Employment after incarceration: exploring the influence of cumulative disadvantage on multiple employment outcomes

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Employment after Incarceration: Exploring the influence of cumulative disadvantage on multiple employment outcomes

by

Amanda D. Emmert

A Dissertation
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ABSTRACT

Prisons and jails across the Unites States release more than 600,000 inmates each year. Scholars have posed and addressed numerous concerns for the reintegration outcomes and prospects that face ex-inmates. Yet, little is known about the cumulative employment and economic disadvantages faced by ex-inmates who experienced their first incarceration at early ages or experience multiple incarcerations throughout their lives. Using event history and fixed effects analyses on longitudinal data from the Rochester Youth Development Study (RYDS), this project explores employment acquisition, employment tenure, employment stability, and use of public assistance for signs of differential ex-inmate disadvantages. While cumulative disadvantage is not universally evident, disaggregating ex-inmates based on incarceration history is fruitful, and this study’s findings contribute to our understandings of previous empirical research.
CHAPTER 1. Introduction

A concerning cycle exists in the criminal justice system. Research suggests that incarceration has negative effects on the employment outcomes of ex-inmates (Apel and Sweeten 2010; Bushway 1998, 2004; Freeman 1992; Grogger 1995; Holzer 1996; Hunter and Borland 1997, 1999; Kerley and Copes 2004; Lopes, Krohn, Lizotte, Schmidt, Vásquez, and Bernburg 2012; Sampson and Laub 1993; Waldfogel 1994; Western and Beckett 1999; Western 2002; Witte and Reid 1980). While the relationship between employment and recidivism is complex, employment has the potential to reduce criminal behavior and recidivism. Therefore, reliance on incarceration as a punishment or containment method of crime prevention can inadvertently hurt the employment outcomes of ex-inmates and produce more crime.

Previous research establishes that incarceration negatively affects a breadth of employment outcomes. For example, incarceration between the ages of 17 and 23 is negatively correlated with work commitment, employment, and continuity of employment at ages 25 to 32 (Sampson and Laub 1993). Similarly, ex-inmates earn 10 to 20 percent lower wages than their never-incarcerated peers (Kling 2006; Western 2002), and the discrepancy in wages and wage mobility between ex-inmates and never-incarcerated individuals can persist for over a decade after initial incarceration experiences (Freeman 1992; Sampson and Laub 1993; Western and Beckett 1999).

Despite the attention scholars have paid to employment post-incarceration, a few areas have yet to be explored. First, previous studies have explored both limited and broad samples to observe the effect of arrest, conviction, and incarceration on employment outcomes. As potential turning points in ex-inmates’ life courses, understanding the effect of each of these factors on employment is important. However, two aspects of life course theory, the influence of cumulative disadvantage and the age at which ex-inmates experience their first life course
interruptions due to incarceration, have yet to be addressed when considering the effect of incarceration on ex-inmates' employment outcomes.

While previous studies have employed numerous different sampling restrictions and controls, each has been limited in its ability to assess cumulative disadvantage. For example, some studies have limited their samples to concentrate on the effect of first time incarceration on employment (see Apel and Sweeten 2010; Kerley and Copes 2004; Lalonde and Cho 2008; Pettit and Lyons 2009), while others have sampled ex-inmates regardless of the number of times they have experienced incarcerations (see Freeman 1992; Kling 2006; Loeffler 2013; Lyons and Pettit 2008; Western and Pettit 2000; Western 2002, 2006). The disadvantage of limiting samples and populations in these ways is that neither sample can address whether ex-inmates experience cumulative disadvantages. This is because studies of first time ex-inmates only address employment after the first incarceration, and studies of ex-inmates, regardless of the number of incarcerations experienced, deliver estimates of the average employment outcome across all ex-inmates. A few studies address life course concerns by limiting their samples to specific age groups (see Freeman 1992; Sampson and Laub 1993). However, without the ability to compare the employment outcomes of individuals who experience their first incarceration at a young age to those who experience it at older ages, it is impossible to ascertain whether employment outcomes differ based on age of interruptions in the life course.

By disaggregating the incarceration sample based on the number of incarcerations ex-inmates have experienced, it is possible to measure whether cumulative employment disadvantage occurs with additional incarceration experiences. A simplified study of cumulative disadvantage is also possible when ex-inmate samples are disaggregated based on whether they have experienced one or multiple incarcerations.
Similarly, previous research has only briefly considered the influence of incarceration age on employment outcomes (see Emmert 2014; Sampson and Laub 1993). For example, Pettit and Lyons (2009) and Witte and Reid (1980) measure the influences of ex-inmates’ age at their most recent incarceration admission and age of incarceration release (respectively) on employment outcomes. Yet, there is equal, if not greater, significance in measuring the age at which individuals begin their first incarcerations. Age at first incarceration indicates a crucial life course interruption, signifying the moment human capital attainment stops, slows, or stalls. Kerley and Copes (2004) emphasize that contact with the criminal justice system at younger ages is especially devastating, as harm accumulates as individuals age. Moreover, the finding that individuals incarcerated at younger ages experience more non-employment than individuals incarcerated later in life (Emmert 2014), suggests that age at first incarceration may prove to predict ex-inmate employment outcomes more successfully than measures of admission age or release age.

As such, post-incarceration employment literature still has several avenues for growth. I propose to expand the current state of research by exploring the research questions below.

**Research Question 1**

What are the effects of cumulative incarceration experiences and age at first incarceration on ex-inmates’ employment acquisition rates?

**Research Question 2**

How do cumulative incarceration experiences and age at first incarceration affect the employment stability and tenure of ex-inmates?
Research Question 3

Do cumulative incarceration experiences and age at first incarceration affect how long individuals spend relying on public assistance, and do ex-inmates resort to public assistance at faster rates than never-incarcerated individuals do?

The remainder of this chapter outlines theoretical and empirical concerns regarding the influence of employment on recidivism, and the effects of incarceration on employment outcomes. More specifically, this project outlines how social bonding/control theory and strain theory explain the influence of employment on crime, and draws on life course perspective, stigma, and loss of human capital as foundations for post-incarceration negative employment outcomes. Similarly, I address the current state of literature regarding employment following arrest, conviction, or incarceration.

Theories and Literature Regarding Employment and Crime

Numerous theories establish theoretical foundations for studying the influence of incarceration on employment outcomes. Social bonding/control theory posits that establishing healthy social bonds during adolescence through participation and commitment in institutions such as employment encourages conformity to social norms and prevents participation in deviant behaviors (Hirschi 1969). Sampson and Laub (1993) suggest that social bonds can also redirect adult life course trajectories away from crime. Alternatively, employment and crime may exist as alternative approaches to achieving financial goals. As such, the financial compensation achieved through employment can prevent individuals from pursuing illicit activities that pay well. Below I discuss social bonding/control theory and financial rewards theory in detail, before exploring empirical findings on the matter.
Social Bonding/Control Theory

Social bonding/control theory suggests that attachment, commitment, involvement, and belief are four elements that bond individuals to “conventional society” (Hirschi 1969). Scholars often attribute the positive influence of employment over adolescents and adults behaviors to the elements of commitment and involvement. Commitment emphasizes that when individuals commit their time, energy, and effort towards conventional activities and institutions, such as employment, they are less likely to jeopardize their position and achievements with criminal activities (Hirschi 1969). Similarly, involvement in conventional activities and employment leaves little time for individuals to become involved in delinquent and criminal behaviors (Hirschi 1969). As such, social bonding/control theory suggests that commitment and involvement in legal employment opportunities deter individuals from participating in criminal activities (Hirschi 1969).

However, it is also important to note that commitment and involvement in conventional activities can also serve as the framework for establishing attachment. Attachment highlights the positive influence others have over our behaviors when our affection, admiration, and close identification with others lead us to care about others’ expectations of us (Hirschi 1969). Participation in conventional activities can encourage adolescents and adults to bond or form attachments to individuals who explicitly or implicitly encourage them to conform to conventional behaviors and avoid delinquency and crime.

While Hirschi’s (1969) social bonding/control theory focused on the influence of attachment, commitment, involvement, and beliefs on adolescent conformity, scholars have suggested that the theory applies to adult conformity also (see Sampson and Laub 1990, 1993; Uggen 1999, 2000; Wadsworth 2006). For example, Sampson and Laub (1993) suggest that the
informal social bonds that adults form or strengthen through family and employment can alter
criminal trajectories in adulthood and result in decreased participation in crime.

**Financial Rewards**

An alternative theory suggests that employment and crime are simply alternative
approaches to achieving financial goals. Ehrlich (1973) suggests that when individuals consider
legal and illegal opportunities they use cost-benefit analyses to determine whether the
opportunity is worth pursuing. When legal opportunities, such as employment, produce sufficient
benefits, individuals are less likely to pursue criminal opportunities because the cost of arrest is
too high when financial goals are already being achieved (Ehrlich 1973). Thus, depending on the
lucrative nature of employment and crime opportunities, individuals can switch between or
consistently pursue one approach to financial stability/success (Ehrlich 1973).

Similarly, Merton (1938) and other strain theorists suggest that crime results from
individuals’ motivations to pursue socially encouraged goals, such as financial success, without
legitimate means of pursuing these goals. Thus, when obstacles such as criminal records or lack
of education make employment inaccessible, individuals are prone to pursue financial rewards
through crime.

**Employment and Crime Research**

The relationship between employment and crime is complex and nuanced. In general,
there is support for the concept that employment reduces the likelihood that individuals will
participate in criminal behavior or recidivate (Crutchfield and Pitchford 1997; Engelhardt 2010;
Good, Pirog-Good, and Sickles 1986; Good and Pirog-Good 1987; Gould, Weinberg, and
Mustard 2002; Grogger 1998; Sampson and Laub 1990, 1993; Tripodi, Kim, and Bender 2010;
Uggen 1999; Verbruggen et al. 2013; Wadsworth 2000, 2006; Weiss and Reid 2005; Witte and
Tauchen 1994; Witte 1980). However, previous research suggests that job quality (Crutchfield and Pitchford 1997; Sampson and Laub 1990, 1993; Uggen 1999; Wadsworth 2006), wages (Engelhardt 2010; Gould et al. 2002; Grogger 1998; Witte 1980), race (Engelhardt 2010; Good and Pirog-Good 1987), and individuals’ abilities to supplement low earnings with public assistance (Verbruggen et al. 2013) all play a role in whether employment prevents crime. In general, when individuals hold jobs that they find to be mentally or financially rewarding, they are less likely to participate in crime (Wadsworth 2006). These rewards manifest as employment stability, salaried income (as opposed to hourly wages), perceived upward mobility, position longevity, perceived meaningfulness, vacation leave, retirement benefits, and job satisfaction (Crutchfield and Pitchford 1997; Sampson and Laub 1990, 1993; Uggen 1999; Wadsworth 2006). Wadsworth (2006) concludes that employment quality is the most important factor in deterring criminal involvement. This being said, while Wadsworth (2006) and Witte and Tauchen (1994) do not find income or job stability influential over criminal behavior and recidivism, other scholars do (see Engelhardt 2010; Gould et al. 2002; Grogger 1998; Witte 1980). Engelhardt (2010, p. 677) finds that “individuals who are unemployed are caught committing crimes and imprisoned two times faster than low-wage workers and four times faster than high-wage workers.” Similarly, Verbruggen et al. (2013) find that males who are employed or are receiving public assistance are less likely to reoffend, suggesting that financial rewards or accessibility deters criminal behavior.

However, while general consensus suggests that employment can reduce participation in crime, this is not universally found (see Good and Pirog-Good 1987; McMorris and Uggen 2000; Mortimer et al. 1996; Piquero 2004; Sviridoff and Thompson 1983). In a study of adult male misdemeanants released from New York City’s Rikers Island correctional facility, Sviridoff and
Thompson (1983) find that low-level employment can be accompanied by or alternated with criminal behavior. Participants described using employment income to support or supplement illicit activities, and using criminal behaviors to supplement employment income (Sviridoff and Thompson 1983), in much the same way that Good and Pirog-Good (1987, p. 91) conclude that white teenage males use employment to mask criminal behaviors or “moonlight in crime.” Similarly, employment can have detrimental influences on adolescent delinquency and substance use when it interferes with school involvement and achievements (McMorris and Uggen 2000; Mortimer et al. 1996).

**Theories and Literature Regarding Incarceration and Employment**

Three theories – stigmatization, loss of human capital, and life course interruption – have held center stage in exploring the potential effects of incarceration on post-incarceration employment outcomes. Stigma negatively affects ex-convicts’ employment potentials in three prominent ways. Stigma leads to employer reluctance to hire offenders (Holzer 1996; Pager, Western, and Sugie 2009; Pager 2003; Petersilia 2003; Schwartz and Skolnick 1962). Similarly, Western (2002) suggests that the government has reinforced ex-offender stigmas and labels by imposing employment restrictions against felons. Additionally, building upon Becker's (1963) discussion of stigmatization, Apel and Sweeten (2010) have voiced concern that ex-inmates incorporate stigmas and labels into their self-concept, resulting in subsequent deviant behavior or a reluctance to pursue employment. Meanwhile, a separate, but possibly concurrent theory suggests that ex-inmates experience more difficulty with acquiring, maintaining, and advancing employment because they experience erosion of skills (or human capital) and interruptions to employment while incarcerated (Kling 2006; Lalonde and Cho 2008; Western, Kling, and Weiman 2001; Western 2002). Finally, Sampson and Laub's (1993) life course theory suggests
that incarceration interrupts key life transitions and social bonds, resulting in negative employment outcomes upon release. Below I discuss in detail the influence of labeling, erosion of skills, and life course perspective on employment post-incarceration.

Stigmatization

Social stigmatization and labeling negatively affect employment and income prospects following arrest, conviction, and incarceration (Becker 1963; Lopes et al. 2012; Sampson and Laub 1993). This occurs in multiple ways. Incarceration negatively affects employment and wages by triggering negative impressions of former offenders, such as untrustworthy, dangerous, and unable or unwilling to appropriately deal with authority. This can lead potential employers to feel concern over future liability if they were to hire an ex-convict, and in cases where alternative noncriminal candidates exist, a former offender may appear to be an unnecessary risk (Bushway 2004; Holzer 1996; Petersilia 2003; Schwartz and Skolnick 1962; Western 2002). Alternatively, employers may subconsciously link incarceration with underclass populations. In such situations, employers could proscribe inappropriate stereotypes of poor populations, such as lazy, crude, and having poor social skills, to ex-inmates (Apel and Sweeten 2010). Scholars also suggest that government policies that disqualify individuals with felony records from employment in licensed, skilled, or professional careers are driven by stigma and distrust of ex-convicts (Western 2002). Yet these same policies can inhibit former offenders from access to lucrative and often continuous employment opportunities (Western 2002).

Another concern of labeling theory is the potential for ex-inmates to begin to believe negative stigmas, incorporating societal labels into their self-concepts. When this occurs, it is possible for offenders to become entrenched in the roles, behaviors, and relationships proscribed by society, which begins a self-fulfilling prophecy cycle (Jensen 1972; Schur 1971).
Alternatively, when ex-inmates are aware of the stigma and hesitance of employers to hire ex-convicts, they may be reluctant or dissuaded from seeking employment opportunities because they anticipate being rejected by employers (Apel and Sweeten 2010).

Loss of Human Capital

A separate, but possibly concurrent theory to labeling and stigmatization suggests that ex-inmates experience more difficulty with gaining, maintaining, or advancing employment because they experience erosion of skills (or human capital) and interruptions in employment while incarcerated. Loss of human capital theory argues that while in prison, offenders experience erosion of skills and fail to gain the abilities they would presumably acquire through work experience if they were not incarcerated. As such, ex-convicts are at a distinct disadvantage when attempting to acquire employment, as they present skills below those of other potential candidates (Kling 2006; Lalonde and Cho 2008; Western et al. 2001; Western 2002).

Additionally, “[l]ife course analysis of occupations finds that earnings mobility depends on stable employment in career jobs” (Western 2002:526). As such, ex-convicts can experience difficulty gaining employment or reaching income levels that would have been possible if not for incarceration.

Human capital theory (Becker 1964) suggest that “career” employment is a symbiotic relationship between employer and employee. In these types of employment, both the employer and employee invest in the employee gaining industry-specific human capital such as training, experience, and education. Since both the employee and employer are investing in the employee’s skill acquisition, it is in both parties’ best interest that the employment be stable and long-term. Then, as employees gain human capital over time, their wages gradually increase to reflect the increased value they bring to employers and the decline in employers’ cost of training
the employees. Nagin and Waldfogel (1998) suggest that “career” employers are looking for individuals who are trustworthy, capable of maintaining employment stability, and worthy of the investment employers make towards training and educating employees. However, individuals with criminal records fail to inspire these assessments among employers. Therefore, individuals with criminal records are often left to pursue “spot market jobs,” which are rarely stable employment opportunities and illicit little if any investment in human capital growth by the employer or the employee (Nagin and Waldfogel 1998). As such, ex-convicts and -inmates have little hope for finding upwardly model employment or wage opportunities.

Incarceration can prove to be extremely detrimental for employment in industries that evolves over time (due to either new techniques, new technology, or new policies), as the failure to accumulate work experience, training, and education result in substantial gaps between ex-inmates and other potential employees. Thus, “incarceration is likely to disrupt human capital accumulation and to weaken what might already be a tenuous connection to the formal labor market” (Apel and Sweeten 2010:452). Meanwhile, to survive incarceration (both emotionally and physically) inmates can develop habits and ways of socializing that are innate to prison life (termed prison socialization or “prisonization”), but are neither easily discarded nor transferable to society or workplace interactions (Apel and Sweeten 2010; Clemmer 1940). Thus, some of the skills that can evolve during incarceration are detrimental to acquiring employment and participating in the workforce.

*Life Course Perspective*

Life course perspective suggests that incarcerations interrupt and divert life transitions and trajectories (Sampson and Laub 1993). More specifically, the life course perspective suggests that incarceration has negative influences on multiple areas of offenders’ lives by
interrupting key life transitions and disrupting social bonds that are necessary for informal social control to occur. By disrupting or redirecting the employment trajectories of ex-inmates, incarceration prevents individuals from obtaining the same jobs, wages, and wage mobility demonstrated by their never-incarcerated peers. Incarceration also weakens social ties that could help ex-inmates find employment (Sampson and Laub 1993). As such, Hagan (1993) suggests that contact with the criminal justice system sets off a snowball process. Kerley and Copes (2004) argue that contact with the criminal justice system during adolescence is especially devastating, as harm accumulates as individuals age. In other words, individuals who experience incarceration during late adolescence miss developing early employment experiences, skills, and social contacts that act as building blocks to later employment. Without these employment experiences and skills, and with the added disadvantage of having criminal records, individuals incarcerated during early adolescence have little to recommend them for steady and upwardly mobile employment after their release from jail or prison. Ultimately, this disadvantage leaves individuals trailing the employment trajectories of their never-incarcerated peers, if they are ever able to move past entry-level jobs. Unfortunately, few studies have explored the effect of incarceration when interacted with age.

*Employment Post-Contact with the Criminal Justice System*

Scholars have consistently found arrest, conviction, and incarceration negatively affect employment and financial outcomes (Bushway 1998, 2004; Engelhardt 2010; Freeman 1992; Grogger 1995; Holzer 1996; Hunter and Borland 1997, 1999; Kerley and Copes 2004; Lopes et al. 2012; Nagin and Waldfogel 1998; Sampson and Laub 1993; Waldfogel 1994; Western and Beckett 1999; Western 2002; Witte and Reid 1980). Previous research has employing a variety of theories, methodologies, and data sets to approach the issue of employment outcomes post-
incarceration. Yet, despite the variety of measures and methods utilized, prior literature consistently finds that incarceration has grim long-term effects on ex-inmates’ employment outcomes.

Holding a Job

Overwhelming consensus suggests that incarceration reduces employment among ex-inmates (Freeman 1992; Holzer 1996; Sampson and Laub 1993; Waldfogel 1994; Western and Beckett 1999; Witte and Reid 1980). In a study of males incarcerated as juveniles, Sampson and Laub (1993) find that incarceration between ages 17 and 23 is negatively correlated with work commitment, employment, and continuity of employment between 25 and 32 years-old.

Sampson and Laub (1993) contend that these and other findings support life course theory, as incarceration weakens social bonds between ex-inmates and society, and interrupts important life transitions.

Research regarding the effect of criminal conviction on employment outcomes suggests that ex-offenders experience poor employment stability, holding more jobs and for shorter periods of time than prior to their conviction (Nagin and Waldfogel 1995). “The stigma of conviction is especially prohibitive of entry into high-status or career jobs” (Western 2002). Thus, ex-offenders are often relegated to flexible or contingent employment that is characterized as unstable and lacking continuity. A study of employment among parolees supports this theory, suggesting that while employment is not hard to find, the jobs acquired lack in “character and quality” (Evans 1968).

Conviction and Employment

Similar literature on criminal conviction finds that conviction has detrimental effects on employment even when offenders are not incarcerated (Nagin and Waldfogel 1995; Pager et al.
2009; Pager 2003; Waldfogel 1994). For example, in field experiments in which male applicants submitted resumes and job applications that only differed in disclosed race and criminal background, Pager et al. (2009) and Pager (2003) observed that both race and criminal background have negative effects on low-wage employment application success. In fact, applicants with criminal records are one-half to one-third as likely to be interviewed or hired for employment opportunities compared to applicants without criminal histories (Pager 2003). This is quite concerning when paired with Apel and Sweeten's (2010) finding that incarcerated individuals demonstrate significantly lower employment probabilities compared to individuals who are convicted but never incarcerated. Based on these findings, Apel and Sweeten (2010) conclude that incarcerated offenders experience negative employment outcomes beyond the influence of stigma associated with criminal conviction.

Non-Employment and Participation in the Labor Market

In a previous study (Emmert 2014), I explore the effect of various incarceration experiences on the lengths of time individuals are non-employed and the lengths of time individuals spend looking for employment. I find that individuals who have multiple incarceration experiences demonstrate longer non-employment periods compared to their never-incarcerated peers (Emmert 2014). However, incarceration is not responsible for the observed difference in non-employment lengths (Emmert 2014). Thus, the increase in non-employment observed among ex-inmates with multiple incarceration experiences results from a previous incarceration, the experience of being arrested, or from incarceration during adolescence or young adulthood (since first incarceration occurs before age 23 for this subsample) (Emmert 2014). In testing the latter, I find that individuals who are younger during their first incarceration
demonstrate longer non-employment periods than their peers who experience their first incarceration at older ages.

Emmert (2014) also explores the influence of incarceration on the amount of time individuals spend looking for employment. Apel and Sweeten (2010) raise the concern that initial rejections during ex-inmates’ pursuits of employment, or anticipated rejections, lead ex-inmates to voluntarily opt-out of participating in the labor market (Apel and Sweeten 2010). I find that individuals in the incarceration and never-incarcerated subsamples spend equivalent amounts of time looking for employment (Emmert 2014). Considering that individuals with multiple incarceration experiences and/or individuals who experience their first incarceration at earlier ages spend more time non-employed than their peers, one would expect these individuals to spend more time looking for employment (Emmert 2014). Yet, this is not the case. Despite spending more time non-employed, individuals with multiple incarceration experiences or incarceration experiences at young ages do not spend more time looking for employment (Emmert 2014). I conclude that this phenomenon supports Apel and Sweeten's (2010) deduction, since at least a portion of the ex-inmates sampled in the study appear to voluntarily opt-out of participating in the labor market.

My findings in Emmert (2014) prove important to this study because I use the same dataset in this project. I do this in order to gain a more comprehensive understanding of how incarceration influences employment outcomes while using a single study sample. I also use the same independent measures of incarceration to ensure continuity between this and my previous study.
Employment Rate

Kling (2006) and Lalonde and Cho (2008) both perform fixed effects analyses on administrative data, and find that immediately following incarceration, employment rates increase for offenders compared to their own pre-incarceration employment histories. However, in the long term, these studies find that employment rates drop to mirror pre-incarceration rates (Kling 2006; Lalonde and Cho 2008). Thus, while Kling’s (2006) and Lalonde and Cho’s (2008) findings of post-incarceration employment rate improvements stand out among reentry employment studies, the temporary nature of these improvements suggest that ex-inmates are not better off in the long-term.

It is important to note, that in a natural experiment in which criminal defendants are randomly assigned to judges who demonstrate statistically significant imprisonment sentencing disparities, Loeffler (2013) finds that the employment rates of ex-inmates five years after indictment fail to be statistically significantly different. Loeffler (2013) suggests that his results address two large concerns in incarceration research: isolation of the effect of incarceration on offenders, and selection bias. In other words, scholars point out that prior to incarceration, most individuals have experience with the criminal justice system, including prior arrests, prosecutions, convictions, and probation. Each of these previous experiences can instigate cumulative disadvantage in addition to the effect of incarceration, and making it difficult to separate out the effect of incarceration from the effect of previous criminal justice experiences. Additionally, numerous scholars suggest that individuals who demonstrate preexisting characteristics of disadvantage (minority, urban poor, non-employment, insufficient education, etc.) are more likely to be arrested, convicted, and imprisoned (Loeffler 2013; Manski and Nagin 1998; Nagin, Cullen, and Jonson 2009; Smith and Paternoster 1990). As such, the negative life
outcomes demonstrated by ex-inmates may reflect systemic social selection and stagnation of negative attributes during incarceration. Loeffler (2013) argues that random assignment to judges who demonstrate statistically significant imprisonment patterns controls for incarceration selection bias. Thus, his finding that incarceration fails to affect employment may suggest that employment differences observed in other research reflect preexisting disadvantage among individuals who experience incarceration, but that incarceration itself does not add additional disadvantage.

Wages and Income

In studies where arrest records are compared to earnings data from unemployment insurance reports, scholars find that arrest and conviction have short-term effects on individuals’ wages and earnings (Grogger 1995; Waldfogel 1994). Meanwhile, incarceration has been found to have a long-term effect on ex-inmate incomes, with ex-inmates experiencing 10 to 30 percent lower wages compared to their peers (Grogger 1995; Kling 2006; Western 2002). Similarly, youths incarcerated in correctional facilities before age 20 continue to experience negative income and wage outcomes for over a decade after their initial incarceration experience (Freeman 1992; Sampson and Laub 1993; Western and Beckett 1999). Scholars suggest that the interruptions ex-inmates’ experience in their life courses because of incarcerations negatively influence their future wages and incomes.

A life course perspective on crime indicates that incarceration can disrupt key life transitions. Life course analysis of occupations finds that earnings mobility depends on stable employment in career jobs. These two lines of research thus suggest that incarceration reduces ex-inmates’ access to the steady jobs that usually produce earnings growth among young men. (Western 2002:526)

Moreover, by disrupting the relationships, education, skill acquisition, and employment trajectories of individuals, incarceration leaves ex-inmates at a distinct disadvantage for finding
employment opportunities that are continuous and pay well. “If ex-convicts ultimately recover
their pre-incarceration wage level, the life course perspective suggests they may still be worse
off because wages would have grown even higher without incarceration” (Western 2002).

In addition to earning lower wages and incomes than their peers, ex-inmates also
demonstrate poor earnings mobility, as incarceration reduces the rate of wage growth for ex-
inmates by 31 percent compared to at-risk men who never experience incarceration (Western
2002). This may result from stigmas connected to incarceration.

Public Assistance

In a study of Dutch men and women treated in a juvenile detention center as youths,
Verbruggen et al. (2013) find that 64 percent of the men and 83 percent of the women sampled
receive public assistance during the study’s observation period. Both the types of aid received
and the lengths of time individuals used aid varied by gender. Males more frequently received
unemployment benefits. By comparison, females often utilized disability benefits, and did so for
almost three times as long as men would use benefits.

Race and Education

Whether through interruptions in the life course or stigmatization, researchers suggest
that the inaccessibility of career jobs to ex-offenders and -inmates causes large reductions in
earnings for these individuals. Racial disparities in employment accessibility among individuals
with criminal records prove to be even more concerning. Research suggests that race can have a
direct effect on post-offense employment, as employers are less likely to interview job applicants
who are black offenders than job applicants who are white offenders (Pager 2003). A similar
relationship exists between incarceration and education level. Western (2002) suggests that not
achieving academic milestones such as high school and college graduation exacerbates negative
wage outcomes for ex-inmates. This is particularly concerning considering Apel and Sweeten's (2010) observation that incarceration can lead offenders to drop out of high school. While high school drop outs can pursue high-school equivalency diplomas (GED), research demonstrates that high-school equivalency degrees are not equivalent in the job market (Cameron and Heckman 1993). Moreover, Holzer, Offner, and Sorensen (2004) conclude that declines in employment activity by young black males with lower educations result from post-incarceration effects, particularly among 25 to 34 year-olds. Thus, the effect of incarceration on educational attainment limits the wage and income mobility of adolescents (Apel and Sweeten 2010).

**Looking Ahead**

Chapter 2 provides an overview of the analyses and data I use in the studies. More specifically, I discuss event history analysis, fixed effects analysis, the Rochester Youth Development Study (RYDS), and the employment outcomes and incarceration measures I use in this project.

Subsequent chapters (Chapters 3 to 5) address the results, and discussion sections for each question posed above. Chapter 3 uses event history analyses to study the influence of incarceration on ex-inmates’ employment acquisition rates. Chapter 4 considers the effects of incarceration on employment stability and tenure through fixed effects analyses. Chapter 5 explores the effects of incarceration on use of public assistance, using both fixed effects and event history analyses.

Finally, Chapter 6 provides a general discussion of the three research questions. More specifically, this chapter includes a summary of the key findings, a discussion of the contributions made by this study to current literature, and a discussion of the directions for future research in the study of post-incarceration outcomes.
CHAPTER 2. Overview of Analyses and Data: Event history analysis, fixed effects analysis, RYDS, employment outcomes, and incarceration measures

In Chapter 1, I pose three research questions: (1) What are the effects of cumulative incarceration experiences and age at first incarceration on ex-inmates’ employment acquisition rates? (2) How do cumulative incarceration experiences and age at first incarceration affect the employment stability and tenure of ex-inmates? And (3) Do cumulative incarceration experiences and age at first incarceration affect how long individuals spend relying on public assistance, and do ex-inmates resort to public assistance at faster rates than never-incarcerated individuals do? Table 1 offers an overview of the analytical techniques used and the dependent, independent, and control variables employed to address each research question introduced. Below, I describe in detail these techniques, measures, and the data employed in these studies.

Analytical Strategies

I utilize two methodological techniques – event history analysis and fixed effects analysis – across the three studies I perform. More specifically, I use event-history analysis to explore whether incarceration influences the rates at which ex-inmates gain employment and resort to using public assistance. I employ fixed effects analyses to explore how ex-inmates differ from their never-incarcerated peers regarding employment stability, employment tenure, and use of public assistance. Below I describe each technique and how it aids in the study of ex-inmates’ post-incarceration employment outcomes.

Event History Analyses

I use Cox proportional hazards models to estimate event history analyses (also known as time to event analyses or survival analyses). Event history analysis measures the amount of time from the beginning of an observation until individuals experience the event of interest. Thus, this
<table>
<thead>
<tr>
<th>Chapter</th>
<th>Research Question</th>
<th>Analytical Technique</th>
<th>Dependent Variables</th>
<th>Independent Variables</th>
<th>Controls</th>
</tr>
</thead>
</table>
| 3       | What are the effects of cumulative incarceration experiences and age at first incarceration on ex-inmates’ employment acquisition rates? | Event history analyses   | Time to employment                   | Incarceration  
 First incarceration  
 Multiple incarcerations  
 Number of incarcerations  
 Age at first incarceration | Prior employment |
| 4       | How do cumulative incarceration experiences and age at first incarceration affect the employment stability and tenure of ex-inmates? | Fixed effects analyses   | Employment tenure  
 Employment stability           | Incarceration  
 First incarceration  
 Multiple incarcerations  
 Number of incarcerations  
 Age at first incarceration | Observation length  
 Post-contact         |
| 5       | Do cumulative incarceration experiences and age at first incarceration affect how long individuals spend relying on public assistance? | Fixed effects analyses   | Public assistance                   | Incarceration  
 First incarceration  
 Multiple incarcerations  
 Number of incarcerations  
 Age at first incarceration | Observation length  
 Post-contact         |
| 5       | Do ex-inmates resort to public assistance at faster rates than never-incarcerated individuals do? | Event history analyses   | Time to public assistance            | Incarceration  
 First incarceration  
 Multiple incarcerations  
 Number of incarcerations  
 Age at first incarceration | Male  
 Black  
 High school graduate |
technique serves to explore whether incarceration affects the rates at which ex-inmates gain employment and use public assistance.

To determine whether incarceration influences the time to event (in this case, employment acquisition or use of public assistance), it is necessary to include a comparison group in the sample. By including a comparison group of individuals who have never experienced incarceration, I am able to compare the employment outcome hazard rates of individuals who experience incarceration with those of individuals who never experience incarceration. The comparison group for this project is composed of individuals who experience an arrest or are convicted of a crime and sentenced to serve probation at some point during the observation period. For the never-incarcerated subsample, “time to event” measures the amount of time between experiencing an arrest or a conviction to serve probation and an employment outcome of interest. By comparison, for the incarceration subsample, “time to event” measures the amount of time between incarceration release and an employment outcome of interest.

Not every individual in the sample experiences the event of interest during the observation period, but it is possible that the event would have occurred had the observation period lasted longer. For this reason, a censor, or binary control measure, is included in each model to denote whether the time measure signifies the time to event or represents the end of the observation period because the event never occurs.

As I stated above, the event history analysis allows me to consider whether incarceration affects the rate at which individuals experience specific employment outcome events. For each independent measure I include in the model, the Cox proportional hazards model returns a hazard rate that demonstrates the probability that individuals experience an employment outcome at time t while the individuals are at risk for experiencing the event. While the hazard rate is an
unobserved variable, it controls both the occurrence and the timing of employment outcome experiences.

I produce five models for each dependent variable I explore with event history analyses. Each model includes a different independent measure of incarceration, but holds all other variables the same between models.

**Fixed Effects Analyses**

I use fixed effects analyses to explore how incarcerations influence ex-inmates’ employment stability, employment tenure, and use of public assistance post-incarceration. Fixed effects analyses accomplish this by comparing ex-inmates’ employment histories pre-incarceration with their employment outcomes post-incarceration. Fixed effects analysis assumes that behaviors and outcomes, such as employment, maintain relatively consistent trajectories throughout individuals’ lives. Thus, by comparing peoples’ employment histories prior to interventions, such as incarcerations, to their employment histories following interventions, it is possible to determine whether the interventions influence employment outcomes.

By permitting each individual’s previous employment history to act as a control by which to compare their employment outcomes post-contact with the criminal justice system, fixed effects analysis holds time-invariant unobservables (such as personality) constant. Similarly, the analysis technique holds observables, such as gender and race, constant. This allows me to focus strictly on the effect of incarceration on ex-inmates’ post-incarceration employment outcomes. In order to compare ex-inmates to themselves across time, fixed effects analyses include a dichotomous control variable for each individual in the sample, a dichotomous variable to distinguish between pre- and post-contact measures, an independent measure of incarceration, and an interaction variable for incarceration and post-contact.
One of the incarceration measures used in this project, *age at first incarceration*, necessarily requires incarceration, thus, eliminating any potential comparisons to never-incarcerated individuals. Therefore, only ex-inmates are included in *age at first incarceration* models. However, it is also important to compare ex-inmates’ employment outcomes to the employment outcomes of their never-incarcerated peers. By including an independent incarceration measure that differentiates between the incarceration subsample and a comparison group, it is possible to estimate both within person and between person comparisons (akin to difference-in-differences analyses). Again, the within person analysis compares respondents’ employment outcomes prior to their contact with the criminal justice system to their outcomes after contact. Then, by including an incarceration term that distinguishes between groups, the differences observed between pre- and post-periods are compared for respondents who have experienced incarceration and individuals in the comparison group who have never experienced incarceration, thus establishing between person comparisons.

To estimate fixed effects analyses, it is necessary to have employment histories for two periods for both the incarcerated and never-incarcerated subsamples. For the incarcerated subsample, the pre-contact period documents individuals’ employment histories prior to incarceration. The post-contact period documents individuals’ employment histories after release from jail or prison.

The comparison group for this study is composed of individuals who experience an arrest or are convicted of a crime and sentenced to serve probation at some point during the observation period. For the never-incarcerated subsample, the pre-contact period documents individuals’ employment histories prior to their arrest or probation conviction. The post-contact period documents individuals’ employment histories after their arrest or probation conviction.
This study utilizes data from the Rochester Youth Development Study (RYDS), an ongoing longitudinal study examining antisocial and delinquent behaviors. Beginning in 1988 with a selection of 1,000 seventh- and eighth-grade students in the Rochester (New York) Public School System, RYDS has interviewed the panel of students from their early teenage years through their adult years. To date, RYDS has completed 14 waves of interviews for the panel, spanning ages 14 through 31.

The original RYDS sample was stratified on two dimensions in order to target subjects at high risk for violence and serious delinquency. First, males were oversampled (75 percent versus 25 percent), as they are more likely than females to engage in serious and violent offenses (Blumstein et al. 1986; Huizinga, Morse, and Elliott 1992). The study oversampled students living in areas of Rochester that experienced high residential arrest rates under the assumption that adolescents living in these areas were at greater risk for offending than adolescents living in areas with lower residential arrest rates. High residential offender areas were determined by assigning each census tract in Rochester a resident arrest rate that reflected the proportion of the tract’s total adult population arrested by the Rochester police in 1986. The RYDS sampled students proportionate to the rate of offenders living in each tract. Students in the top one-third of arrest rate tracts were sampled with certainty.

From Waves 2 to 14, RYDS experienced less than 2 percent attrition per year. In an effort to maximize sample retention, the RYDS utilizes participant-tracking techniques and provides survey incentives. Similarly, the RYDS attempts to track and interview all subjects who move from Rochester, and conduct in-institution interviews of participants who are institutionalized during data collection periods. At Wave 14, the subject panel was 68.9 percent black, 16.2
percent Hispanic, and 14.9 percent white. These proportions are close to expectation, given the population characteristics of Rochester schools and the decision to oversample high-risk youth. Additionally, 79 percent of the original sample was still participating at Wave 14. A formal test of subject attrition within RYDS revealed that the subjects retained do not significantly differ from those not retained on multiple dimensions, including gender, social class, family structure, drug use, delinquency, property crime, and violent crime (see Krohn and Thornberry 1999). None of the difference tests reach statistical significance (p < 0.05).

Table 2 presents basic characteristics of the total panel, the Wave 1 sample, and the Wave 14 sample. As one can see, the distributions are similar at both Waves. For example, 72.9 percent of the original panel was male at sampling, and at Wave 14, it was 70.2 percent.

Current Study

To conduct the necessary analyses for this study, I use data collected in Waves 13 and 14. During these waves, interviewers ask subjects to complete a life history calendar (LHC) in which

<table>
<thead>
<tr>
<th></th>
<th>RYDS Sample</th>
<th>Study Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Total panel</td>
<td>Wave 1 (%)</td>
</tr>
<tr>
<td><strong>Gender</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>72.9</td>
<td>74.1</td>
</tr>
<tr>
<td>Female</td>
<td>27.1</td>
<td>25.9</td>
</tr>
<tr>
<td><strong>Race/ethnicity</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>68.0</td>
<td>68.9</td>
</tr>
<tr>
<td>Hispanic</td>
<td>17.0</td>
<td>17.3</td>
</tr>
<tr>
<td>White</td>
<td>15.0</td>
<td>13.8</td>
</tr>
<tr>
<td>N</td>
<td>1000</td>
<td>956</td>
</tr>
</tbody>
</table>
they report on life events that have occurred since they were last interviewed. The LHC covers approximately six years’ time for the majority of respondents, when respondents are approximately 23 through 29 years-old.\(^1\) In addition, I use police data collected from the Rochester Police Department (RPD) and the New York Division of Criminal Justice Services (DCJS), which spans law enforcement interactions with respondents between Waves 1 and Wave 12.

The study sample is limited to a subset of LHC participants who experience incarceration or contact with the criminal justice system in the form of an arrest or conviction to probation. The study sample includes 184 individuals who experience an incarceration during the time covered by the LHC. Additionally, the comparison group includes 66 respondents who experience an arrest or conviction to probation during the LHC, but have no incarceration experiences during the period.\(^2\) While individuals who experience incarceration necessarily experience arrests at some point prior to incarceration, I only observe these individuals pre- and post-incarceration. In addition, individuals included in the arrest/probation subsample do not demonstrate incarcerations before or during the LHC period. In other words, each individual is included in only one of the subsamples (incarceration or arrest/probation), with no overlap between subsamples.

---

\(^1\) Prior to the LHC, RYDS surveys do not inquire into participants’ employment activities in enough detail to discern the employment outcomes of interest to this project. As such, I limited my analyses to explore employment outcomes during the LHC period, when participants are approximately 23 to 29 years-old.

\(^2\) While conviction to probation or conviction not resulting in incarceration would have been the most ideal comparison group, too few cases experience either event without future incarceration during LHC. As such, it was necessary to include individuals who experience arrest regardless of conviction status in the comparison group. Similarly, it would be advantageous to explore the sensitivity of the methods utilized to the counterfactual. However, the only two groups who demonstrate breaks from the “conventional” labor market are military personnel on reserve and former homemakers. While both groups are identifiable via RYDS data, there are too few military personnel to simulate an adequate comparison. Former homemakers also prove to be problematic counterfactual samples, because they often did not distinguish between the period when they were a homemaker and the period when they were looking for employment.
Table 2, columns 5 and 6 list descriptive statistics for each subgroup – arrest/probation and incarceration. The table demonstrates that the arrest/probation subsample is comparable in demographic makeup to that of the incarcerated subsample. While the incarceration subgroup consists of a slightly higher concentration of males and blacks compared to the arrest/probation subsample, this likely reflects bias in incarceration convictions in the criminal justice system (Steffensmeier, Kramer, and Streifel 1993; Steffensmeier, Ulmer, and Kramer 1998).

Table 3 outlines demographic characteristic descriptive statistics for the arrest/probation comparison subsample (columns 4 and 5), the individuals experiencing their first ever incarcerations (columns 6 and 7), and individuals with multiple incarceration experiences (columns 8 and 9). The comparison group is dissimilar from the incarceration subsamples in many regards, including gender and race demographics, and cumulative delinquency throughout the life course. However, the arrest/probation comparison group proves to be a closer fit to the incarceration samples than are the RYDS participants I exclude from this project (see columns 2 and 3). It is also interesting to note that on multiple dimensions the first incarceration and multiple incarceration subsamples vary from one another by similar amounts as they do from the arrest/probation subsample (although in many regard, the arrest/probation subsample is more similar to first-time ex-inmates than to ex-inmates with repeated incarceration experiences).

**Employment Outcomes**

I explore four employment outcomes in the studies below. These are employment, employment stability, employment tenure, and public assistance. While I discuss each employment outcome at length later, I include a brief synopsis of employment measures below.

*Employment*

A common concern in the literature is that ex-inmates have more difficulty gaining
Table 3. Sample Demographic Characteristics

<table>
<thead>
<tr>
<th></th>
<th>RYDS Sample Excluded From Study</th>
<th>Arrest/ Probation Subsample</th>
<th>Study Sample First Incarceration Subsample</th>
<th>Multiple Incarceration Subsample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean/% SD</td>
<td>Mean/% SD</td>
<td>Mean/% SD</td>
<td>Mean/% SD</td>
</tr>
<tr>
<td><strong>Gender</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>35.4 21.0</td>
<td>21.0 13.0</td>
<td>10.4</td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>64.6 79.0</td>
<td>86.8 89.6</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Race/Ethnicity</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>66.1 72.6</td>
<td>75.4 74.8</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hispanic</td>
<td>17.1 11.3</td>
<td>13.0 15.7</td>
<td></td>
<td></td>
</tr>
<tr>
<td>White</td>
<td>16.8 16.1</td>
<td>11.6 9.6</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Have a Partner</td>
<td>67.4 68.0</td>
<td>71.7 61.4</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Have Children</td>
<td>57.6 53.2</td>
<td>60.9 53.0</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Education</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High school graduate</td>
<td>51.7 50.0</td>
<td>55.7 48.0</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Employment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percent of time non-</td>
<td>23.0 28.4</td>
<td>22.3 29.2</td>
<td>24.6 30.9</td>
<td>44.0 33.3</td>
</tr>
<tr>
<td>employed between</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ages 19-21</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Delinquency</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>General ever-variety</td>
<td>3.8 3.9</td>
<td>4.6 3.4</td>
<td>6.1 4.1</td>
<td>8.1 4.7</td>
</tr>
<tr>
<td>General incidence</td>
<td>165.7 357.9</td>
<td>171.2 249.6</td>
<td>409.8 580.9</td>
<td>509.6 714.6</td>
</tr>
<tr>
<td>Serious ever-variety</td>
<td>0.7 1.2</td>
<td>0.6 1.1</td>
<td>1.4 1.6</td>
<td>2.0 1.6</td>
</tr>
<tr>
<td>Serious incidence</td>
<td>4.0 14.1</td>
<td>3.5 11.6</td>
<td>6.5 14.4</td>
<td>12.4 23.7</td>
</tr>
<tr>
<td>Violence ever-variety</td>
<td>1.2 1.3</td>
<td>1.5 1.2</td>
<td>2.2 1.4</td>
<td>2.6 1.4</td>
</tr>
<tr>
<td>Violence incidence</td>
<td>7.4 19.9</td>
<td>9.4 20.3</td>
<td>15.6 31.2</td>
<td>21.3 29.7</td>
</tr>
<tr>
<td><strong>Socioeconomic Level</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lower class</td>
<td>62.3 63.6</td>
<td>63.8 69.2</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Middle class</td>
<td>37.7 36.4</td>
<td>36.2 30.8</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>540 66 70 114</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

employment post-incarceration than their never-incarcerated peers do. In order to explore this concern, I construct a dependent measure, *employment*, which compares employment acquisition rates for individuals who experience incarceration and those who do not.
Employment Stability

Previous literature suggests that individuals who have been convicted of a crime experience greater employment turnover than their never-convicted peers (Nagin and Waldfogel 1995). However, it is important to explore whether employment turnover is also an issue for ex-inmates compared to their never-incarcerated peers. For this reason, employment stability explores the number of jobs each respondent holds, thus allowing me measure whether employment turnover differs between incarcerated and never-incarcerated individuals.

Employment Tenure

I continue expanding on the research performed by Nagin and Waldfogel (1995) by exploring whether ex-inmates hold jobs for shorter lengths of time than individuals in the never-incarcerated comparison subsample. Employment tenure does this by measuring the average length of time individuals hold jobs. With this measure, it is possible to determine whether incarcerated and never incarcerated individuals hold jobs for equal lengths of time, or whether one subsample demonstrates shorter employment lengths than the other.

Public Assistance

With research indicating that ex-inmates demonstrate negative employment outcomes, it is possible that ex-inmates have difficulty making living wages. It is conceivable then that incarceration and poor employment outlooks following imprisonment lead ex-inmates to rely on public assistance. I explore the effect of incarceration on public assistance by measuring the length of time respondents report using public assistance and the rate at which individuals report using public assistance after contact with the criminal justice system.
Incarceration Measures

I use police data and LHC responses to construct five independent incarceration measures. The first incarceration measure is a binary measure that differentiates individuals in the sample who experience incarceration during the LHC observation period from those who do not. This measure identifies the 73.6 percent of the overall sample subcategorized for having experienced incarceration from the never-incarcerated comparison subsample. To subdivide the incarceration subsample further, I create additional dichotomous variables. First incarceration identifies individuals in the incarceration subsample whose LHC incarcerations are the first incarcerations they have ever experienced. Similarly, the multiple incarcerations measure identifies individuals in the incarceration subsample who experience a minimum of one incarceration before the incarceration observed in the LHC period. Individuals in the first incarceration and multiple incarcerations subsamples make up 28.8 percent and 44.8 percent of the overall sample respectively.

Number of incarcerations, is a count of the number of incarcerations each respondent has experienced by the pre- and post-contact periods separately. I create these measures to determine whether a cumulative disadvantage develops with each additional incarceration experienced. The average number of incarcerations experienced by individuals in the sample is 1.2 for the pre-contact period; however, the measure ranges in size from 0 to 9 incarcerations in the period. By comparison, the average number of times incarcerated in the post-incarceration period is 1.9, with individuals having between 0 and 10 incarceration experiences.

A second relevant measure of accumulative disadvantage would be cumulative incarceration length. While it is possible to determine via LHC measures how long participants

---

3 I use police data to supplement LHC and Wave 1 through 12 data regarding the number of times participant have been incarcerated and at what age they were first incarcerated.
are incarcerated between ages 23 and 29, the RYDS does not record incarceration lengths prior to age 23. Without participants’ incarceration or sentence lengths prior to the LHC period, I was not able to construct cumulative incarceration length measures for this study.

The final incarceration measure I include is *age at first incarceration*. *Age at first incarceration* measures how old individuals in the incarceration subsample are/were at their first experience of incarceration. I create this measure to determine whether incarceration has a greater effect on ex-inmates’ employment outcomes based on when individuals experience their first incarceration. The average age at first incarceration in the incarceration subsample is 23 years-old, with a range from 15 to 32 years-old.
CHAPTER 3: Vocations after Incarceration: An event history analysis of employment following incarceration

Introduction

Incarceration has the potential to severely affect ex-inmates’ employment potentials following release from prison or jail (Bushway 1998, 2004; Freeman 1992; Grogger 1995; Holzer 1996; Hunter and Borland 1997, 1999; Kerley and Copes 2004; Lopes et al. 2012; Nagin and Waldfogel 1998; Sampson and Laub 1993; Waldfogel 1994; Western and Beckett 1999; Western 2002; Witte and Reid 1980). Research has thoroughly explored the extent of this effect, and explained the negative employment outcomes as resulting from losses of human capital, stigmatization, and interruptions in the life course. Furthermore, literature proves that ex-inmates continue to experience negative employment outcomes for a decade after reentry into society (Sampson and Laub 1993). However, scholars have yet to demonstrate the rate at which ex-inmates successful gain employment in the years following incarceration. With better knowledge of how quickly ex-inmates find employment, it is possible to focus reentry services on ex-inmates who are most in need of assistance. To explore this outcome, it is necessary to perform event history analyses.

Method

Variables

Dependent Variable: Employment

A common concern in the literature is that ex-inmates have more difficulty gaining employment post-incarceration than their never-incarcerated peers do. In order to explore this concern using event history analysis, I construct a dependent measure, employment. Employment compares the employment acquisition rates of individuals who experience incarceration and their never-incarcerated peers between approximately 23 and 29 years-old. For individuals in the
incarceration subsample, *employment* measures the amount of time between release from incarceration and gaining employment. For the never-incarcerated subsample, *employment* measures the length of time between individuals’ experience of arrest or conviction to probation and gaining employment.\(^4\)

Table 4 shows the means, standard deviations, and ranges of *employment*, independent measures, and control measures utilized in this study. Among individuals in the never-incarcerated subsample, *employment* has a mean of 742 days and a standard deviation of 598 days. Thus, on average, individuals in this sample obtain employment 742 days (2 years) after arrest or conviction to probation. Surprisingly, by comparison, individuals who experience incarceration, on average, find employment more quickly with a mean of 449 days, however the standard deviation is much larger at 623 days. In general, *employment* demonstrates a standard deviation that is larger than its mean. In a regression analysis, this would be a concerning discovery, as this suggests significant skew in the data. However, non-normality is not a concern in survival analysis.

Control Variables

I also include several control variables that scholars have previously found to influence, or could influence, employment acquisition. The variables *male* and *black* are both dichotomous measures of demographic information about each individual in the sample. Similarly, I include a dichotomous measure, *high school graduate*, to denote individuals in the sample who graduated from high school. Finally, the measure *prior employment* is a dichotomous variable included to distinguish whether respondents report having employment at any point prior to their contact with the criminal justice system. It is important to include this measure in order mitigate any

\(^4\) Some individuals in the sample maintain employment during their encounter with the criminal justice system. These individuals are still included in the sample.
potential influence prior employment may have on obtaining or maintaining employment.

Results

Overall, 84 percent of study participants acquire employment during the observation period. Table 5 compares employment acquisition speeds following contact with the criminal justice system. In each of the five model estimations, gender, race, and high school graduation fail to predict employment acquisition rates (although I will later discuss the importance of the interaction between gender and incarceration in several models). The finding that race does not influence the rate at which individuals gain employment in any model is reassuring, as it suggests that employers are not discriminating based on race, and individuals are not voluntarily leaving the labor market due to actual or perceived racial discrimination in the labor market.

Table 4. Descriptive Statistics of Employment, Incarceration, and Control Measures

<table>
<thead>
<tr>
<th>Dependent Measure</th>
<th>Arrest/Probation Subsample</th>
<th>Incarceration Subsample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (SD)</td>
<td>Range</td>
</tr>
<tr>
<td>Employment</td>
<td>742.4 (598.1)</td>
<td>2618</td>
</tr>
<tr>
<td>Incarceration</td>
<td>0.7 (1)</td>
<td></td>
</tr>
<tr>
<td>First incarceration</td>
<td>0.3 (1)</td>
<td></td>
</tr>
<tr>
<td>Multiple incarcerations</td>
<td>0.5 (1)</td>
<td></td>
</tr>
<tr>
<td>Number of incarcerations</td>
<td>1.9 (2.0)</td>
<td>10</td>
</tr>
<tr>
<td>Age at first incarceration</td>
<td>23 (4.2)</td>
<td>17</td>
</tr>
<tr>
<td>Male</td>
<td>0.8 (1)</td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>0.7 (1)</td>
<td></td>
</tr>
<tr>
<td>High school graduate</td>
<td>0.5 (1)</td>
<td></td>
</tr>
<tr>
<td>Prior employment</td>
<td>0.5 (1)</td>
<td></td>
</tr>
</tbody>
</table>
### Table 5. Employment Event History Models

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Beta</td>
<td>HR</td>
<td>Beta</td>
<td>HR</td>
<td>Beta</td>
</tr>
<tr>
<td>Male</td>
<td>-0.186</td>
<td>0.830</td>
<td>-0.110</td>
<td>0.896</td>
<td>-0.261</td>
</tr>
<tr>
<td>Black</td>
<td>-0.244</td>
<td>0.783</td>
<td>-0.213</td>
<td>0.808</td>
<td>-0.333</td>
</tr>
<tr>
<td>High school graduate</td>
<td>0.248</td>
<td>1.281</td>
<td>0.361</td>
<td>1.434</td>
<td>0.428</td>
</tr>
<tr>
<td>Prior employment</td>
<td>0.592***</td>
<td>1.808</td>
<td>0.581**</td>
<td>1.788</td>
<td>-1.030</td>
</tr>
<tr>
<td>Time-dependent prior employment</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.321**</td>
</tr>
<tr>
<td>Incarceration</td>
<td>-0.492</td>
<td>0.611</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Incarceration*M</td>
<td>1.109*</td>
<td>3.032</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>First incarceration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.506*</td>
</tr>
<tr>
<td>Multiple incarcerations</td>
<td>-1.105</td>
<td>0.331</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Multiple incarcerations*M</td>
<td>1.964**</td>
<td>7.129</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of incarcerations</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.105*</td>
</tr>
<tr>
<td>Age at first incarceration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.054</td>
</tr>
<tr>
<td>N</td>
<td>195</td>
<td>107</td>
<td>137</td>
<td>195</td>
<td>100</td>
</tr>
</tbody>
</table>

**NOTE:** Hazard rates (HR). * p < .05 ** p < .01 *** p < .001
Similarly, the finding that graduation from high school does not influence employment acquisition can be both a positive and negative observation. While it is encouraging to observe that individuals are able to acquire employment, even if they lack a high school diploma, having a high school diploma is often considered (whether accurate or appropriate) a valid means of judging potential employees’ skills and appropriateness for the job. Thus, if high school education does not stratify the employment acquisition rate of individuals, what does? It is possible that Rochester, an urban environment, possesses a labor market that includes employment opportunities for individuals with variety of education levels. Alternatively, Rochester employers may segregate worthy potential employees based on labels they feel are more important than simply high school graduate versus high school dropout, such as ex-inmate versus never incarcerated. A third circumstance may be that the labor market in Rochester is primarily composed of low level or entry-level employment opportunities, in which case, high school graduation likely has little to no influence on job acquisition.

Before I explore this further, it is important to note that in Model 1, Model 2, and Model 4, having employment prior to contact with the criminal justice system is a highly significant predictor of employment acquisition. Comparably, in Model 3, the time-dependent form of prior employment is statistically significant. In Models 1, 2, and 4, the hazard of employment is 74 to 81 percent higher for persons with prior employment. In other words, prior employment is associated with a 74 to 85 percent increase in employment. In Model 3, prior employment has a non-linear relationship with time to employment (see Table 6). The hazard of employment is 85 percent higher for individuals with prior employment 500 days (approximately 1 year and 4 months) after coming in contact with the criminal justice system, and continues to improve over time. For example, 2000 days (approximately 5.5 years) after coming in contact with the
Table 6. Hazard Ratios for Prior Employment

<table>
<thead>
<tr>
<th>Time to Employment (days)</th>
<th>HR</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>0.357</td>
</tr>
<tr>
<td>500</td>
<td>0.849</td>
</tr>
<tr>
<td>1000</td>
<td>0.935</td>
</tr>
<tr>
<td>1500</td>
<td>0.991</td>
</tr>
<tr>
<td>2000</td>
<td>1.030</td>
</tr>
<tr>
<td>2500</td>
<td>1.063</td>
</tr>
<tr>
<td>3000</td>
<td>1.091</td>
</tr>
<tr>
<td>3500</td>
<td>1.112</td>
</tr>
</tbody>
</table>

criminal justice system, the hazard of employment is 103 percent higher for individuals with previous employment experience. These results are not surprising, considering that Witte and Reid (1980) find previous employment to significantly affect parolees’ employment outcomes.

In considering the influence of incarceration on employment acquisition, it is interesting to note that in Model 1, incarceration fails to reach significance. However, the interaction incarceration*male proves to be significant. As such, incarceration alone fails to affect the rate at which individuals gain employment, but a distinct difference in employment rates occurs between male and female ex-inmates. The hazard of employment is three times higher for male ex-inmates than for their female and never-incarcerated peers. While it seems odd that male ex-inmates acquire employment three times more quickly than their peers, this finding supports Evans’ (1968) conclusion that employment is not difficult for ex-inmates to find. However, Evans (1968) goes on to point out that the jobs available to ex-inmates often lack in “character and quality.” Based on my finding in Emmert (2014), it is clear that ex-inmates spend less time employed than their never-incarcerated peers do. Therefore, despite finding employment faster, male ex-inmates still spend more time than their never-incarcerated peers non-employed. This
coincides with previous studies’ conclusions that ex-inmates are often relegated to flexible or contingent employment that is characterized as unstable and lacking continuity (Evans 1968; Nagin and Waldfogel 1995; Western 2002). Alternatively, it is also possible that ex-inmates find employment more quickly than their never-incarcerated peers do because of reentry employment programs and parole requirements. Some reentry programs place ex-inmates with work opportunities directly after incarceration. By comparison, ex-inmates released on parole can be required, or heavily encouraged, to find employment as a condition of parole. When ex-inmates face the option of finding employment or returning to jail or prison, ex-inmates may accept employment even if the jobs are undesirable or lacking in quality.

Unlike the incarceration measure in Model 1, first incarceration in Model 2 is significant. In fact, the hazard of employment is 1.7 times higher for first time ex-inmates than for their never-incarcerated peers. However, gender fails to be a significant predictor of employment acquisition in this model.

In Model 3, individuals with multiple incarcerations alone fail to be statistically significantly different from their never-incarcerated peers in the length of time it takes to obtain employment. However, the interaction multiple incarcerations*male is significant. The hazard for employment is seven times higher for male ex-inmates with multiple incarceration experiences than for their female and never-incarcerated peers. As in Models 1 and 2, the speed that ex-inmates acquire employment likely reflects that ex-inmates are finding jobs that others have deemed undesirable. This may account for my previous finding that ex-inmates with multiple incarceration experiences spend longer non-employed than their never-incarcerated peers (Emmert 2014).
The effect of incarceration on the length of time it takes ex-inmates to find employment is also significant when considering the number of incarcerations ex-inmates experience. More specifically, the hazard of employment is 1.1 times higher with every additional incarceration experience. Therefore, one incarceration increases ex-inmates’ employment acquisition rates by 1.1, while ex-inmates with five incarcerations acquire employment at 5.5 times the rate of their never-incarcerated peers. Interestingly, while gender proves to be significant in previous models, it fails to reach significance when number of incarcerations controls for incarceration frequencies. This suggests that gender is significant when I measure incarcerations in aggregate or semi-disaggregated because gender reflects differences in incarceration frequency (males experience more incarcerations than females). When I control for the number of incarcerations ex-inmates have experienced, gender drops out of the equation.

Interestingly, I find that age at first incarceration fails to reach significance in Model 5. This suggests that age at first incarceration does not affect ex-inmates’ employment acquisition rates. Previously, Emmert's (2014) finds that age at first incarceration affects ex-inmates’ lengths of non-employment. Therefore, while ex-inmates who experience their first incarcerations at younger ages show longer periods of non-employment, they do not demonstrate more difficulty finding their first job post-incarceration compared to individuals who are older when first incarcerated.

Discussion

As theory and numerous previous studies have attested, ex-inmates demonstrate clear disadvantages when it comes time to find employment post-incarceration. However, this study

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5 Based on the finding that number of incarcerations influences employment acquisition speed, I suspect that cumulative employment length would also successfully – and potentially more accurately – predict employment acquisition.
expands on previous findings by disaggregating the employment outcomes of ex-inmates based on gender and the number of incarceration experiences they have. Similarly, event-history analysis adds a new dimension to scholars’ understandings of ex-inmates’ employment struggles by highlighting the degree to which incarceration slows employment attainment.

I find that when I study ex-inmates in aggregate, male ex-inmates demonstrate higher rates of employment acquisition post-incarceration compared to their peers. However, the importance of disaggregating ex-inmate employment outcomes based on the number of times participants have experienced incarceration becomes clear when comparing first-time ex-inmates to individuals with multiple incarceration experiences. Males and females who have just experienced their first incarcerations demonstrate equivalent employment acquisition rates. By comparison, males who have multiple incarceration experiences gain employment at a faster rate than their female and never-incarcerated peers do. Going one step farther, I find that when incarceration experiences are disaggregated by number of incarcerations, gender is not significant. This suggests that gender is significant when I measure incarcerations in aggregate for the incarceration model or semi-disaggregated for the multiple incarcerations model because gender reflects differences in incarceration frequency (males experience more incarcerations than females). When I control for the number of incarcerations ex-inmates experience, gender drops out of the equation. Thus, disaggregating the incarceration experiences of ex-inmates proves to be extremely important in understanding the influence of incarceration over employment acquisition rates.

As opposed to demonstrating cumulative disadvantages, it would seem that ex-inmates demonstrate cumulative advantages in employment acquisition rates, since with every additional incarceration experience, ex-inmates acquire employment even more quickly post-incarceration. However, it seems likely that more is going on. Evans (1968) suggests that employment is not difficult for ex-inmates to find. However, Evans (1968) goes on to point out that the jobs
available to ex-inmates often lack “character and quality.” Similarly, it is possible that ex-
inmates’ employment acquisition rates reflect two other aspects of the criminal justice system, 
prisoner reentry programs and parole requirements. Some reentry programs place ex-inmates 
with work opportunities directly after incarceration. By comparison, ex-inmates released on parole can be required, or heavily encouraged, to find employment as a condition of parole. 
However, despite ex-inmates’ advantageous employment acquisition speeds, Emmert (2014) finds that ex-inmates spend more time non-employed than their never-incarcerated peers. Therefore, despite finding employment faster, ex-inmates still spend more time non-employed than their never-incarcerated peers do. This coincides with previous studies conclusions that ex-
inmates are often relegated to flexible or contingent employment that is characterized as unstable and lacking continuity (Evans 1968; Nagin and Waldfogel 1995; Western 2002).

One surprise in this study is the finding that race and education do not influence employment acquisition speed post-incarceration. Considering the notable findings of previous studies (Lyons and Pettit 2008; Pager et al. 2009; Western and Pettit 2000), I expected stigmatization and skill requirements to lead participants who are black or have not graduated high school to acquire employment more slowly than their peers, and especially when participants have incarceration experiences. However, this is not the case. Both race and educational attainment fail to influence employment acquisition rates. However, my findings do not directly conflict with the findings of previous studies. This study differs from previous works in the employment outcome observed and the type of data used. This likely explains the finding that race and educational attainment do not play a role in the rate at which ex-inmates acquire employment.
It is important to note that limitations do exist with the present study. In addition to having a small study sample, this study is geographically limited to respondents who lived in Rochester, New York at approximately 14 years-old, and is predominantly composed of males. Similarly, this study does not address concerns over ex-inmates’ employment quality post-incarceration. To expand on these findings in the future, it is important to consider the influence of cumulative incarceration length on a larger and more diverse sample. Additionally, future research should explore ex-inmates’ employment quality post-incarceration using RYDS data.
CHAPTER 4. Jobs Held after Being Celled: The effect of incarceration on holding a job

Introduction

A concerning cycle exists in the criminal justice system. Research suggests that incarceration has negative effects on the employment outcomes of ex-inmates (Apel and Sweeten 2010; Bushway 1998, 2004; Freeman 1992; Grogger 1995; Holzer 1996; Hunter and Borland 1997, 1999; Kerley and Copes 2004; Lopes et al. 2012; Sampson and Laub 1993; Waldfogel 1994; Western and Beckett 1999; Western 2002; Witte and Reid 1980). While the relationship between employment and recidivism is complex, employment has the potential to reduce criminal behavior and recidivism. Therefore, reliance on incarceration as a punishment or containment method of crime prevention can inadvertently make crime and the life outcomes of ex-inmates more problematic in the future. By disaggregating the effect of imprisonment on offenders, research and scholars have an opportunity to add to the knowledge base currently in existence.

I set out to expand on the current state of research by conducting fixed effects analyses of employment stability and tenure. In other words, I use fixed effects analyses to compare the employment outcomes of ex-inmates to themselves, and determine whether incarceration produces negative employment outcomes.

Method

Variables

Dependent Variables: Employment Outcomes

The RYDS questions interview subjects at Waves 13 and 14 about a variety of topics including employment, incarceration, arrest, and probation experiences since last interviewed. In addition to being asked the start and end dates of these experiences, the surveys include follow
up questions regarding the details of events. Respondents are able to report multiple and concurrent events. I use the responses from these questions to create two dependent employment outcome measures *employment stability* and *employment tenure*.

As discussed above, previous literature suggests that individuals who have been convicted of crimes experience greater employment turnover than their never-convicted peers (Nagin and Waldfogel 1995). However, it is important to explore whether employment turnover, or employment stability as I refer to it in this paper, is also an issue for ex-inmates compared to their never-incarcerated peers. For this reason, I create a dependent variable, *employment stability*, to measure the number of jobs each respondent holds between Waves 12 and 14 (covers approximately 23 to 29 years-old). To accommodate the format of fixed effects analysis, I create two *employment stability* measures, one to reflect the number of jobs respondents hold during the pre-contact period and a second for the number of jobs held during the post-contact period.

Table 7 shows the means, standard deviations, and ranges of both the employment outcomes explored in this study, including *employment stability*. In the pre-contact period, the mean number of jobs held by the arrest/probation subsample is 1.9 jobs. The average number of jobs held during the pre-contact period by individuals in the incarceration subsample is 1.7 jobs. In the post-contact period, the arrest/probation subsample demonstrates a decreased average of 1.4 jobs held, while the mean number of jobs held by the incarceration subsample remains at 1.7 jobs. A concern with the dependent measure *employment stability* is the comparative size of the means to the standard deviations for the incarceration subsample. With a mean of 1.7 jobs held in the pre-contact period, and an equally large standard deviation of 1.7 jobs, the *employment stability* measure demonstrates skew towards larger values of *employment stability* for the incarceration subsample. This phenomenon is even more of an issue with the next dependent
Table 7. Descriptive Statistics of Employment Stability, Tenure, and Incarceration Measures

<table>
<thead>
<tr>
<th>Dependent Measures</th>
<th>Arrest/Probation Subsample</th>
<th>Incarceration Subsample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre-Contact Period</td>
<td>Post-Contact Period</td>
</tr>
<tr>
<td>Mean</td>
<td>SD</td>
<td>Range</td>
</tr>
<tr>
<td>Employment Stability</td>
<td>1.9</td>
<td>1.5</td>
</tr>
<tr>
<td>Employment Tenure</td>
<td>363.1</td>
<td>569.2</td>
</tr>
<tr>
<td>Incarceration Measures</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Incarceration</td>
<td>1.0</td>
<td>0</td>
</tr>
<tr>
<td>First Incarceration</td>
<td>0.4</td>
<td>1</td>
</tr>
<tr>
<td>Multiple Incarcerations</td>
<td>0.6</td>
<td>1</td>
</tr>
<tr>
<td>Number of Incarcerations</td>
<td>1.6</td>
<td>1.9</td>
</tr>
<tr>
<td>Age at First Incarceration</td>
<td>23.0</td>
<td>4.2</td>
</tr>
<tr>
<td>Observation length</td>
<td>1517.5</td>
<td>762.4</td>
</tr>
</tbody>
</table>
measure *employment tenure*, which demonstrates standard deviations larger than the means of each period. In the next section, I will discuss how I account for this skew by logging the dependent measures.

I continue expanding on the research performed by Nagin and Waldfogel (1995) by exploring whether ex-inmates hold jobs for shorter lengths of time than individuals in the never-incarcerated comparison sample. To do this, I construct a dependent variable *employment tenure* that measures the average length of time a respondent holds a job, with separate measures for the pre- and post-contact periods. Individuals in the comparison group, who experience arrests and/or convictions that result in probation sentences, demonstrate a mean *employment tenure* length of 363.1 days during the pre-contact period, and 134.2 days during the post-contact period. Comparatively, individuals in the incarceration subsample hold jobs during the pre-contact period for an average of 415.5 days, and a mean of 468.4 days during the post-contact period. Based on previous research, my finding that the mean tenure length drops significantly for the comparison group during the second period is unexpected. Research suggests that as people get older, they settle into employment, thus demonstrating more employment stability and longer employment lengths (DiPrete 1989; Rosenfeld 1992; Spilerman 1977). Therefore, the employment tenure of the entire population should increase between the pre- and post-contact periods, not decrease. This suggests that an historical effect occurs during the observation period. In other words, the study sample reflects a national drop in employment, hours, and earnings between 2000 and 2004 (Bureau of Labor Statistics 2013). While this historic effect could potentially mask the effect of incarceration on employment tenure, it poses less of a threat as long as the employment drop affects the entire sample. Moreover, while the means could suggest that historical effects disproportionately influence subsamples of this study, additional analysis is
necessary to reach such a conclusion. By including an interaction term between incarceration measures and post-contact in the fixed effects models, I am able to measure whether the employment drop affects the entire sample, thus, using employment tenure as a measure is not problematic.

Control Variables

Since incarceration, arrest, or conviction to probation occur at different times in the LHC period for each respondent, the length of the pre- and post-contact periods vary for each individual. As such, it is necessary to control for the observation length of each period for each respondent. Pre-contact observation length measures the amount of time in days between a respondent’s last interview and their first experience of incarceration, arrest, or probation during the LHC period. The average observation lengths for the arrest/probation subsample and the incarceration subsample are comparable. The pre-contact period for the arrest/probation subsample is 1517.5 days (4.2 years) and is 1334.1 days (3.7 years) for the incarceration subsample. However, observation length ranges from 151 to 2922 days (7.6 years) for the arrest/probation subsample and from 28 to 3228 days (8.8 years) for the incarceration subsample during this period. Post-contact observation length measures the amount of time in days between a respondent’s experience of arrest, probation, or release from incarceration and either the end of the LHC period, or a second occurrence of the experience (incarceration, arrest, or probation) during LHC. The average observation length in the pre-contact period is 1670.4 days (4.6 years) for the arrest/probation subsample and 1181.6 days (3.2 years) for the incarceration subsample. However, observation lengths range from 59 to 3377 days (9.1 years) for the arrest/probation subsample and from 31 to 3226 days (8.7 years) for the incarceration subsample.

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6 While the pre- and post-contact periods demonstrate a large range of lengths, observation lengths for each period are evenly distributed.
Results

Employment Stability

In Table 8, I explore *employment stability*, or the number of jobs held over the pre- and post-contact periods, using fixed effects analyses. I hypothesize that incarceration negatively affects individuals’ employment stability by reducing the ability of the individual to maintain a single job, or by increasing the number of jobs individuals need to hold in order to make a living wage.

The main purpose of *observation length* is to control for respondents’ varying observation lengths during the pre- and post-contact periods. Similarly, *post-contact* acts as a control in the model, differentiating between the pre- and post-contact periods. However, it is also possible to use these measures to determine the relationship between *observation length* or *post-contact* and employment stability. In doing so I find that, *observation length* and *post-contact* are statistically significant across all five employment stability models. *Observation length* suggests that for every additional observation day, there is an expected increase of 0.02 percent in the number of jobs held during the observation. As such, *observation length* has a small effect on the number of jobs respondents hold. *Post-contact* indicates that never-incarcerated individuals and ex-inmates hold about 14 percent fewer jobs during the post-contact period than in the pre-contact period. Based on my hypothesis, this is a positive outcome, demonstrating that, as time goes on, individuals experience greater stability in employment.

It is important to note that individual level differences (which the fixed effects method of analysis hold constant), *observation length*, and *post-contact* account for the majority of the predictive power of the models. While the models in Table 8 demonstrate $R^2$ values that range

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7 To make the fixed effects results of the log transformed *employment stability* measure interpretable, Table 8 reflects inverse transformed estimates.
Table 8. Inverse Transformed* Fixed Effects Estimates of Employment Stability

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observation length</td>
<td>0.0002***</td>
<td>0.0002***</td>
<td>0.0002***</td>
<td>0.0002***</td>
<td>0.0003***</td>
</tr>
<tr>
<td>Post-contact</td>
<td>-0.137*</td>
<td>-0.136*</td>
<td>-0.136*</td>
<td>-0.137*</td>
<td>0.053</td>
</tr>
<tr>
<td>Incarceration</td>
<td>-0.489*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact * Incarceration</td>
<td>0.183*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>First incarceration</td>
<td></td>
<td>-0.571**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact * First incarceration</td>
<td></td>
<td>0.125</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Multiple incarcerations</td>
<td></td>
<td>-0.504*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact * Multiple incarcerations</td>
<td></td>
<td>0.221*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of incarcerations</td>
<td></td>
<td></td>
<td>0.165</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact * Number of incarcerations</td>
<td></td>
<td>0.006</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age at first incarceration</td>
<td></td>
<td></td>
<td></td>
<td>-0.015</td>
<td></td>
</tr>
<tr>
<td>Post-contact * Age at first incarceration</td>
<td></td>
<td></td>
<td></td>
<td>-0.018</td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>.686</td>
<td>.674</td>
<td>.686</td>
<td>.686</td>
<td>.703</td>
</tr>
<tr>
<td>N</td>
<td>230</td>
<td>128</td>
<td>158</td>
<td>230</td>
<td>132</td>
</tr>
</tbody>
</table>

*a To compensate for the pre-analysis transformation of employment stability, estimates are inverse transformed in the table.

* p < .05 ** p < .01 *** p < .001

between 67.4 and 68.6 percent, the incarceration measure in each model only explains between 0.6 and 1.3 percent of variation observed within the sample. Thus, most of the variance explained results from comparing participants to themselves.

In Model 1, the incarceration measure and the interaction measure (post-contact*incarceration) are statistically significant. This suggests that individuals in the incarcerated subsample hold 49 percent fewer jobs than their never-incarcerated peers. However, during the post-incarceration period, the incarcerated subsample demonstrates an 18 percent
increase in the number of jobs held, a change that the never-incarcerated subsample do not mirror during the same period. The previously discussed finding that individuals with prior incarcerations demonstrate longer periods of non-employment provides context here. Since non-employment necessarily means not holding a job, it is understandable that individuals in the incarceration subsample also hold fewer jobs. Unfortunately, this means that holding fewer jobs may not be a positive employment outcome for this group. Instead, this subsample holds fewer jobs because they experience more non-employment. During the post-incarceration experience, the incarceration subsample demonstrates a small increase in the number of jobs held, but this adjustment does not help the incarceration subsample reach the “healthier” employment stability level demonstrated by the never-incarcerated subsample.

Model 2 shows that compared to those who never experience incarceration, individuals who experience their first incarceration during the LHC period hold 57 percent fewer jobs. However this difference is not a result of the incarceration experienced, since the interaction measure (post-contact*first incarceration) fails to predict employment stability. This suggests that unlike above, where all ex-inmates clump together and demonstrate lower employment stability pre-contact, with increased employment stability post-incarceration, individuals who experience their first incarceration hold fewer jobs pre-incarceration compared to their never-incarcerated peers, and fail to show any improvement in employment stability post-incarceration.

By comparison, Model 3 demonstrates that both the main effects and interaction effects of multiple incarcerations are statistically significant predictors of employment stability. Similar to the findings in Model 1, individuals who have experienced prior incarceration(s) hold 50 percent fewer jobs compared to individuals in the never-incarcerated subsample. However, post-incarceration, individuals with multiple incarceration experiences demonstrate a 22 percent
increase in the number of jobs they hold. This finding mirrors my findings for Model 1. Incarcerated individuals experience fewer jobs over the pre- and post-contact periods because they spend more time unemployed. During the post-incarceration experience, the multiple incarcerations subsample demonstrates a small increase in the number of jobs held, but this adjustment does not help the multiple incarcerations subsample reach the “healthier” employment stability level demonstrated by the never-incarcerated subsample during the post-contact period.

Interestingly, my finding that post-incarceration employment stability increases compared to pre-incarceration for the aggregated incarceration subsample and the multiple incarceration subsample mirrors the findings of Kling (2006) and Lalonde and Cho (2008). However, by comparing the employment stability of the incarceration and multiple incarceration subsamples to that of the never incarcerated subsample, I discover that the improvements observed in employment stability fail to compensate for the negative employment outcomes demonstrated by the incarceration subsamples prior to incarceration. Therefore, I am able to add context to the findings of Kling (2006) and Lalonde and Cho (2008).

I also explore the possibility that the effect of incarceration on employment stability compounds with each additional incarceration experience in Model 4, using a measure of incarceration that counts the number of times each individual experiences incarceration. This model finds the control measures observation length and post-contact to be statistically significant, but the incarceration measures are not. While it is possible that each additional incarceration fails to create cumulative disadvantage among individuals who experience incarceration, it is also important to note that this measure does not, and cannot address the effect of cumulative incarceration length on ex-inmates’ employment stability. Therefore, I suspect that number of incarcerations fails to predict employment outcomes for ex-offenders because this
measure does not capture the catalyst of negative employment outcomes, and not because the number of incarcerations experienced by subjects fails to influence employment outcomes. While it is not possible to measure cumulative incarceration length based on the data currently available via the RYDS, future research would benefit from including cumulative incarceration length in exploring employment outcomes.

Similarly, in Model 5, observation length proves to be statistically significant, however age at first incarceration does not. Therefore, regardless of the age at which individuals experience their first incarceration, ex-inmates are likely to hold fewer jobs prior to incarceration compared to their never-incarcerated peers, and individuals with multiple incarcerations are likely to demonstrate a small increase in the number of jobs they hold post-incarceration.

Considering the five models measuring employment stability, I find a complex story of employment disadvantage. In contrast to my hypothesis that individuals who experience incarceration will demonstrate less job stability by holding larger numbers of jobs, I find that individuals in the incarceration subsample hold fewer jobs over the pre- and post-contact periods. While this finding differs from that of Nagin and Waldfogel (1995) and Waldfogel (1994), based on the findings of Emmert (2014) it is possible to conclude that individuals in the incarceration subsample hold fewer jobs because they spend longer periods non-employed. The finding that post-incarceration employment stability increases compared to pre-incarceration for the aggregated incarceration subsample and the multiple incarceration subsample mirrors the findings of Kling (2006) and Lalonde and Cho (2008). However, by comparing the employment stability of the incarceration and multiple incarceration subsamples to that of the never-incarcerated subsample, it is clear that the improvements observed in employment stability fail to compensate for the negative employment outcomes demonstrated by the incarceration
subsamples prior to incarceration. Again, this adds context to the findings of Kling (2006) and Lalonde and Cho (2008).

*Employment Tenure*

Using a sample of men in London, Nagin and Waldfogel (1995) suggest that, in addition to influencing the number of jobs individuals hold, conviction negatively affects the tenure of employment – the length of time individuals hold jobs. By comparison, I consider the influence of incarceration on employment tenure, in much the same way I have employment stability.

Table 9 reflects inverse transformed estimates (necessary because employment tenure is log transformed pre-analyses to account for skew), and demonstrates consistent findings for each of the five incarceration measures. As in past models, the control variables observation length and post-contact reach statistical significance in each of the models measuring employment tenure. Observation length suggests that for each additional day of observation, individuals demonstrate an increase in employment tenure of 0.1 percent. Thus, the effect of observation length on employment tenure is small. Meanwhile, post-contact suggests that both the incarcerated and never-incarcerated subsamples demonstrate about a 70 percent drop in average employment length in the post-contact period. Based on the findings of previous research, this outcome is unexpected. Research suggests that as people get older, they settle into employment, thus demonstrating more employment stability and longer employment lengths (DiPrete 1989; Rosenfeld 1992; Spilerman 1977). Therefore, the employment tenure of the entire population should increase between the pre- and post-contact periods, not decrease. This suggests a historical effect occurs during the observation period. In other words, the study sample is very likely reflecting regional or national changes in employment or economics between 1997 and 2005. Moreover, my finding is confirmed by data from the Bureau of Labor Statistics (2013)
Table 9. Inverse Transformed\textsuperscript{a} Fixed Effects Estimates of Employment Tenure

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observation length</td>
<td>0.001***</td>
<td>0.001***</td>
<td>0.001***</td>
<td>0.001***</td>
<td>0.001***</td>
</tr>
<tr>
<td>Post-contact</td>
<td>-0.698***</td>
<td>-0.698***</td>
<td>-0.697***</td>
<td>-0.698***</td>
<td>-0.655***</td>
</tr>
<tr>
<td>Incarceration</td>
<td>-0.988**</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact*Incarceration</td>
<td>0.099</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>First incarceration</td>
<td>-0.995***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact*First incarceration</td>
<td>0.254</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Multiple incarcerations</td>
<td>-0.988**</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact*Multiple incarcerations</td>
<td>-0.012</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of incarcerations</td>
<td>0.429</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact*Number of incarcerations</td>
<td>-0.107</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age at first incarceration</td>
<td>-0.087</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact*Age at first incarceration</td>
<td>0.036</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(R^2)</td>
<td>.884</td>
<td>.914</td>
<td>.862</td>
<td>.885</td>
<td>.894</td>
</tr>
<tr>
<td>(N)</td>
<td>230</td>
<td>128</td>
<td>159</td>
<td>230</td>
<td>132</td>
</tr>
</tbody>
</table>

\textsuperscript{a} To compensate for the pre-analysis transformation of employment tenure, estimates are inverse transformed in the table.

\* p < .05 ** p < .01 *** p < .001

which demonstrates drastic reductions in employment, hours, and earnings nationally between mid-2000 and mid-2003.

As was discussed above, individual level differences (which the fixed effects method of analysis controls for), observation length, and post-contact account for the majority of the predictive power of the models for employment tenure. While the models in Table 9 demonstrate \(R^2\) values that range between 86.2 and 91.4 percent, the incarceration measure in each model
only explains between 0 and 3 percent of variation observed within the sample. Thus, most of the variance explained results from comparing participants to themselves.

Models 1, 2, and 3, which measure the effect of *incarceration*, *first incarceration*, and *multiple incarcerations* on employment tenure, each find the incarceration main effect to be statistically significant. In Model 1, individuals who experience incarceration have an average employment length that is 99 percent shorter than their never-incarcerated peers’ employment lengths. When the incarcerated subsample is broken down into *first* and *multiple incarceration* subsamples, I find similar results. Individuals who experience their first incarceration during the observation period have an average employment length that is nearly 100 percent (99.5) shorter than their never-incarcerated peers (Model 2). Meanwhile, individuals who have previous incarceration experience(s) have employment tenures that are almost 99 percent shorter than the never-incarcerated subsample (Model 3). However, in each of these models, the interaction measure between the respective incarceration subsample and *post-contact* fail to be statistically significant. Thus, while individuals who experience incarceration, whether it is a first experience or one of multiple incarceration experiences, demonstrate significantly shorter employment tenures than their never-incarcerated peers, this pattern is evident prior to the incarceration observed in this study. This supports the findings of previous research, in that ex-inmates demonstrate considerable employment disadvantages even prior to experiencing incarceration (Caspi et al. 1998; Gottfredson and Hirschi 1990; Moffitt 1993; Sullivan 1989).

However, Model 4 demonstrates that when incarceration is measured as a count of imprisonment experiences, only *observation length* and *post-contact* successfully predict employment tenure. Again, this finding may reflect a weakness in the measure *number of incarcerations*, as this measure does not, and cannot address the effect of cumulative
incarceration length on ex-inmates’ employment tenure. As such, it is possible that number of incarcerations fails to predict employment outcomes for ex-offenders because it does not capture the catalyst of negative employment outcomes, and not because the number of incarcerations experienced by subjects fails to influence employment outcomes.\textsuperscript{8} Similarly, in Model 5, age at first incarceration fails to have a significant effect on employment tenure. Therefore, regardless of the number of incarceration experiences individuals may have, or the age at which individuals experience their first incarceration, ex-inmates demonstrate the same trend in employment tenure as their never-incarcerated peers.

Taking the findings of each model into consideration, two trends emerge. The entire sample demonstrates a decreasing trend in employment tenure over the observation periods. This finding reflects a historic effect that occurs during the observation period. As mentioned above, the study sample reflects a national drop in employment, hours, and earnings between 2000 and 2004 (Bureau of Labor Statistics 2013). The second trend observed is that individuals with incarceration experience hold jobs for slightly shorter lengths of time. However, this trend is not a result of the incarceration experienced during the observation period.

\textbf{Discussion}

Nagin and Waldfogel (1995) find that individuals who have been convicted of a crime experience greater employment turnover and shorter employment tenures than their never-convicted peers. Building on these findings, I explore whether incarceration adds to the employment disadvantages of convicted individuals by comparing individuals who have experienced incarcerations to those who have been arrested or convicted of a crime but never

\textsuperscript{8} While it is not possible to measure cumulative incarceration length based on the data available in this study, future research would benefit from including cumulative incarceration length in exploring employment outcomes.
incarcerated. This allows me to determine whether employment disadvantages accumulate as individuals experience more contact with the criminal justice system.

As was the finding of Nagin and Waldfogel (1995), I hypothesized ex-inmates would hold more jobs than their never-incarcerated peers, demonstrating employment instability. In reality, this was not what I find. In general, ex-inmates hold fewer jobs than their never-incarcerated peers do, but a nuanced story emerges when employment stability is observed separately for first-time ex-inmates and ex-inmates with multiple incarcerations.

When ex-inmates are studied in aggregate, I find they hold fewer jobs prior to incarceration than their never-incarcerated peers do. However, post-incarceration, ex-inmates hold more jobs than they did pre-incarceration. Disaggregating employment outcomes based on ex-inmates’ incarceration experiences reveals a more detailed story. In reality, only ex-inmates with multiple incarceration experiences hold more jobs post-incarceration than pre-incarceration. By comparison, ex-inmates with single incarceration experiences maintain similar numbers of jobs pre- and post-incarceration. Thus, disaggregating ex-inmates based on number of incarcerations experienced proves to be extremely important, and possibly hints at a cumulative effect when multiple incarcerations are experienced.

Although my findings seem to contradict Nagin and Waldfogel’s (1995), both suggest the same concern: ex-convicts and ex-inmates are not holding equivalent numbers of jobs as their peers. In a previous study, I find that ex-inmates with multiple incarceration experiences demonstrate longer cumulative non-employment than their peers (Emmert 2014). When considered in unison with the finding that many ex-inmates hold fewer jobs than their peers do, it would appear holding fewer jobs is not a sign of employment stability among ex-inmates in this study, but actually a symptom of non-employment.
Also of note is my finding that ex-inmates with multiple incarceration experiences hold more jobs post-incarceration than they did prior to incarceration. This conclusion mirrors the employment findings of Kling (2006) and Lalonde and Cho (2008), and appears to suggest that ex-inmates demonstrate employment advantages post-incarceration. However, by comparing the employment stability of ex-inmates to their never-incarcerated peers, it is possible to conclude that this improvement comes with a caveat. Despite holding more jobs post-incarceration than prior to incarceration, ex-inmates with multiple incarceration experiences still hold fewer jobs post-incarceration than their never-incarcerated peers do. This provides additional context for Kling's (2006) and Lalonde and Cho's (2008) findings.

I find general support for Nagin and Waldfogel’s (1995) conclusions regarding the effect of negative contact with the criminal justice system on employment tenure. Individuals who experience incarceration, whether it is a first experience or one of multiple incarceration experiences, demonstrate significantly shorter employment tenures than their never-incarcerated peers. In addition, my finding that ex-inmates demonstrate reduced employment tenures even prior to experiencing incarcerations adds to a growing literature regarding employment disadvantages prior to incarceration (Caspi et al. 1998; Gottfredson and Hirschi 1990; Moffitt 1993; Sullivan 1989). Yet, I add to Nagin and Waldfogel’s (1995) findings by concluding that incarceration experiences do not exacerbate or add to ex-inmates’ pre-existing employment tenure disadvantages.

The presence of history effects on employment during the observation period, and the unique nature of Rochester, New York both prove to be limiting factors in this study. An unexpected downward trend in employment tenure exhibited by the entire study sample across the observation period reflects a national drop in employment, hours, and earnings between 2000
and 2004 (Bureau of Labor Statistics 2013). While I control for this trend, future studies would benefit from exploring employment outcomes during stable economic periods to minimize external threats. Similarly, the sample and data used for this study is geographically limiting and cannot address cumulative incarceration length. Continued research using diverse geographical samples to explore the effect of incarceration on employment stability and tenure is warranted.

Several opportunities and programs that currently exist can improve the employment outcomes of ex-inmates following incarceration. Inmates who participate in Florida’s work release program are significantly less likely to recidivate, and experience significantly better employment and earnings outcomes over a three-year follow-up period (Berk 2008). Similarly, Washington State runs a Correctional Industries program that emphasizes job skill development and work ethics. Research suggests that Washington’s Correctional Industries program has positive effects on offenders recidivism rates, odds of acquiring employment, wages earned, and employment tenures (Cox 2009; Drake 2003). To make the most of this and other programs across the United States, states and the federal government need to expand work release programs to cover more areas of the country and accommodate additional eligible offenders.
CHAPTER 5. Subsistence via Public Assistance: Exploring ex-inmates’ use of public assistance following incarceration

Introduction

With research demonstrating that ex-inmates suffer negative employment outcomes, it is vitally important to understand how incarceration influences individual and societal finances and resource use. When incarceration compromises wages, income, and wage mobility, ex-inmates can find it difficult to make a living wage. As such, it is conceivable that ex-inmates rely more heavily on public assistance to supplement their earnings. While many have come to accept the financial cost of incarceration, it is possible that reliance on public assistance post-incarceration results in unintended costs to ex-inmates and society in the long-term. Before it is possible to determine the monetary cost of incarceration, it is necessary to explore whether ex-inmates use or rely on public assistance at greater rates than their never-incarcerated peers do. I attempt to do just this using fixed effects and event history analyses to consider the lengths of time ex-inmates use public assistance, and whether ex-inmates turn to public assistance more quickly than individuals who have never experienced incarceration.

Method

Variables

Dependent Variables: Public Assistance

The RYDS interviews subjects at Waves 13 and 14 about a variety of topics including employment, incarceration, arrest, probation, and financial experiences since last interviewed. Questions included start and end dates of these experiences, as well as additional follow up questions regarding the details of events. Respondents were also able to report multiple and concurrent events. I use the responses from these questions to create two dependent measures concerning public assistance.
In the fixed effects models, *public assistance* is a dependent measure of the cumulative length of time respondents report using public assistance. *Public assistance* is measured with separate pre- and post-contact period variables for the fixed effects model. It is important to note that in New York, public assistance is available in many forms, including food stamps, housing assistance, childcare services, employment training, and other services. Eligibility for public assistance and the duration of public assistance differs among services. However, many of the services offered are available to both unemployed and employed individuals. Eligibility is often determined based on income, resources, household composition, and citizenship. Similarly, in New York, public assistance and food stamp eligibility is not restricted for individuals with criminal histories (Legal Action Center 2009). However, the Housing Authority can consider relevant criminal history when assessing eligibility for public housing (Legal Action Center 2009).

Table 10 shows the means, standard deviations, and ranges of all the variables used in this study. In the pre-contact period, the arrest/probation subsample uses public assistance for a mean length of 68 days (over 2 months), while the mean length of public assistance use for the incarceration subsample is 87 days (almost 3 months). The mean length of time using public assistance in the post-contact period is similar, averaging 73 days (over 2 months) for the arrest/probation subsample, and 72 days (over 2 months) for the incarceration subsample. A concern evident with the dependent measure *public assistance* is the comparative size of the mean to the standard deviation. With subsample standard deviations nearly three to five times larger than their respective means, *public assistance* demonstrates a large skew towards larger values. While initially I corrected for this issue by logging the dependent variable, the estimates still appeared skewed towards larger values. A Box-Cox transformation analysis of public
Table 10. Descriptive Statistics of Public Assistance, Time to Public Assistance, Incarceration, and Control Measures

<table>
<thead>
<tr>
<th></th>
<th>Arrest/Probation Subsample</th>
<th>Incarceration Subsample</th>
<th>Incarceration Subsample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre-Contact Period</td>
<td>Post-Contact Period</td>
<td>Pre-Contact Period</td>
</tr>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Range</td>
</tr>
<tr>
<td>Dependent Measures</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Public assistant</td>
<td>68.1</td>
<td>293.2</td>
<td>1916</td>
</tr>
<tr>
<td>Time to public assistance</td>
<td>1698.4</td>
<td>789.1</td>
<td>3254</td>
</tr>
<tr>
<td>Incarceration Measures</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Incarceration</td>
<td>0.7</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>First Incarceration</td>
<td>0.3</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Multiple Incarcerations</td>
<td>0.4</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Number of Incarcerations</td>
<td>1.2</td>
<td>1.8</td>
<td>9</td>
</tr>
<tr>
<td>Age at First Incarceration</td>
<td>23.0</td>
<td>4.2</td>
<td>17</td>
</tr>
<tr>
<td>Control Measures</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observation Length</td>
<td>1517.5</td>
<td>762.4</td>
<td>2771</td>
</tr>
<tr>
<td>Male</td>
<td>0.8</td>
<td>1</td>
<td>0.9</td>
</tr>
<tr>
<td>Black</td>
<td>0.7</td>
<td>1</td>
<td>0.8</td>
</tr>
<tr>
<td>High School Graduate</td>
<td>0.5</td>
<td>1</td>
<td>0.3</td>
</tr>
</tbody>
</table>
assistance revealed the necessity to transform the dependent measure by the $\lambda = -1.25$ in order to achieve normality. This transformation provided similar (but smaller) estimates compared to those produced by the logged dependent variables.

In order to explore use of public assistance by ex-inmates using event history analysis, I construct a dependent measure, *time to public assistance*, which compares the rate of public assistance use by individuals who experience incarceration and those who do not. For individuals in the incarceration subsample, *time to public assistance* measures the amount of time (in days) between release from incarceration and initial use of public assistance. For the never-incarcerated subsample, *time to public assistance* measures the length of time (in days) between individuals’ experience of arrest or conviction to probation and use of public assistance.\(^9\)

*Time to public assistance* has a mean of 1698 days (4.6 years) and a standard deviation of 789 days (2.1 years) for the arrest/probation subsample. By comparison, individuals in the incarceration subsample wait 1070 days (2.9 years) before using public assistance, with a standard deviation of 862 days (2.4 years).

Control Variables

Since incarceration, arrest, or conviction to probation occurs at different times in the LHC period for each respondent, the *observation length*, or length of pre- and post-contact periods, vary for each individual. As such, it is necessary to control for the observation length of each period for each respondent in the fixed effects analyses. Pre-contact *observation length* measures of the amount of time in days between a respondent’s last interview and their first experience of incarceration, arrest, or probation during the LHC period. The average *observation lengths* for the arrest/probation subsample and the incarceration subsample are comparable. The

\(^9\) Some individuals in the sample never use public assistance. These individuals are still included in the sample.
pre-contact period for the arrest/probation subsample is 1517.5 days (4.2 years) and is 1334.1 days (3.7 years) for the incarceration subsample. However, observation length ranges from 151 to 2922 days (5 months to 8 years) for the arrest/probation subsample and from 28 to 3228 days (1 month to 8.8 years) for the incarceration subsample during this period. Post-contact observation length measures the amount of time in days between a respondent’s experience of arrest, probation, or release from incarceration and either the end of the LHC period, or a second occurrence of the experience (incarceration, arrest, or probation) during LHC. The average observation length in the pre-contact period is 1670.4 days (4.6 years) for the arrest/probation subsample and 1181.6 days (3.2 years) for the incarceration subsample. However, observation lengths range from 59 to 3377 days (2 months to 9.2 years) for the arrest/probation subsample and from 31 to 3226 days (1 month to 8.8 years) for the incarceration subsample.

I also include several control variables in the event history analyses that previous research finds influential to employment. The variables male and black are both dichotomous measures of demographic information about each individual in the sample. Similarly, I include a dichotomous measure, high school graduate, to denote individuals in the sample who graduated from high school. I include this measure to reflect Western's (2002) finding that education is a mitigating factor of wage and wage mobility among ex-inmates. This is important since wages and wage mobility influence ex-inmates’ necessity to use public assistance to supplement earnings.

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10 While the pre- and post-contact periods demonstrate a large range of lengths, observation lengths for each period are evenly distributed.
Results

Fixed Effects Analyses

Table 11 demonstrates that only *observation length* and *multiple incarcerations* predict the length of time individuals rely on public assistance. As stated previously, the purpose of the variable *observation length* is to hold constant respondent’s observation length in the pre- and post-contact periods. *Observation length* also demonstrates that when respondents have longer pre- and post-contact periods there is an increased chance that they will use public assistance for slightly longer. While this finding is logical, as the longer the observation period, the longer the period of *public assistance* can last, it is also trivial due to its extremely small influence (0.00004 to 0.0001 percent increase) on *public assistance* length.

Despite being control variables in the model equations, and having little to no impact on the model estimates, individual level differences (which the fixed effects method controls for), *observation length*, and *post-contact* account for the majority of the predictive power generated in each model. In the five *public assistance* models, these three controls account for between 55.6 and 66.5 percent of the predictive power of each model. Thus, the incarceration measures account for approximately 5 percent of each models’ explained variance.

More interesting is my finding that individuals who have experienced multiple incarcerations use public assistance for 70 percent longer than individuals who have never experience incarceration. However, the interaction term *post-contact* *multiple incarcerations* fails to be significant, thus, individuals who have previously experienced incarceration demonstrate longer use of public assistance even prior to the incarceration observed in this study.

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11 To make the fixed effects results of the transformed *public assistance* measure interpretable, Table 11 reflects inverse transformed estimates.
Table 11. Inverse Transformeda Fixed Effects Estimates of Public Assistance Use

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observation length</td>
<td>0.00004**</td>
<td>0.00004*</td>
<td>0.00002</td>
<td>0.00004**</td>
<td>0.0001*</td>
</tr>
<tr>
<td>Post-contact</td>
<td>-0.017</td>
<td>-0.017</td>
<td>-0.017</td>
<td>-0.017</td>
<td>0.044</td>
</tr>
<tr>
<td>Incarceration</td>
<td>0.522</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-contact* Incarceration</td>
<td>0.033</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>First incarceration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.433</td>
</tr>
<tr>
<td>Post-contact* First incarceration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.089</td>
</tr>
<tr>
<td>Multiple incarcerations</td>
<td></td>
<td></td>
<td></td>
<td>0.531*</td>
<td></td>
</tr>
<tr>
<td>Post-contact* Multiple incarcerations</td>
<td></td>
<td></td>
<td></td>
<td>-0.011</td>
<td></td>
</tr>
<tr>
<td>Number of incarcerations</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.057</td>
</tr>
<tr>
<td>Post-contact* Number of incarcerations</td>
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<td></td>
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<td>-0.010</td>
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</tr>
<tr>
<td>Age at first incarceration</td>
<td></td>
<td></td>
<td></td>
<td>-0.008</td>
<td></td>
</tr>
<tr>
<td>Post-contact* Age at first incarceration</td>
<td></td>
<td></td>
<td></td>
<td>0.008</td>
<td></td>
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<tr>
<td>$R^2$</td>
<td>.710</td>
<td>.715</td>
<td>.771</td>
<td>.710</td>
<td>.606</td>
</tr>
<tr>
<td>$N$</td>
<td>220</td>
<td>123</td>
<td>152</td>
<td>220</td>
<td>113</td>
</tr>
</tbody>
</table>

a To compensate for the pre-analysis transformation of public assistance, estimates are inverse transformed in the table.

* p < .05 ** p < .01 *** p < .001.

For multiple incarcerations to significantly affect public assistance length, while post-contact*multiple incarcerations, post-contact*first incarceration, and number of incarcerations do not,a difference between the first incarceration and multiple incarceration subsamples must exist beyond the mere number of times each subsample has experienced incarceration. Emmert (2013) finds that age at first incarceration explains why individuals with multiple incarcerations

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12 While it is not possible to measure cumulative incarceration length based on the data available in this study, future research would benefit from including cumulative incarceration length when exploring employment outcomes.
(incarcerated before age 23) demonstrate employment disadvantages while individuals experiencing their first incarcerations (incarcerated after age 23) do not. However, age at first incarceration is not a significant predictor of the length of time ex-inmates use public assistance post-incarceration compared to never-incarcerated individuals.

Event History Analyses

Overall, 21 percent of respondents report using public assistance post-contact with the criminal justice system. During the corresponding period, between 1997 and 2006, between 5.8 and 10.3 percent of people in the United States received public assistance (Aud, KewalRamani, and Frohlich 2011). Thus, this sample shows a higher rate of public assistance use than was observed in the U.S. population during the same time.

Table 12 shows the analyses that estimate the effect of incarceration on the rate that participants use public assistance. In Model 1, gender and incarceration both significantly influence the rate at which individuals use public assistance. More specifically, at any given time, the hazard of public assistance is 43 percent as large for males as it is for females in Model 1. This finding is not surprising considering that females in the RYDS sample are more likely to be primary caregivers for children compared to males (Lizotte et al. 1994). As primary caregivers, female participants are either financially dependent on an outside source of income while they care for children, or responsible for earning an income that can cover the costs of childcare and living expenses. This likely contributes to female participants’ use of public assistant. Additionally, as of 2013, women earn, on average, 77 cents for every dollar men earn.

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13 To ensure that having children does not have an impact on public assistance use, I separately tested whether having children and the number of children participants have influence this model. Both child measures failed to reach significance, and are not included in the model in Table 12.
Table 12. Public Assistance Use Event History Analysis Models

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th></th>
<th>Model 2</th>
<th></th>
<th>Model 3</th>
<th></th>
<th>Model 4</th>
<th></th>
<th>Model 5</th>
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<tbody>
<tr>
<td></td>
<td>Beta</td>
<td>HR</td>
<td>Beta</td>
<td>HR</td>
<td>Beta</td>
<td>HR</td>
<td>Beta</td>
<td>HR</td>
<td>Beta</td>
<td>HR</td>
</tr>
<tr>
<td>Incarceration</td>
<td>1.278**</td>
<td>3.591</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>First incarceration</td>
<td>1.258**</td>
<td>3.519</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Multiple incarcerations</td>
<td>1.368**</td>
<td>3.928</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of incarcerations</td>
<td>0.078</td>
<td>1.081</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age at first incarceration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.020</td>
<td>1.020</td>
</tr>
<tr>
<td>Male</td>
<td>-0.835*</td>
<td>0.434</td>
<td>-1.050*</td>
<td>0.350</td>
<td>-0.680</td>
<td>0.507</td>
<td>-0.817*</td>
<td>0.442</td>
<td>-0.730</td>
<td>0.482</td>
</tr>
<tr>
<td>Black</td>
<td>0.312</td>
<td>1.366</td>
<td>0.357</td>
<td>1.429</td>
<td>0.190</td>
<td>1.209</td>
<td>0.373</td>
<td>1.453</td>
<td>0.434</td>
<td>1.543</td>
</tr>
<tr>
<td>High school graduate</td>
<td>-0.209</td>
<td>0.812</td>
<td>-0.403</td>
<td>0.669</td>
<td>0.104</td>
<td>1.110</td>
<td>-0.272</td>
<td>0.762</td>
<td>-0.416</td>
<td>0.660</td>
</tr>
<tr>
<td>N</td>
<td>245</td>
<td>133</td>
<td>173</td>
<td>245</td>
<td>125</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

NOTE: Hazard rates (HR).  * p < .05  ** p < .01  *** p < .001
(DeNavas-Walt and Proctor 2014). This wage gap similarly leaves females at a financial disadvantage compared to males.

Unsurprisingly, I also find that the hazard of public assistance is 3.6 times higher for ex-inmates than for their never-incarcerated peers. Based on previous findings that ex-inmates spend more time unemployed, earn lower wages, and experience less wage and income mobility (Emmert 2014; Freeman 1992; Grogger 1995; Kling 2006; Sampson and Laub 1993; Waldfogel 1994; Western 2002), it is unsurprising that ex-inmates turn to public assistance at higher rates than their never-incarcerated peers.

Similarly, when I compare first-time ex-inmates to individuals who have never experienced incarceration in Model 2, gender and first-incarceration predict time to public assistance use. Again, the hazard of public assistance for males is 35 percent of what it is for females. While post-incarceration, the hazard of public assistance is 3.5 times higher for first-time ex-inmates than for their never-incarcerated peers.

In Model 3, multiple-incarcerations predicts how long individuals wait before using public assistance. The hazard of public assistance is 3.9 times higher for ex-inmates with multiple incarceration experiences than for their never-incarcerated peers.

Model 4 finds that while gender predicts the rate at which individuals using public assistance, the number of times individuals experience incarcerations does not. More specifically, the hazard of public assistance for males is 44 percent of what it is for females. However, each individual incarceration experienced by the incarceration subsample fails to have a cumulative effect on time to public assistance. While it is possible that each additional incarceration fails to create cumulative disadvantage among individuals who experience incarceration, it is also important to note that this measure does not, and cannot address the effect
of cumulative incarceration length on ex-inmates’ time to public assistance. Therefore, I suspect that number of incarcerations fails to predict employment outcomes for ex-offenders because this measure does not capture the catalyst of negative employment outcomes, and not because the number of incarcerations experienced by participants fails to influence employment outcomes.\textsuperscript{14}

And in Model 5, I find that age at first incarceration, gender, race, and education each fail to predict how quickly individuals report using public assistance. Since gender proves to be significant in all other models, except Model 3, the finding that gender fails to be significant here likely results from model error.

**Discussion**

Previous research establishes that ex-inmates experience negative employment outcomes following incarcerations. If economic restraints, such as lower earned wages and incomes, force ex-inmates to rely more heavily on public assistance, incarceration could inadvertently result in high costs for ex-inmates and society. Previously, Verbruggen et al. (2013) determine that 64 percent of males and 83 percent of females incarcerated in a Dutch juvenile detention center as youths later receive some form of government benefit. In this study, I expand upon these findings using fixed effects and event history analyses to consider the length of time ex-inmates use public assistance, and whether ex-inmates turn to public assistance more quickly than their never-incarcerated peers do following contact with the criminal justice system.

Again, it is important to note that in New York, public assistance is available in many forms, including food stamps, housing assistance, childcare services, employment training, and other services. Eligibility for public assistance and the duration of public assistance differs

\textsuperscript{14} Relevant incarceration data is not available to measure cumulative incarceration length using RYDS data.
among services. However, many of the services offered are available to both unemployed and employed individuals. Eligibility is often determined based on income, resources, household composition, and citizenship. Similarly, in New York, individuals with criminal histories are still eligible for public assistance and food stamps (Legal Action Center 2009). However, the Housing Authority can consider relevant criminal history when assessing eligibility for public housing (Legal Action Center 2009).

Using fixed effects analyses, I find that first-time ex-inmates and ex-inmates with multiple incarceration experiences vary significantly in their use of public assistance. More specifically, first-time ex-inmates report using public assistance for equivalent lengths of time as their never-incarcerated peers. By comparison, ex-inmates who have experienced multiple incarcerations report using public assistance for 70 percent longer than their never-incarcerated peers do. This outcome is not surprising, considering ex-inmates with multiple incarcerations have previously been found to report longer non-employment (Emmert 2014). Paired together, the finding that individuals with multiple incarceration experiences demonstrate longer non-employment and public assistance use is concerning, and would seem to suggest that ex-inmates’ inabilities to acquire and maintain employment result in increased reliance on public assistance.

Why ex-inmates with multiple incarceration experiences report differential use of public assistance compared to their first-incarceration and never-incarcerated peers, is not immediately clear. While Emmert (2013) finds that age at first incarceration explains differences in non-employment length between individuals with multiple incarcerations (incarcerated before age 23) and individuals experiencing their first incarcerations (incarcerated after age 23), this does not prove to be the case for public assistance use. Ultimately, additional analyses are necessary to
understand the influence of multiple incarceration experiences on the length of time ex-inmates use public assistance.

Considered from an alternative angle, event history analyses demonstrate that incarceration and gender both significantly influence the rate at which participants use of public assistance. More specifically, the hazard of public assistance for males is 35 to 44 percent of what it is for females. Thus, females use public assistance at a higher rate than males. Family composition and structure likely explains these findings. Females in the RYDS’s sample are more likely to be primary caregivers for children compared to male participants (Lizotte et al. 1994). In this role, female participants are either financially dependent on an outside source of income while they care for children, or are responsible for earning an income that can cover both childcare and living expenses. The financial difficulty of earning enough to cover childcare and living expenses likely contributes to female participants’ use of public assistant. Additionally, as of 2013, women earn, on average, 77 cents for every dollar men earn (DeNavas-Walt and Proctor 2014). This wage gap similarly leaves females at a financial disadvantage compared to males.

Furthermore, post-incarceration, the hazard of public assistance is 3.5 times higher for first-time ex-inmates than for their never-incarcerated peers. By comparison, the hazard of public assistance is 3.9 times higher for ex-inmates with multiple incarceration experiences than for their never-incarcerated peers. As such, ex-inmates use public assistance far more quickly than their never-incarcerated peers.

While ex-inmates’ lengths and rates of public assistance use are concerning, Verbruggen et al. (2013) establish that supplementing income with public assistance reduces recidivism among ex-inmates. This being said, the most favorable outcome would be for ex-inmates to have the necessary resources to earn living wages. Several programs that currently exist can improve
ex-inmates’ employment outcomes. A work release program in Florida and Correctional Industries program in Washington both improve ex-inmates’ recidivism, employment, and earnings outcomes post-incarceration (Berk 2008; Cox 2009; Drake 2003). If programs such as these were utilized more widely across the United States, it might be possible to lower ex-inmates’ reliance on public assistance post-incarceration.

Limitations do exist for this study. One potential concern regards the use of self-reported data to measure use of public assistance. Using public assistance in the United States is highly stigmatized, and might prevent participants from honestly disclosing when they use these services. Similarly, self-reported public assistance data relies on participants’ accurately remembering when and how long they used these services. Each of these concerns limit the validity and reliability of self-report data and any findings resulting from its’ use. Having said this, recent studies find that the vast majority of RYDS participants accurately self-report the number of times that they have been arrested (Emmert et al. 2015; Krohn et al. 2013). Thus, there is reason to believe that participants’ accuracy in self-reporting on one set of stigmatizing life events likely suggests that participants accurately self-report on other stigmatizing life events. A second concern regarding use of RYDS data is that the sample is geographically limited to a set of adults who all attended seventh or eighth grade in Rochester, New York. As such, future studies that use official data and geographically diverse samples to explore ex-inmates’ use of public assistance are warranted. Finally, while it is not possible to measure cumulative incarceration length with the data currently available via the RYDS, future research would benefit from including cumulative incarceration length when exploring employment outcomes.
CHAPTER 6. Discussion

This chapter summarizes the research findings and literary contributions of each research question posed. Following this, I discuss the overarching theoretical and methodological contributions discovered, and potential directions for future research to explore.

Summary of Research Findings

Chapter 3 utilizes event history analyses to explore whether cumulative incarceration experiences and age at first incarceration influence ex-inmates’ employment acquisition rates. The findings suggest that ex-inmates who experience their first ever incarceration between 23 and 29 years-old acquire employment at faster rates than their never-incarcerated peers do. By comparison, gender proves to be an important contributing factor to employment acquisition among ex-inmates with multiple incarceration experiences. While female ex-inmates with multiple incarcerations and individuals who have never experienced incarceration acquire employment at equivalent rates, the hazard of employment is 7 times higher for male ex-inmates with multiple incarceration experiences than for their female and never-incarcerated peers. Thus, male ex-inmates gain employment more quickly than their female and never-incarcerated peers do. When the relationship between incarceration and employment acquisition is broken down by number of incarcerations, gender fails to reach significance and each additional incarceration experience increases ex-inmates’ employment acquisition rates. This suggests that gender is significant when I measure ex-inmates in aggregate or semi-disaggregated because gender reflects differences in incarceration frequency (males experience more incarcerations than females). When I control for the number of incarcerations ex-inmates experience, gender is no longer relevant.

I use fixed effects analyses in Chapter 4 to explore how incarceration affects ex-inmates’ employment stability and tenure. I conclude that ex-inmates with a single or multiple incarceration experiences hold fewer jobs than their never-incarcerated peers do, even prior to their incarceration
experiences. However, post-incarceration, ex-inmates with multiple incarceration experiences hold more jobs than they did prior to incarceration. These findings expand upon those of Nagin and Waldfogel (1995) by suggesting that holding fewer jobs does not always equate with greater employment stability. In this case, ex-inmates likely hold fewer jobs because they spend less time employed. Additionally, while my findings for ex-inmates with multiple incarceration experiences mirror those of Kling (2006) and Lalonde and Cho (2008), I conclude the positive increase in the number of jobs ex-inmates hold post-incarceration do not overcome the disparity that exists in employment stability between individuals with multiple incarceration experiences and those who never experience incarceration. In support of Nagin and Waldfogel’s (1995) findings, I conclude that individuals who experience incarceration demonstrate significantly shorter employment tenures than their never-incarcerated peers. Moreover, the finding that ex-inmates demonstrate reduced employment tenures even prior to experiencing incarcerations adds to a growing literature concerned with the presence of employment disadvantages even prior to incarceration (Caspi et al. 1998; Gottfredson and Hirschi 1990; Moffitt 1993; Sullivan 1989). My findings add to Nagin and Waldfogel’s (1995) by concluding that incarceration does not exacerbate or add to ex-inmates’ pre-existing employment tenure disadvantages.

Chapter 5 considers whether cumulative incarceration experiences and age at first incarceration influence how long ex-inmates use public assistance and how quickly ex-inmates use public assistance following release from incarceration. First, I explore the influence of incarceration on the length of time ex-inmates utilize public assistance using fixed effects analyses. I find that ex-inmates with multiple incarceration experiences report longer use of public assistance than their never-incarcerated peers do. However, they use public assistance more even prior to their latest incarceration experience. Then, I utilize event history analyses to discern whether ex-inmates use public assistance at faster rates than never-incarcerated individuals do. The findings suggest that
overall, females use public assistance more quickly after coming in contact with the criminal justice system compared to males. Moreover, both first-time ex-inmates and ex-inmates with multiple incarceration experiences use public assistance at faster rates than their never-incarcerated peers.

**Overarching Conclusions and Contributions**

When considered as a whole, several overarching conclusions and contributions emerge from this project. These conclusions and contributions inform both theoretical and methodological techniques for exploring the effect of incarceration on ex-inmates’ employment outcomes. Specifically, these studies offer insight into post-incarceration cumulative employment disadvantages, the importance of disaggregating ex-inmates based on their incarceration experiences, and the ineffectiveness of incarceration counts in predicting employment outcomes.

The foundation of this project is a concern pose by life course theory that cumulative disadvantages emerge when interruptions occur early in the life course, or repeated interruptions occur. In this project, I explore cumulative disadvantage by separating ex-inmates based on whether they have experienced one or multiple incarcerations, controlling for the number of incarcerations ex-inmates have experienced, and controlling for ex-inmates’ ages at their first incarceration experiences. Ultimately, I discover that all ex-inmates demonstrate significant employment disadvantages. In addition, ex-inmates with multiple incarceration experiences demonstrate additional disadvantages in the rates they use public assistance. However, I do not find cumulative disadvantages for employment acquisition, employment stability, or employment tenure.

Nevertheless, regardless of the presence (or lack thereof) of cumulative disadvantages, it is still imperative to disaggregate incarceration subsamples. While in most cases it is not necessary to disaggregate ex-inmates based on the number of times they have experienced
incarceration, multiple models in this project prove the necessity of distinguishing between ex-inmates with one or multiple incarceration experiences. Often times these ex-inmates demonstrate significantly different pre-incarceration employment histories. When considered as an aggregate, these differences can mask or inflate estimated effects of incarceration on employment outcomes.

Finally, I conclude that in general, the measure number of incarcerations is not an effective predictor of cumulative disadvantage or incarceration’s effect on employment outcomes. While any incarceration interrupts the life course, it is conceivable that longer incarcerations are more interruptive than shorter incarcerations. Thus, a strict count of the number of times individuals experience incarcerations misses an important element of the interrupt, cumulative duration. Moreover, Kling (2006) has established the value of predicting employment outcomes based on incarceration lengths. As such, I conclude that incarceration frequencies may prove informative in conjunction with or interacted with cumulative incarceration length, but prove to be less informative by themselves. Additionally, while it is not possible to measure cumulative incarceration length with the data currently available via the RYDS, future research would benefit from including cumulative incarceration length when exploring employment outcomes.

**Future Directions for Research on Post-Incarceration Outcomes**

Based the conclusions above, it is imperative that future research consider the influence of cumulative incarceration length on cumulative disadvantage. This is especially important considering criminal sentence lengths have increased in the United States over the last two decades. Numerous scholars have explored the influence of incarceration length on employment outcomes (Kling 2006; Lott 1992a, 1992b; Lyons and Pettit 2008; Sampson, Laub, and Wimer
2006). For example, Kling (2006) explores ex-inmates’ wage outcomes using a sample of first-time offenders and administrative data. However, a disadvantage of administrative data is that it only accounts for legal and documented income. Similarly, income is only one aspect of employment, and as Wadsworth (2006) pointed out, financial rewards may not be the best crime deterrent. As such, it would be informative to explore the effect of cumulative incarceration length on various employment outcomes and using self-reported data.

Additionally, future research can use propensity score matching techniques to replicate and expand upon the current analyses and findings. Like fixed effects analysis, propensity score matching is one of many advanced techniques that controls for ex-inmates’ non-random selection into incarceration (i.e. selection bias). Multiple studies have used propensity score matching or matched pair techniques to explore the influence of incarceration on ex-inmates’ employment outcomes (see Apel and Sweeten 2010; Pager 2003; Pettit 2005). Replicating this project’s studies using matching techniques would provide additional reassurance that the findings are robust across multiple techniques.

Similarly, future research can benefit from performing comparable analyses on a larger sample. The sample size of RYDS data limits construction of a “true” counterfactual comparison group, the construction of mixed-effects analyses, the inclusion of additional predictive measures, and likely suppresses some statistically significant findings. Ex-inmates’ employment outcomes can be more thoroughly explored with larger samples.

Conclusions

This project addresses three concerns regarding ex-inmates’ employment outcomes following incarcerations: the length of time it takes ex-inmates to acquire employment, their employment stability and tenure post-release, whether incarcerations affect how long individuals
spend relying on public assistance, and the length of time ex-inmates use public assistance. These studies contribute to a growing literature on reentry employment outcomes and consider cumulative employment disadvantages among ex-inmates.
References


