages and employment among former prisoners. A review of the various approaches used to estimate the impact of incarceration on subsequent employment and earnings follows. The available evidence evaluating program and policy interventions aimed at improving the employment outcomes of those previously incarcerated is then considered.

MECHANISMS

In a recent research review, Pager (2007) identifies three mechanisms that could explain the poor employment outcomes for ex-prisoners: (1) selection, (2) transformation, and (3) labeling. All three mechanisms are likely to contribute to some extent, with some perhaps being more dominant than others for certain types of inmates or institutional experiences.

Selection

Virtually all research on employment and earnings finds that people who have been incarcerated do very poorly in the labor market; there is less consensus as to whether these poor outcomes are an effect of incarceration. As discussed elsewhere in this report, those who are incarcerated have certain characteristics associated with both the risk of incarceration and poor labor market outcomes: they average less than 12 years of schooling; have low levels of functional literacy; score low on cognitive tests; often have histories of drug addiction, mental illness, violence, and/or impulsive behavior; and have little work experience prior to incarceration, with at least one-quarter to one-third of inmates being unemployed at the time of their incarceration (Travis, 2005; Bureau of Justice Statistics, 1994). Unemployment and low wages among the formerly incarcerated may therefore result not from incarceration but from preexisting low employability and productivity.

As noted in earlier chapters and discussed further in Chapter 10, many of the incarcerated come from marginalized communities. Because of shifts in the American labor market (Wilson, 1987, 1997; Kalleberg, 2011), these communities often have fewer quality jobs and more unstable, low-paying, low-quality jobs—the kind of jobs for which those released from prison are most likely to compete when they are able to compete at all. And it is to those neighborhoods—where others are marginally employed and

where the social networks needed to link to quality employment are most disrupted or nonexistent—that most men and women released from prison return. It may be, then, that the employment challenges of the formerly incarcerated are driven largely by characteristics of those who end up in prison and the communities from which they come, rather than by any direct consequence of incarceration itself.

Transformation

This second explanation suggests that the experience of incarceration changes inmates in ways that are detrimental to their job readiness. Here there is likely to be significant heterogeneity in effects, with variation across both inmates and institutions. While some inmates advance their education, develop job skills, and/or stabilize their lifestyle during their time in prison, many others are worse off when they leave prison than when they arrived as a result of a range of disruptive and debilitating features of prison life. Moreover, behaviors that are adaptive for survival in prison—a taciturn demeanor, a suspicious approach to human relationships, and resistance to authority, for example (Irwin and Austin, 1997, p. 121)—often are counterproductive for stable employment (see the discussion in Chapters 6 and 7).

In addition, extended periods of absence from the labor market can erode skills and create large gaps in work histories, in turn raising questions about individuals' preparation for work. Extended periods away also can disrupt social and familial relationships (Hagan, 1993), which often are critical to securing employment. (The consequences of incarceration for families are discussed further in Chapter 9.)

Labeling

The legal and social stigma of a criminal record,[1] especially now that criminal record information is widely available to employers, may mean that mere contact with the criminal justice system can have lasting employment consequences. The labeling due to criminal conviction can result in both legal and social exclusion. Formal exclusion is imposed through the web of federal and state laws that restrict those with a criminal record from a range of labor market activities (Olivares et al., 1996; Petersilia, 2003).

The number of barred occupations and limits on employment for those with a criminal record has increased substantially during this period. The nature of the restrictions can vary from bans for anyone with a criminal record to bans for certain crimes; the restrictions can be time-limited or

[1]Employers often lack information on incarceration from official record sources, so many hiring decisions likely are made on the basis of convictions rather than time served.

lifetime bans. Some restrictions offer employers hiring discretion, and some provide the job seeker avenues for demonstrating rehabilitation. These hiring restrictions have been adopted by legislatures and state agencies overtime in response to diverse events. As such, these policies are spread across chapters of state laws and records and have become quite complex to navigate for those seeking employment, those seeking to hire, as well as those trying to aid persons with criminal records. There are also a number of federal and local restrictions. Neither the number of legal restrictions nor the jobs subject to restrictions have been quantified nationally.[2] However, some states have initiated inventories of their policies and restrictions. Florida, for example, identified state-created restrictions on 40 percent of the jobs in large employment sectors (Mills, 2008).

Beyond legal restrictions, employers express a reluctance to hire individuals with a criminal record, which often is viewed as a sign of untrustworthiness or unreliability (Holzer, 1996). Over time, employers have become increasingly likely to ask job applicants about their criminal history and substantially more likely to conduct official criminal background checks to verify applicants' reports on their prior criminal convictions (Bureau of Justice Statistics, 2003; SEARCH, 2005). To the extent that job applicants are eliminated from consideration on the basis of their criminal record, the labeling consequences of criminal justice contact will result in reduced opportunities for employment. These dynamics could likewise cause ex-inmates to concentrate their job search outside the formal sector of the labor market.

APPROACHES TO STUDYING EMPLOYMENT EFFECTS

This section reviews the various approaches that can be used to study the effects of incarceration on employment. We caution that, when assessing the labor market consequences of incarceration, much of the existing literature takes as its relevant comparison group those who are similar to incarcerated individuals in all ways apart from the criminal conviction that led to their incarceration. The "effect" in this case captures the consequences of both the conviction and the period of incarceration.

[2]The American Bar Association has begun assembling a database of collateral consequences of conviction for each U.S. jurisdiction. This work is supported through the National Institute of Justice under a provision in the Court Security Improvement Act of 2007. To date, this database contains more than 30,000 state laws that restrict access to employment, occupational and professional licenses, and other basic rights. The database is expected to include information from all states by 2014. For more information, see http://www.abacollateralconsequences.org/ [May 2013].

Surveys of Employer Attitudes

Surveys of employers have examined attitudes toward hiring individuals with a criminal record. In contrast with other sensitive topics, such as race or gender, employers do not appear reluctant to express negative views about those who have had trouble with the law. According to Holzer (1996), for example, roughly 40 percent of employers in a sample of four large urban labor markets reported that they would not knowingly hire someone with a criminal record (see also Holzer et al., 2004a, 2004b, 2006, 2007; Husley, 1990, pp. 40-41; Pager, 2007, Chapter 7). Another 25-35 percent of the employers responded "it depends" (Holzer et al., 2004a; Pager and Quillian, 2005), which suggests that at least for some employers, the type of crime or the circumstances of the conviction provide relevant information beyond the simple fact of conviction. Overall, though, the plurality of employers appear highly reluctant to hire those with a prior criminal record.

Some survey research suggests that employer attitudes vary by type of occupation, with greater restrictions being placed on sales and clerical jobs than on those entailing more manual skill (Husley, 1990, p. 43; Holzer, 1996, pp. 58-62; Pager, 2007, Chapter 7). Employers filling positions that require contact with customers and handling of cash are less receptive than those with jobs not requiring these tasks (Stoll et al., 2004). Large firms are more willing than small firms to hire someone with a criminal record (although the latter firms are less likely to conduct criminal background checks); manufacturing firms are more likely to hire such an individual than those in finance, insurance, or real estate; and employers located in the central city are more willing to do so than those in the suburbs (Stoll et al., 2004, p. 219). Characteristics of the offense and program participation also appear to matter. In particular, violent and property crimes evoke more negative reactions than drug crimes, and employers appear to be responsive to evidence of rehabilitation, such as participation in a drug treatment program or transitional work (Holzer, 2007; Pager, 2007).

Survey research allows for the exploration of a wide range of considerations among employers. At the same time, the validity of survey research is dependent on the accuracy of respondents' reports. Although employers in general appear willing to express their honest views about employing individuals with a criminal record, some ambiguity remains in determining how employer attitudes correspond to subsequent actions (Pager and Quillian, 2005). Employer attitudes are only one factor shaping hiring decisions, and abstract survey responses do not take into account other relevant considerations, such as those related to the available labor pool, the number of vacancies, and the process of conducting a criminal background check.

Nevertheless, it appears that given the choice, employers would consistently prefer to avoid hiring individuals with a criminal record.

It is worth noting that the legal system can be hard on employers, holding them liable for acts of their employees under "negligent hiring laws," which could shape hiring decisions. Complicating matters, existing laws can impose contradictory expectations, with negligent hiring laws and fair employment laws often working at cross purposes (Watstein, 2009; Pager, 2007). Little is known about how the legal context shapes employer behavior with respect to applicants with a criminal record, and case law points to rather ambiguous patterns (e.g., Mukamal, 2003). Some states have recently adopted laws, in conjunction with other reforms, designed to limit the liability of employers that hire people with a criminal record (Rodriguez et al., 2011). More research is needed to understand what specific concerns underlie employers' reluctance to hire such individuals.

Ethnographic and Other Qualitative Studies

Researchers who have studied firsthand the experiences of individuals released from prison have consistently documented the range of hardships facing those seeking employment (Sullivan, 1989; Duneier, 1999; Anderson, 1999; Goffman, 2009; Gonnerman, 2004). In a study of black men in Chicago, Young (2003, p. 95) concludes, "Nothing created as great a stigma for them than the possession of a criminal record. Each knew very well that a record was a severe detriment to finding work." Sullivan (1989, p. 69) documents some of the concrete experiences in which employment difficulties appeared to follow directly from criminal justice involvement. "Gaspar Cruz lost one job that he had held for a year after his employer found out that he had been in jail. . . . Miguel Tirado lost four different jobs in the course of a six-month period during which he had to make weekly court appearances. He did not want to tell his employers that he had to go to court and could not otherwise explain his absences." Sullivan also finds that individuals who had assistance through employment services or personal networks were more likely to obtain a job than those without such help.

These cases illustrate the stigma and subsequent disruption associated with incarceration, as well as the importance of the reentry context. Eli Anderson's (1999, p. 244) research illustrates the psychological toll exacted by such experiences. He describes John Turner, a young man whose initially minor contact with the criminal justice system triggered a sequence of adverse events. "After John had finished completing the successive weekends in jail, there was no job waiting for him. He then looked for a new job, without success, for many weeks. The places where he inquired told him they needed no help or that they would call him—which they never did. As his best efforts repeatedly proved unsuccessful, he became increasingly

demoralized" (see also Harding, 2003). These accounts suggest a possible negative feedback cycle through which repeated encounters with rejection may lead to cynicism and withdrawal from formal labor market activity (see also Black, 2009).

Ethnographic studies offer a detailed view of the complex pathways that lead from prison to home. However, this complexity makes it difficult to draw simple conclusions about the net impact of any single factor. As Goffman's (2009) recent study shows—criminal justice entanglements can sometimes become an excuse invoked by young men to relieve themselves of the responsibility of getting a job, regardless of whether they could have done so absent the criminal record. Yet even without a clear causal story, the pervasive finding that respondents *perceive* their record to be a significant impediment to finding work is important in its own right. To the extent that individuals become discouraged in their search for work or avoid formal employment opportunities preemptively, real distortions in labor market outcomes based on these supply-side responses may occur.

Experimental Approaches to Studying Criminal Stigma

With experimental methods, researchers can control for nonrandom selection into a treatment group (e.g., incarceration) to isolate causal pathways. Several studies have used experiments to examine the labeling effects of incarceration on employment decisions. Much of the rigorous work accomplished to date was inspired by a classic study by Schwartz and Skolnick (1962) in which researchers prepared four fictitious resumes to present to prospective employers for an unskilled hotel job. Three of the four résumés reflected varying levels of criminal justice contact related to an assault charge, ranging from conviction to arrest to acquittal; the fourth résumé reflected no criminal record. Each of the applicants with a criminal record was less likely to be considered for the job relative to the noncriminal control, even when the individual had subsequently been cleared of any wrongdoing. Although the severity of the criminal record mattered, these results suggest that mere contact with the criminal justice system can have serious negative effects on employment.

Several later studies have formalized and extended Schwartz and Skolnick's design, varying the types of crimes committed by the hypothetical applicants or the national context (Finn and Fontaine, 1985; Boshier and Johnson, 1974; Buikhuisen and Dijksterhuis, 1971). Most recently, Pager (2003) and Pager and colleagues (2009b) conducted a series of experimental in-person audit studies of entry-level jobs in Milwaukee and New York City, respectively. In these studies, résumés reflecting equivalent schooling and work histories were assigned to pairs of trained testers, with one tester in the pair receiving a criminal record condition; the member of

each pair receiving this condition alternated each week. The results from both cities indicate that employers strongly disfavored job seekers with a criminal record (with reductions in callbacks of 30-60 percent), the penalty of a criminal record being especially large for blacks.[3]

Beyond the effect of a criminal record in these studies, the direct effect of race also loomed large. In both Milwaukee and New York, blacks with a clean record experienced callback rates similar to those of whites with a felony conviction. Some have argued that contemporary racial discrimination can in part be explained by employers' concerns about crime, with race being used as a proxy for criminality (Pager, 2007; Alexander, 2012). Holzer and colleagues (2006) suggest that employers who are reluctant to hire those with a criminal record and who conduct criminal background checks are more likely to hire blacks than those who do not conduct background checks (who may instead engage in statistical discrimination) (see also Bushway, 2004). To the extent that the growth of incarceration and racial disproportionality therein may contribute to perceptions of widespread criminality among young black men, the estimated impact of incarceration on employment may be understated. In this case, the rise of incarceration may have consequences above and beyond its individual-level effects. Pervasive contact with the criminal justice system at today's scale has consequences for racial stratification that extend well beyond individuals behind bars.

Experimental studies offer a rigorous measure of causality, eliminating many of the problems of selection endemic to observational research. At the same time, experiments have their limitations. For one thing, the applicant profile used in experiments cannot capture the diversity of characteristics represented among the ex-prisoner population; estimates then may be generalizable only to those of the chosen profile (e.g., conviction type, age, education level). Further, experiments rely on samples of help-wanted ads and direct application procedures. To the extent that ex-prisoners find work through networks or intermediaries, audit studies may overestimate the barriers they encounter in the open labor market. Likewise, job applicants with a criminal record may apply to systematically different kinds of jobs from those that are audited, further limiting the external validity of the results. Despite these limitations, field experiments provide compelling evidence that, under specific conditions, the stigma of a criminal record is substantial for those seeking employment.

[3]In Milwaukee, whites with no criminal record received callbacks 30 percent of the time, compared with 17 percent for whites with a criminal record, 14 percent for blacks with no criminal record, and 5 percent for blacks with a criminal record. In New York City, whites with no criminal record received callbacks or job offers 31 percent of the time, compared with 22 percent for whites with a criminal record, 25 percent for blacks with no criminal record, and 10 percent for blacks with a criminal record.

Analysis of Survey Data

It may be possible to address limits on the external validity of audit studies by analyzing surveys drawn from the population of workers with an incarceration record. Using survey results to analyze the labor market effects of incarceration is challenging because few data collections follow people in and out of institutional settings. Panel data from the 1979 and 1997 cohorts of the National Longitudinal Survey of Youth (NLSY) are exceptional because interviews are conducted with respondents if they become incarcerated. Likewise, the Fragile Families Survey of Child Well-Being, a panel survey of mainly low-income parents in urban areas, began interviewing incarcerated male respondents a few years after the first-round survey was fielded. Information on the incarceration status of survey respondents also has been collected indirectly, with retrospective reports for the NLSY cohorts, Fragile Families, and the National Youth Survey and with an item for survey nonresponse in the Panel Study of Income Dynamics, which records incarceration as a reason for noninterview.

Besides the possibility of population inference, survey data provide detailed measurement of labor market outcomes. Respondents often are asked about their current employment status, earnings, occupation, hours worked, job tenure, and multiple job holding. Measures of earnings are not confined to those on which taxes and unemployment insurance contributions are assessed.

Against these advantages, survey data have two main limitations. First, surveys may measure criminal justice system involvement imprecisely or with error. Questions about prior incarceration in the NLSY79 and the Fragile Families Survey, for example, do not obtain information about the timing of incarceration, preventing estimation of the pre/postincarceration difference in earning. Survey respondents also may be unwilling to report prior incarceration, and some may count short terms of jail incarceration while others count only imprisonment. Problems of measurement can be overcome when incarceration is directly observed, with survey interviews conducted in prison being recorded. This direct observation of incarceration has been used in a number of studies of wages and family income (e.g., Western, 2002; Raphael, 2007; Geller et al., 2011).

The second important limitation of survey data is that sample sizes for incarcerated respondents are relatively small. In the NLSY79, around 7 percent of approximately 5,000 male respondents reported incarceration or were interviewed in prison at some point before age 40. With such small samples, analyses of population subsets, say, by race or age, will have low statistical power.

Data limitations aside, all observational studies—survey, qualitative, and administrative—pose the problem of nonrandom selection into

incarceration. Survey studies have dealt with this threat to causal inference in three main ways. First, to adjust for selection on observables, survey analysis has controlled for variables that are unusual in standard labor market studies but useful for the estimation of incarceration effects. Measures of drinking, smoking, drug use, aggressive or impulsive personality, juvenile incarceration and criminal involvement, and domestic violence all have been used as controls in regression analyses of employment and income. Second, panel data studies can control for all time-invariant factors correlated with incarceration and earnings with individual-level fixed effects. Finally, Lalonde's (1986) classic study of the National Supported Work Demonstration shows that treatment effects estimated with observational data can be aligned with experimental results when analysis is restricted to subsets of the population likely to receive the treatment. Researchers have thus analyzed subsets of crime-involved survey respondents instead of entire samples (Western, 2006, Chapter 5; Grogger, 1995).

Each of these strategies defines different comparison groups for the estimation of incarceration's consequences. Regression adjustment compares formerly incarcerated workers with observably similar workers who have not been incarcerated. Fixed effects analysis removes cross-sectional variation and compares the formerly incarcerated with themselves prior to incarceration. Sample restrictions compare the formerly incarcerated with those who are not incarcerated but observably at risk of incarceration, including those who may later be incarcerated.

Table 8-1 summarizes several studies of survey data. The survey studies consistently indicate reductions in employment, wages, and annual incomes associated with incarceration. The findings suggest that employment declines 10 to 20 percent after incarceration. A similar incarceration consequence is estimated for hourly wages. In one analysis of the 1979 cohort of the NLSY, annual incomes are estimated to fall by more than 30 percent (Western, 2006). In the general population, wages grow strongly with age at least until the mid-40s. Formerly incarcerated respondents in the NLSY exhibit very little earnings growth, a pattern clearly evident in the administrative data as well.

Use of Administrative Data

While survey studies are relatively rare, a number of researchers have analyzed administrative data linking court or correctional records to earnings data obtained from state unemployment insurance (UI) systems. Analyzing administrative records enables the collection of very large samples, often with thousands of formerly incarcerated workers. Administrative data also can provide detailed information about the court process and sentencing. Thus studies have been able to distinguish prison from jail incarceration

TABLE 8-1 Analyses of the Labor Market Effects of Incarceration Using Survey Data

Study	Data Source	Outcome Studied	Negative Effect?
Freeman (1992)	NLSY79	Annual employment	Yes
	BYS	Current employment	Yes
	ICY	Current employment	Yes
Western (2006)	NLSY79	Hourly wages	Yes
		Annual employment	Yes
		Annual earnings	Yes
Raphael (2007)	NLSY79	Annual employment	Yes
		Hourly wages	Mixed
Apel and Sweeten (2010)	NLSY97	Annual employment	Yes
		Hourly wages	No

NOTE: BYS = 1989 Boston Youth Survey; ICY = 1979-1980 Inner City Youth Survey; NLSY = National Longitudinal Survey of Youth.

(Grogger, 1995), and the random assignment of judges to cases has been a source of exogenous variation used to identify incarceration effects through an instrumental variables approach (Kling, 2006; Loeffler, 2012).

Despite their advantages, administrative data suffer from three main limitations. First, court and correctional records used for analysis often include relatively little covariate information. Detailed controls for such factors as schooling, work experience, juvenile criminal involvement, or cognitive ability that are available in survey data are rarely included in administrative records.

Second, most administrative studies are based on linkage of court and UI records. Record linkage generally is based on names, social security numbers, and dates of birth. These identifiers can be unreliable in court records for a population for whom varying identifying information is presented. Some of the difficulty posed by the quality of identifiers for record linkage is reflected in the match rate between criminal records and UI earnings histories. Match rates of around 60 to 70 percent are common, so earnings for 30 to 40 percent of the administrative samples remain unobserved.[4] Little is known about the biases that might be introduced by match failure, although it appears unlikely to be randomly distributed.

[4]As discussed further below, these poor match rates are likely due to some combination of inferior data quality and the prevalence of sample members who have not worked in UI-covered employment.

The third difficulty posed by administrative studies concerns the measurement of earnings with UI records. If the formerly incarcerated are working in non-UI-covered (e.g., informal or casual) or interstate jobs, they will not be included in UI records. Indeed, a longitudinal study of ex-prisoners in three states found that 8 months after release, respondents were more likely to have received income from informal work (47 percent) than from formal employment (41 percent) (Visher et al., 2011). Likewise, analyzing a sample of male youth with a prior arrest, Kornfeld and Bloom (1999) show that UI earnings are 70 to 100 percent lower than self-reported earnings. Holzer (2009, p. 252) similarly observes "that the very low quarterly employment rates [in UI data] are dramatically lower than those found in any of the NLSY studies or in any other survey of those incarcerated." Holzer (2009) goes on to note that the direction of bias will depend on the incidence of underreporting before and after incarceration. If younger (preincarceration) workers are more involved in casual employment and postincarceration workers are more involved in the formal labor market while on parole or subject to child support obligations, the post-pre difference will be biased upwards (i.e., will lead to the inaccurate conclusion that incarceration is associated with higher rates of employment). Indeed, in a study by Piehl (2009, p. 8), individuals in a prerelease program in Maryland referred to the onerous requirement that they participate in formal employment despite more lucrative opportunities to be found in informal (non-UI-covered) work. To the extent that the formal requirements of parole induce movement from informal to formal employment, pre/postincarceration estimates of employment based on administrative records may overstate the relative employment successes of those recently released from prison. In summary, the limitations of administrative data may contribute to both noise and a positive bias in the estimation of incarceration's effects on labor market outcomes.

Despite these challenges, several studies yield evidence of the negative effects of incarceration on quarterly employment and earnings (see Table 8-2). Grogger (1995) and Waldfogel (1994) both report reductions in employment of around 5 percent. These two studies also report similar earnings losses of 10 to 30 percent. Grogger's (1995) analysis is able to distinguish the effects of arrest, probation, jail incarceration, and imprisonment. Six quarters after admission to incarceration, Grogger (1995) reports large negative effects of jail and prison on earnings and employment. The imprisonment effect may reflect time out of the labor market due to incarceration, although jail terms typically are shorter than a year, so low

TABLE 8-2 Analyses of the Labor Market Effects of Incarceration Using Administrative Data

Study	Data Source	Outcome Studied	Negative Effect?
Waldfogel (1994)	Probation reports	Monthly income	Yes
		Monthly employment	Yes
Grogger (1995)	UI and court (California)	Quarterly UI employment	Yes
		UI quarterly earnings	Yes
Kling (2006)	UI and court records (California and Florida)	Quarterly UI employment	No
		UI quarterly earnings	No
		UI poverty	No
Sabol (2007)	UI and DOC (Ohio)	Quarterly UI employment	Mixed
Pettit and Lyons (2007)	UI and DOC (Washington)	Quarterly UI employment	Mixed
Lalonde and Cho (2008)*	UI and DOC (Illinois)	Quarterly UI employment	No
Loeffler (2012)	UI and jail (Cook County, Illinois)	Quarterly UI employment	No

NOTE: DOC = department of corrections; UI = unemployment insurance.
*Analysis is confined to women enrolled in welfare and social service programs.

employment and earnings after a year are interpreted as postrelease effects.[5] Negative long-term effects of imprisonment also were found in a sample of prisoners in Washington State (Pettit and Lyons, 2007) and in Ohio (Sabol, 2007). Characteristic of recent studies using administrative data, Pettit and Lyons (2007) and Sabol (2007) also find small increases in UI employment immediately after release compared with preincarceration levels, perhaps reflecting the necessity of formal-sector employment as a condition of parole (see, e.g., Piehl, 2009, p. 8).

Several recent studies of administrative data provide much weaker evidence for the negative effects of incarceration on labor market outcomes. In

[5]Note that Grogger was unable to observe actual release dates, and thus some estimates may be biased downward (away from zero) because of continued incarceration. Estimates for jail populations are less affected by this concern as it is uncommon for inmates to remain in jail longer than 1 year.

particular, studies by Kling (2006), Loeffler (2012), and Lalonde and Cho (2008) all suggest that incarceration has no significant negative effect on these outcomes. In contrast to much of the research comparing those who have and have not been incarcerated, Kling (2006) studies the effects of the length of incarceration among those serving time in prison. Analyzing UI data for federal prisoners in California and state prisoners in Florida, he finds small short-term gains in UI employment and earnings for those serving longer sentences. Kling (2006, pp. 873-874) speculates that additional programming, particularly work releases, may be associated with improved employment for those serving longer sentences. He also provides a set of analyses in which the causal effect of incarceration is identified by comparing otherwise similar individuals who received shorter or longer prison sentences as a result of the relative leniency of the judges to which they were randomly assigned (an instrumental variables approach). The instrumental variables point estimates also indicate a positive (although insignificant) effect of incarceration on employment.

Analyzing data from Cook County, Illinois, Loeffler (2012) also uses the random assignment of judges to identify the effect of incarceration. Similar to Kling's (2006) results, Loeffler's instrumental variables estimates have standard errors about 10 times larger than least-squares standard errors, and none of the Cook County incarceration effects is significant. Instrumental variables estimates are sometimes taken as a gold standard, although these assessments never consider the cost of bias reduction in additional variance. Loeffler's employment data come from administrative UI records, representing a 67 percent match rate for his sample. As with other analyses based on UI data, positive effects may reflect the short-term impact of parole supervision for the subset of ex-inmates who are moved into formal employment. Little is known about the work activities of the 33 percent of the sample not identified by UI records, or about the distribution of non-UI-covered work before and after the relevant periods of supervision.

Finally, Lalonde and Cho (2008) analyze linked incarceration and UI records for a sample of welfare-enrolled women in Illinois state prison in the 1990s. Examining employment dynamics surrounding incarceration, they find that quarterly UI employment is above preprison levels for three quarters after incarceration. They find that the positive effects of prison on employment are largest for women with children. This increase in employment may be due to greater financial need experienced by those with dependants. The positive employment consequence may also be confounded with 1996 welfare reform that imposed more stringent work requirements on poor single women with children applying for welfare.

Discussion

The balance of quantitative results points to the negative consequences of incarceration for employment. These consequences have been found in survey data (Freeman, 1992; Western, 2006; Raphael, 2007) and administrative data (Waldfogel, 1994; Grogger, 1995; Pettit and Lyons, 2007; Sabol, 2007) with a reasonably stringent variety of specifications fitting individual fixed effects. A few recent studies of administrative data find no negative effect, as well as short-term positive effects that may reflect increased formal-sector employment while under community supervision and shifting institutional conditions associated with welfare reform.

Studies of the labor market experiences of people released from incarceration have adopted a wide variety of methods, examined both the supply and demand sides of the labor market, and analyzed different kinds of qualitative and quantitative data in a wide range of times and places. With such heterogeneity in research designs, it is not surprising that the many different findings of these studies fail to point in a single direction. Still, several general conclusions can be drawn, and specific areas in which more research is needed can be identified.

Studies employing a variety of methods point to similar conclusions. Audit studies of employers and analyses of survey data on formerly incarcerated respondents consistently show the negative effect of incarceration on employment. Survey analyses also show the negative effect on earnings and, less consistently, on hourly wages. The estimated effects in these studies often are substantively large, with reductions in employment outcomes of 10 to 30 percent. Audit studies and survey analyses also are able to impose strong controls for selection—experimentally in the case of audit studies and through a combination of fixed effects, regression adjustment, and sample restrictions in the case of survey analyses. This evidence for the negative effect of incarceration is buttressed by qualitative research, employer surveys, and some but not all studies of administrative data.

Yet a few recent administrative studies reject the hypothesis of a negative incarceration effect and show small increases in employment immediately after release. An urgent question here concerns the utility of administrative data for describing the employment experiences of the formerly incarcerated. The nonrandom observation of employment and earnings in administrative data produced by incomplete record linkage and incomplete UI coverage of postrelease work likely introduces bias, although the direction and magnitude of that bias are poorly understood.

Aggregate Studies

Looking beyond the consequences for individuals, researchers have asked whether the impact of incarceration on labor market outcomes is sufficiently large to result in distortions in aggregate patterns of employment. As the numbers of incarcerated individuals have grown exponentially, it has become important to consider such macro-level consequences. Two lines of research have studied the aggregate significance of incarceration for the labor market: the first views incarceration as a source of "invisible inequality" unmeasured by standard methods such as household surveys; the second assesses the effects of incarceration on aggregate outcomes such as the unemployment rate for different segments of the labor market.

The hypothesis of invisible inequality claims that large institutionalized populations significantly distort official measures of employment based on household surveys (Western and Beckett, 1999; Western and Pettit, 2005). The simplest analyses have compared employment-to-population ratios based on household surveys with adjusted employment ratios that include prison and jail inmates in the population count. Note that these analyses make no claim about the causal effect of incarceration. They simply aim to measure population counts more accurately than the usual household surveys, yielding more accurate estimates of labor utilization and economic well-being in the population. When inmates are included in the denominator of these employment ratios, conventional statistics, particularly for low-educated black men, are altered substantially. According to Western and Pettit (2010, p. 12), "Conventional estimates of the employment rate show that by 2008, around 40 percent of African American male dropouts were employed. . . . Once prison and jail inmates are included in the population count (and among the jobless), employment among young African American men with little schooling fell to around 25 percent by 2008. Indeed, by 2008 these men were more likely to be locked up than employed." Official statistics based on household surveys thus overlook the degree to which contemporary employment patterns are affected by high rates of incarceration.

The second line of research on aggregate effects examines whether the micro-level effects of incarceration accumulate to produce large effects on aggregate measures such as the unemployment rate. Holzer and colleagues (2005) thus examine the aggregate relationship between incarceration and employment among young black men, but in this case focus on the longer-term impact of those reentering society after incarceration. Using data from the outgoing rotation groups of the Current Population Survey between 1979 and 2000 and controlling for a range of state-level social and demographic characteristics with state and year dummies, the authors find that a

1 percent rise in incarceration of blacks (lagged by 3 years to match typical reentry flows) is associated with a roughly 1 percent decline in employment and labor force participation among young black men. Controlling for similar patterns among whites—using a difference-in-difference approach in which effects for whites are attributed to omitted variables—yields comparable results (see also Freeman and Rodgers, 1999).

Sabol and Lynch (2003) use county-level data to estimate the aggregate effects of changes in incarceration on family structure and employment. Their instrumental variables approach relies on an indicator for states that did or did not adopt mandatory or determinate sentencing laws during the 1980s. The sentencing variables are correlated with imprisonment, but not related to changes in family structure or employment. Using data from 280 counties in 96 metropolitan areas, the authors find that the annual number of male prisoners released back into the county from which they were sentenced is negatively related to employment levels among black men; the effects for whites are not significant. Change models show similar effects, but with state fixed effects, these results are not significant.

Useem and Piehl (2008) rely on aggregate data to investigate both the short-term effects of incarceration on employment via population removal and the longer-term consequences for reentry. Based on analyses that include state-level incarceration rates (lagged at multiple intervals) with state and year fixed or random effects, their results yield smaller estimates than those of previous researchers, suggesting that the aggregate impact of incarceration may be less than previously indicated. One key difference among estimates is that the Useem-Piehl analyses focus on employment outcomes for men of all ages, whereas the effects of incarceration may be concentrated among young men.

Overall, these aggregate analyses generate mixed findings about the overall relationship between incarceration and employment: while the general aggregate association between incarceration and employment may be quite weak, this relationship appears more substantial among prime-age men, and particularly black men with no college education. Perhaps not surprisingly, these results suggest that labor market distortions are likely to be greatest among those demographic groups most affected by the high levels of incarceration (e.g., young black men). Of course, aggregated data present only a coarse view of this relationship, and state-level data in particular may obscure important within-state heterogeneity. Paired with the individual-level analyses discussed above—pointing to employment and wage penalties experienced by ex-prisoners, in some cases more so for blacks—these results help tell a fuller story about the negative impact of incarceration on labor market outcomes.

PROGRAMS AND POLICIES FOR IMPROVING EMPLOYMENT OUTCOMES

The challenge of improving the employment outcomes of ex-prisoners is in many ways similar to that of improving the employment outcomes of all low-skilled men. Spotty work histories, low education, and poor social capital make the transition to stable employment difficult. For a thorough review of the challenges and possibilities of increasing employment among poor men, see Mead (2011). In this section, we briefly review the state of the evidence on reentry programs that focus on employment and then discuss recent efforts to limit employer access to criminal record information as a strategy for increasing the employment opportunities of those released from incarceration.

Employment Reentry Programs

Western (2008, pp. 10-13) categorizes four types of programs aimed at increasing employment and reducing recidivism "among people with criminal records: (1) transitional employment programs; (2) residential and training programs for disadvantaged youth; (3) prison work and education programs; and (4) income supplements for the unemployed." Evaluations of such programs are limited, but a growing body of findings indicates that intensive and directive interventions, provided immediately after release, are more likely to be successful than less concerted services.

Transitional employment programs provide temporary subsidized work to those released from prison. Participants often work in small groups with a high level of supervision to support the development of behaviors useful for permanent employment. They also have access to job placement services. Several of these programs have been tested empirically. A study of the National Supported Work (NSW) demonstration, carried out during the 1970s, found positive employment effects as well as reduced recidivism for older participants but not for those younger than age 26. According to Western (2008, p. 10), the "early randomized experiment . . . placed parolees and probationers in construction industry jobs. Three years after entry to the program, about 42 percent of NSW clients over the age of twenty-six had been rearrested, compared with 54 percent in the control group (Uggen, 2000). NSW participants over age twenty-six were also less likely to report illegal earnings. There were no significant differences between program and control groups among those aged twenty-six and younger."

A 3-year evaluation of the Center for Employment Opportunities (CEO) in New York City, using random assignment, found that the effects of increased employment early in the follow-up period declined over time. At the first-year follow-up, there was little difference in employment and

earnings between the program and control groups. However, the evaluation found that CEO significantly reduced recidivism, especially for participants that enrolled within 3 months of being released from prison. The researchers also conducted a cost-benefit analysis and determined that because of reduced criminal justice system expenditures, the financial benefits exceeded the costs (Redcross et al., 2012). Since the evaluation, CEO has improved its program to address low job retention after participants leave the program. According to Mead (2011, p. 62), "CEO created a retention unit to follow up with clients on the job and address problems there. It also has instituted Rapid Rewards, a bonus of up to $200 a year . . . for achieving certain milestones in job retention. The program claims to have improved its job retention rate at 180 days after placement from 40 to 60 percent."

Another program in New York, the ComALERT Program, provides substance abuse treatment with subsidized employment and housing. An evaluation of this program found that "participation was associated with significant improvements in employment and a 18 percent reduction in arrest rates compared to a matched control group with similar demographics and criminal history" (Jacobs and Western, 2007; Western, 2008, p. 10).

The Transitional Jobs Reentry Demonstration (TJRD), an evaluation using rigorous random assignment, tested transitional employment programs for former prisoners in four cites (Chicago, Detroit, Milwaukee, and St. Paul) (Jacobs, 2012). It was designed to examine whether transitional employment programs were more effective than less costly options—those that provided services for job search, referral, and placement but no subsidized employment. The study found that at first-year follow-up, during a period of recession and increased unemployment rates in the four cities, early employment gains for the program group faded as transitional jobs ended, and in contrast with CEO, there was no difference in recidivism between the program and control groups, although the researchers note that most subsequent "prison admissions were for violations of parole rules, not new crimes" (Redcross et al., 2010, p. iii). The results of this study support findings of earlier evaluations that transitional employment programs have yet to be effective at helping former prisoners obtain and hold permanent jobs. However, participation in the transitional jobs is generally high, suggesting that former prisoners welcome the income and the opportunity to work.

Residential and training programs for disadvantaged youth target a specific population and offer services, often combined, such as housing, drug treatment, education, and job training. According to Western (2008, p. 13), the "Opportunity to Succeed (OPTS) program (1994-97) provided mandatory substance-abuse treatment in intensive residential placements, as well as job readiness training. A year after random assignment, the treatment group had accumulated an extra month of full-time employment and

were 9 percent more likely to have held a full-time job. Recidivism was also modestly lower in the treatment group, although the program effect was not significant" (Rossman et al., 1999).

Job Corps, established by the Economic Opportunity Act of 1964 and operated now under the Workforce Investment Act of 1998, is a national program targeting economically disadvantaged youth aged 16 to 24. A comprehensive experimental evaluation, using random assignment from 1994 to 1996 and data collection covering a 48-month period after assignment, found that Job Corps improves outcomes for participants—with large effects in obtainment of general equivalency diplomas (GEDs) and vocational certificates and increased earnings for disadvantaged youth—but at a high financial cost (Schochet et al., 2008). The researchers conclude that for older participants, the benefits appear to offset the costs and note that across the full sample, "benefits exceed costs for the participants themselves [thus] effectively redistribut[ing] resources toward low-income youth" (Schochet et al., 2008, p. 1883). The evaluation found employment and earnings gains across a number of subgroups of participants, including those who had been arrested for less serious crimes[6] prior to participation. The study examined the program's impact on crime and found that the program group was significantly less likely than the control group to be arrested, particularly for less serious crimes, during the first 2 years of follow-up. The program group also was less likely to be convicted and spend time in jail.

More conventional vocational training is provided in a nonresidential setting under the Job Training Partnership Act (now called the Workforce Investment Act). An earlier evaluation of this program found no effect on earnings and rearrest rates among the male participants with prior arrest records (Bloom et al., 1997), which may be because the program is not intensively directive, having a focus on general job skills aimed largely at adults.

A recent study examined training programs that target industry-specific needs for a given area to prepare disadvantaged populations to fill local positions and connect them with employers (Maguire et al., 2010). A random assignment research design was used to study three such "sectoral employment" programs in Boston, Milwaukee, and New York City. Seventeen percent of the adult sample (N = 1014) had been formerly incarcerated. The 2-year evaluation found that the nonprofit-led sector-focused training programs did increase the earnings of the program group compared with the control group across a range of program participants. Notably, at two

[6]Disorderly conduct and trespassing were considered less serious crimes. Serious crimes included aggravated assault, burglary, murder, and robbery (Schochet et al., 2008).

of the three program sites, formerly incarcerated individuals showed significant earnings gains.

Another intensive residential program for disadvantaged youth—the National Guard Youth ChalleNGe Program—was evaluated with a random assignment design (Millenky et al., 2011). This program combines a 20-week quasimilitary residential phase with a 1-year postresidential-supported mentoring phase. Researchers surveyed the sample of 1,200 young people 3 years after the start of the study (or about 1.5 years after the end of the 17-month ChalleNGe Program) and found that the program group was more likely than the control group to have obtained education credentials (GED, high school diploma, or college credits) and to be employed, earning about 20 percent more than the control group. The evaluation found no statistically significant difference between the groups in whether they had been arrested (about 50 percent of both groups) or convicted (about 25 percent) by the 3-year point. The study did find significant effects on arrests or convictions after 1 or 2 years, but the effects were not significant after 3 years (Bloom et al., 2009; Millenky et al., 2010). The program is open only to youth aged 16 to 18 who are drug free and not "heavily" involved with the justice system,[7] so the above findings may not apply to youth who have substance abuse problems or have committed serious crimes and/or been in contact with the adult criminal justice system.

Prison work and education programs, offered by nearly all state and federal prison systems, are provided to prisoners either throughout their sentence or just prior to release. These programs serve multiple purposes, such as addressing deficient education and skills, reducing idleness, and providing social benefits. As discussed in Chapter 6, it is possible with available statistics to estimate how many prisons offer such programs and how many prisoners participate, but much is unknown about correctional education and employment, such as the quality and the level of engagement and completion. Unfortunately, most studies compare those who participated in such programs with those who did not, thus biasing the findings toward those who self-select to participate, and little information is collected about the characteristics of participants in comparison with those that do not participate. Nonetheless, a number of recent meta-analyses and reviews conclude that programs such as basic education, GED, postsecondary education, and vocational training can be cost-effective, have lowered the risk of recidivism, and hold promise for increasing future employment, while correctional industries and work programs are less or not effective (Cecil et al., 2000; Davis et al., 2013; Fabelo, 2002; Gerber and Fritsch,

[7]Those "heavily" involved with the justice system included those currently on parole or on probation for anything other than juvenile status offenses, serving time or awaiting sentencing, under indictment or charged, or convicted of a felony or a capital offense.

1995; MacKenzie, 2006, 2012; Steurer et al., 2001; Western, 2008; Wilson et al., 1999, 2000).

Western (2008, p. 13) points to three large-scale studies whose findings indicate the beneficial effects of prison education:

> The PREP study (1983-85) found that participation in vocational training and work programs was associated with reduced rates of reincarceration in federal prison as long as twelve years after release (Saylor and Gaes, 1997). The Three-State Recidivism Study (1997-98), named for study groups in Maryland, Minnesota, and Ohio, examined a variety of educational programs including basic education, GED preparation, and secondary and postsecondary schooling. Although the study does not distinguish the effects of different types of educational programs, those who participated in classes in prison were only 48 percent likely to be rearrested after a year, compared with a 57 percent rearrest rate for the comparison group (Steurer et al., 2001). Program participants had higher earnings in the first year after release, but this earnings advantage disappeared after three years. Similar to the Three-State Recidivism Study, the Florida GED study (1994-99) found no enduring gains to earnings or employment for those who obtained a GED in prison. Still, some immediate improvements in earnings were found, particularly for nonwhite GED holders (Tyler and Kling, 2007).

It is difficult to draw strong conclusions about the impact of prison work and education programs on later criminal behavior and employment because participants often are involved in other institutional services. Thus, the possibility that the impact is due to a combination of factors and not the program alone cannot be ruled out. Does the link between education and training and recidivism depend only on cognitive and/or skill change or some combination of such change and increased opportunities for additional schooling or employment in the community? Strong arguments have been made about the importance of gender- and race/ethnicity-sensitive programming. However, evidence is insufficient to know whether such "responsive" programming would increase academic progress, be more effective in reducing recidivism, or assist in employment success. Work programs face additional challenges if the institutional goals of producing products and maintaining facilities conflict with rehabilitation goals for work programs. Considering the large number of vocational and academic education, prison industry, and work programs, the research in this area is sparse and severely limited by flaws in the research methodology. More rigorous research using randomized trials would greatly increase knowledge of how to provide effective, evidence-based correctional education and work programs.

Finally, **income supplements** involve paying unemployment benefits to released prisoners to spur economic opportunities. Western (2008, p. 13) reviews early research on income supplements:

> Beginning in 1971, the Baltimore LIFE (Living Insurance for Ex-Prisoners) experiment (1972-74) randomly allocated released state prisoners to a thirteen-week treatment consisting of weekly $252 payments and job placement in some cases, while a control group received no treatment. After twelve months, 49.5 percent of the treatment group had been rearrested compared with 56.9 percent of the controls (Mallar and Thornton, 1978). The LIFE program was replicated on a larger scale in Texas and Georgia in the TARP (Transitional Aid for Released Prisoners) experiment (1975-77). The TARP participants had higher rates of unemployment than the control group, however, and were no less likely to recidivate (Rossi et al., 1980).

Limits on Access to Criminal Records

Beyond programs that directly intervene in the job readiness or placement of ex-prisoners, one final approach to improving their employment outcomes is to reduce the labeling consequences of their criminal record. "Ban the Box" campaigns have received popular support in recent years, promoting policies that limit employers' exposure to criminal background information until later in the hiring process. As of August 2013, Ban the Box legislation had been passed in more than 50 cities and counties.[8] In most cases, the legislation applies primarily to city employers, but it extends in some locations to vendors or private contractors doing business with the city. Unfortunately, no systematic evaluation of the impact of Ban the Box legislation has yet been conducted. Whether or how—through increased supply or demand—these policies affect the overall employment rates of ex-prisoners is currently unknown.

Efforts also have been made to regulate the dissemination of criminal record information. For example, the Criminal Offender Record Information (CORI) reform in Massachusetts prohibits the dissemination of misdemeanor records after 5 years from release from supervision or custody or after 10 years for felony convictions (except for murder, manslaughter,

[8]As of August 2013, Ban the Box legislation had been passed in Alameda County, Atlanta, Atlantic City, Austin, Baltimore, Berkeley, Boston, Bridgeport, Buffalo, Cambridge, Canton, Carrboro, Carson, Chicago, Cincinnati, Cleveland, Compton, Cumberland County, Detroit, Durham City, Durham County, East Palo Alto, Hartford, Jacksonville, Kalamazoo, Kansas City, Memphis, Minneapolis, Multnomah County, Muskegon County, New Haven, New York, Newark, Newport News, Norfolk, Norwich, Oakland, Philadelphia, Pittsburgh, Portsmouth, Providence, Richmond, San Francisco, Santa Clara, Seattle, Spring Lake, St. Paul, Tampa, Travis County, Washington, DC, Wilmington, and Worcester (National Employment Law Project, 2013).

or sex offenses). This legislation goes well beyond the provisions in most states. While most states make some provision for the sealing or expungement of records (Love, 2006), particularly in the case of nonviolent or first-time offenses, credit reporting agencies and criminal background services typically are subject to little oversight in distributing this information. At present, there exists no federal legislation comparable to the Fair Credit Reporting Act, which prohibits credit agencies from disseminating information dating back more than 7 years. Again, little evidence is available with which to evaluate how the regulation of criminal record information at the state or federal levels may affect the employment rates of ex-prisoners. In considering policies of this kind, it is useful to keep in mind that the predictive value of a criminal record declines over time. Seven to 10 years following an arrest, the likelihood of arrest for young men with a record looks indistinguishable from that of those with no criminal history (Kurlycheck et al., 2006; Blumstein and Nakamura, 2009; see also Chapter 3). Thus there may be good reason to pursue policies that regulate the introduction of stigmatizing information beyond this window.

KNOWLEDGE GAPS

We offer the following observations regarding gaps in knowledge on the issues examined in this chapter and suggestions for addressing those gaps.

Directions for Future Research

Current research findings do not make it possible to distinguish among the effects of criminal behavior, criminal conviction, and the experience of incarceration as they relate to subsequent labor market experiences. A clearer understanding of the mechanisms by which criminal justice involvement leads to poor employment outcomes is critical for addressing the central policy concern of whether and to what extent reductions in or alternatives to incarceration can improve employment outcomes.

Research more explicitly comparing estimates derived from administrative data on employment and earnings with those derived from survey data would be useful. A better understanding is needed of the extent to which discrepancies across these literatures are the result of poor data matches, poor data coverage (e.g., of informal or casual employment), supervision effects, errors in self-reports, or something else.

The collection of longitudinal data tracking individuals before and after their contact with the criminal justice system is needed. Partnering with existing longitudinal studies (e.g., the Panel Study of Income Dynamics [PSID], AdHealth) would be a useful avenue to explore. Tracking

participation in the informal labor market would enable a better understanding of the continuum of economic activity that characterizes the survival strategies of ex-prisoners.

Labor Market Context

Attention to the broader labor market context is needed in examining the consequences of incarceration. Limited research has focused on the question of how tight or slack labor markets may affect the reentry experiences of individuals leaving prison. Going forward, it will be important to understand how macroeconomic conditions interact with criminal justice policy to produce observed labor market outcomes.

Little is known about how formal barriers and the experience of prison affect the job search strategies of former prisoners. Do the experiences of overcrowding, prison violence, the quality of health care, or prison staff interactions, for example, influence, whether positively or negatively, the trajectories of inmates following release? To what extent do the supply-side adaptations of ex-prisoners (e.g., search intensity, search strategy) produce distortions in their labor market outcomes? Both qualitative and quantitative research would be useful in understanding how perceived and real barriers to employment affect ex-prisoners' persistence and placement in the labor market.

Programs to Improve Employment and Other Outcomes

Large-scale, long-term, and experimental evaluations of in-prison education and therapeutic programming, job training, and job placement programs are critical for directing policy activity in this area. Policy development to date has been to some extent stymied by contradictory findings among relatively small-scale studies. Investment is needed in broader evaluations that can inform efforts to improve experiences during and after imprisonment. The interdependence between employment and other reentry outcomes also needs to be considered. The experimental evaluation of CEO's transitional employment program, for example, found that early placement in transitional work significantly reduced recidivism but had no measurable effect on longer-term employment. This puzzling finding warrants further research into the role of early intervention in the postrelease process, how short- and long-term employment may be linked, and how they in turn are linked to recidivism. Evaluations of programs that address the needs of ex-prisoners across multiple dimensions (e.g., housing, substance abuse, employment) may likewise better inform understanding of pathways to desistance than those focused on employment alone. Institutional aspects of work and reentry programs need to be examined. What

allowed CEO to generate impacts where NSW largely failed? How are such programs built and improved over time? Answering these questions will require field research, of which there has as yet been far too little.

CONCLUSION

Research to date on the employment and earnings consequences of incarceration has taken many forms, from employer surveys to ethnographic observation, survey research, analysis of administrative data, and studies of aggregate effects. Results across this broad field vary, but the bulk of the evidence supports the conclusion that incarceration is associated with poor employment outcomes. Employers express a reluctance to hire those with a criminal record, and field experiments confirm this reluctance in measures of real-world behavior. Individuals see having a criminal record as a significant barrier to employment, although little is known about how these perceptions may affect one's strategy or persistence in job search.

Evidence from survey research consistently finds a significant negative relationship between incarceration and employment, wages, and annual income. Studies using administrative data come to somewhat more mixed conclusions, with some recent work reporting a boost in employment immediately following incarceration. In most cases, however, that boost is short-lived, giving way to longer-term null or negative effects. The aggregate effects of incarceration, or of having a criminal record are difficult to detect for general populations, but appear significant for young black men. These results suggest both the direct consequences for the employment prospects of those returning from prison and the consequences for the broader population of young black men, who are viewed with suspicion in the labor market by virtue of membership in a high incarceration group.

As noted throughout this report, the incarcerated population in the United States disproportionately comprises individuals with low levels of schooling and histories of mental illness and substance abuse—generally poor human capital and "work readiness." These individuals also often have quite limited access to social networks that could yield jobs with high growth prospects. The experience of incarceration thus both reflects and exacerbates persistent labor market inequalities.

Considering the mechanisms by which incarceration affects employment is critical to our evaluation of potential policy alternatives. For example, if the negative consequences of incarceration for employment are due primarily to its "transformative" impact on the physical, psychological, or social well-being of inmates—through attrition of human capital, weakening of prosocial ties, or development of coping mechanisms incompatible with life on the outside—then substituting community supervision for prison would reduce those consequences. If, on the other hand, the consequences

of incarceration for employment are due primarily to the stigma of having serious criminal justice contact, policies reducing incarceration by assigning individuals to other formal sanctions (e.g., probation, fines, treatment) would do little to reduce this labeling effect. Many of these alternative sanctions also come with a permanent criminal record. To the extent that the employment consequences associated with incarceration are driven in part by the effect of a criminal record (with or without incarceration), policies aimed at mitigating these consequences must go beyond reducing the numbers of people behind bars.

The evaluation literature examining programs designed to improve postrelease employment outcomes yields a mixed record (see also reviews by Mead, 2011, Chapter 4; Bushway and Reuter, 2002; Bloom, 2006; and Visher et al., 2005). These programs vary greatly in their content and in their clients. Many appear to generate only short-term effects or effects that generalize only to subsets of the population. Less intensive interventions, such as the income supplements of TARP and the training under the Job Training Partnership Act, and interventions directed at male youth have been unsuccessful. More intensive interventions that are directive as to desired behavior tend to be more successful, particularly if they target adults who are known to be motivated to desist from crime. The results of CEO and ComALERT also suggest that timely interventions focused on the period immediately after prison release are likely to have a greater chance of success.

Most program evaluations conducted to date have focused on the reentry process. Less is known about the impact of policies designed to reduce the flow of individuals into prison, which may represent a more powerful mechanism through which to mitigate the negative consequences of incarceration for employment. To the extent that incarceration results in a decay of human capital, a disruption of family ties, or psychological/interpersonal adaptations not conducive to stable employment, programs or policies focused on front-end diversion may have greater scope for success. At the same time, as noted above, it is important to remember that the problems of criminal activity and a criminal record present their own difficulties for employment, even absent incarceration.