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IMPACT OF INCARCERATION ON EMPLOYMENT PROSPECTS

Robert Apel and Anke Ramakers

Introduction

At this point in history, incarceration represents unprecedented government intervention in the lives of millions of Americans, especially poor, undereducated, and minority citizens. Punishment scholars have introduced terms such as “mass imprisonment” (Garland, 2001) and the “prison boom” (Wakefield & Wildeman, 2013) to characterize the scale of incarceration and its growth in the last two generations. Although aggregate prison growth in the United States slowed in the 2000s and even reversed direction around 2008, the incarceration rate nevertheless remains at historic levels and will remain so for a much longer time to come.

One “collateral consequence” that has acquired sustained scholarly attention, especially in the last 15 years, is the employment barrier for formerly incarcerated individuals. A number of excellent reviews of the incarceration–employment relationship already exist and are essential reading for scholars in this tradition (Uggen, Wakefield, & Western, 2005; Wakefield & Uggen, 2010; Western, Kling, & Weiman, 2001; Wildeman & Muller, 2012). Our objective in this chapter is to consider this research in quite a bit more detail than has heretofore been possible. Our review draws together two broad types of incarceration–employment scholarship: research on formerly incarcerated individuals and employer–focused research. Within these broad areas, we categorize what we believe are distinct strands of each research type, summarize individual studies within each category, and characterize overall findings and patterns.

Review of Research Findings on Formerly Incarcerated Individuals

One frustration that is bound to be experienced by a scholar who reviews extant research on the incarceration–employment relationship is the seeming lack of consistency in the findings. It is not difficult to find persuasive evidence that incarceration corrodes employment prospects, but it is also not difficult to find equally persuasive evidence that incarceration bears no relationship with employment, and possibly even improves employment prospects in the short term. That being said, and at the risk of making what must seem like a nonsensical observation, there is consistency in the inconsistencies concerning the incarceration–employment relationship.

In the paragraphs that follow, we classify existing research into five broad categories: non-representative research, administrative research, survey research, reentry research, and cross-national research. Within each of these categories, it is possible to draw more dependable conclusions about

the nature of the incarceration–employment relationship. It would seem that inconsistencies mostly only arise when conclusions are drawn across these research traditions.

Findings From Non-Representative Research

One prominent strand of research on the incarceration–employment relationship utilizes a variety of high-risk but non-representative samples comprising adjudicated delinquents as well as individuals recently released from prison (Gottfredson & Barton, 1993; Laub & Sampson, 2003; Matsueda, Gartner, Piliavin, & Polakowski, 1992; Nagin & Waldfogel, 1995; Sampson & Laub, 1993). These studies typically benefit from having a comparison (non-incarcerated) sample that is demonstrably at high risk of incarceration, lending more weight to the interpretation of any differences in employment or earnings as attributable to the incarceration experience. On the other hand, the results from these kinds of studies are far less likely to generalize, not to mention that findings about the impact of incarceration on employment tend to be as frustratingly mixed as the study designs.

Matsueda et al. (1992) re-analyzed data from the subsamples of recently incarcerated individuals and individuals addicted to drugs in the National Supported Work Demonstration. They observed no difference between the two groups in the likelihood of employment as well as mean earnings (although there was a surprisingly positive correlation of the number of weeks spent in jail with employment and earnings). On the other hand, recently incarcerated individuals reported significantly higher probability and amount of illegal income earning. They also discovered that recently incarcerated individuals rated certain professional occupations (e.g., teacher, construction worker, factory worker) significantly less favorably than individuals addicted to drugs, despite the fact that both groups were chronically unemployed.

Sampson and Laub (1993; see also Laub & Sampson, 2003) re-analyzed data from a sample of youth sentenced to a Boston-area reform school matched to school-going youth, finding that length of juvenile and adolescent incarceration was inversely correlated with adult job stability. In contrast, Needels (1996) re-analyzed data from formerly incarcerated men in Georgia in the Transitional Aid Research Project, finding no relationship between the length of incarceration and earnings once she adjusted for time free in the community.

Gottfredson and Barton (1993) exploited the closing of the Montrose Training School in Maryland as a natural experiment for studying the effect of residential placement on juvenile experiences. When they compared youth who were institutionalized to youth who were supervised in the community but would have been institutionalized had the facility remained open, they failed to find any differences in post-release work experiences between the two groups. Although a study of conviction rather than incarceration, Nagin and Waldfogel (1995) did find in the Cambridge Study of Delinquent Development—a study of white men from working-class London—that conviction exacerbated work instability by increasing unemployment, decreasing tenure, and increasing the number of jobs held. Unexpectedly, conviction was also associated with significantly higher weekly earnings by more than 10 percent above the sample average. To explain this apparent contradiction, they argued that criminal conviction relegates individuals to less stable but higher-paying “spot market jobs” rather than “career jobs.”¹¹

In sum, non-representative research yields a somewhat mixed set of findings about the relationship between incarceration and employment, indicating either an adverse effect or a null relationship. Null findings tend to characterize studies of youth with criminal justice contacts, as well as studies that utilize incarceration length as the criterion measure. On the other hand, adverse effects of incarceration on employment prospects tend to characterize studies of men who had criminal justice contacts during adulthood (or studies that follow youth with criminal justice contacts into adulthood), and studies that utilize more diverse employment measures than employment status. Although there are a number of mixed findings from this research tradition, one notable characteristic of these

data sources is that they were collected prior to the 1980s, a period that witnessed substantial growth in the prison population.

Findings From Administrative Research

A second strand of research on the incarceration–employment relationship analyzes earnings data from administrative sources. One such administrative source includes presentence investigation (PSI) and probation/parole reports on individuals convicted in federal courts (Benson, 1984; Kerley & Copes, 2004; Kerley, Benson, Lee, & Cullen, 2004; Kling, 2004, 2006; Lott, 1992a, 1992b; Waldfogel, 1994). A second administrative source comprises data from state correctional and state unemployment insurance (UI) systems for samples of recently arrested or incarcerated individuals, including California (Grogger, 1995), Florida (Kling, 2004, 2006), Ohio (Sabol, 2007), Washington State (Pettit & Lyons, 2007, 2009), and Illinois (Cho & LaLonde, 2008; Jung, 2011; Loeffler, 2013). A distinct advantage of administrative studies is that they are frequently capable of compiling data on employment and earnings for as few as eight and as many as 20 quarters prior to and following incarceration. On the other hand, they are frequently limited to studying only the correlation between incarceration length and employment, conditional on incarceration. They also rely exclusively on official measures of employment, which can lead to distortion if formerly incarcerated individuals are frequently employed in “uncovered jobs.” We return to this latter point after considering the research findings.

Lott (1992a) failed to find any relationship between prison incarceration and earnings among individuals federally convicted of drug crimes, net of their conviction. On the other hand, he found in a companion study that incarceration reduced earnings among persons convicted for larceny but not among those convicted for fraud (Lott, 1992b). In Waldfogel’s (1994) study, individuals with a federal conviction experienced a relative decline of 9 percent in their employment likelihood and a 16 percent penalty in their monthly earnings. Grogger (1995) showed that individuals arrested in California also experienced a sizable penalty for jail confinement that persisted for at least six quarters following release. Specifically, the relative (to the sample mean) erosion in employment was over 15 percent, whereas the relative penalty in earnings was 14 percent.

An unexpected finding that often emerges in many state administrative datasets is that the probability of employment actually increases relative to pre-prison employment, and is unexpectedly higher among those who serve longer sentences. For example, Kling (2004, 2006) observed in Florida that employment rates were 40 percent among those incarcerated for one year, and more than 50 percent among those incarcerated for four years. Similarly, mean earnings in the peak quarter were approximately \$800 among those incarcerated for one year and \$1,600 among those incarcerated for four years. These differentials were relatively short lived, however, as employment and earnings converged after two years elapsed. Furthermore, Kling observed that post-prison employment rates eventually returned to their pre-prison level, irrespective of incarceration length.

Pettit and Lyons (2007, 2009; Lyons & Pettit, 2011) similarly reported that incarceration length was positively and significantly correlated with employment rates among formerly incarcerated men in Washington State. As in the Florida study, there was a tendency for employment to return to pre-prison levels within two years. Jung (2011) reported the same kind of convergence at the two-year mark among males in Cook County, Illinois (see Cho & LaLonde, 2008, for evidence on formerly incarcerated females in the same jurisdiction). Also like the Florida study, significant earnings differentials were found in favor of those who served longer terms of confinement in Washington State and Illinois, but these differentials disappeared over time.

Loeffler (2013) employed a unique research design to study the impact of incarceration on UI employment among individuals convicted of felonies in Cook County, Illinois. Specifically, he took advantage of the fact that cases are randomly assigned to circuit court judges, each of whom has a peculiar tendency to sentence differently from his or her judicial peers (on average, at least). In light

of random assignment to judges, these inter-judge differences become the source of exogenous variation in the likelihood of imprisonment and a powerful way of overcoming selection bias. Using a set of unique judge identifiers as “instrumental variables” for incarceration, then, Loeffler found that there was no relationship between incarceration and five-year employment rates.²

To summarize the findings from administrative data sources, among individuals convicted by the federal government, incarceration seems to worsen employment prospects relative to other criminal justice sanctions. On the other hand, somewhat unexpectedly, in studies of individuals serving state prison terms, those who serve longer terms tend to have better prospects with respect to both employment and earnings than those who serve shorter terms. It is very possible that this finding is due, in part, to the conditions of community supervision for formerly incarcerated individuals with different lengths of confinement, or else to differential exposure to prison programs that emphasize job skills (in which case individuals with longer “exposure” to prison are administered a larger “dosage” of in-prison programming). However, it is noteworthy that these differentials systematically erode with the passage of time.³ A notable feature of most of these studies is that the subjects serve a sentence longer than one year in state prisons; in fact, the average subject in these studies served approximately two years behind bars. The estimates should therefore be interpreted as the correlation between imprisonment length and employment prospects, conditional on serving a prison sentence much longer than one year.

There is one important qualification to the paradoxical finding that longer versus shorter prison terms are correlated with better employment and earnings prospects, at least in the short run. Administrative earnings data come from state tax records and are based on the earnings reported by employers to the state UI system. They therefore fail to capture income from “uncovered jobs”—for example, self-employment income and out-of-state income.⁴ Comparisons of self-reported and administrative data indicate that survey earnings are routinely higher than UI earnings, although program impacts tend to be similar (Kornfeld & Bloom, 1999). The singular exception is for young males with a criminal record, for whom the discrepancy between survey and UI earnings is greatest, and for whom program impact estimates qualitatively differ depending on the source (Schochet, Burghardt, & McConnell, 2008). These males are precisely the subjects of interest in the studies cited previously, suggesting that post-prison employment prospects measured from tax records miss many sources of income for samples entangled in the criminal justice system—self-employment, informal employment, short-term employment, and employment that is cash only or “off the books.” Similar findings have been reported among formerly incarcerated men in the Netherlands, wherein informal employment seemed a plausible explanation for the discrepancy between self-report and official data sources (Ramakers, Nobbe, Nieuwebeerta, & Dirkzwager, 2017).

In short, if the tendency to work in UI-covered jobs varies systematically by the length of time served in prison, then the positive correlation between incarceration length and employment will partially be an artifact of the tendency to work in covered jobs relative to uncovered jobs. This finding suggests that self-reported employment and earnings, although undoubtedly subject to their own peculiar sources of measurement error, probably yield fewer biases in samples of formerly incarcerated individuals (see also Ramakers, Apel, Nieuwebeerta, Dirkzwager, & van Wilsem, 2014).

Findings From Survey Research

A third prominent strand of research on the incarceration-employment relationship employs the National Longitudinal Survey of Youth 1979 (NLSY79), until recently the only large-scale, self-report survey permitting scholars to study the incarceration-employment relationship in a representative sample (Bound & Freeman, 1992; Davies & Tanner, 2003; Fagan & Freeman, 1999; Freeman, 1992; Huebner, 2005; Maroto, 2015; Monk-Turner, 1989; Raphael, 2007; Western, 2002, 2006; Zaw, Hamilton, & Darity, 2016). One distinct advantage of the NLSY79 is that the survey has been

ongoing since its inception, allowing very long-term follow-up of respondents into their 50s. However, this advantage is offset by limitations in the measurement of incarceration, which can only be ascertained by the location of the interview and which is consequently likely to capture confinement in prison rather than jail. A more recent survey now exists in the form of the National Longitudinal Survey of Youth 1997 (NLSY97), comprising annual self-report information about incarceration in either jail or prison, and permitting the study of the incarceration-employment relationship in a contemporary sample of young people surveyed into their early 30s (Apel & Sweeten, 2010). While the NLSY79 and NLSY97 have become common fixtures in research on the incarceration-employment relationship, there are other recent and notable data sources that also provide broad generalizability. These include the Fragile Families and Child Wellbeing Study (Geller, Garfinkel, & Western, 2006; Turney & Schneider, 2016; Western, 2006), the Survey of Income and Program Participation (Sykes & Maroto, 2016), and the National Longitudinal Study of Adolescent Health (Brayne, 2014; Dennison & Demuth, in press).

Freeman was among the first to explore the relationship between incarceration and employment in the NLSY79 (Bound & Freeman, 1992; Fagan & Freeman, 1999; Freeman, 1992). In a study of just the male high school dropouts in the sample, for instance, he documented a shockingly large 21-point reduction in the likelihood of employment between ages 18 and 26 (a 34 percent relative decline from the sample mean), and a 17-point disparity five years later between ages 23 and 31 (a 22 percent relative decline from the sample mean) (Bound & Freeman, 1992). In a companion study, the relative differences were even larger among young black male high school dropouts compared to the differences for all male high school dropouts (Freeman, 1992).

The use of the NLSY79 to study the incarceration-employment relationship has been reinvigorated by Western (2002; see also Western, 2006), who used the dataset to estimate the effect of incarceration on wage levels and wage growth. Prior incarceration had a significantly depressive effect on current wages, creating a wage gap of about 16 percent between non-incarcerated individuals and those with a history of incarceration. Importantly, Western also found that incarceration deflected individuals onto a much flatter wage trajectory, slowing wage growth by 31 percent relative to high-risk men who were never incarcerated.⁵

Huebner (2005) further confirmed that a history of incarceration led to significant erosion in the long-term likelihood of employment, after controlling for an extensive set of confounding variables. Raphael (2007) explored a variety of statistical models and found that having ever been incarcerated was correlated with a significant reduction in the annual number of weeks worked, as well as a significant decline in hourly wages of about 15 percent. Jung (2015) explored whether age of first confinement in the NLSY79 moderated the incarceration-employment relationship. He found that incarceration in a youth correctional facility was correlated with significant reductions in wages and labor supply (e.g., number of weeks worked) by age 40, whereas first incarceration during the 20s was correlated with a significant wage penalty but no impact on labor supply.

Also using the NLSY79, Maroto (2015; see also Zaw et al., 2016) documented substantial differences in wealth accumulation between individuals who had ever been incarcerated and their never-incarcerated peers. For example, in a well-controlled model, she estimated a mean difference in net worth (assets less debts) on the order of \$42,000. Furthermore, the difference grew substantially over time, from \$19,000 among those incarcerated in the previous year to \$105,000 among those incarcerated more than 10 years ago. The primary mechanism for the size of the wealth disparity was attributable to the differences in home ownership, which was exacerbated by the differences in marriage and earnings.⁶

Apel and Sweeten (2010) utilized the NLSY97 to explore a number of facets of the incarceration-employment relationship. Using a variety of different statistical methods, they estimated the employment likelihood to be about 11 percent lower among individuals who were incarcerated following their first criminal conviction, compared to comparably high-risk individuals who were also convicted of a

crime for the first time but were not incarcerated. This effect was persistent for up to six years following incarceration. When they further probed the employment differential, they discovered that most of the difference was due to the fact that incarcerated individuals dropped out of the labor force (and for a significantly longer number of weeks), meaning they were neither employed nor looking for work. Interestingly, there was no difference in the likelihood or duration of unemployment, referring to a state in which someone was not employed but was actively searching for a job. The overall pattern of results compelled Apel and Sweeten to conclude that incarceration caused sustained withdrawal and detachment from work. Further comparisons showed a wage disparity of 9 percent among those who were employed, and although this estimate was not statistically significant, there was evidence of deterioration in wages over time for the comparatively young sample.

Geller et al. (2006; see also Western, 2006) performed an analysis of the incarceration-employment relationship among new fathers surveyed in the Fragile Families and Child Wellbeing Study (FFCWS).⁷ Men who had ever been incarcerated possessed a relative employment rate about 4 percent lower than non-incarcerated men, as well as a wage rate about 31 percent lower. However, the magnitude and significance of these differences varied a great deal depending on what statistical adjustments were applied, not to mention that sensitivity analysis indicated the results were not particularly robust to confounding by unobserved variables. Even more recent FFCWS research by Turney and Schneider (2016) found significant disparity in the accumulation of assets—including a bank account, a vehicle, and home ownership—that was not only limited to formerly incarcerated individuals but extended to their romantic partners, as well.

Sykes and Maroto (2016) creatively merged data from multiple sources, but primarily the Survey of Income and Program Participation (SIPP), to study the relationship between incarceration and wealth. Their important results showed that incarceration (as proxied by a measure of institutionalization) led to significant erosion in household wealth, producing a 64 percent decline in assets as well as an 86 percent decline in debts. While the latter finding would not seem to be obviously problematic at first glance, in fact it suggests that households impacted by incarceration are locked out of opportunities to obtain credit and thus to build wealth. Sykes and Maroto further estimated substantial differences in the likelihood of employment among households with an incarcerated family member, suggesting spillover effects of incarceration.

Brayne (2014) utilized the National Longitudinal Study of Adolescent Health (Add Health) to investigate the degree to which various forms of criminal justice involvement resulted in “system avoidance,” or the curtailing of involvement with surveilling institutions. Having been incarcerated was correlated with significant withdrawal from the labor market, and in fact other forms of criminal justice contact (e.g., arrest, conviction) were uncorrelated with employment. Dennison and Demuth (in press) also utilized Add Health to evaluate the impact of incarceration on socioeconomic status (a composite variable including educational attainment and occupational status). They treated incarceration as the most extreme status on a continuum of criminal justice involvement and found that formerly incarcerated individuals possessed significantly lower socioeconomic achievement, over and above less serious forms of criminal justice involvement.

To summarize the findings from survey research, it should be clear that this research tradition exhibits a great deal of consistency in the finding that incarceration is correlated with worse employment and socioeconomic prospects. The employment differential tends to fall between 10 and 20 percent, which is a relative difference in the probability of working between incarcerated and non-incarcerated individuals. The wage disparity is more variable and tends to be between 5 and 30 percent, although the differential is not always statistically significant. The most recent research examines wealth disparity and provides estimates that are statistically significant and quite large.

There are a number of contributions and advantages of the survey research tradition for the questions that interest us here. First, the available samples are large and nationally representative, which ensure that the results generalize to known populations. Second, the surveys are frequently

longitudinal, which facilitates the study of not only short-term consequences of incarceration but long-term erosion in employment. Third, the employment outcomes are manifold, which allows unpacking of some of the complexities of the incarceration-employment relationship. Fourth, there is a great deal of attention devoted to the selection problem, which strengthens causal inference. Yet true to the adage that there is no such thing as a free lunch, one important tradeoff of these advantages is the fact that survey research is frequently limited in the available measures of incarceration and criminal justice involvement generally. For example, with possibly one exception (the NLSY97), researchers working in the survey tradition are unable to distinguish jail from prison confinement, to measure length of confinement, and to study the “filtering” of criminal suspects from arrest, through intermediate criminal justice decisions (e.g., charging, prosecution, conviction), to incarceration as the final disposition (one wave of Add Health allows these filtering distinctions as well).

Findings From Reentry Research

One final, much more recent research tradition that speaks to the nature of the incarceration-employment relationship is composed of samples of incarcerated individuals who have recently left jail or prison (Freudenberg, Daniels, Crum, Perkins, & Richie, 2005; Sugie & Lens, 2017; Visher, Debus-Sherrill, & Yahner, 2011; Western, Braga, David, & Sirois, 2015). These reentry studies are not evaluations of the impact of incarceration on employment and earnings, *per se*, simply because they lack comparison samples of individuals who were not recently incarcerated. They nevertheless allow consideration of how circumstances change before and after confinement for those who experience incarceration. They also speak with some authority to the manifold reentry challenges that recently incarcerated individuals face in the labor market and other domains, especially in the more volatile months immediately following their release.

Freudenberg et al. (2005) conducted before/after comparisons of employment among adolescent men and adult women incarcerated in New York City jails. Relative to their employment status prior to the arrest leading to their incarceration, neither group exhibited any significant change in employment about 15 months after release. What is notable, however, is that no more than one-third of either sample was employed at either time period. Notably, the adult women reported significantly more reliance on government sources and family members for income following release, whereas adolescent men reported significantly less reliance on these sources. The adult women also reported a significant increase in health problems (e.g., depression, anxiety) and emergency room visits, while adolescent men again reported significant reductions in all of these experiences.

Visher et al. (2011) reported results from the Returning Home project, a three-state, longitudinal study of individuals leaving state prisons. About two-thirds (68 percent) of the respondents held a job prior to prison, but only 31 percent reported current employment after two months in the community, and just 45 percent reported current employment after eight months in the community. At both time periods, about 75 percent reported actively searching for a job, but a large share (71 percent) felt that their criminal record had affected their job search. An examination of income sources at the eighth-month interview revealed that a higher percentage reported income from informal work (47 percent) and family and friends (48 percent), as opposed to legal work (41 percent). In a regression model of the total percentage of time employed since release, Visher et al. found that the pre-prison work history was an important correlate of post-prison employment, as was in-prison work experience and arrangement of a job prior to release. Drug use, however, was inversely correlated with post-release employment, as were physical and mental health conditions.⁸

Western et al. (2015; see also Western, Braga, Hureau, & Sirois, 2016) oversaw the Boston Reentry Study, a longitudinal study of men and women who were recently incarcerated in Massachusetts state prisons. Immediately following release, respondents did not report heavy work-related activity (e.g., employment, job search), and although labor force participation increased steadily it reached

only about 15 percent by the end of the first week. Rather, about half of the sample was idle and reported no activity at all during this time. After two months, 43 percent reported paid employment, and after six months this figure increased to 53 percent.⁹ Many of the reported jobs were classified as “day labor” (e.g., construction, home improvement, snow removal), suggesting a high degree of instability and seasonality. Interestingly, public assistance was far more common than employment and characterized over 70 percent of the sample at both time periods. In a regression model of employment, Western et al. reported that respondents who were more socially isolated during the first week—spending time without family and involved in no productive activity—were less likely to be employed, a result that was marginally significant.

Sugie and Lens (2017) conducted a three-month smartphone study of self-report daily employment among men who were recently released from New Jersey state prisons. The novel use of smartphones allowed them to incorporate global positioning system (GPS) tracking of the respondents’ daytime whereabouts. In a model of the timing of first employment, Sugie and Lens found that only the density of low-skill, low-wage job openings in proximity to a respondent’s residence was significantly correlated with his employment likelihood, suggesting “spatial mismatch” between where formerly incarcerated individuals live and where good-paying work opportunities are available. On the other hand, any kind of job availability in proximity to daytime activity spaces was strongly correlated with job acquisition, suggesting that efforts to subsidize transportation to job-rich areas might hold promise as an employment-focused intervention for individuals who have recently left prison.

In sum, research from reentry studies points to a number of challenges that face formerly incarcerated individuals in the very first few months during their return to the community. Among the challenges that have been identified in the studies reviewed above are substance use and abuse, relatively unstable and inaccessible work opportunities, mental and physical health problems, and overall social isolation (especially among formerly incarcerated males). These are challenges that are likely to severely restrict employment prospects. Yet these by no means exhaust all of the challenges, as very recent attention has been devoted to the problems of legal debt accumulation (Cook, Kang, Braga, Ludwig, & O’Brien, 2015; Harris, Evans, & Beckett, 2010) and housing insecurity (Geller & Curtis, 2011; Harding, Morenoff, & Herbert, 2013).

Findings From Cross-National Research

Lest the review thus far leave the impression that research on the relationship between incarceration and employment is limited to the United States, it is worth drawing attention to a vibrant tradition of similar research in Western Europe. One point of contrast between the U.S. and Europe is the typical length of confinement, as prison sentences in Europe are far shorter than their American counterparts. In fact, European prison confinement resembles American jail confinement with respect to its length. A second point of contrast concerns the legal system itself, as most European legal systems are inquisitorial rather than adversarial. By comparison, the U.S. represents the quintessential adversarial system of justice. A third point of contrast is related to the conditions of prison confinement, which are far more humane in Europe than in the U.S. (Johnston, 2000). A fourth point of contrast concerns the presence of a more liberal welfare state that provides for a stronger social safety net for all individuals, including the formerly incarcerated.¹⁰ Findings from these contexts can speak to the generalizability of the large body of work based on American samples and provide insight into whether theoretical mechanisms are context-specific or universal.

A distinct advantage of cross-national research is that European scholars frequently have access to population-wide digital registries on many individual circumstances, including education, marriage and cohabitation, fertility, military service, and residence, in addition to criminal justice involvement and employment. To date, the most rigorous research on the incarceration-employment relationship outside the U.S. has been performed in the Netherlands (Ramakers et al., 2014; Ramakers, van

Wilsem, & Apel, 2012; Van der Geest, Bijleveld, Blokland, & Nagin, 2016; Verbruggen, 2016) and the Nordic countries (Aaltonen et al., 2017; Andersen, 2015; Landersø, 2015).

Using administrative data from the Netherlands, Ramakers et al. (2012) estimated the timing of employment of formerly incarcerated individuals compared to unemployed to-be-incarcerated individuals. These groups were selected from the population of incarcerated males who entered Dutch penitentiaries within a two-year window and were in the risk pool for employment (i.e., they were recently employed). They found that individuals who were recently incarcerated had a significantly higher probability of employment than unemployed individuals who had not yet been incarcerated. Specifically, the employment likelihoods were 80 and 55 percent, respectively. Recently incarcerated individuals also obtained a job more quickly—12 months versus 18 months, respectively.

In another recent study of Dutch males surveyed for the Prison Project, Ramakers et al. (2014) examined the relationship between incarceration length and a variety of self-report employment outcomes within the six-month window following release. The most consistent finding was that, for prison spells longer than six months in duration, the likelihood of employment declined with incrementally longer incarceration length. For prison spells shorter than six months, on the other hand, longer incarceration length was uncorrelated with employment. This threshold relationship was not explained by differences in in-prison programming or post-release recidivism. Interestingly, there was no clear relationship between incarceration length and measures of job quality such as wages and occupational status, nor with measures of job stability, among men who obtained employment. One suggestive finding from this study was that re-employment by pre-prison employers was partly able to explain these differentials, as males who were imprisoned for short periods of time were more likely to maintain their employment ties.

Van der Geest et al. (2016) combined treatment files on Dutch males treated in a juvenile justice institution with administrative data until age 32. He first estimated different adult employment trajectories for the sample (“normative,” “delayed onset,” “dropouts,” and “non-participants”), and then documented heterogeneity in the incarceration-employment relationship. Among the roughly 64 percent of the sample characterized by normative and delayed onset employment trajectories, incarceration resulted in a substantial decline in the probability of employment in the subsequent year, over and above the impact of conviction. There was no incarceration-employment relationship for the balance of the sample. Verbruggen (2016) used the same dataset and included both the men and women from this high-risk sample. She found that neither a conviction nor a prison spell had any impact on employment chances for men once unemployment history was taken into account. Among women, recent conviction (but not incarceration) was associated with a significantly lower employment likelihood, and like the men, unemployment history was strongly related to current employment.

Aaltonen et al. (2017) investigated employment before and after incarceration in four Nordic welfare states: Denmark, Finland, Norway, and Sweden. The samples were composed of individuals imprisoned for the first time for a maximum of one year, and differed considerably in the degree of labor market attachment prior to incarceration as well as employment prospects following incarceration. To illustrate, 39 percent of Finnish individuals (aged 25–30 at the time of incarceration) earned any income in the year prior to their imprisonment, and this increased to 48 percent in the year after imprisonment. By comparison, the corresponding employment changes were 72 to 74 percent in Denmark, 68 to 43 percent in Sweden, and 78 to 71 percent in Norway. In all four countries, however, employment rates exhibited an overall downward trend starting five years prior to incarceration and spanning five years following incarceration.

Andersen (2015) investigated the effect of community service versus incarceration by exploiting a policy reform in Denmark, which made a large number of criminal offenses newly eligible for community service as punishment (e.g., misdemeanors, drunk driving, simple violence). Her results indicated that, for up to three years following conviction, individuals given custodial sentences exhibited

the same levels of income and social welfare dependence as those sentenced to community service. On the other hand, there was a significant divergence in long-run prospects, as those sentenced to incarceration had lower income and higher social welfare dependency. For example, five years after conviction, the formerly incarcerated individuals earned 17 percent lower income and experienced 25 percent longer duration of welfare dependency.¹¹

Landersø (2015) exploited another Danish reform that increased incarceration length by roughly one month, to obtain causal estimates of the effects of incarceration length on unemployment and earnings. Focusing on individuals incarcerated for violent offenses, most of whom were confined for two months or less, he compared those incarcerated after the reform to those incarcerated prior to the reform. The results indicated that the longer prison sentences actually lowered unemployment rates for up to two years (but not beyond), and although earnings were also higher, the differences were not statistically significant. Landersø suggested that the reform may have reduced unemployment by enabling incarcerated individuals to participate in and benefit from rehabilitation programs versus merely experiencing the costs of incarceration, since participation rates for prison rehabilitation programs increased steeply with incarceration length.

To summarize the findings from cross-national research, the mix of negative, positive, and null effects of incarceration on employment is also evident in Western contexts other than the United States, namely in the Netherlands and the Nordic countries. Null findings are evident from long-term studies of high-risk samples, namely individuals confined in reform institutions as juveniles (Netherlands). Evidence that incarceration improves employment prospects, for at least some period of time following release, stems from studies of shocks to incarceration length among individuals with very short sentences (Denmark), and from studies comparing incarceration spells to unemployment spells (Netherlands). Evidence that incarceration worsens employment prospects is strongest in studies comparing prison sentences to non-custodial punishments (Denmark), and in studies comparing prison sentences longer than six months to comparatively shorter sentences (Netherlands). The latter set of findings suggest that collateral consequences in the labor market are not exclusive to contexts like the U.S., with its dissimilarity in many social and legal respects to Western Europe.

Review of Research Findings on Employers

A great deal of research on the incarceration-employment relationship implicitly concerns the “supply side” of the labor market, referring to the skills and work experiences possessed by potential job seekers that make them more or less attractive hires to employers. A supply-side explanation of the incarceration-employment relationship emphasizes simply that formerly incarcerated individuals are objectively less employable than their non-incarcerated counterparts. For instance, this might be because of erosion in their human capital due to time behind bars and thus being out of the labor market, which makes them less experienced, less productive, or less capable employees. Or it might be because of erosion in their social capital, which might constrain access to job referral networks. Yet there is another tradition of research that focuses on the “demand side” of the labor market, referring to the willingness of employers to knowingly hire formerly incarcerated individuals. This shifts the analytical focus from employees to employers and emphasizes the stigmatizing effect of incarceration and what it represents about a formerly incarcerated individual’s reputation, reliability, or trustworthiness.

Clear evidence for exclusion of formerly incarcerated individuals in the labor market is the variety of statutory restrictions that prohibit employment in certain sectors (e.g., public employment), catering to certain vulnerable clientele (e.g., children), and professional licensing and bonding in certain occupations (Burton, Cullen, & Travis, 1987; Dale, 1976; Harris & Keller, 2005). Beyond these statutory exclusions, however, there is also a compelling body of research based on employers themselves that criminal justice involvement (even if it does not culminate in incarceration) stigmatizes

job seekers. We discuss evidence from two distinct kinds of studies: audit and correspondence studies as well as employer attitude surveys.

Experimental Audits and Correspondence Studies

Experimental audits or correspondence studies are used in a variety of disciplines to test for discrimination. In a typical study of employment, for example, a pair of applicants, known as “auditors” or “testers,” applies for the same job. Relevant background characteristics of the tester pair (e.g., gender, race, education, work history) are matched as best as possible while the key characteristic under study—namely, possession of some kind of criminal history—is randomly varied between the testers. In an audit study, the testers apply in person for posted job openings, whereas in a correspondence study, résumés or applications with fictitious credentials are submitted. The key outcome in an audit or correspondence study is the “callback,” or any form of favorable follow-up from an employer to a tester (e.g., offer of hire, invitation for an interview, solicitation of more information).

In what is probably the earliest version of a correspondence study, Schwartz and Skolnick (1962) solicited the assistance of a confederate to disseminate employment folders to business establishments in the Catskill region of New York State. They documented a much lower callback rate for employment files reporting a conviction and sentence for assault (4 percent), relative to files reporting no criminal record (36 percent).¹² Remarkably, despite the fact their sample size was too small for conventional statistical tests, the difference in those percentages was statistically significant (this was determined by our calculation of Fisher’s exact test from Schwartz and Skolnick’s data).

In a widely read experimental audit, Pager (2003; see also Pager, 2007) conducted a Milwaukee study of matched pairs and found that employers advertising entry-level job openings were less than half as likely to call back applicants who reported a prison record—a felony conviction with prison time—relative to temporary employment.¹³ Specifically, the callback rates were 10 percent and 23 percent, respectively, for applicants with and without a prison record. Her conclusion was that “criminal records close doors in employment situations” (p. 956). In a replication of the audit design in New York City, Pager, Western, and Sugie (2009; see also Pager, 2007; Pager, Western, & Bonikowski, 2009) again found a significantly lower callback rate among men with a prison record. Specifically, callback rates were 15 percent and 28 percent, respectively, among applicants with and without a prison record. One important finding from their analysis concerned opportunities for applicants to establish rapport. Applicants who were given a chance to interact with a hiring manager experienced greatly improved hiring chances, and this was especially true for applicants with a prison record. This finding led Pager and colleagues to conclude that “[p]ersonal contact thus seems to play an important role in mediating the effects of criminal stigma in the hiring process” (p. 200).

One additional finding that has consistently emerged from Pager’s series of audit studies concerns the influence of racial discrimination as well as the dual influence of race and incarceration (Pager, 2003, 2007; Pager et al., 2009; Pager, Western, & Sugie, 2009). Namely, black applicants received significantly fewer callbacks than white applicants, but most alarming was her finding that black applicants without a prison record had a similar callback rate to white applicants with a prison record. This provided evidence of racial discrimination that intersected in a complicated way with incarceration, in such a way that being black and possessing a prison record constituted a “double jeopardy” in low-wage labor markets (Pager, 2005, 2007).

Galgano (2009) carried out a correspondence study in Chicago, with a design very similar to Pager (2003) but involving the submission of online applications rather than in-person applications. Her testers were also female rather than male. She did not observe any difference in callback rates, suggesting that “a criminal history is not as universally stigmatizing for women” (p. 33). Decker, Ortiz, Spohn, and Hedberg (2015) replicated Pager’s (2003) design in Phoenix using both an audit study and a correspondence study. Interestingly, there was no difference in the chance of a callback

in the correspondence portion (online applications) of the study—8 percent of the applicants in the control group received a callback compared to 7 percent of the applicants with a prison record. On the other hand, in the audit portion of the study (in-person applications), 18 percent of the applicants in the control group received a callback compared to 10 percent of the applicants with a prison record, a difference that was statistically significant.

In sum, audit studies consistently find that the rate of callback is lower for formerly incarcerated males who apply in person for job postings, relative to men reporting no criminal record. Indeed, their callback rate is about one-half the size of the callback rate among men without any criminal history. By comparison, there does not seem to be any relationship between incarceration and callback in the correspondence studies conducted to date, for either men or women. The audit research tradition also points to the joint influence of race and criminal history. Formerly incarcerated black job applicants are especially disadvantaged, in such a way that black applicants with no criminal history tend to experience fewer callbacks than white applicants who have recently been incarcerated. However, the interaction of race with criminal history is frequently not statistically significant; thus, this joint relationship is suggestive but has not yet been definitively established.

Employer Willingness to Hire

Another strand of employer-focused research surveys the perceptions and attitudes of business owners or hiring personnel towards individuals formerly involved in the criminal justice system. To this end, multiple measures with Likert-type scales are typically used and are sometimes combined with hiring vignettes. An obvious downside of this approach is that responding to a hypothetical job applicant could produce different outcomes than authentic hiring situations. Indeed, in a follow-up survey of employers in the Milwaukee audit, Pager and Quillian (2005) documented no racial difference in employer expressions of willingness to hire in a vignette involving an applicant with a prison record, despite their finding of a racial difference in the likelihood of a callback to an applicant with a prison record. This suggests that there could be a large discrepancy between what employers say they would do (as measured by survey responses) versus what employers actually do (as measured by callback rates). While surveys might not be suitable for revealing the degree of discrimination, this method can nevertheless examine how a wide range of characteristics of the applicant, the employer, or the organizational context relate to willingness to hire from this disadvantaged group. This line of work helps to reveal these biases and understand why employers hold a generally unfavorable attitude toward these individuals.¹⁴

Finn and Fontaine (1983) surveyed individuals enrolled in personnel management classes and found that respondents preferred applicants without a criminal record over those with different forms of criminal records, and especially those who had served time. Good job qualifications overcame the disadvantages of a criminal record but only when they substantially exceeded the qualifications of applicants without a record.¹⁵ In a later study, Finn and Fontaine (1985) extended their investigation to include more students (N=225) as well as different types of crime, types of employment, and the sex of the applicant. While they found no evidence for a gender bias, significantly lower scores were given to applicants who attempted armed robbery (versus no crime, drugs possession, and shoplifting) and applicants for salesperson jobs (versus hand packager, general clerk).

Albright and Denq (1996) concluded that a college degree and certain types of educational prison programs increased the willingness of employers to hire formerly convicted individuals in Houston and Dallas. The employers were very opposed to hiring individuals convicted of violent crimes but were more likely to hire formerly convicted individuals when government incentives were available and no relationship existed between the crime and the job to be filled. Albright and Denq furthermore concluded that “[e]mployers in this study indicated that the more information they received about the applicant . . . the more likely they are to consider hiring them” (p. 133). In a small-scale

study in Baltimore, Giguere and Dundes (2002) examined employers' willingness to hire using a vignette that described an individual who was recently released from prison and had applied for an entry-level job. They identified people skills and customer discomfort as major employer concerns about hiring from this group. They also showed that employers who had (non-business related) contact with formerly convicted individuals held more favorable attitudes towards them, and concluded social contact with this group "helps to offset the stigma" (p. 404).

Haslewood-Pócsik, Brown, and Spencer (2008) examined several topics related to employing formerly convicted individuals in northwest England and found support for many of the above-mentioned factors.¹⁶ Noteworthy is that, next to job skills, several "soft skills" such as honesty and reliability were rated as highly relevant characteristics. Their study furthermore revealed that employers rated a mentor most useful among a wide variety of support options. A sizeable proportion of the respondents (43 percent) had prior experience with employing formerly convicted individuals, enabling the researchers to compare the views of employers with and without this experience. Remarkably, they concluded that employers with such prior experience were more concerned about the practical risks such an employee might pose, but were also less averse to consider hiring them, regardless of their criminal record. These employers were also more likely to rate educational and professional qualifications as less critical.

Lukies, Graffam, and Shinkfield (2011; see also Graffam, Shinkfield, & Hardcastle, 2007) were the first to examine the relative importance of the criminal history of the job-seeker, personal characteristics of the employer, and organizational context variables for employer attitudes. Based on questionnaires completed by Australian employers, they concluded that the nature of the criminal history was the most salient correlate of perceived employability, although employer and organizational characteristics were also relevant correlates. While industry type and firm location had no influence on attitudes, importantly, employers in larger organizations and those who previously employed individuals involved in the criminal justice system showed a significantly more favorable attitude toward this group.

In a very recent Australian study, Reich (2017) administered vignettes to a large number of employers. She examined the role of several of the abovementioned objective factors as well as employers' subjective beliefs and found that "belief in redeemability" was a significant determinant of the willingness to hire (in her study, persons were convicted but sentenced to community service rather than to prison).¹⁷ She also found that applicants who possessed a variety of hard and soft skills were perceived more favorably.¹⁸

In sum, individuals involved in the criminal justice system in general, and formerly incarcerated persons in particular, not only face a variety of statutory restrictions that categorically prohibit certain types of employment, but must also confront the seemingly intractable attitudes of hiring personnel about their unemployability. On a more positive note, several hard and soft skills appear to mitigate employers' attitudes, implying that other job qualifications might serve as fruitful targets for intervention to improve employment prospects. Furthermore, having access to more information about the applicant and prior experience with hiring from this worker pool are related to more favorable employer attitudes. While these findings are often based on small unrepresentative samples, they add a great deal of nuance to the blunter conclusion that employers possess biases against hiring formerly convicted persons.

Concluding Remarks

This chapter has been limited in scope to a review of the quantitative relationship between incarceration and employment. Out of necessity, there are several strands of research and commentary that we have had to omit in order to be as exhaustive as possible in our review. First, we did not include forms of criminal punishment other than incarceration. The rare exceptions (e.g., conviction) are

for findings that help provide greater understanding of mechanisms for the relationship between incarceration and employment, but it goes without saying that other forms of punishment such as arrest and probation are worthy of review in their own right. Second, we did not include qualitative research findings. This does not stem from a lack of interest in the findings from this research tradition, but rather from the sheer number of quantitative studies that exist, coupled with the need to bring some coherence to seemingly disparate conclusions from the quantitative tradition. There are very good examples of qualitative studies that draw out some of the mechanisms that quantitative scholars are incapable of measuring, and these mechanisms are essential for theoretical clarity about the incarceration–employment relationship. Third, we did not include a lengthy policy discussion. Given the length of the current chapter, there was little space to devote to an extensive consideration of policy proposals or to the promise and pitfalls of employment–focused programs for formerly incarcerated individuals. Although existing research establishes the basic nature of the incarceration–employment relationship, policy proposals have not been met with a great deal of success; yet these challenges should not deter continued effort in the future.

As is probably obvious from this chapter, there is a healthy volume of quantitative research on the incarceration–employment relationship. We have sought to identify the key lines of inquiry, to summarize most of the key studies and results, and to point out the divergent findings and possible sources of the discrepancies. Overall, there is remarkable consistency in the finding that incarceration is highly disruptive for certain aspects of the employment experience—even relative to other highly disadvantaged individuals and even in countries with far more liberal and humane justice systems, formerly incarcerated individuals experience a great deal of instability in the labor market. Moreover, two related lines of quantitative inquiry, for which there was insufficient space in this review, imply that the collateral consequences of mass incarceration extend beyond the lives of those individuals who experience incarceration. First, aggregate research shows that the effects of mass incarceration lead to distortion in measures of the economic health of the country, particularly among the young African American men who fill the nation’s prisons (e.g., Western & Beckett, 1999; Pettit & Western, 2004). To illustrate, the Current Population Survey (CPS) is used to provide estimates of the racial gap in high school dropout and wages, but it misses a sizable fraction of socially marginalized men, by design, because they reside in prisons rather than households.

Second, findings concerning statistical discrimination imply that mass incarceration can sabotage the employment prospects of all young black males. One obvious solution to the reported employer discrimination experienced by formerly incarcerated individuals could seem to impose statutory limits on the availability of criminal history information. It is possible, however, that employers who are denied access to such information might use demographic information in a statistically discriminatory way (Solinas–Saunders, Stacer, & Guy, 2015). Recent studies suggest that some employers indeed use an applicant’s race (along with gender and age) as a demographic marker for possession of a criminal record, and this prejudicial exclusion is worsened if employers lack access to criminal history information (Bushway, 2004; Holzer, Raphael, & Stoll, 2006). This puts advocates in the uncomfortable position of having to decide between the social good that derives from restricting employer access to criminal history repositories or employer ability to inquire about criminal histories on job applications (e.g., “Ban the Box”), and thus improving the employment chances of formerly incarcerated individuals, versus the social harm done by statistical discrimination toward young male African American job applicants who have no criminal justice involvement.

Despite all that has been learned from the research reviewed herein, there is still a great deal of room for advancement of this research tradition. In our concluding comments, we just focus on three of these. First, it is essential to avoid treating employment in isolation from other life domains, since evidence is building that incarceration touches many aspects of an individual’s life other than his or her employment prospects, including housing (Kirk in Chapter 3) health (Fahmy & Wallace in Chapter 6, LeBel & Richie in Chapter 7), and family life (Northcutt Bohmert & Wakefield in Chapter 8)

(see also Wakefield & Uggen, 2010; Wildeman & Muller, 2012). This suggests that efforts to estimate the incarceration–employment relationship in a longitudinal setting will be complicated by the fact that non–employment aspects of a formerly incarcerated individual’s life might actually serve as mediators between incarceration and later employment prospects. If so, standard panel designs risk underestimating the incarceration–employment relationship by treating these non–employment aspects as control variables rather than as causal mediators (e.g., Baćak & Kennedy, 2015).

Second, it is probably a good time to move beyond point estimation altogether, or efforts to identify the “average effect” of incarceration controlling for confounders, and to instead focus future efforts on estimation of heterogeneity in the effects of incarceration. Despite the fact that formerly incarcerated individuals tend to be drawn disproportionately from the lower rungs of the social ladder, there is still likely to be considerable heterogeneity in effects. Heterogeneity can come in manifold forms. It can refer to *groupwise heterogeneity* resulting from measureable characteristics of punished individuals that can moderate incarceration effects, for example, the severity of an individual’s background risk (e.g., Wakefield & Powell, 2016). Yet it can also refer to *distributional heterogeneity* resulting from a shift in the location or scale of an outcome for punished individuals relative to their non–punished counterparts. For instance, formerly incarcerated individuals might not only exhibit lower mean wages, but also experience compression in their wage distribution, relative to non–incarcerated individuals.

Third and finally, most existing research is concerned with prison incarceration, which is quite natural given the greater ease of access to state correctional data. But it is also important to develop a much better understanding of the consequences of jail incarceration, or the impact of comparatively short sentences of incarceration. The jail population on any given day tends to be about one–half the size of the prison population—the prison incarceration rate is about 500 per 100,000 (roughly 1.5 million people) and the jail incarceration rate is about 250 per 100,000 (roughly 750,000 people). However, the average daily jail population underestimates by a very large margin the number of individuals who actually pass through the nation’s jails in a given year, which the Bureau of Justice Statistics estimates at slightly less than 13 million in 2010 (Minton, 2011). Accounting for the fact that a little less than 40 percent of individuals are in jail serving a sentence (rather than awaiting trial) still yields approximately 5 million who are incarcerated in jail for a crime in a given year. Short sentences of incarceration are clearly the norm, yet they continue to be the least studied form of incarceration.

Notes

- 1 This is a distinction that roughly corresponds with work in the secondary and primary labor markets, respectively.
- 2 Our interpretation of Loeffler’s (2013) study suggests more ambiguity in the key finding than what is reported. Although the details are somewhat technical, one limitation of instrumental variables models is their tendency to be inefficient, so much so that they can frequently be no better than standard regression models, especially when the instruments are only weakly correlated with the “causal” variable. In Loeffler’s sample, the point estimates for incarceration barely differed between the standard regression model and the instrumental variables model, while the standard errors were considerably inflated in the latter model and therefore rendered the incarceration effect non–significant. In the standard regression model, on the other hand, incarceration was associated with a significant four–point reduction in the probability of employment. Given a roughly 18–percent mean employment rate, this amounts to about a 20–percent relative difference in employment.
- 3 Similar findings of short–term improvement followed by long–term erosion have even been observed for very short custody spells for post–prison parole violations (Harding, Siegel, & Morenoff, in press).
- 4 These studies can also suffer from a substantial amount of missing data due to inability to match individuals across state correctional and employment databases. For example, Kling (2004) reported the loss of up to 42 percent of his sample (individuals involved in the federal criminal justice system) because of match difficulty.

- 5 It should be pointed out that most but not all NLSY79 studies find that incarceration has a robust impact on employment. For one exception, see Monk-Turner (1989), who found no relationship between incarceration in a juvenile institution and occupational status among white males in the NLSY79.
- 6 For state-level evidence on the homeownership gap, see Schneider and Turney (2015).
- 7 The Fragile Families and Child Wellbeing Study is a nationally representative study of non-marital child-births in large cities.
- 8 In their model, Visher et al. (2011) also estimated a significantly positive relationship between employment and possession of photo identification at the time of release. Although it is impossible to ignore the possibility of selection bias, this finding does point to one inexpensive strategy for prison pre-release programming.
- 9 These estimates were driven by the male respondents, as females reported just 27 percent employment at both time periods.
- 10 For cross-national evidence on the use of imprisonment, see Lappi-Seppälä (2011).
- 11 For additional evidence on social welfare dependency, see Andersen and Andersen (2014).
- 12 The most alarming finding from the Schwartz and Skolnick (1962) study was that employment files reporting a trial and acquittal—in other words, an applicant who was accused of assault but proclaimed to be without guilt—also exhibited a lower callback rate (18%) than files reporting no criminal record. This suggests that individuals who have even minor brushes with the law can experience hiring difficulty if potential employers are able to find out about it. For even more recent evidence to this effect, see Vuolo, Lageson and Uggen (2017).
- 13 In Pager's (2003) study, the individual involved in the criminal justice system was assigned a felony cocaine trafficking conviction with 18 months total prison time, the last six months of which included employment in a prison industry (which was reported in the application filled out at the workplace). To account for the same sequence of 12 months of labor force non-participation followed by six months of employment, the other individual was assigned 12 months of schooling (after being held back one year) followed by six months of employment in low-skill jobs for a temp agency.
- 14 Type of punishment (e.g., custodial versus non-custodial) is not always specified in these studies. More often, studies measure employers' attitude towards hiring formerly convicted individuals in general, and distinguish between different types of crime (e.g., violent, non-violent).
- 15 See Varghese, Hardin, Bauer, and Morgan (2010) for a similar approach and findings. In their study, college students rated applicants with different charges and job qualifications. Severity of charge lowered employability, and qualifications could increase employability among applicants with a misdemeanor but not among those with a felony record.
- 16 Haslewood-Pócsik et al. (2008) found that offense type influenced employer attitudes, and concluded that the potential risk an individual might pose to staff or customers was the most important employer concern. They also concluded that many employers wanted to know about the criminal record of the applicant in the earliest stage of the hiring process, and found that most employers had little knowledge about the rules concerning the disclosure of criminal convictions.
- 17 In Reich's (2017) study, employer belief in redeemability was measured by responses to the following four items: "Most offenders can go on to lead productive lives with help and hard work"; "even the worst young offenders can grow out of criminal behavior"; "most offenders really have little hope of changing for the better"; and "some offenders are so damaged that they can never lead productive lives."
- 18 Reich (2017) operationalized two types of skill sets that she referred to as "desistance signals" in her study. Hard skills were measured by an applicant's willingness to openly disclose details about past offending, completion of an employment program following conviction, and receipt of a certificate of rehabilitation, among others. Soft skills were measured by self-presentation, communication ability, and positive attitude, among others.

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