

Chapter 24

Employment, Crime, and the Life Course

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This chapter focuses on life-course studies of employment and crime. It draws predominantly on quantitative results, but evidence from qualitative life-course studies are discussed as well. The purpose here is to provide an informed assessment of state-of-the-art scholarship. This chapter reviews studies that examine the capacity of employment (job entries) to curb criminal involvement. There are strong theoretical reasons to expect transitions to stable employment to contribute to the desistance process. Hence, the chapter explores the life-course studies of unemployment effects on criminal behavior (job exits). It concludes with a summary of the evidence and discusses implications for the next generation of studies on employment and crime.

Keywords: life course, employment, criminal involvement, stable employment, unemployment, job exits, job entries

The association between employment and crime has been examined widely in the social scientific literature (Uggen and Wakefield 2008; Bushway 2011; Crutchfield 2014). The appeal of this topic is easy to understand given that several perspectives expect employment to limit criminal offending. The proverb “idle hands are the devil’s workshop” is particularly consistent with routine activities theory as it suggests that involvement in legitimate work protects from criminal temptations (Felson 1998). From the perspective of social bonding theory (Hirschi 1969), employment has the potential to restrain individuals from offending because going to work increases involvement in conventional activities, attachment to conventional peers, and commitment to conventional life goals, such as a steady paycheck. Some theories, such as anomie theory and rational choice theory, view crime and work as alternative methods of making ends meet. Merton’s (1938) anomie theory argues that by failing to provide legitimate opportunities to pursue normative success goals, some social systems create pressures for the disadvantaged classes to use illegal means of securing those goals. Research by economists and ethnographers alike suggests that lack of attractive job opportunities among disenfranchised youth contributes to their widespread participation in the illegal economy (Sullivan 1989; Freeman 1996).

Crutchfield (1995, 2014) has argued that having a low-quality job is not any less criminogenic than having no job at all, especially among those living in disadvantaged communities. The idea that the effect of employment depends on job characteristics is consistent with several theories of crime. Social control theory argues that the restraining effect of employment is contingent on its capacity to

generate prosocial capital (Laub and Sampson 2003). A dead-end job is unlikely to produce such effects. Jobs that do not pay well and are inherently undignified or taxing are more likely to rather increase than reduce economic and psychological strain (Grogger 1998; Agnew 2006). Rational choice theory suggests that people may choose to commit crime even at the risk of punishment if the benefits of work are exceedingly low compared to the rewards of crime (Fagan and Freeman 1999). An adequate study of employment effects should examine those characteristics of the job that are expected to produce the hypothesized effects. As a sophisticated example of such research, Wadsworth (2006) examined the impact of several theoretically grounded job characteristics on criminal offending using data from the National Longitudinal Study of Youth. His results highlight the importance of two characteristics above others: rewarding job attributes (subjective quality) and benefits (paid leave, health insurance, etc.). By contrast, job stability and earnings were unrelated to either property crime or violent crime in the fully specified model.

Although most perspectives expect crime to be negatively related to employment, some question the causal nature of the association. The general theory of crime (Gottfredson and Hirschi 1990) argues that low self-control, a stable dispositional characteristic emerging in early childhood, is the fundamental cause of individual differences in criminal offending. Under this theory, we should expect crime-prone individuals to fare poorly in the labor market as a predictable consequence of low self-control: “The instability of offenders’ careers in the legitimate labor market is consistent with the absence of persistence in most

ordinary obligations whether they be interpersonal or school- or job-related” (Gottfredson and Hirschi 1990, p. 165). In support of this argument, developmental research on socioeconomic attainment has shown that childhood deficits in behavioral regulation are strongly related to the risk of unemployment in adulthood (Caspi et al. 1998; Kokko, Pulkkinen, and Puustinen 2000). A key challenge to life-course research of employment effects is to address the selection of antisocial individuals into unemployment and unstable working careers. We should expect a great deal of spuriousness in the longitudinal association between criminal offending and employment outcomes.

The idea that criminal behavior may affect employment, rather than vice versa, is also suggested by labeling theory, which draws attention to how social reactions to formal punishment shape individual life outcomes (Fagan and Freeman 1999; Bernburg and Krohn 2003). Using data from an experimental employment audit, Pager (2003) studied the impact of criminal record on labor market success. Employment audit involves sending matched pairs of individuals to apply for real jobs in order to see if employers respond differently to applicants based on the characteristics manipulated in the experiment (Pager 2003, p. 945). In this case, the pairs were identical with the exception of one characteristic—the appearance of a (fabricated) criminal record, which was randomly assigned so that by the end of the experiment, each member of the pair had served in the criminal condition an equal number of times. The results from this experiment are compelling. Among the job applicants of white ethnicity, having a criminal record reduced the probability of callbacks by 50 percent. The effect was even larger among African Americans

because employers were three times more likely to contact those without a criminal record.

It is, of course, possible for both sets of influences to contribute to the negative association between employment and crime: employment may reduce crime and crime may reduce employment. Evidence for reciprocal effects of this kind has been documented in prior research on unemployment and crime (Thornberry and Christenson 1984). This bidirectional perspective is also consistent with the interactionist theory of desistance articulated recently by Massoglia and Uggen (2010).

So far we have focused on theories that expect employment to reduce crime, crime to reduce employment, as well as theories that assume no causal association between the two. Are there any perspectives that would expect employment to increase crime? Drawing on differential association theory (Warr 2002), one can identify examples where participation in legitimate work might stimulate offending. A historical study of “occasional” offenders in an English railroad town found that rather than suppressing crime, getting a job in the railway industry facilitated participation in a lifestyle of heavy drinking with male peers, which, as a rule, increased rather than decreased criminal offending (Godfrey, Cox, and Farrall 2007, p. 108). Studies of frontier violence offer several examples of situations where employment opportunities attract large concentrations of single men to distant or isolated locations (Savolainen, Lehti, and Kivivuori 2008; Courtwright 2009). These men spend much of their time drinking, gambling, and competing over women, who tend to be in short supply. Settings like these are particularly

conducive to interpersonal violence. Cowboys of the Wild West are perhaps the most famous example of this phenomenon. Initial reports focusing on the recent energy boom in North Dakota finds that the sudden influx of mostly male migrant labor may be linked to increased levels of both violent and property crime in shale-rich counties (James and Smith 2014; Ruddell et al. 2014).

Research on the effects of adolescent part-time work on delinquency is another example of a literature that considers employment as potentially criminogenic. Theoretical reasons for expecting a positive association between employment and crime at this stage of the life course include increased exposure to delinquent peers, increased availability of alcohol and drugs due to increased earnings and associations with older peers, and increased strain due to the dual demands of working and going to school. Early research by Steinberg et al. (1982) found that working long hours (at least 15 to 20 hours per week) in a job while going to school was associated with a variety of risky outcomes, including increased use of cigarettes and marijuana. Although the association between work intensity and delinquency has been replicated in a number of subsequent studies (e.g., Steinberg, Fegley, and Dornbusch 1993; Ploeger 1997; Staff and Uggen 2003), recent evidence from more rigorous designs suggests that the association is likely to be spurious rather than causal (Apel et al. 2008; Staff et al. 2010).

The focus of this review is on life-course studies of employment and crime. The relevant research engages longitudinal data at the individual level of analysis. In basic terms, we are interested in studies that examine criminal behavior from the point of view of employment transitions: job entries and job exits. Thus, cross-

sectional and aggregate-level studies of (un)employment and crime are outside the scope of this chapter. The review draws predominantly on quantitative results, but we discuss evidence from qualitative life-course studies as well. The purpose is to provide an informed assessment of the state-of-the-art scholarship. Although we wish to be comprehensive, the goal is not to mention every longitudinal study that has examined employment and criminal outcomes. Instead, the focus is on key studies with an emphasis on evidence on causality. Thus, we prioritize studies that are methodologically sophisticated with respect to causal inference and theoretical mechanisms.

The chapter is organized into two main sections. Section I reviews studies that examine the capacity of employment (job entries) to curb criminal involvement. There are strong theoretical reasons to expect transitions to stable employment to contribute to the desistance process. Section II reviews life-course studies of unemployment effects on criminal behavior (job exits). Section III concludes the chapter by summarizing the evidence and discussing implications for the next generation of studies on employment and crime.

I. Job Entries: Does Employment Reduce Crime?

A. Experimental Evidence

From the perspective of causal analysis, employment is an advantageous variable because it lends itself to random assignment. To study the effect of, say, marriage on criminal offending, it would not be feasible to randomly “assign” spouses for unmarried offenders. By contrast, not only are experimental designs possible in studies of job effects, there is an extensive literature evaluating the effectiveness of

labor market programs on criminal offending, and some of those studies use data from randomized controlled trials (Wilson, Gallagher, and MacKenzie 2000; Bushway and Reuter 2001; Visher, Winterfield, and Goggeshall 2005).

Overall, the results from job experiments have been disappointing. According to Bushway and Apel (2012, p. 28), “the most defensible conclusion from experimental evaluations of work programs is that the programs have a dismal record of jointly improving employment outcomes and of lowering recidivism.” A meta-analysis of the effects of employment programs on recidivism echoes this pessimistic conclusion: following the protocol endorsed by the Campbell Collaboration, Visher, Winterfield, and Goggeshall (2005) identified eight methodologically rigorous randomized controlled trials. The average effect size calculated from this set of studies was not statistically different from zero, which implies that employment programs tend to have no impact on offending. Only one of the eight studies reported a statistically significant negative effect on recidivism (Uggen 2000), but this effect was limited to the older members of the sample and based on data collected in the 1970s.

Because age is likely related to increased motivation to “go straight,” the result could mean that employment programs are effective among sufficiently motivated clients (Uggen and Wakefield 2008). To temper this conclusion, many of the relevant programs require evidence of motivation or readiness to change as a prerequisite for participation. For example, in order to be eligible for Job Corps, one of the programs included in the meta-analysis, applicants must “be free of serious behavioral problems” and “have the capability and aspirations to participate

in Job Corps.” Despite these and nine additional screening criteria, Job Corps has not proven to be effective in curbing crime among participants with prior criminal history (Visher, Winterfield, and Goggeshall 2005).

Using the data from the same 1970s experiment as Uggen (2000), a recent study by Uggen and Shannon (2014) focused on participants who were heavy drug users. In this hard-to-employ population, participation in supported work was found to reduce predatory property crime (robberies and burglaries) substantially but had no impact on drug use. The finding is interesting because it suggests that the effect of employment may be crime-dependent. It makes sense that having access to steady income would reduce motivation for economic crime. An optimistic interpretation is that work helps transform drug use from a crime problem into a health problem. On the other hand, if employment sustains the drug habit, there is a strong possibility these individuals will eventually lose their jobs and presumably recidivate as predatory offenders. Also, Uggen and Shannon (2014) did not examine other forms of property crime, such as theft. Perhaps participation in employment provides opportunities to steal valuable items, making it less necessary for motivated offenders to commit robberies or burglaries—crimes that are arguably more risky than, say, embezzlement or larceny.

Although randomized experiments are methodologically compelling, a major limitation with the evidence from job programs is that the jobs they provide tend to be of low quality (Uggen and Wakefield 2008; Apel and Horney 2017). They are almost invariably what Crutchfield (1995) has described as “secondary-sector” jobs, i.e., menial jobs that offer low pay, no health care benefits or

retirement plan, and limited opportunities for career advancement. As discussed earlier, most criminological perspectives expect jobs to reduce offending only in so far as they increase bonds to conformity and/or increase the economic benefits of legitimate work relative to the gains from crime. To appreciate the importance of job quality in the desistance process, consider the life history of Wes Moore (Moore 2010), a persistent offender from the Baltimore area who enrolled in Job Corps, a program that has been described as “the most prominent” residential program providing vocational and life skills training, general education, and job placement after graduation (Bushway and Reuter 2001, p. 210).

Wes Moore learned about Job Corps through a friend who was about to enter the program. At this time of his life, Wes, too, was ready to turn his life around: “I’m done, man. I want to get out. Do something different with my life. But I’m not sure what. I’m not going back to high school. I’m too old for that. But I’m tired of running these streets” (Moore 2010, p. 138). This quote conveys heightened awareness of being in the wrong path and increased openness to change. In the vernacular of desistance research, Wes was going through cognitive transformation (Giordano, Cernkovich, and Rudolph 2002)—a perfect moment for entering a high-end program targeting disadvantaged dropouts like him. When he arrived at the campus of the Job Corps Center in Maryland, he could not have been more impressed with the physical setting, a sharp contrast to the streets of the ghetto. He excelled as a student, earning his GED near the top of his class. He selected carpentry as his vocational specialty and enjoyed “the quiet thrill of a job well done” (Moore 2010, p. 142). After seven months in the residential program, Wes

graduated from Job Corps and was excited to face the real world. However, despite doing everything right in the program, finding consistent work proved difficult.

Wes moved from landscaping to home construction to working in a food court of a shopping mall. None of these jobs paid living wages: “He worked ten hours a day and came home with barely enough energy to play with his kids and barely enough money to feed and clothe them” (Moore 2010, p. 144). After trying to stay straight for more than a year, the temptation of making money from cooking and selling crack proved too hard to resist, and eventually Wes resumed his criminal activities. Soon after, he was arrested and convicted for his (alleged) involvement in an armed robbery of a jewelry store during which the security guard was killed. Wes Moore was sentenced to life in prison without the possibility of parole.

Aside from being tragic, the story of Wes Moore is instructive in many different ways, but here our focus is on the observation that his promising path toward desistance was interrupted by the failure of the labor market to provide him with an opportunity to support his family through hard work. The effectiveness of Job Corps in reducing criminal offending has been evaluated in a nationally representative randomized trial (Schochet, Burghardt, and Glazerman 2001). This research found a small beneficial treatment effect in the general client population but no statistically significant differences between the control and the treatment groups among those with prior convictions. Consistent with the experience of Wes Moore, this otherwise model program was found to have one major shortcoming: “placement services provided after participants left the centers were found to be

limited in scope and substance” (Schochet, Burghardt, and McConnell 2008, p. 1866). Recent evidence from a randomized re-entry experiment suggests that employment programs are more effective when bolstered with a comprehensive system of services that not only prepare ex-offenders for the labor market but continue to support them with equal vigor during the transition process (Cook et al. 2015). The research by Cook et al. (2015) evaluated the effectiveness of the Milwaukee Safe Street Prisoner Release Initiative (PRI) and found improvements in both employment and recidivism outcomes in the treatment group. The unique aspect of the PRI was the provision of frequent and multi-systemic assistance to the clients during the critical post-release period.

B. Work as a Turning Point: The Effect of Good Jobs

Sampson and Laub’s (1993) age-graded theory of informal social control is arguably the most influential contemporary life-course theory of desistance from crime. As the term “age-graded” suggests, this perspective is focused on explaining changes in the criminal trajectory at different stages of the life course. Although Sampson and Laub do not deny the role of stable individual differences (e.g., low self-control) in criminal behavior, their goal is to elucidate the social-environmental processes that sustain or modify the effect of such propensities. As far as desistance, the age-graded theory has emphasized the role of marriage and employment as two adult social bonds with the potential to redirect behavior away from crime. A key aspect of the argument is that the level of bonding must be sufficiently strong and of high quality in order to contribute to the process of desistance.

With regard to employment, the initial research by Sampson and Laub (1993) underscored job stability as an influential factor in promoting desistance: having a stable job in early adulthood was found to restrain criminal offending at later stages. The relevant regression models were estimated controlling for individual differences in cognitive ability, antisocial traits, and other potential sources of spuriousness. However, critical examinations of Sampson and Laub's (1993) index of job stability have revealed that labor market bonding was not the only thing it measured (Uggen 2000; Wright and Cullen 2004). One of the items included in this construct is "work habits." It indicates the level of reliability and effort in the job performance and the degree to which the employer considered the subject as "an asset to the organization" (Sampson and Laub 1993, p. 144). As observed by Uggen (2000, p. 531): "If employment effects are conditional on good work habits, the putative 'job effects' are tainted by 'person effects' or pre-existing worker characteristics." In other words, instead of measuring job stability, this index appears to capture individual differences in such characteristics as maturity, sobriety, and motivation.

Attempts to replicate the employment effects reported by Sampson and Laub (1993) have produced mixed results. Using contemporary data on juvenile offenders in the United States, Giordano, Cernkovich, and Rudolph (2002) found no association between job stability and offending rates. On the other hand, studies using nationally representative data from the United States (Wright and Cullen 2004) and Finland (Savolainen 2009) report longitudinal evidence of reduced offending as a function of labor market attachment. Although the Finnish study did

not attend to employment quality, the argument was made that in a generous welfare state, such as Finland, where even the chronically unemployed are guaranteed a decent standard of living, choosing to work in the absence of economic necessity can be interpreted as a signal of job quality. Another European study from the Netherlands found that “regular” jobs were more effective than jobs acquired through a temporary work agency (van der Geest, Bijleveld, and Blokland 2011). As the jobs of the former variety are associated with better prospects of long-term employment, this result is consistent with the age-graded theory of informal social control. On the other hand, this same study did not find any association between employment duration and offending rate. It may be that the “regular” jobs are not only more stable but also more rewarding in terms of pay as well as non-economic attributes (Wadsworth 2006).

Evidence from ethnographic research suggests that regular paycheck may not be the only mechanisms that helps offenders remain in the straight and narrow (Laub and Sampson 2003). One of the “persistent thieves” interviewed in Shover’s (1996) monograph underscores the importance of interpersonal bonding with a conventional other as the key factor in his employment experience: “The guy liked me from the jump. And that’s when I hooked up with him. And I went straight a long time without the intentions of going straight” (Shover 1996, p. 127). Similar to quantitative studies of observational data, ethnographic research offers conflicting accounts concerning the role of employment in desistance. Giordano, Cernkovich, and Rudolph (2002, p. 1033) report that both male and female respondents “were very unlikely to build a story of change around the development of a rewarding

career, and only a few focus heavily on stable employment.” Maruna (2001, p. 25) has argued that it would be unrealistic to expect employment to trigger self-transformation among most addicts and offenders.

C. Selection Bias in Observational Studies

If the results from labor market experiments have been mostly dismal, the evidence from observational studies is mixed. Some studies find evidence that work matters while others do not. The main challenge for non-experimental research is addressing selection bias. The decision to seek employment, the employer’s decision to hire a person, and the worker’s ability to hold on to a job are not random outcomes but exhibit a great deal of selectivity. It is safe to say that an offender who gets a job and manages to stay employed is different from an offender that fails to do so. For example, we might expect the employed offender to be more motivated to change, less likely to struggle with addiction problems, and more likely to have children.

Traditional methods of multivariate analysis reduce selection bias by adjusting for differences in observable characteristics presumed to influence the association between employment and crime. For example, in their research showing a link between stable employment and desistance, Sampson and Laub (1993) estimated models controlling for differences in childhood antisocial tendencies, family socioeconomic status, educational attainment, prior offending, and many other potential sources of bias. In addition to using control variables, the study by Savolainen (2009) reduced selection bias by focusing on a homogeneous sample of offenders, all of whom were weakly tied to conventional institutions

(including the labor market) and had similar criminal histories at the start of tracking. Propensity score matching is a more sophisticated way to harmonize the employment and comparison groups. Under this technique, the comparisons of the employment effect are limited to cases that are individually matched using information from observable covariates, and those without an acceptable match are excluded from the analysis (Apel and Sweeten 2010).

Although these approaches are useful, it is ultimately unrealistic to expect any data set to be able to capture all the relevant differences that might bias the comparisons. As a matter of logic, critics may always point to variables that were not held constant or included in the matching procedure. Unobserved heterogeneity is the technical term for this problem. Cognizant of the limits of between-individual comparisons, a number of scholars have gravitated to designs in which the employed individual him- or herself serves as the comparison case. This is possible with sufficiently frequent longitudinal observations of within-individual change over time. For example, using data generated by retrospective life-history calendars over a three-year period, Horney, Osgood, and Marshall (1995) linked monthly changes in life circumstances, such as employment, to self-reported rates of criminal offending in a sample of men released from prison. In this path-breaking study, within-individual changes in employment status were unrelated to all measures of criminal activity except one. Contrary to expectations, the odds of property crime were higher during periods of employment. As discussed above, in the context of Uggen and Shannon (2014), it is possible that, for crime-prone individuals, work settings present superior opportunities for theft and other non-

predatory property offenses. (Because studies that examine within-individual changes in employment status can be understood as studies of job exits as well as job entries, we discuss this literature in more detail in the next section, which is dedicated to unemployment effects.)

Focusing on within-individual change is an effective way to eliminate selection bias in such time-stable characteristics as cognitive ability, personality, educational failure, and exposure to childhood maltreatment. However, as most scholars recognize, this approach is limited in its ability to address unobserved heterogeneity in time-variant individual differences, such as motivation or recovery from addiction. To illustrate this point, consider a recent within-person analysis of life history calendar data from the Second Nebraska Inmate Study (Apel and Horney 2017). In an effort to shed light to the mechanisms responsible for the inverse association between employment and crime, the study examined which job characteristics mediated the association. The results showed that objective job characteristics (income and hours worked) did not matter, but that the person's subjective commitment to the job did. Although it is possible, as the authors seem to assume, that increased commitment was caused by the employment experience (such as bonding with coworkers), the results cannot rule out the possibility that these individuals express more commitment to their jobs because they are more committed to changing their lives, and they realize employment supports this goal. Thus, it is possible that commitment to change precedes the decision to become and remain employed. As we demonstrate below, because results from intra-individual models compare average rates of offending between states of employment and

unemployment, they are unable to demonstrate the correct time order between the employment event and the onset of desistance.

To appreciate the point about time order, consider, once more, the case of Wes Moore. Recall that he enrolled in Job Corps after a period of initial desistance and then cleaned up his act by the time of program graduation. However, after a series of failed attempts to secure stable employment with living wage, he returned to selling drugs and eventually became involved in a robbery-homicide. The criminal trajectory related to this sequence of events is depicted in [Figure 24.1](#), along with a trajectory representing the turning point hypothesis derived from the age-graded theory. The two pathways are very different. The curve representing the turning point hypothesis depicts active but fluctuating levels of criminality during the pre-employment period, followed by gradual but steady decline in the offending rate during the employment period. This pattern is consistent with the hypothesis that employment has a causal effect on desistance. The curve describing Wes Moore's offending trajectory is clearly inconsistent with the causal effect because his offending increases after the employment transition. However, if we compare the rates of criminal offending between the two trajectories, we find that, in each situation, the average rates (represented by the dotted horizontal lines) are lower in post-employment period than on the pre-employment period. In this example, the amount of reduction (i.e., the effect size) is identical across the two hypothetical trajectories.

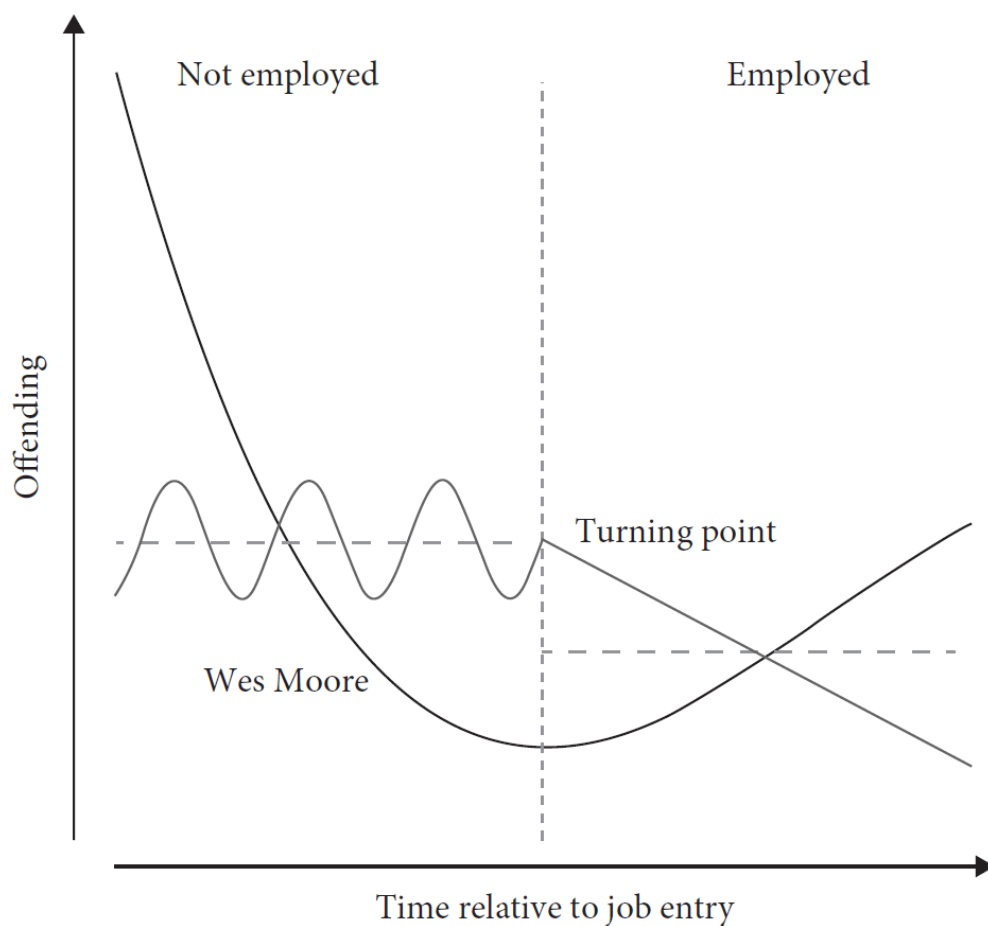


Fig 24.1. Two Ideal- Typical Offending Trajectories Around the Point of Job Entry: Wes Moore and The Turning Point Process. (The Dotted Line Shows the Average Offending Rate During the Pre- and Post- Employment Periods.)

The point of this example is to show why standard methods of estimation used in observational research are inadequate for addressing a key element of causal inference—the timing of change in the criminal trajectory. In order for a job to qualify as a turning point it is obvious that the employment transition must take place before desistance rather than vice versa. This critical issue was addressed in an article by Skardhamar and Savolainen (2014). Using monthly data on recidivist

male offenders from Norway, the authors examined changes in criminal trajectories around the point of entry to stable employment (defined as a job lasting for a minimum of 6 months). The results showed that most offenders had desisted long before the employment transition and that becoming employed was not associated with additional reductions in criminal behavior. The study further examined if these patterns varied depending on the age of the offender and the stage of the criminal career. They did not observe meaningful evidence of age interaction but were able to identify a subset of offenders who became employed during an active phase of the criminal career, and who experienced substantial reductions in criminal offending thereafter. To the extent this can be interpreted as evidence in support of the turning point hypothesis, it is marginal to say the least: this trajectory described less than 2 percent of the sample. The main conclusion from this research is that that transition to employment is best viewed as a consequence rather than a cause of desistance.

In a more recent contribution to this literature, Loughran, Nagin, and Nguyen (2016) attempted to salvage the turning point hypothesis by emphasizing the gradual and interdependent nature of the association between legal employment and criminal offending. Modeling desistance as a discrete-time Markovian process, they found that the probability of “transitioning to a crime-free state” increased with the number and length of prior employment periods. In other words, the results show that the longer or more frequently the individuals were working in legal employment, the more likely they were to end up desisting from crime. However, as the authors acknowledge, these results have no direct bearing on the

causal effect on employment crime since the reported pattern does not rule out the selection hypothesis.

Instead, the main implication seems to be that if employment does have an effect on crime, this effect is likely to unfold gradually via a mutually reinforcing process. This kind process is not, however, consistent with the turning point hypothesis, which does indeed assume a gradual process of desistance, but only in response to an exogenous “triggering event,” such as a transition to marriage or employment (e.g., Laub, Nagin, and Sampson 1998).

The process described by Loughran, Nagin, and Nguyen (2016) is rather more consistent with the interactionist theory of desistance advanced by Massoglia and Uggen (2010), which, similar to Loughran, Nagin, and Nguyen’s (2016) research, is focused on adolescent transitions to adulthood. By contrast, the focus of the turning point hypothesis is on adult offenders’ disengagement from career criminality. Thus, contrary to what the authors claim, none of the evidence presented in their research “calls into question the conclusions of Skardhamar and Savolainen” (Loughran, Nagin, and Nguyen 2016, p. 49) because, consistent with the turning point hypothesis, that research was concerned with investigating the timing of change in criminal offending vis-à-vis a transition to stable employment—a hypothetical turning point—within a population of recidivistic adult offenders.

II. Job Exits: Research on Unemployment and Crime

Although criminological research on unemployment is extensive, there are surprisingly few studies with an explicit focus on job loss as a life-course event.

This may have to do with the fact that most longitudinal studies of crime are based on high-risk (offender) samples where unemployment is the “normal” starting point. Whatever the reason, life-course criminologists have been far more interested in estimating the effect of employment on desistance than the effect of unemployment on offending. As a notable exception from the early days of modern life course criminology, Farrington et al. (1986) analyzed the crime rates of late adolescent males in the Cambridge Study, finding that levels of property crime were higher during unemployment, while rates of violent crime did not vary by employment status. Using birth cohort data from New Zealand, Fergusson, Horwood, and Woodward (2001) examined the longitudinal within-individual association between unemployment and a number of problem outcomes during late adolescence and emerging adulthood. Similar to the Farrington study, they found rates of property crime to be higher during periods of unemployment. As noted in the previous section, studies of within-individual change can be understood as estimating either the effects of employment or unemployment. More often than not, the relevant studies are not framed as studies of “job entries” or “job exits,” as they simply examine the time-varying association between (un)employment and offending using one of the two possible states as the focal category.

Research on short-term changes in “local life circumstances” have utilized the life history calendar method to create longitudinal data sets. Using monthly data from Second Nebraska Inmate Study (Horney 2001), Felson et al. (2012) found unemployment to be positively related to drug dealing but unrelated to violent and property crimes during the three-year observational period. In the same study, a

measure of financial stress (which could be related to employment quality) was associated with increases in both drug dealing and property crime. Slocum et al. (2005) examined monthly changes in employment and crime in a sample drawn from a women's detention center and found rates of property crime and drug use to be higher during times of unemployment. Meanwhile, Griffin and Armstrong (2003) found that only drug dealing increased during periods of unemployment in their female jail inmate sample. Piquero, MacDonald, and Parker (2002) examined within-individual changes in employment status and violent and non-violent offending during a seven-year follow-up, finding a positive association between unemployment and violent offending only for whites; none of the other associations were statistically significant. In their updated re-analysis of the Glueck study, Laub and Sampson (2003) found levels of both alcohol/drug crime and predatory crime to be higher during unemployment in a 16-year follow-up of 419 delinquent men. However, as they point out, because the reference category includes not just employment but also incarceration, the results could be inflated (Laub and Sampson 2003, p. 270).

Drawing on individual-level data from administrative records, Aaltonen et al. (2013) examined a large sample of Finnish men ($n = 15,658$) with at least one recorded criminal offense over a six-year observational period. Results from fixed-effects regression models of within-individual change showed that unemployment was associated with higher levels of property crime but unrelated to violent crime or drunk driving. This study also found evidence suggesting that the duration of unemployment was positively related to property crime. Similar patterns were

observed in a Dutch study (Mesters, van der Geest, and Bijleveld 2014), where regular employment (but not temporary employment) was negatively associated with property crime but unrelated to violent crime. Attending to the duration of both employment and unemployment, and using both random and fixed effects models, Verbruggen, Blokland, and van der Geest (2012) found employment to be associated with lower levels of offending among males and females, but employment duration was significant only for males. On the other hand, the duration of unemployment was associated with increased offending among females but decreased offending among males. According to the authors, this surprising finding may be caused by the higher likelihood of institutionalization (incapacitation) among the chronically unemployed men.

Unlike with job entries, there are no randomized experiments manipulating job exits. The reasons are obvious: it would be highly unethical to ask employers to fire their workers in order to study the effects of unemployment. However, some scholars have used data generated from natural experiments to approximate a randomized design (Mustard 2010). Most of these studies have been conducted at the aggregate level and thus fall outside the scope of this essay. For example, Raphael and Winter-Ebmer (2001) used state-level measures for defense contracts and exposure to oil price shocks as instruments for state-level unemployment rate and found that levels of property crime increased when unemployment rates went up as a consequence of these exogenous shocks. We are aware of two individual-level studies of unemployment and crime that exploit data from natural

experiments. Both are based on administrative microdata tracking officially recorded criminal offending before and after large-scale layoffs.

Using data from Norway, Rege et al. (2014) showed that men who became unemployed as a consequence of plant downsizing had higher offending rates after job displacement compared to men working in similar plants who did not experience layoffs (see also Rege et al. 2009). Although the relative increases in crime rates were large, the base rates were low in this normal population study. For example, the 60 percent increase in property offending was produced by a change from approximately 0.1 to 0.16 crimes per 100. Using similar data from Denmark, Bennett, and Ouazad (2016) examined changes in crime among individuals who had lost their jobs as a result of mass layoffs (defined as a termination of more than 30 percent of the employees). This study identified causal effects similar in magnitude to the Norwegian study. A comparison of persons who were laid off and those who kept their jobs showed no differences in pre-displacement levels of recorded offending, indicating that the layoff experience was not systematically related to individual differences in criminal propensity. Among those who lost their jobs, the offending rate increased by 0.5 percentage point and remained at a higher level during the entire four year tracking period. Most of this effect was related to increased involvement in property crime by individuals with low educational attainment. This pattern points to material hardship as the mechanisms accounting for the increased offending levels. Together, these two Scandinavian studies offer the most compelling evidence to date regarding the causal effect of unemployment on criminal behavior at the individual level of analysis.

Scholars in the Nordic countries have also examined the link between unemployment benefits and crime. A Danish evaluation study of active labor market programs (ALMPs) found that individuals randomly assigned to the treatment group receiving more intensive activation measures (including a 2-week job search program and more meetings with caseworkers) committed fewer crimes than those in the control group of standard ALMP (Andersen 2012). Similar results were obtained in a Finnish (non-experimental) study that showed that the same individuals committed more property crimes during “passive” unemployment periods when compared to activation periods (Aaltonen et al. 2013). These Nordic results are consistent with the previously noted evaluation study from Milwaukee showing the potential for employment to reduce recidivism when coupled with enduring and comprehensive assistance toward successful labor market attachment (Cook et al. 2015).

III. Conclusion

The assumption that employment reduces crime is both logical and attractive. As noted above, employment is an outcome that can be targeted through realistic policy interventions. It would be comforting if it turned out that problems of crime and violence can be addressed effectively by offering training and jobs for individuals with antisocial tendencies. From the perspective of public good, turning criminals into taxpayers amounts to a win-win proposition. Unfortunately, evidence from life-course criminology finds little support for the hypothesis that work reduces crime.

The evidence from observational (i.e., non-experimental) studies is mixed. The most consistent finding emerging from the literature suggests that employment is associated with reductions in economic crime, although notable exceptions to this generalization exist. This finding is intuitive: given that work provides a source of income, there is less need to resort to crime as a means to earn a living or to finance a drug habit, for example. On the other hand, it is not surprising if changes in employment status are weakly related to interpersonal violence given that most jobs do not prevent aggressive people from having disputes or developing grievances with other people (Felson 2014). However, the main shortcoming of observational studies is their inability to demonstrate causality. Studies of within-individual change are superior to between-individual comparisons because they rule out the confounding influence of unobserved heterogeneity in time-stable individual differences, but this is not sufficient because such time-varying characteristics as motivation or maturation are likely to bias the longitudinal association between employment and criminal offending.

As long as the research design allows for systematic (i.e., non-random) selection into employment, the problem of unobserved heterogeneity is going to be a factor. However, it is a problem that can be managed more effectively with additional investment in data collection. Most criminological studies of within-individual change rely on two kinds of sources: life-history calendars and official statistics. The former are typical in the Anglo-American context dominated by survey data, while scholars in northern European countries (e.g., Finland, Norway, and the Netherlands) are able to create individual-level data sets drawing on

interlinked population registries (Lyngstad and Skardhamar 2011). If the purpose of research is to establish associations between objective life circumstances, such as employment, and criminal offending, both of these methods of data collection are good options. However, it is clear that data available from population registries are limited in their capacity to measure changes in motivation, maturity, and other time-varying psychological characteristics assumed to influence the association between employment transitions and desistance. These types of measures can be included in life history surveys, but we suspect that retrospective self-reports of psychological change are unreliable because one's life circumstances at a given time are likely to bias subjective assessments of pre-transition psychological states. For example, all else equal, a person who is currently working in a stable job is likely to see him- or herself as having been more motivated prior to the employment transition than a person who remained unemployed or lost their job (Massoglia and Uggen 2010).

Thus, as an improvement to the status quo, we propose prospective data collection of short-term change in both objective life circumstances and psychological states implicated in theories of desistance. Although this approach would not eliminate the problem of unobserved heterogeneity, it could reduce it significantly. For example, a prospective study that controls for weekly changes in motivation to “go straight” is less vulnerable to the claim that the association between employment and desistance is spurious than a study that fails to attend to this confounder. Moreover, in addition to addressing spuriousness, this analytic strategy could be used to examine the mechanisms expected to produce the

employment effect: changes in peer context, income, time use, etc. Methodological examples of this kind of research can be found in the literature on college drinking and its consequences (Patrick and Maggs 2008; Patrick, Maggs, and Lefkowitz 2014).

Although observational data are not ideal for demonstrating causality, they can be used productively to evaluate causal claims. This point is illustrated well by Skardhamar and Savolainen's (2014) study focusing on the timing of employment entries in the criminal trajectory. In order for employment to serve as a turning point, or to assist in the desistance process, we should observe reductions in criminal offending after the transition to employment. This basic point was largely ignored in prior research focusing on changes in the average offending rate (see [Figure 24.1](#)). The evidence from Skardhamar and Savolainen (2014) showed that an overwhelming majority of offenders had desisted from crime before they were able to make transitions to stable employment. Moreover, their analysis found virtually no evidence of reduced offending following the point of job entry. Because a single study is never a sufficient foundation for drawing firm conclusions, we encourage replications of this style of analysis using data from different national contexts. We also call for an end to studies that ignore the timing component in the longitudinal association between employment and crime.

In terms of methodological rigor, randomized controlled trials are the best option to study the causal effect of employment on crime. As discussed above, the weight of experimental evidence favors the null hypothesis by an overwhelming margin, especially among studies focusing on individuals with prior criminal

history. There is some evidence that employment may protect at-risk individuals from criminal temptations, but even these effects are relatively small. Although it would be a mistake for life-course criminologists to ignore this literature, results from experimental research are devastating only if you assume that merely providing jobs to crime-prone individuals will have a significant impact, regardless of the quality of the job and the readiness of the person. We find this assumption naïve and inconsistent with most theories of criminal desistance.

There is occasional evidence suggesting that, under the right conditions, participation in high-quality training and employment programs may help individuals reduce their involvement in crime (Uggen and Wakefield 2008; Bushway 2011; Andersen 2012; Cook et al. 2015). The challenge for future research is to specify these conditions. As an innovative step, Bushway and Apel (2012) have proposed using evidence of clients' performance in an employment program as an objective signal that could be used to sort "good bets" from those who may not be ready to take on steady job. In addition, as illustrated in the case of Wes Moore, once the "good bet" has been identified, it is important to connect the person to a job with qualities that help sustain the process of desistance. Given that employment quality has been widely recognized as an important factor, it is striking how little attention has been devoted to this issue in either experimental or observational research. Following Sampson and Laub's (1993) original formulation, most studies have focused on job stability as the only aspect of employment quality. It would seem that such additional characteristics as wages,

autonomy, benefits, and how well the job matches the worker's skills and aspirations merit more systematic scrutiny in future research (Wadsworth 2006).

While the evidence for the crime restraining effect of becoming employed is weak, the best available evidence—from two quasi-experimental studies—supports the assumption that involuntary unemployment increases the rate of criminal offending (Rege et al. 2014; Bennett and Ouazad 2016). This observation suggests that the effect of employment on crime may be asymmetrical (Lieberman 1985). In other words, the causal effect of employment may be limited to a situation where regularly employed people become criminally active when coping with reduced income and increased leisure. Employment is far less likely to have a crime-reducing effect on the behavior on antisocial individuals whose personal history indicates weak attachment to the labor market.

In conclusion, life-course research on employment and crime remains a vibrant field of inquiry. The literature has made a great deal of progress in the past 30 years or so. The current state of knowledge offers limited evidence of causality in the longitudinal association between employment transitions and rates of criminal offending. However, there are several ways in which the evidence base can be improved by the next generation of studies. We find this agenda worth pursuing because it has the potential to inform policies that are politically feasible, cost-effective, and morally superior to the more punitive alternatives.

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