



Criminal Justice, Self-Sufficiency, and the Life Course: Social and Economic Insecurity After Incarceration and Conviction

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**Criminal Justice, Self-Sufficiency, and the Life Course:
Social and Economic Insecurity After Incarceration and Conviction**

A dissertation presented by

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to

The Committee on Higher Degrees in Social Policy

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ABSTRACT

This dissertation examines previously unexplored aspects of the socioeconomic wellbeing of individuals who have passed through the American criminal justice system, expanding upon prior work both substantively and temporally. First, I consider housing stability among a group of individuals overlooked in prior research on the consequences of criminal justice contact: the 12 million Americans who have been convicted of a felony but never incarcerated. Using data from the National Longitudinal Survey of Youth 1997 cohort, I compare the experiences of individuals with a felony conviction but no history of incarceration to those of formerly-incarcerated individuals as a means of disentangling the effects of incarceration from the independent effect of felon status. I find that never-incarcerated individuals with felony convictions, like formerly-incarcerated individuals, experience an elevated risk of housing instability and residential mobility relative to their never-convicted peers, even when likely mechanisms like financial resources and behavioral characteristics are controlled for.

In Chapter 3, I use data from the National Longitudinal Survey of Youth 1979 cohort (NLSY79) to examine how formerly-incarcerated individuals interact with social safety net institutions by examining usage of six programs: cash welfare (AFDC/TANF),

Supplemental Security Income (SSI), disability insurance, unemployment insurance, food stamps/SNAP, and the earned income tax credit (EITC). Contrary to prior research that suggests criminal justice contact suppresses engagement with record-keeping institutions, I find no evidence that formerly-incarcerated individuals avoid safety net programs with greater administrative burden. Instead, I find that formerly-incarcerated white individuals appear to engage in assistance-seeking behavior with regard to means-tested program, receiving benefits more often than their observably similar never-incarcerated counterparts. I also find that, regardless of race, formerly-incarcerated individuals are less likely to benefit from contributory social insurance programs like disability and unemployment.

In Chapter 4, I examine long-term total income trajectories over the life course following incarceration. Using NLSY79 data, I examine the value and composition of total income packages before and after incarceration, considering how earned income, spouse income, transfer income, and other income change following incarceration and across the life course. I find that all types of income decline significantly following incarceration, but some recover, eventually returning to pre-incarceration levels. Using cluster analysis, I also find that, while the modal formerly-incarcerated man has very low income and limited income growth across the life course, approximately one in five have income trajectories and levels similar to those of never-incarcerated men.

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1. Introduction

Over the past forty years, the American criminal justice system has grown to a scale unprecedented in both historical and global perspective. The incarceration rate rose sharply from 161 inmates per 100,000 U.S. residents in 1972 to 670 per 100,000 in 2015 (Kaeble and Glaze 2016; National Research Council 2014c). On any given day, approximately 2.2 million individuals are incarcerated in American prisons and jails (Kaeble and Glaze 2016). While this figure marks a slight decline from the peak of 2.3 million incarcerated adults and an incarceration rate of 760 per 100,000 in 2008 (Kaeble et al. 2015), the U.S. continues to lead the world in both incarceration rate and number of people incarcerated (Walmsley 2015). However, growth of the American criminal justice system has not been limited only to correctional facility populations. Since 1980, the number of Americans on probation has also expanded dramatically, rising from about 1.1 million in 1980 to approximately 3.8 million adults in 2015 (Kaeble and Glaze 2016; Snell 1995).

As a result, the number of Americans who have passed through and been marked by the criminal justice system has also increased greatly over the past four decades. While there are no estimates of the total number of Americans who have previously

served prison, jail, or probation sentences that include misdemeanor convictions, recent estimates indicate that the number of individuals convicted of felony charges in the United States rose from approximately 4 million in 1980 to more than 14 million in 2010 (Shannon et al. 2017). Whereas former felons made up approximately 2.4 percent of the U.S. adult population in 1980, they represented about 6.2 percent of the total adult population in 2010. If current prisoners and probationers are included, the share of current and former felons in the U.S. increased from about 3 percent of the adult population in 1980 to more than 8 percent in 2010 (Shannon et al. 2017).

This growth in the scale of the U.S. criminal justice system has not been evenly distributed throughout the population, however. While just over 6 percent of the full voting age population had a prior felony record in 2010, over 18 percent of black adults had prior felonies as of 2010. Among black men, a full quarter had a prior felony, and one-third were either current or former felons as of 2010 (Shannon et al. 2017). Minorities, especially black men, are disproportionately likely to be under probationary supervision. Phelps (2017) estimates that in 2007, one in every 12 black men (and one in 21 black adults) were currently serving probation sentences, compared to one in 41 white men (and one in 65 white adults). Racial disparities in incarceration rates are even more extreme. In 2008, one in 12 black men (8 percent) and one in 36 Hispanic men (2.7 percent) were currently incarcerated, compared to just one in every 87 white men (1.1 percent) (Pew Charitable Trusts 2010).

Disparities in exposure to incarceration are not only race-based, however, but also class-based. Since 1980 there has been almost no change in incarceration rates among highly-educated men – instead, the growth in incarceration rates over this period has occurred almost exclusively among less educated men. While incarceration rates

among college-educated men aged 20 to 34 barely changed from 1980 to 2008, they doubled among young men with a high school diploma or GED and grew 350 percent and 500 percent, respectively, among black and white male high school dropouts over this period (Pew Charitable Trusts 2010). As a result, 68 percent of black male high school dropouts, 28 percent of white male dropouts, and 20 percent of Hispanic male dropouts born in the late 1970s are estimated to have served time in prison by their mid-30s (Western and Pettit 2010).

Such disparate exposure to the sanctions imposed by the criminal justice system – e.g., financial sanctions, missed work, legal restrictions on employment opportunities – is likely to have important implications for race- and class-based stratification in the U.S. in its own right (Uggen, Manza, and Thompson 2006; Wakefield and Uggen 2010). But prior work also suggests that criminal justice contact, particularly incarceration, has additional enduring negative effects on a variety of socioeconomic and health outcomes following release (National Research Council 2014c). Previous studies link prior incarceration to labor market discrimination (Pager 2003; Pager, Western, and Bonikowski 2009), lower wages (Apel and Sweeten 2010; Western 2002), decreased employment levels (Holzer 2009), diminished earnings (Western, Kling, and Weiman 2001), and very low upward economic mobility (Pew Charitable Trusts 2010). Another vein of research connects prior incarceration to poorer mental health (Schnittker, Massoglia, and Uggen 2012; Turney, Wildeman, and Schnittker 2012), diminished physical health (Massoglia 2008a, 2008b; Schnittker and John 2007), and poorer health behaviors (Porter 2014) among formerly incarcerated adults.

While health and employment-related outcomes have received the bulk of scholarly attention in the area of individual-level consequences of incarceration,

additional research suggests that incarceration leads to subsequent relationship dissolution (Lopoo and Western 2005; Turney and Wildeman 2013), housing instability (Geller and Curtis 2011; Harding, Morenoff, and Herbert 2013; Warner 2015), decreased asset ownership (Turney & Schneider), and diminished civic participation (Lerman and Weaver 2014; Weaver and Lerman 2010). Thus, given the demographic concentration of criminal justice contact among already marginalized segments of the population, it appears that the growth of penal institutions is exacerbating pre-existing disadvantages and disparities in American society.

In this dissertation, I expand on prior work considering the individual-level implications of criminal justice contact along both substantive and temporal dimensions. First, I take a deeper dive into exploring the socioeconomic wellbeing of individuals who have passed through the criminal justice system by considering outcomes that have received relatively little, if any, attention in the prior literature: housing stability, social safety net program participation, and total income packages. Additionally, I extend the literature in this area by considering a broader set of individuals marked by the criminal justice system than typically studied and by employing a longer time frame than usually used.

Most of the previous research literature that considers the subsequent consequences of criminal justice contact has focused on formerly-incarcerated individuals (Kirk and Wakefield 2018). But the number of formerly-incarcerated Americans (4.9 million in 2010) is dwarfed by the number of current and former felons who have never been imprisoned (12 million in 2010) (Shannon et al. 2017). Moreover, both the legal sanctions and stigma that accompany felony conviction are likely to apply not just to formerly-incarcerated individuals but also to the millions of Americans with

felony records who have never done time behind bars (The Council of State Governments Justice Center n.d.; Uggen et al. 2014, 2006). Thus, in Chapter 2 I consider the implications of criminal justice contact for the 12 million Americans who have been convicted of a felony but never incarcerated.

Prior research examining the consequences of incarceration has largely been unable to disentangle the mechanisms that link criminal justice contact to subsequent poor outcomes (Kirk and Wakefield 2018), but I argue that comparing the experiences of individuals with a felony conviction but no history of incarceration to those of formerly-incarcerated individuals provides a means of disentangling the effects of incarceration – and the removal from one’s community and the labor force that it necessarily entails – from the independent effect of felon status per se. In particular, I examine experiences of housing instability after felony conviction, and incarceration, among members of the National Longitudinal Survey of Youth 1997 cohort. Using a variety of modeling strategies, including sibling fixed effects and restricted comparison groups, I find that never-incarcerated individuals with felony convictions, like formerly-incarcerated individuals, experience an elevated risk of housing instability and residential mobility relative to their never-convicted peers, even when likely mechanisms like financial resources and behavioral characteristics are controlled for. Thus, this chapter makes an important contribution to the literature by highlighting how conviction, not just incarceration, can introduce instability into the lives of the millions of Americans who have passed through the criminal justice system. Moreover, these findings highlight that criminal justice reform efforts focused on increasing the use of community corrections over incarceration may do less to reduce the harm of criminal justice contact than reformers expect.

In Chapter 3, I return to the fate of formerly-incarcerated individuals, examining a rarely-considered dimension of socioeconomic wellbeing and institutional engagement: social safety net program participation. Each year, more than 600,000 Americans are released from prison (Carson 2018). Formerly-incarcerated individuals often face extreme financial hardship and a tenuous attachment to the formal labor market following release (Visher, Debus-Sherrill, and Yahner 2011; Western et al. 2015). Prior research has extensively analyzed employment, but few studies examine the non-market sources of income that are crucial for economic wellbeing after incarceration. Social safety net programs, including social insurance programs like disability (SSDI) and income support programs like Temporary Assistance for Needy Families, are intended to provide support to Americans facing periods of financial uncertainty or employment gaps, but the formerly incarcerated are not usually thought of as members of the “deserving poor” class that these programs often aim to benefit (Moffitt 2015). Moreover, other research suggests that contact with the criminal justice system may lead to avoidance of institutions that keep formal records and lower trust in government (Brayne 2014; Weaver and Lerman 2010). Thus, even when formerly-incarcerated individuals qualify for social safety net benefits, they may fail to utilize them, particularly those that require in-person interactions with government offices.

In this chapter, I consider how formerly-incarcerated individuals interact with social safety net institutions by examining usage of six safety net programs: cash welfare (AFDC/TANF), Supplemental Security Income (SSI), disability insurance, unemployment insurance, food stamps/SNAP, and the earned income tax credit (EITC). In so doing, I provide the first estimates of the extent to which formerly incarcerated adults utilize social safety net resources. By comparing usage patterns across these

different programs, I also provide insight into how program structure and administrative burden influence participation rates among the formerly incarcerated. Rather than finding support for the system avoidance hypothesis, I find that formerly-incarcerated white individuals appear to engage in assistance-seeking behavior with regard to means-tested program, receiving benefits more often than their observably similar never-incarcerated counterparts. I also find that, regardless of race, formerly-incarcerated individuals are less likely to benefit from contributory social insurance programs like disability and unemployment, perhaps locked out by their more tenuous attachment to the formal labor market.

In Chapter 4, I take a step back and examine total income trajectories over the life course following incarceration. Prior research on the financial wellbeing of formerly-incarcerated individuals has tended to be limited either in the type of income sources or the time period considered. Studies of recently-released prisoners have examined financial wellbeing from a holistic perspective – examining earned income, as well as public benefits receipt and support from family – but these studies only follow former prisoners over a relatively short time span following release (Harding et al. 2014; Western et al. 2015). On the other hand, studies using longitudinal data to examine financial circumstances of former prisoners have, to my knowledge, only examined earned income (Apel and Sweeten 2010; Bartik and Houseman 2008; Western 2006). Thus, I consider the long-term *total* income packages and trajectories of formerly-incarcerated men in this chapter. Using National Longitudinal Survey of Youth 1979 data, I examine the value and composition of total income packages both before and after incarceration, considering how earned income, spouse income, transfer income, and other income change following incarceration and across the life course. I consider

both mean income trajectories and variation among the formerly incarcerated in income trajectory patterns through their mid-50s. I find that all types of income decline significantly following incarceration, but some recover, eventually returning to pre-incarceration levels. Using cluster analysis, I also find that, while the modal formerly-incarcerated man has very low income and limited income growth across the life course, approximately one in five have income trajectories and levels similar to those of never-incarcerated men. In keeping with prior work on desistance from crime, I find that marriage and higher levels of employment are associated with achieving a traditional-looking income trajectory after incarceration. I also find that black men and men with disabilities are disproportionately likely to experience extremely disadvantaged post-incarceration income trajectories.

In Chapter 5, the conclusion, I discuss the implications of these findings for criminal justice reform efforts, social safety net policy, and the social (re)integration of individuals who have been involved in the American criminal justice system, suggesting directions for future research.

2. Housing Instability Following Incarceration and Conviction

A notable amount of both scholarly and political attention has been devoted to considering the consequences of mass incarceration in the U.S. in recent years. Researchers and advocates alike have pointed to the sharp increase in national incarceration rates over the last four decades and the consequently large population of former prisoners as cause for concern (Charles Koch Institute n.d.; National Research Council 2014c). According to recent estimates, there were 5 million former prisoners in the American population in 2010, up from a historic average of about 1 million throughout most of the 20th century (Shannon et al. 2017).

A sizeable literature has established that the consequences of incarceration do not stop at the prison gate, but that incarceration appears to lead to greater disadvantage and marginalization in individuals' lives along almost every dimension, from health to socioeconomic well-being, and extending even to their children's well-being (Adams 2018; Bryan 2017; Massoglia and Pridemore 2015; Western et al. 2015). Much of this research suggests that the link between incarceration and these various forms of marginalization and disadvantage is not driven purely by selection into incarceration but is causal. Moreover, in addition to exacerbating disadvantage in the individual life course, the fact that incarceration is unequally distributed in the population –

concentrated among racial minorities and the less educated – has led researchers to highlight incarceration as a driver of both the production and reproduction of poverty and inequality in American society (National Research Council 2014c; Wakefield and Uggen 2010; Western and Pettit 2010).

However, most previous research literature on the implications of the massive growth of the American criminal justice system in recent decades has failed to consider that the footprint of the criminal justice system extends far beyond just those who have been incarcerated in prisons and jails to include millions of other Americans who have passed through and been marked by the justice system without being physically incarcerated (Phelps 2017; Uggen et al. 2014). In 2006, the most recent year for which data are available, 1.2 million individuals were convicted of a felony in the U.S. (Rosenmerkel, Durose, and Farole 2009). While most felony convictions lead to a prison or jail sentence, approximately 30 percent do not (Durose and Langan 2003, 2007; Rosenmerkel et al. 2009). Instead, these individuals remain in the community following conviction, receiving probation or other penalties, like fines, community service or periodic drug testing. Despite avoiding incarceration, however, they do acquire the status of former felon. Recent estimates put the number of Americans who have been convicted of a felony crime but never served time in prison at 12 million, or 8.4 percent of the total working age population – more than double the size of the former prisoner population (Shannon et al. 2017).¹

While incarceration marks the most serious form of punishment, prior felony *conviction* status is associated with a broad variety of disadvantages and prohibitions that may follow individuals for many years. Former felons, particularly drug offenders,

¹ Calculated from Shannon et al. (2017) by subtracting 2010 “currently in prison or on parole” estimated count in Table 1 from the 2010 “former felons” estimated count in Table 2.

can legally be denied access to a wide variety of rights and benefits, ranging from voting and jury service to postsecondary education assistance (GAO 2005; Uggen et al. 2006). They can also be denied housing, employment, and occupational licenses in most states as a result of their conviction records (Legal Action Center 2004). Moreover, the easy accessibility of criminal background checks (Bushway et al. 2007) means that gatekeepers in both the housing and labor markets can and do discriminate on “criminal history” broadly, not just prior incarceration (Holzer, Raphael, and Stoll 2007).

Although these 12 million Americans with felony records who have never been imprisoned are likely to experience significant repercussions as a result of being marked as former felons, their experiences have been almost wholly overlooked in the prior research literature (National Research Council 2014c). Therefore, this chapter directly considers whether lesser criminal justice system involvement – namely, felony conviction *without incarceration* – introduces similar instability into one’s life.

In addition to being a large population worthy of study in its own right, focusing on the experiences of formerly-convicted-*but-never-incarcerated* individuals can provide important insight into the mechanisms linking prior incarceration to the variety of subsequent disadvantages identified in prior research. Thus, I compare the experiences of individuals with felony convictions but *no* history of incarceration to those of formerly-incarcerated individuals as a way to begin disentangling the effects of *incarceration* proper from the effects of being *marked* as a felon. Specifically, I do so with regard to housing stability, which prior scholars have found is greatly diminished by incarceration (Geller and Curtis 2011; Herbert, Morenoff, and Harding 2015; Warner 2015). I review this literature below before returning to a fuller discussion of potential

mechanisms and hypotheses with regard to the experience of formerly-convicted-but-never-incarcerated individuals.

A final motivation, to which I return in greater depth in the conclusion, is to complicate how we conceive of the problem of social integration following criminal justice contact. Most current discussions of this issue focus on former prisoners and the challenges of reentry, with reformers often proposing reduced incarceration rates and more supportive reentry programming as the solution (e.g., #cut50 n.d.). I argue, however, that neglecting to consider the experiences of the millions of Americans who bear the stigma of felon status but have never passed through a prison gate blinds us to the full set of challenges born of the American criminal justice system.

Housing Challenges After Exiting the Criminal Justice System

Prior research suggests that incarceration leads to increased housing instability, generally finding that prior incarceration is associated with experiencing a higher number of residential moves (Geller and Curtis 2011; Harding et al. 2013; Warner 2015). Scholars investigating this topic often point to the relationship between housing instability and recidivism in making the case for housing stability as an important outcome. Indeed, homelessness and greater residential mobility following release are associated with higher risk of rearrest and reincarceration (Metraux and Culhane 2004; Steiner, Makarios, and Travis 2015). However, housing instability is also related to a number of other outcomes relevant to individuals' quality of life and opportunities. In the domain of health, housing instability has been linked to poorer access to health care (Kushel et al. 2006; Reid, Vittinghoff, and Kushel 2008), lower birthweight among pregnant mothers (Carrion et al. 2015), and greater incidence of depression and generalized anxiety disorder among women (Suglia, Duarte, and Sandel 2011).

Moreover, housing instability is an important form of social exclusion (Foster and Hagan 2007; Lee, Tyler, and Wright 2010) that may hinder individuals' ability to achieve stability more generally. Edin and Shaefer (2015:55) offer qualitative evidence of how housing instability can complicate the job search, while Desmond et al. (2016) find that housing insecurity may lead to employment loss and job insecurity. Qualitative accounts also document how housing instability can limit individuals' ability to take advantage of and maintain access to resources like cash assistance, food stamps, and even internet access at the local library (Desmond 2016:63, 216; Edin and Shaefer 2015:100).

Despite the importance of housing for individual opportunity and stability, federal law permits both public housing authorities and private landlords to reject prospective applicants based on their criminal history. At their discretion, public housing authorities may reject applicants with felony convictions who apply for subsidized units or housing vouchers, and many housing authorities do so (Curtis, Garlington, and Schottenfeld 2013). Moreover, in many cities, individuals already living in public housing or receiving a housing voucher can lose their housing assistance for permitting someone with a felony conviction to move in with or even visit them (Blidner 2014; GAO 2005). In the private rental housing market, landlords are legally permitted to ask prospective tenants about their criminal history and run criminal records checks when deciding whether to rent to a prospective tenant, and prior research establishes that they routinely do so, often turning away applicants who reveal felony records (Delgado 2005; Helfgott 1997; Leasure and Martin 2017; Thacher 2008).

Unpacking Mechanisms

As prior studies note, legal housing market discrimination is likely to be at least partially responsible for the higher levels of housing instability observed among formerly-incarcerated individuals, but returning prisoners also face barriers in the form of strained relationships, poor employment history, lack of financial resources, and the general stigma of having been incarcerated, all of which are likely to affect their ability to find and maintain stable housing (Geller and Curtis 2011; Harding et al. 2013; Warner 2015). Therefore, because the physical removal from one's community entailed by incarceration affects individuals in so many ways, it is impossible to know how much of the post-incarceration housing instability observed in prior research results from the stigma and discrimination that accompany the "mark of a criminal record" versus from the incarceration and physical removal itself.

However, by focusing on individuals who have been convicted of a felony *but never incarcerated*, we can start to disentangle the effect of felon status and the discrimination it is likely to entail from all of the bundled intermediary effects of incarceration itself. If, after controlling for common confounders, formerly-convicted individuals who have never done time have housing experiences that do not differ greatly from those of observably similar never-incarcerated individuals, then we can assume that it is not the fact of having been marked as a felon that increases housing instability among the formerly incarcerated but instead something about the actual experience of incarceration and community removal itself. Therefore, in this chapter I compare the housing instability experiences of individuals who have been convicted of a felony but never incarcerated to those of formerly-incarcerated individuals as a means of teasing apart the effect of being *marked* as a felon from the effect of having been *locked up*.

Moreover, once I account for differences in financial resources, which may stem from both financial sanctions (Harris 2016) and labor market discrimination (Pager 2003; Pager et al. 2009) experienced by individuals with felony convictions, and behavioral differences, like hard drug use, that may affect an individual's ability maintain a stable residence, remaining differences in housing instability between formerly-convicted-but-never-incarcerated individuals and never-incarcerated individuals would provide greater support for the hypothesis that housing market discrimination is partially responsible for higher housing instability among both the previously-incarcerated and formerly-convicted population.

Data & Methods

I use data from the National Longitudinal Survey of Youth 1997, which has collected detailed information on employment, education, criminal activity, household characteristics, and more from a nationally-representative sample of 8,984 U.S. men and women since 1997, when they were ages 12-16. From 1997 to 2011, the NLSY97 surveys were conducted annually; as of 2013 data collection is biennial.

The NLSY97 includes extensive self-reported data on arrests, convictions, and incarceration spells since age 12, which allow me to construct detailed incarceration and conviction histories for all respondents. NLSY97 also includes considerable information about respondents' current housing situation and residential moves that occurred between interview waves. In particular, I use information about the type of housing unit each respondent is living in at each survey wave, whether the respondent considers that or any other dwelling to be their permanent residence, and the number of times the respondent has moved since the last survey wave to gauge respondents' housing stability.

The most recent survey for which data are available is 2015, at which point sample members were 30-36 years old; 79 percent of original sample members participated. Because NLSY97 respondents are still relatively young, some may still be in the midst of their criminal careers as of 2015. Criminal offending usually peaks in the late teens, however, and most respondents should be aging out of offending by their late 20s (Hirschi and Gottfredson 1983; Loeber and Farrington 2014). Moreover, although respondents were just 30-36 in 2015, the median time since last arrest that lead to a felony conviction was 9.75 years (117 months)², and the median time since last incarceration was 5.75 years (69 months).

I examine housing instability experiences at age 25 and older because housing instability in the early 20s is normative in modern American society. The “emerging adulthood” period, from ages 18 to 25, is a demographically dense period in which young adults explore and transition between a variety of roles, including child, student, romantic partner, and employee (Arnett 2000; Rindfuss 1991). While specific trajectories and ordering of events vary widely among young adults in this period, residential instability is the norm, as young adults move out of parents’ households, into dorms or first apartments, in with partners, to new cities for work, back into parents’ homes, etc. (Arnett 2000; Goldscheider and Goldscheider 1994). Accordingly, rates of residential mobility rise sharply in the 18-24 age range, peaking at age 23 (Benetsky, Burd, and Rapino 2015). But residential mobility rates begin to decline sharply around age 25 as young adults move into more stable roles (Arnett 2000; Benetsky et al. 2015). Thus, I consider experiences of residential (in)stability at and after age 25, as this is the first period of adulthood in which residential instability marks a deviation from the

² The median time from arrest to sentencing is roughly six months for felony convictions (Durose and Langan 2003, 2004; Rosenmerkel, Durose, and Farole 2009).

norm. The NLSY97 data contain 50,763 person-year observations in which respondents (N=8,285) were age 25 and above.³

Incarceration & Felony Conviction History

In the previous National Longitudinal Survey of Youth (NLSY79), incarceration history could only be discerned based on current dwelling type at each survey. As a result, prior research that has used NLSY79 data to examine the collateral consequences of criminal justice system contact, including Warner’s research on post-incarceration residential mobility (Warner 2015, 2016), has only been able to examine outcomes for the subset of formerly-incarcerated individuals observed in prison or jail at the time of the annual, and later biennial, survey.⁴ The more detailed NLSY97 data, however, allow the identification of not just formerly-incarcerated individuals – including those who are incarcerated and released between survey waves – but also individuals who have been convicted of or pled guilty to a crime, whether or not it resulted in prison or jail time.

I use data on the broad category of crime (e.g., assault, robbery, drug possession) for which a respondent pled guilty or was convicted to identify likely felony convictions. Because felony thresholds and sentencing guidelines vary from state to state, I rely upon

³ In such a volatile period of the life course, both increased measurement error and effect heterogeneity may threaten accurate inference. Still, I have also run models on all person-year observations in which respondents are 20 and older, employing age fixed effects to account for the strong relationship between criminal offending, housing instability and youth. The results of these models are substantively consistent with those presented in the main analyses and are available upon request.

⁴ A quick analysis of the NLSY97 incarceration history data suggests that the NLSY79 method of identifying formerly-incarcerated individuals based on their residence type at each survey wave undercounts the share of sample members who have been previously incarcerated by nearly 40 percent. By age 29, 9.2 percent of all NLSY97 respondents report having been incarcerated at least once, but only 5.6 percent are ever observed in prison or jail at the time of the survey. This observed-in-jail method of identifying formerly-incarcerated respondents particularly undercounts incarceration among women (3.6 percent ever incarcerated by age 29 vs. 1.6 percent ever *observed* in prison or jail by age 29), who serve shorter sentences, on average.

broad assumptions about the categories of crime that most often fall into the felony category. I code assault, robbery, burglary, theft⁵, drug sales, and drug possession as felonies, excluding destruction of property, “other property crimes,”⁶ major traffic offenses, public order offenses⁷, parole and probation violations, and the general “other offense” catchall category. This is a conservative approach, as the broad crime categories I bundle together as felony convictions will capture some misdemeanor offenses (e.g., misdemeanor drug possession, misdemeanor theft). Because misdemeanor offenses are less likely to bear the same level of stigma as felony convictions, however, the potential inclusion of misdemeanor convictions in my felony conviction indicator variable is likely to bias the coefficient towards zero.

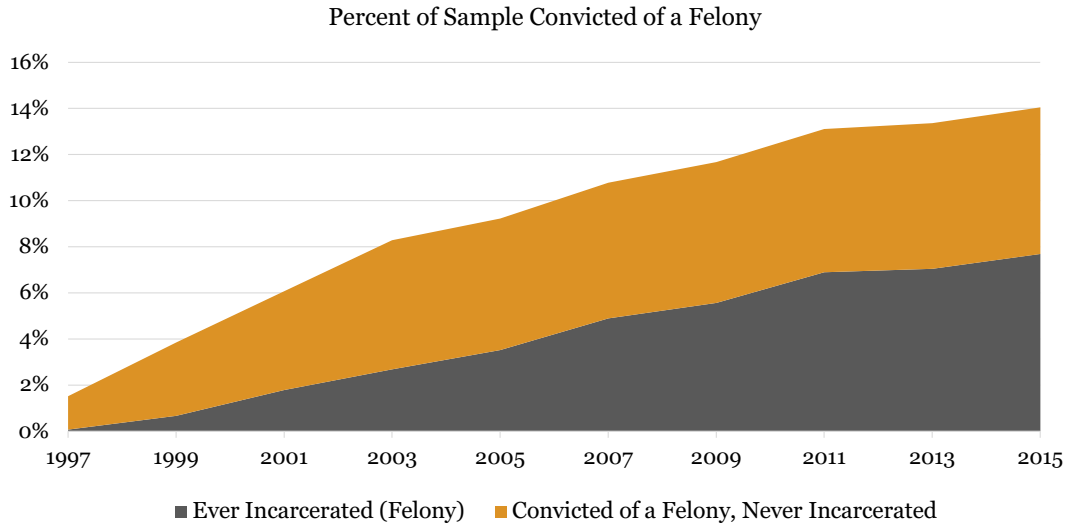
In order to gauge the effect of felony conviction independent of incarceration, I create an indicator variable identifying respondents *ever convicted of a felony but never incarcerated*, which I then pair with dummy variables identifying respondents who have *ever been incarcerated* and who are *currently incarcerated* in any given survey year. As Figure 2.1 shows, 14 percent of NLSY97 respondents have ever been convicted of a felony by 2015, and just under half (6.4 percent) of those respondents have never been incarcerated.

⁵ The theft prompt includes auto theft, larceny, and shoplifting.

⁶ The prompt for “other property crimes” specifies “fencing, receiving, possessing or selling stolen property” as falling into this category.

⁷ While the public order category can include felonies because weapons offenses are classified as public order offenses, weapons convictions (without an accompanying more serious felony conviction) make up only about 3 percent of all felony convictions in state courts (Rosenmerkel et al. 2009) and approximately 8 percent of federal felony convictions (Schmitt and Jones 2017).

Figure 2.1. Criminal Justice Contact, NLSY79



Note: Unweighted percentages.

Housing Instability Outcome Variables

The first housing outcome I consider is a common one in the housing (in)stability literature: *number of residential moves* since last interview. At each survey wave, respondents are asked to report the number of different addresses at which they have lived since the last interview date. Because the exact amount of time between interviews varies across respondents, I use Poisson models with an offset to account for different lengths of between-survey time over which respondents may have moved (i.e., exposure).

My second outcome variable, whether the respondent's current dwelling type at the time of the interview is some sort of *temporary housing*, is also commonly used in analyses of housing instability. This is a dummy variable set equal to one if the respondent currently resides in a hotel, motel, rooming house or boarding house; shelter or on the street; hospital; or group home or treatment center at the time of the survey.⁸

⁸ Because currently incarcerated respondents are by definition residing in a jail or prison, they are excluded in models that estimate the probability of residence in temporary housing.

Finally, I create a third measure of housing instability from respondents' answers to two questions asking whether they consider their current place of residence to be temporary or permanent and, if temporary, whether they have some other place that they consider their permanent residence. These questions are asked at each survey wave for the purpose of helping interviewers identify family and "household members" about whom they should collect information (e.g., in the case of a college student interviewed at her dorm). For my purpose, however, these questions provide novel insight into respondent's own self-perceptions of unstable housing.

For example, given the prevalence of "doubled up" households, in which adults coreside out of economic necessity (Mykyta and Macartney 2012; Pilkauskas, Garfinkel, and McLanahan 2014), a sizeable proportion of individuals may appear stably housed at a point in time because they are living in a traditional dwelling unit, rather than temporary housing, even if they are truly unstably housed in a doubled up situation.⁹ Therefore, the *no permanent residence* measure I create, which is set equal to one if a respondent indicates that they consider neither their current place of residence nor any other to be their permanent place of residence, provides a way of capturing an additional type of housing instability not well captured by the prior two measures of housing stability.¹⁰

⁹ Fewer than half of all doubled up housing arrangements last more than one year (Glick and Van Hook 2011).

¹⁰ I also considered the possibility that formerly-incarcerated and previously-convicted individuals are more likely to live in crowded, doubled up or unstable housing arrangements by examining their overall household size, the number of unrelated individuals in their household (not counting cohabiting partners and their children), and whether they reported neither owning nor renting their current dwelling unit (if not in temporary housing). I found no meaningful and consistent differences based on criminal justice history in any of these outcomes, however, once confounding characteristics were added to the models. Additionally, I examined measures of the duration of housing instability based on the number of consecutive survey years in which

Control Variables

In the first model I control only for basic demographic characteristics that relate to probability of having been incarcerated or convicted or of experiencing housing instability and residential mobility: age, gender, and race/ethnicity. I control for *age* with a fully flexible set of dummy variables, with 25 as the reference age. Gender is an indicator variable set equal to one if the respondent is *female*. Respondents' *race and ethnicity* are captured in the following four discrete categories: white non-Hispanic, black non-Hispanic, Hispanic, and other. White non-Hispanic is the reference category.

In the second model, I control for additional characteristics that are unlikely to have been affected by individual experiences of incarceration or conviction but that may confound the relationship between criminal justice contact and housing instability. This model includes family background traits that may affect respondent's proclivity to move and their access to family resources that could assist in avoiding criminal justice penalties or increasing housing stability in early adulthood. Given that higher residential mobility during childhood may indicate a more unstable family of origin and is linked to higher residential mobility during early adulthood (Myers 1999), I include a count of the *number of times the respondent moved between ages 12 and 16* (inclusive).¹¹ I also control for the respondent's *household structure in 1997*, measured as a categorical variable containing the following four categories: lived with both biological parents

respondents reported having no permanent residence, living in temporary housing, and/or neither owning nor renting their residence. I found no discernible patterns with regard to duration of these housing situations based on incarceration or conviction history, however. Results are available upon request.

¹¹ I have also tried a version of this variable that includes residential moves up to age 18 and found consistent results. I use age 16 as the cutoff because it is more often exogenous, as only 2.2 percent of ever-incarcerated respondents and 10.5 percent of ever-convicted respondents experienced incarceration or conviction by age 16, compared to 14 percent and 29 percent, respectively, by age 18.

(reference category), lived with one biological parent and one stepparent, lived with one biological parent only, and some other living arrangement. Finally, I include parents' education, coded as the highest degree completed by either of the respondent's resident parents in 1997. NLSY97 collected information on the highest grade completed by the respondent's residential parents (biological, step, adoptive, or foster) as part of the 1997 parent interview. To make coding comparable to that used for respondent education, highest grade level completed is translated into highest degree received using standard assumptions about length of time to degree. Parents who reported fewer than 12 years of completed education are coded as having no diploma or degree (reference category), those with exactly 12 years are coded as having a high school diploma, those with 13-15 years are coded as having completed some college, those with exactly 16 years are coded as college graduates, and those reporting more than 16 years are coded as having a graduate or professional degree.

In addition to these family background characteristics, I also include respondent's age-adjusted percentile score on the Armed Services Vocational Aptitude Battery test (*ASVAB percentile score*), which the NLSY97 administered to respondents in the first two survey waves, as a rough measure of respondent's cognitive ability. Additionally, I include in this model a binary indicator identifying respondents who *received a high school diploma or GED by age 19*. High school completion by age 19 is considered plausibly exogenous as the median age of first incarceration and first conviction in the NLSY97 data are 23 and 21, respectively. (Mean age at first incarceration and first conviction are 23.5 and 22, respectively.) Thus, the goal of this second model is to help reduce concerns about selection bias by accounting for some of the additional factors that help predict which individuals are more likely to select into

criminal justice system contact (Kirk and Wakefield 2018). In doing so, the results from this model can help put a more plausible upper bound on the relationship between felony conviction and housing instability.

The third model adds in a full set of covariates meant to account both for mediating characteristics (i.e., intervening mechanisms) and other potentially confounding characteristics. In addition to the covariates included in the second, pre-treatment controls model, this third model adds controls for time-varying individual achieved and behavioral characteristics, including marital status, parenthood, financial resources, and proxies for criminal proclivity. In this model, respondent education is recorded as the *highest degree completed* to date among the following five categories: none (reference category), high school diploma or GED, Associate's or some college, Bachelor's, or graduate or professional degree. I also include an indicator variable to identify *current students*. Marital status is captured by a variable set equal to one if the respondent is currently *married* on the interview date. *Parenthood* is identified by an indicator variable set equal to one if the respondent has at least one biological child residing in her household at the time of the survey.

I account for respondents' financial resources and recent employment history by including a measure of total *wages and salary in the prior year*, adjusted for inflation to 2014 dollars. Additionally, I include a measure of the approximate value of *gift income* respondents received from family and friends in the prior calendar year, adjusted to 2014 dollars, as such cash gifts could be used to assist in the transition to stable housing.¹² I also include a measure of respondents' *assets* to better account for financial

¹² Respondents reported estimated values of gift income using 7 ordinal response categories ranging from "\$1-500" at the low end to "more than \$10,000" at the high end. In order to adjust values for inflation, I assign the midpoint of the range respondents report as the value of their gift

resources.¹³ Finally, I proxy for respondents' criminal activity and/or proclivity by including indicator variables set equal to one if the respondent reported *ever using hard drugs since the last interview* or *ever carrying a gun since the last interview*.

Ideally, I would include a much fuller set of behavioral controls to account for differences in criminal activity. While NLSY97 collects self-reported data on a range of other criminal behaviors (e.g., assault, drug sales, theft) across multiple survey waves, only gun carrying, hard drug usage, and marijuana usage are asked of all respondents at each survey round since 1998. Starting in 2004 (when respondents are 19-25 years old), NLSY79 restricts these questions to respondents who report having previously been arrested and a small subsample of other randomly selected respondents. In the years during which all respondents are asked about criminal behavior, hard drug usage, in particular, correlates moderately highly with other self-reported criminal activities, which is why I choose to include it as a control variable in the third model. Marijuana is weakly correlated, sometimes negatively, with other self-reported criminal activities, so I do not include it as a measure of criminal proclivity. Gun carrying since last interview is correlated weakly to moderately with other criminal activities, but the relationship is consistently positive, so I include it as a control variable in the third model.

By controlling for these various potential intervening mechanisms, I attempt both to further reduce confounding and provide an upper bound for the potential role of housing market discrimination. If felony conviction only affects housing stability via its

income in a given year, then adjust values to 2014 dollars. Results are substantively consistent when I drop the gift income measure from the models.

¹³ NLSY97 collects data on the net worth of respondents and, if applicable, their spouse or partner in the first interview during or after the calendar year in which they turn 20, 25, 30, and 35. I subtract out the value of assets that respondents report their spouses or partners do not share with them, then multiply impute asset values for years in which assets were not collected. Results are substantively consistent when I drop this imputed asset variable from the models.

effect on probability of achieving stable employment, entering into and/or maintaining a stable union, and avoiding hard drug use, then any differences in housing stability between never-convicted and ever-convicted respondents that remained in the second model should disappear in this model. If, alternatively, significant differences in housing stability remain between these two groups after the inclusion of these covariates, then this will provide evidence for the potential role of housing market discrimination against individuals with felony convictions as a mechanism contributing to greater housing instability among this population.

Sibling Fixed Effects Model

Often studies that use longitudinal data like the NLSY79 use individual fixed effects models to account for unobserved characteristics that may confound the relationship being examined. I could employ that approach, except that the individual fixed effect model requires within-individual variation in the covariate(s) of interest for that individual to contribute to parameter estimates. Because I limit analysis of housing outcomes to ages 25 and above – an age-range during which residential instability is no longer normative – individual fixed effects models would only allow me to examine how first conviction *after* age 25 contributes to housing stability. But, because criminal offending peaks in the late teens and early 20s (Hirschi and Gottfredson 1983; Loeber and Farrington 2014), first conviction at such a late age is unusual. Of all NLSY97 respondents ever convicted of a felony by 2015, only 18 percent were first convicted after age 25. Therefore, individual fixed effect models that rely only on the experiences of this minority of ever-convicted respondents are not likely to provide an accurate reflection of the relationship between felony conviction and housing instability for the majority of

formerly-convicted young adults, because individuals who avoid felony conviction until after age 25 are non-representative.

However, if I use biological families, rather than individuals, as the grouping unit for a fixed effect model I should be able to account for some of the most important unobserved characteristics that could confound the relationship between felony conviction and housing instability while still including observations from respondents first convicted of a felony before age 25 – i.e., the majority of ever-convicted respondents. Because NLSY97 sampled at the household level, then enrolled all household residents in the appropriate age range (12-16 years old on Dec. 31, 1996) in the study, close to half (41 percent) of all NLSY97 respondents have at least one biological sibling in the study sample. Moreover, because these biological siblings must have been living in the same household in their teens for both to enter the survey sample, these sibling pairs will share not just genetic material, but also household-level experiences (e.g., housing instability, exposure to neighborhood and domestic violence) and characteristics (e.g., parental temperament, values, and criminal activity) that are not easily observable in the survey questions administered by NLSY79. Thus, with sibling fixed effects models I can account for family-level characteristics that could otherwise confound the relationship between conviction and housing instability by restricting comparison of housing outcomes to biological siblings who differ in their criminal justice contact. Because even biological siblings will differ in their achievements and temperaments, I continue to control for gender, educational attainment, marital status, parenthood, labor income, gift income, assets, cognitive ability, and gun carrying and drug use since last interview in the sibling fixed effects model.

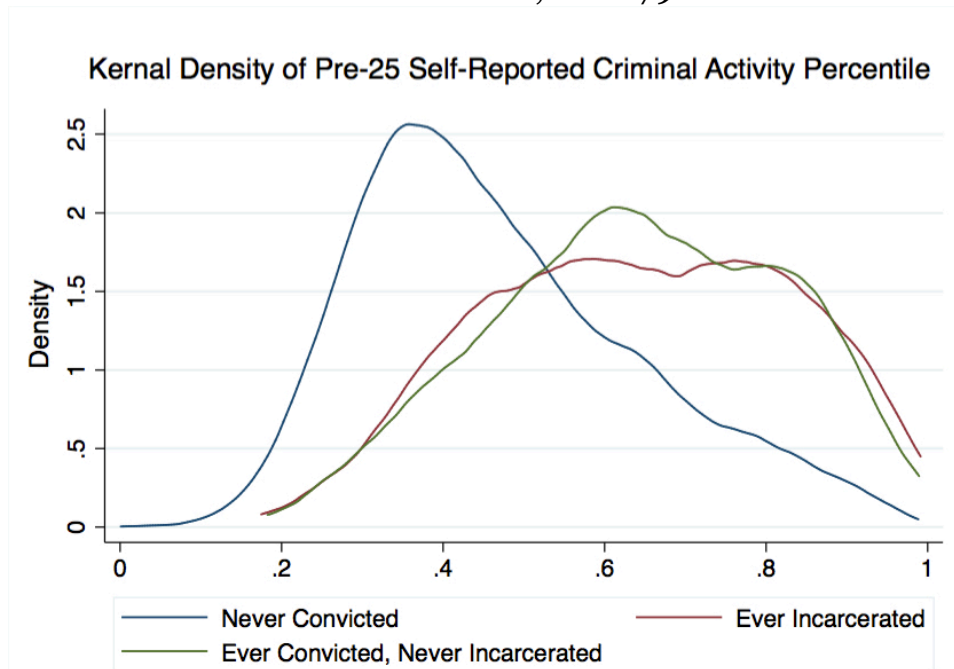
High Crime Comparison Group Model

Another threat to causal inference in this scenario is that behavioral differences, particularly with regard to criminal activity, could confound the relationship between conviction and housing instability. Indeed, as Figure 2.2 shows, ever-incarcerated and ever-convicted-but-never-incarcerated NLSY79 respondents have remarkably similar distributions with regard to self-reported criminal activity prior to age 25, but their self-reported criminal activity distributions are distinctively left-skewed relative to that of the never-convicted respondents.

Although I proxy for time-varying criminal activity in the third model with the two best measures available in all survey years (i.e., hard drug use and gun carrying) that may give insight into criminal behavior, I also run an additional set of models that further accounts for this behavioral difference by restricting the comparison group to a high-crime subsample of NLSY79 respondents, according to their self-reported criminal activity before age 25. Restricting comparison to individuals similar to the treatment group, and thus at risk of receiving treatment (i.e., conviction or incarceration), can significantly reduce bias in the estimation of causal effects compared to relying on a general population comparison group with regression adjustment (LaLonde 1986; Western 2002). For this analysis I drop all never-convicted respondents with below median self-reported early adult criminal activity from the analysis and re-run the final model with full control variables described above on this restricted sample.

If incarceration affects housing instability solely through community removal and the intermediary effects of being physically locked up, then we should expect to see little difference between formerly-convicted-but-never-incarcerated respondents and never-incarcerated respondents with regard to early adulthood housing instability once the

Figure 2.2. Self-Reported Criminal Activity Distributions by Criminal Justice Contact, NLSY79



Note: This figure reflects respondent's responses to questions asking whether the respondent had carried a gun, destroyed property, stolen goods worth less than \$50, stolen goods worth more than \$50, committed any property crimes, assaulted anyone, sold marijuana, sold hard drugs, used marijuana, and/or used hard drugs since their last interview. These questions were asked of all respondents in the 1998-2003 survey years. Because respondents' ages during this period ranged from 12 to 24, I have created an age-adjusted percentile score for self-reported criminal activity that averages across respondent's percentile within the total distribution of self-reported criminal activity at each age. Figure 2.2 displays the distribution of this age-adjusted self-reported criminal activity percentile.

above described observable characteristics are controlled for, and even less so when potentially confounding unobservable characteristics are controlled for in the sibling fixed effect and high crime comparison group models. If, however, we see that formerly-convicted-but-never-incarcerated respondents experience significantly greater levels of housing instability even after these differences are controlled for, this will suggest that felony conviction status and the mark of a criminal record independently affect housing stability. Thus, in the following models I expect the coefficient on *ever incarcerated* to consistently be larger than the coefficient on *ever convicted, never incarcerated*, because the former will reflect all of the intermediary effects of actual incarceration (e.g., removal from labor market, weakened social ties, health effects of confinement) as well as the post-incarceration effects of being marked as a former felon. The coefficient on *ever convicted, never incarcerated*, however, should only reflect this second set of mechanisms (i.e., felon status). Therefore, the relative size of these two coefficients should give us some sense as to the relative importance of incarceration per se versus felon status for experiences of housing instability.

Across all models I use logistic regression to predict no permanent residence and temporary housing and Poisson regression to predict number of residential moves. In all but the sibling fixed effects models, I use respondent-level random effects to account for repeated observation of respondents across survey waves. I multiply impute missing values on control variables. Results produced with multiply imputed datasets are consistent with those produced using casewise deletion.

Results

Descriptive Statistics

Table 2.1 displays descriptive statistics for outcome and control variables, broken out by felony conviction and incarceration history. On average, NLSY79 respondents move less than once every two years (0.46 moves per year), but we see that number of moves within the last year is elevated slightly for formerly-convicted-but-never-incarcerated respondents (0.56) and even more so for ever-incarcerated respondents (0.65). Very few respondents overall (less than one percent) live in temporary housing or have no permanent residence, but these experiences are much more common among formerly-incarcerated respondents (2 percent and 6 percent, respectively). Probability of living in temporary housing and having no permanent residence are also elevated, though to a lesser degree, for respondents who have been convicted but never incarcerated, at about 1 percent each compared to 0.5 and 0.2 percent, respectively, among never-convicted respondents.

That formerly-convicted-but-never-incarcerated respondents fall between the full sample and formerly-incarcerated respondents on each of these outcomes makes sense given that they appear to be a more privileged group, on average, than formerly-incarcerated respondents. Their racial composition is much more similar to that of never-convicted respondents, and they are more highly educated, more likely to have lived with both biological parents in adolescence, and more likely to be married than formerly-incarcerated respondents. They also have more highly educated parents, have higher cognitive test scores, higher income, and higher assets than formerly-incarcerated respondents. But on all of these measures, they still fall behind never-incarcerated respondents. Where formerly-incarcerated and formerly-convicted-but-never-

Table 2.1. Descriptive Statistics

	Criminal Justice Contact			
	Full Sample	Ever Incarcerated	Ever Convicted (Felony), Never Incarcerated	Never Convicted or Incarcerated
Person-Year Level				
<u>Outcome variables</u>				
No permanent residence	0.7%	5.6%	0.8%	0.2%
Temporary housing	0.5%	2.2%	1.1%	0.5%
Number of moves in last year	0.46	0.65	0.59	0.44
<u>Control variables</u>				
Currently incarcerated	1.1%	11.7%	--	--
Age	28.5	28.9	28.5	28.4
Highest degree completed				
None	8.7%	25.7%	19.5%	6.3%
High school diploma or GED	54.8%	67.5%	64.9%	52.9%
Some college/Associate's	7.4%	3.5%	6.4%	7.8%
Bachelor's	23.0%	2.8%	7.5%	25.9%
Graduate or professional degree	6.2%	0.5%	1.8%	7.0%
Current student	11.9%	5.9%	8.4%	12.7%
Married	38.1%	20.4%	25.1%	40.6%
Parent	43.8%	38.2%	43.2%	44.4%
Labor income last year (mean)	\$30,721	\$16,788	\$26,394	\$32,308
Gift income last year (mean)	\$341	\$293	\$240	\$352
Assets (mean)	\$48,331	\$20,011	\$35,821	\$51,817
Used hard drugs (ever) since last interview	3.8%	8.8%	8.7%	3.0%
Carried a gun (ever) since last interview	5.4%	5.4%	6.9%	5.3%
	<i>Person-years</i>	50,763	4,253	3,099
Person Level				
<u>Control variables</u>				
Female	49.0%	21.7%	32.6%	53.2%
Race/Ethnicity				
White	66.1%	57.9%	65.4%	67.1%
Black	15.9%	24.3%	17.0%	14.9%
Hispanic	13.0%	13.5%	13.3%	12.9%
Other	5.0%	4.3%	4.3%	5.2%

Table 2.1. (Continued)

	Full Sample	Criminal Justice Contact		
		Ever Incarcerated	Ever Convicted (Felony), Never Incarcerated	Never Convicted or Incarcerated
Graduated high school by age 19	75.1%	45.4%	56.0%	79.7%
Number of residences lived in from ages 12-16	1.95	2.48	2.49	1.86
Household structure, 1997				
Lived with both biological parents	52.6%	31.3%	35.9%	55.8%
Lived with one biological parent, one stepparent	14.6%	20.4%	18.9%	13.6%
Lived with one biological parent only	28.1%	37.9%	38.6%	26.3%
No biological parents present	5.0%	10.5%	6.6%	4.3%
Parents' education (highest degree)				
None	13.5%	22.6%	19.0%	12.1%
High school diploma or GED	30.3%	40.1%	29.8%	29.3%
Some college/Associate's	26.8%	20.6%	26.6%	27.5%
Bachelor's	15.5%	10.3%	13.6%	16.2%
Graduate or professional degree	14.0%	6.4%	11.0%	15.0%
Armed Services Vocational Aptitude Battery (ASVAB) percentile score	50.3	33.5	40.1	52.7
No biological parents present				
Labor income last year (2015)	\$37,205	\$21,483	\$31,807	\$39,345
Gift income last year (2015)	\$312	\$302	\$252	\$318
Assets (2015)	\$61,126	\$26,261	\$45,652	\$66,131
At least one biological sibling also in sample	40.8%	42.0%	38.2%	40.9%
Number of biological siblings in sample (if any)	1.18	1.26	1.15	1.17
<i>N</i>	8,285	827	518	6,940

Note: Weighted values. Descriptive statistics refer to person-years in which respondents were 25 years or older. All dollar value variables have been adjusted for inflation to 2014 values.

incarcerated respondents are remarkably similar is in their rates of hard drug use (roughly 9 percent) and the amount of residential mobility they experienced during adolescence (2.5 moves on average).

Models

Tables 2.2, 2.3 and 2.4 display results from the Poisson models predicting number of moves since the last interview, logit models of temporary housing residence, and logit models of having no permanent residence, respectively. In each table, the first column shows results from the simplest model that controls only for basic demographic characteristics (race/ethnicity, gender, age). The second column shows results from the pre-treatment controls model that accounts for family background characteristics, cognitive ability, and high school completion by age 19. The final column displays results from the model using the full slate of control variables (individual achieved characteristics and behavior measures, in addition to family background variables). I anticipate that the differences between individuals with and without conviction history and with and without incarceration history observed in the first, demographic controls only model will be larger than the remaining differences in columns two and three, once various behavioral, social origins, and financial resource differences between these groups have been taken into account.

The coefficients in Table 2.2 show that, across models, both felony conviction without incarceration and prior incarceration are associated with significantly greater residential mobility between surveys relative to individuals who have never been convicted. Even when controls are added in the second and third models, the coefficients on both the prior-conviction-without-incarceration and prior-incarceration variables remain highly statistically significant and similar to their magnitudes in the demographic

controls only model in column one. Across the three models, the coefficient on the *ever incarcerated* variable is about 30 percent larger than that on the *ever convicted, never incarcerated* variable, which is unsurprising given the greater disruption that incarceration entails. (These coefficients are significantly different from each other across models.) But the fact that the coefficient on *ever convicted, never incarcerated* remains significant across models provides evidence that felony status independent of incarceration appears to matter for housing stability.

Table 2.2. Poisson Regression Predicting Number of Moves in Last Year, Age 25 and Above

	Demographic Controls	Pre-Treatment Controls	Full Controls
Ever convicted, never incarcerated	0.296*** (0.0328)	0.259*** (0.0327)	0.217*** (0.0325)
Ever incarcerated	0.406*** (0.0291)	0.379*** (0.0296)	0.312*** (0.0296)
Currently incarcerated	-0.0550 (0.0540)	-0.0669 (0.0539)	-0.109** (0.0538)
Race/ethnicity			
Black	-0.00525 (0.0203)	0.0220 (0.0228)	-0.00278 (0.0227)
Hispanic	-0.0857*** (0.0223)	-0.0313 (0.0242)	-0.0339 (0.0240)
Other	0.0136 (0.0460)	0.0162 (0.0454)	-0.00341 (0.0450)
Female	0.0641*** (0.0174)	0.0536*** (0.0172)	0.0590*** (0.0180)
Household structure, 1997			
Lived with one biological parent, one stepparent		0.142*** (0.0261)	0.134*** (0.0258)
Lived with one biological parent only		0.120*** (0.0209)	0.107*** (0.0207)
No biological parents present		0.209*** (0.0361)	0.194*** (0.0357)
Parents' education (highest degree)			
High school diploma/GED		-0.00173 (0.0264)	0.000965 (0.0262)
Some college/Associate's		0.0528* (0.0282)	0.0497* (0.0281)
Bachelor's		0.0739** (0.0338)	0.0686** (0.0336)
Graduate or professional degree		0.134*** (0.0366)	0.126*** (0.0365)
Number of residences lived in from ages 12-16		0.0672*** (0.00515)	0.0650*** (0.00511)
ASVAB percentile score		2.04e-06*** (3.95e-07)	2.13e-06*** (4.06e-07)

Table 2.2. (Continued)

Current student		-0.0839***	-0.103***
		(0.0222)	(0.0223)
Completed high school or GED by age 19		0.220**	
		(0.0931)	
Highest degree completed			
High school diploma/GED			-0.0587**
			(0.0256)
Some college/Associate's			-0.0764
			(0.0771)
Bachelor's			-0.0584
			(0.0529)
Graduate or professional degree			-0.0453
			(0.133)
Married			-0.0674***
			(0.0179)
Parent			-0.0903***
			(0.0176)
Labor income last year			-1.64e-06***
			(3.30e-07)
Gift income last year			1.79e-05***
			(5.87e-06)
Assets			-7.27e-07***
			(1.24e-07)
Used hard drugs (ever) since last interview			0.185***
			(0.0341)
Carried a gun (ever) since last interview			0.0682**
			(0.0327)
Age fixed effect	Yes	Yes	Yes
Individual-level random effects	Yes	Yes	Yes
	<i>Observations (person-years)</i>	47,187	47,187
		47,187	47,187

*** p<0.01, ** p<0.05, * p<0.1

Likewise, in Table 2.3, we see that both former incarceration and having a felony conviction even without incarceration are positively and significantly associated with higher log odds of living in temporary housing. Here, the addition of controls noticeably reduces the magnitude of the coefficient on *ever convicted, never incarcerated*, particularly in the full controls model which includes respondent education, marital status, parent status, income, assets, hard drug use and gun carrying. The difference remains statistically significant and positive, however, in all models. Again, the log odds of living in temporary quarters are higher among the formerly incarcerated, but still we see that felony conviction status, even without the removal from one's community entailed by incarceration, is associated with higher log odds of living in temporary

housing, even when full controls are included in the model. Again, the *ever incarcerated* and *ever convicted, never incarcerated* coefficients are significantly different from each other.

Table 2.3. Log Odds from Logistic Regression Predicting Current Residence in Temporary Housing, Age 25 and Above

	Demographic Controls	Pre-Treatment Controls	Full Controls
Ever convicted, never incarcerated	1.530*** (0.281)	1.268*** (0.283)	0.837*** (0.267)
Ever incarcerated	2.505*** (0.231)	2.173*** (0.237)	1.420*** (0.221)
Race/ethnicity			
Black	0.750*** (0.209)	0.365 (0.233)	0.125 (0.221)
Hispanic	0.0295 (0.255)	-0.0860 (0.274)	-0.0260 (0.253)
Other	0.756* (0.444)	0.697 (0.460)	0.508 (0.432)
Female	0.197 (0.189)	0.111 (0.191)	0.318* (0.190)
Household structure, 1997			
Lived with one biological parent, one stepparent		0.762** (0.297)	0.614** (0.278)
Lived with one biological parent only		0.910*** (0.242)	0.777*** (0.226)
No biological parents present		1.567*** (0.330)	1.281*** (0.303)
Parents' education (highest degree)			
High school diploma/GED		0.0145 (0.266)	0.0828 (0.242)
Some college/Associate's		0.287 (0.296)	0.262 (0.268)
Bachelor's		0.0935 (0.384)	0.0941 (0.355)
Graduate or professional degree		0.154 (0.434)	0.111 (0.413)
Number of residences lived in from ages 12-16		0.114** (0.0529)	0.0830* (0.0488)
ASVAB percentile score		-4.86e-06 (4.11e-06)	-1.87e-06 (3.96e-06)
Current student		-0.828*** (0.319)	-0.915*** (0.314)
Completed high school or GED by age 19		0.903 (0.867)	
Highest degree completed			
High school diploma/GED			0.432* (0.252)
Some college/Associate's			0.753 (0.819)
Bachelor's			-0.514 (1.137)
Graduate or professional degree			-

Table 2.3. (Continued)

Married				-0.673*** (0.238)
Parent				-1.052*** (0.192)
Labor income last year				-3.52e-05*** (5.42e-06)
Gift income last year				-2.16e-05 (6.70e-05)
Assets				-3.18e-06 (2.31e-06)
Used hard drugs (ever) since last interview				1.115*** (0.247)
Carried a gun (ever) since last interview				0.621** (0.314)
Age fixed effect	Yes	Yes	Yes	
Individual-level random effects	Yes	Yes	Yes	
	<i>Observations (person-years)</i>	50,007	50,007	49,897

*** p<0.01, ** p<0.05, * p<0.1

Table 2.4 displays results from the logit model of having no permanent residence. Again, we see that, net of controls, both felony conviction without incarceration and prior incarceration are associated with significantly greater housing instability. In this case, the coefficients reveal that both formerly-incarcerated individuals and individuals with felony convictions but no history of incarceration are significantly more likely to perceive themselves as having no permanent residence than never-convicted individuals. As in the models of number of moves and temporary housing, these differences are highly statistically significant and consistent across models; they are also significantly different from each other.

Table 2.4. Log Odds from Logistic Regression Predicting No Permanent Residence, Age 25 and Above

	Demographic Controls	Pre-Treatment Controls	Full Controls
Ever convicted, never incarcerated	0.674*** (0.257)	0.641** (0.254)	0.539** (0.259)
Ever incarcerated	1.118*** (0.202)	1.030*** (0.202)	0.891*** (0.204)
Currently incarcerated	4.906*** (0.201)	4.834*** (0.194)	4.344*** (0.205)
Race/ethnicity			
Black	0.0276 (0.155)	-0.0216 (0.164)	-0.0767 (0.171)
Hispanic	-0.0419 (0.188)	-0.0483 (0.196)	-0.0511 (0.202)
Other	0.146 (0.416)	0.176 (0.415)	0.0229 (0.422)
Female	-0.426** (0.169)	-0.466*** (0.168)	-0.202 (0.176)
Household structure, 1997			
Lived with one biological parent, one stepparent		0.473** (0.194)	0.447** (0.200)
Lived with one biological parent only		0.342** (0.173)	0.304* (0.177)
No biological parents present		-0.0230 (0.261)	-0.0661 (0.267)
Parents' education (highest degree)			
High school diploma/GED		0.0523 (0.180)	0.0439 (0.185)
Some college/Associate's		-0.121 (0.224)	-0.128 (0.225)
Bachelor's		-0.253 (0.287)	-0.293 (0.288)
Graduate or professional degree		0.170 (0.315)	0.0818 (0.316)
Number of residences lived in from ages 12-16		-0.00465 (0.0417)	-0.000696 (0.0420)
ASVAB percentile score		-7.24e-07 (3.09e-06)	-2.78e-06 (3.25e-06)
Current student		0.311 (0.239)	0.230 (0.242)
Completed high school or GED by age 19		0.621 (0.644)	
Highest degree completed			
High school diploma/GED			0.317* (0.173)
Some college/Associate's			0.739 (0.793)
Bachelor's			-0.446 (1.050)
Graduate or professional degree			-
Married			-0.357* (0.213)
Parent			-1.796*** (0.280)
Labor income last year			-5.62e-06 (4.03e-06)

Table 2.4. (Continued)

Gift income last year			2.02e-05 (6.96e-05)
Assets			-1.72e-06 (1.59e-06)
Used hard drugs (ever) since last interview			0.250 (0.281)
Carried a gun (ever) since last interview			0.0116 (0.328)
Age fixed effect	Yes	Yes	Yes
Individual-level random effects	Yes	Yes	Yes
<i>Observations (person-years)</i>	50,730	50,730	50,619

*** p<0.01, ** p<0.05, * p<0.1

Table 2.5 displays the results from the sibling fixed effect model for all three housing instability outcomes discussed above. Because very few respondents have no permanent residence or live in temporary housing to begin with and the sibling fixed effects model requires variation within families to produce estimates, the sample sizes for models predicting these two outcomes are greatly reduced when employing sibling fixed effects.¹⁴ (Respondents from families in which neither sibling ever reports having no permanent residence and neither sibling ever lives in temporary housing after age 25 are, thus, dropped from the sibling fixed effect model.) Accordingly, coefficients are measured with more error and standard errors are two to three times as large in the sibling fixed effect model as in previous models.

Comparisons within biological sibling pairs reveal that individuals who have been convicted of a felony but never incarcerated move significantly more often and have significantly higher log odds of living in temporary housing and of having no permanent

¹⁴ I have also run this model with person-year observations in which respondents are 20-24 years old included in order to increase sample size and test the consistency of the findings. When I do so, I find that results are consistent with those presented below for the temporary housing and residential mobility outcomes. Coefficients are smaller, though still positive, for the no permanent residence outcome when ages 20-24 are included in the sibling fixed effect model, and, as a result, the coefficient on *ever convicted, never incarcerated* is no longer statistically significant. These results are available upon request.

residence than their siblings who have never been convicted of a crime, net of differences in gender, education, marital status, parenthood, financial resources, gun carrying, drug use, and cognitive test scores. Likewise, individuals who have been previously incarcerated have significantly higher log odds of living in temporary housing and of having no permanent residence than their siblings, all else equal.

Table 2.5. Biological Sibling and Age Fixed Effects Models, Age 25 and Above

	Number of Moves in Last Year	Temporary Housing	No Permanent Residence
Ever convicted, never incarcerated	0.158*** (0.0550)	0.972** (0.465)	1.166* (0.686)
Ever incarcerated	0.196*** (0.0480)	1.414*** (0.460)	0.699 (0.564)
Currently incarcerated	-0.136* (0.0798)	21.12 (1,039)	4.214*** (0.471)
Female	0.00594 (0.0305)	0.251 (0.376)	0.638 (0.483)
Highest degree completed			
High school diploma/GED	0.00197 (0.0530)	0.610 (0.554)	0.956* (0.510)
Some college/Associate's	0.0680 (0.152)	-14.62 (4,229)	-10.55 (1,616)
Bachelor's	-0.170 (0.116)	-14.34 (11,549)	-10.35 (2,048)
Graduate or professional degree	0.162 (0.247)	-16.24 (16,090)	
Current student	-0.148*** (0.0364)	-0.780 (0.562)	0.483 (0.457)
Married	-0.112*** (0.0297)	-0.627 (0.387)	-0.657 (0.517)
Parent	-0.143*** (0.0288)	-0.913*** (0.326)	-2.345*** (0.581)
Labor income last year	-1.90e-06*** (5.38e-07)	-1.90e-05** (8.12e-06)	-5.24e-06 (8.24e-06)
Gift income last year	2.24e-05** (9.83e-06)	-3.44e-05 (0.000174)	1.50e-05 (0.000149)
Assets	-5.91e-07*** (1.76e-07)	-2.56e-06 (2.94e-06)	-7.45e-07 (2.90e-06)
Used hard drugs (ever) since last interview	0.0686 (0.0605)	1.167** (0.509)	0.213 (0.583)
Carried a gun (ever) since last interview	0.0242 (0.0556)	0.345 (0.572)	0.645 (0.673)
ASVAB percentile score	3.04e-06*** (7.61e-07)	-1.12e-06 (9.10e-06)	-4.92e-06 (9.87e-06)
Age fixed effect	Yes	Yes	Yes
Family-level fixed effects	Yes	Yes	Yes
Observations (person-years)	19,173	2,635	1,655
Sibling sets	1,611	186	114

*** p<0.01, ** p<0.05, * p<0.1

Coefficients are actually larger on *ever convicted, never incarcerated* in the biological sibling fixed effect model for temporary housing and no permanent residence than in the full controls model in Tables 2.3 and 2.4, but this is likely due to the greater error in these models. The key takeaway from the sibling fixed effects models is that the differences in housing instability based on criminal justice contact identified in the prior models do not appear to be wholly due to unobserved family-level confounders.

Finally, while the sibling fixed model should account for unobserved family-level confounders, including to some extent heritable biological confounders, the sibling fixed effects model does not necessarily account for the possibility that differences in housing instability are due to unobserved and unaccounted for differences in criminal proclivity that vary within as well as between families. Thus, Table 2.6 displays results from the high crime comparison group model that restricts the never-convicted comparison group to respondents who were at or above the median on age-adjusted self-reported criminal activity in their teens and early 20s.

Table 2.6. Results from High Crime Comparison Group Models, Age 25 and Above

	Number of Moves in Last Year	Temporary Housing	No Permanent Residence
Ever convicted, never incarcerated	0.150*** (0.0335)	0.460* (0.269)	0.626** (0.289)
Ever incarcerated	0.241*** (0.0316)	1.012*** (0.232)	0.984*** (0.242)
Currently incarcerated	-0.129** (0.0543)	-	4.338*** (0.211)
Race/ethnicity			
Black	0.00343 (0.0304)	0.143 (0.250)	-0.149 (0.189)
Hispanic	-0.0274 (0.0327)	0.0940 (0.280)	0.102 (0.219)
Other	-0.0345 (0.0597)	0.308 (0.501)	0.220 (0.480)
Female	0.110*** (0.0250)	0.441** (0.219)	-0.198 (0.212)
Household structure, 1997			
Lived with one biological parent, one stepparent	0.113*** (0.0346)	0.492 (0.320)	0.516** (0.226)

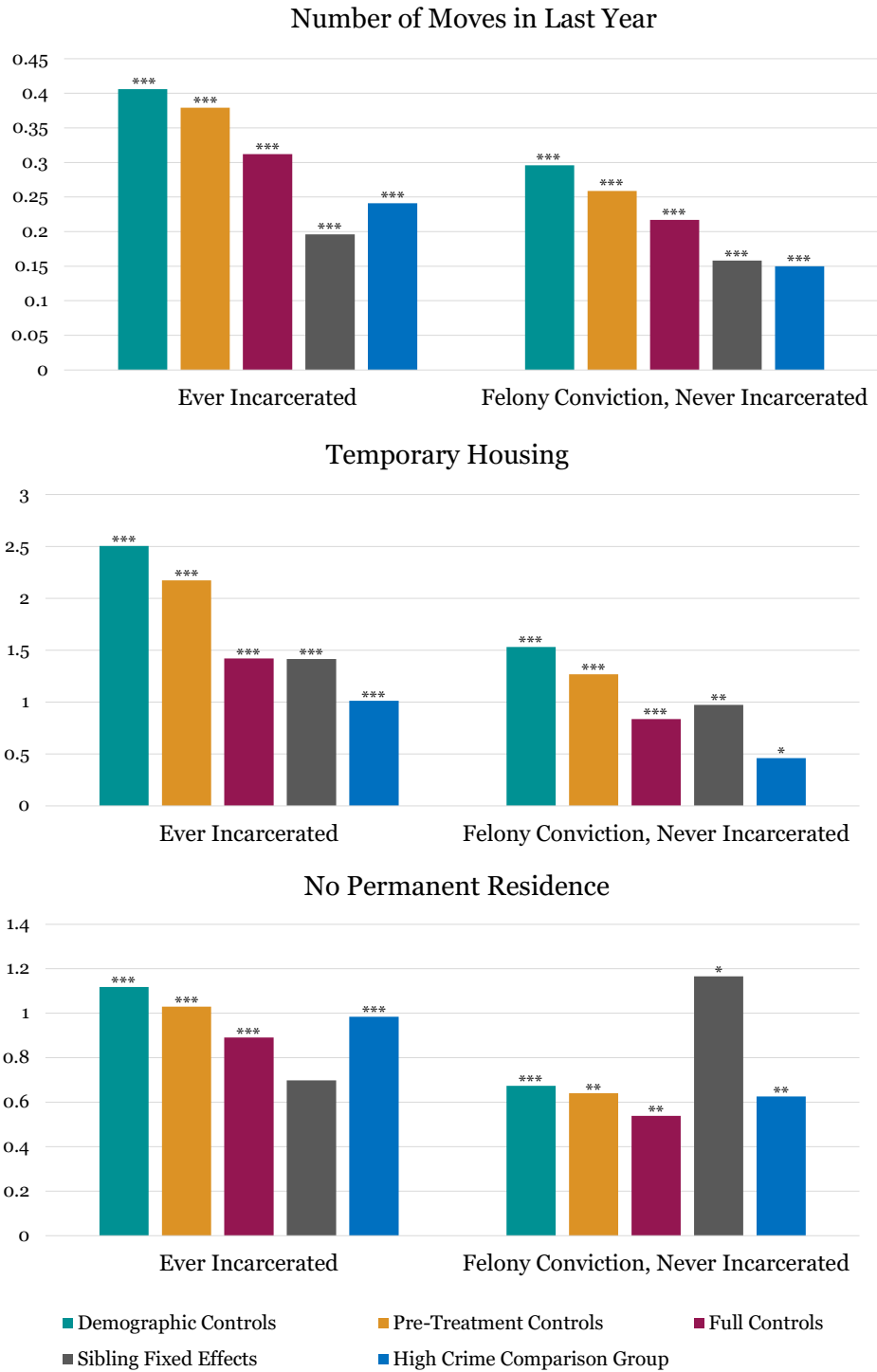
Table 2.6. (Continued)

Lived with one biological parent only	0.125*** (0.0281)	0.791*** (0.261)	0.431** (0.202)
No biological parents present	0.176*** (0.0467)	1.230*** (0.349)	-0.126 (0.299)
Parents' education (highest degree)			
High school diploma/GED	-0.0140 (0.0388)	-0.0275 (0.273)	0.0935 (0.190)
Some college/Associate's	0.0136 (0.0395)	0.0306 (0.318)	-0.0833 (0.253)
Bachelor's	0.00333 (0.0461)	0.196 (0.384)	-0.402 (0.326)
Graduate or professional degree	0.0811 (0.0517)	0.0651 (0.471)	0.250 (0.379)
Number of residences lived in from ages 12-16	0.0600*** (0.00662)	0.0359 (0.0532)	-0.0131 (0.0441)
ASVAB percentile score	1.00e-06* (5.45e-07)	3.25e-06 (4.41e-06)	-8.21e-07 (4.41e-06)
Current student	-0.0844*** (0.0313)	-0.730** (0.346)	0.172 (0.303)
Highest degree completed			
High school diploma/GED	0.0143 (0.0337)	-0.334 (0.245)	-0.00885 (0.177)
Some college/Associate's	0.00160 (0.0544)	-0.368 (0.457)	-0.373 (0.525)
Bachelor's	0.109** (0.0502)	-1.432*** (0.556)	-0.230 (0.499)
Graduate or professional degree	0.153** (0.0726)	-1.718 (1.129)	0.206 (0.794)
Married	-0.0747*** (0.0251)	-0.449* (0.259)	-0.258 (0.237)
Parent	-0.132*** (0.0236)	-1.159*** (0.218)	-1.644*** (0.324)
Labor income last year	-1.67e-06*** (4.54e-07)	-2.74e-05*** (6.08e-06)	-7.71e-06 (5.00e-06)
Gift income last year	-8.98e-07 (7.92e-06)	-5.15e-05 (9.83e-05)	5.72e-06 (6.95e-05)
Assets	-9.52e-07*** (1.72e-07)	-3.05e-06 (2.19e-06)	-1.08e-06 (2.14e-06)
Used hard drugs (ever) since last interview	0.168*** (0.0368)	1.033*** (0.249)	0.351 (0.262)
Carried a gun (ever) since last interview	0.0561 (0.0388)	0.599* (0.326)	-0.136 (0.355)
Age fixed effect	Yes	Yes	Yes
Individual-level random effects	Yes	Yes	Yes
<i>Observations (person-years)</i>	22,831	23,855	24,538

*** p<0.01, ** p<0.05, * p<0.1

For ease of comparison, Figure 2.3 graphically displays the coefficients on *ever incarcerated* and *ever convicted, never incarcerated* across the three outcomes of interest from the models in Tables 2.2-2.6. If we compare across these models, we can

Figure 2.3. Comparison of Coefficients Across Models



Note: Graphs display coefficients from Poisson model predicting number of moves in the last year and logit models predicting temporary housing and no permanent residence.
 *** p<0.01, ** p<0.05, * p<0.1

see that the coefficients on *ever convicted, never incarcerated* are roughly 50 to 70 percent as large in the high crime comparison group model (Table 2.6) as they were in the full controls model for number of moves and temporary housing (Tables 2.2-2.3). (The coefficients on *ever incarcerated* are about 70 to 80 percent as large for these two respective outcomes.) The coefficients on *ever convicted, never incarcerated* and *ever incarcerated* in the restricted comparison group model predicting log odds of no permanent residence are actually quite similar to the coefficients reported in Table 2.4. Most crucially, though, we see that the differences in housing stability outcomes remain significant and positively signed even when comparison is restricted to the high crime subsample. Thus, the consistency in the general pattern of findings across the high crime comparison group and sibling fixed effect models should provide some confidence that the relationship between prior felony conviction *without* incarceration and housing instability identified here is causal.

Robustness Checks

In addition to the models described above, I also tried models that accounted for length of time since conviction and since release from incarceration. I found minor attenuation of the relationship between prior incarceration and temporary housing as well as residential mobility over time, but no significant differences with regard to time since felony conviction without incarceration. This should provide further support for the claim that the *ever incarcerated* variable captures the full effect of incarceration on subsequent housing stability, while the *ever convicted, never incarcerated* variable should primarily capture the effect of felony status alone. We should expect the effects of being removed from one's community via incarceration to attenuate with time, but the effect of being *marked* as a felon is probably less likely to attenuate with time.

I also considered whether housing instability outcomes differed by type of crime for which the respondent was convicted (e.g., drug crimes, violent crimes, property crimes). These results from these models suggest that the relationship between prior felony conviction and current temporary housing is primarily driven by convicted drug offenders. There are no clear differences based on type of conviction, however, with regard to log odds of having no permanent residence or number of moves.

In an attempt to account for the possibility that housing instability following incarceration or conviction may be partially due to strained relationships – strained either by incarceration itself or simply by the fact of having worn out family and partners’ patience while passing through the court system – I also ran models predicting log odds of living with adult family members and log odds of living with a romantic partner. Once control variables are added, there are no statistically significant differences in either of these outcomes.¹⁵ Therefore, I am disinclined to believe that relationship strain is a key mechanism driving housing instability among formerly convicted or incarcerated individuals.

Because Devah Pager’s work suggests that the labor market stigma of being formerly incarcerated varies by race (Pager 2003; Pager et al. 2009), I also examined interactions between race/ethnicity and incarceration and felony conviction history, but I did not find clear patterns of racial differences in the relationship between incarceration or conviction and housing instability. However, it is important to note that even if felony conviction does not appear to have a differential impact on housing instability by race, the distributional differences in criminal justice contact by race mean

¹⁵ If I do not control for marital status, formerly-incarcerated individuals are less likely to live with a romantic partner, and if I do not control for marital status, formerly-convicted individuals appear to be slightly less likely to live with romantic partner, but the difference is not statistically significant.

that felony conviction (and incarceration) is still likely to contribute to greater housing instability among black Americans as a whole simply because a disproportionate share of them are pulled into contact with the criminal justice system.

Finally, gender interacted models suggest that the relationship between prior incarceration and/or felony conviction status is amplified for women, though the gender interaction terms are only consistently statistically significant across models for log odds of living in temporary housing (for both formerly-incarcerated and formerly-convicted-but-never-incarcerated women) and for number of residential moves among previously-incarcerated female respondents.¹⁶ I do not have strong a priori reasons to expect to find these differences, but it is an intriguing pattern that may be worth further exploration and theorizing by other scholars. Future research that tests whether the gendered pattern observed here holds in other data and, if so, explores the mechanisms behind this dynamic could be an important contribution to the literature, especially given the relatively limited understanding we have of collateral consequences of criminal justice system involvement for women due to their lower incarceration rates and the relative dearth of studies that examine the effects of conviction without incarceration.

¹⁶ These gender differences do not appear to be driven by gendered differences in parenthood (i.e., residence with own children). In models that include three-way interactions between gender, parenthood, and conviction/incarceration history, the higher probability of temporary housing and greater residential mobility among formerly convicted and incarcerated females appear to be driven by women without children. The same is not true for probability of having no permanent residence, however. In this case, the higher coefficient among formerly-incarcerated women – which was not statistically significant in the gender-interacted model – appears to be driven by women with children. That higher probability of residing with one’s children cannot explain higher levels of housing instability among previously convicted and/or incarcerated women relative to men is not entirely surprising as prior research identifies parenthood as a stabilizing status, associated with both lower residential mobility and desistance from crime (Benetsky, Burd, and Rapino 2015; Laub and Sampson 2001).

Discussion

I find that felony conviction appears to increase housing instability – as measured by number of residential moves in the last year, likelihood of living in temporary housing, and likelihood of having no permanent residence – even when conviction is not accompanied by a custodial sentence that removes one from one’s community. As would be expected, the magnitude of the relationship between prior incarceration and housing instability is generally greater than the magnitude of the relationship between felony conviction without incarceration and housing instability. This is unsurprising, as incarceration is a multifaceted treatment – involving removal from one’s community, employment disruption, and confinement, in addition to the criminal record stigma likely to follow release – compared to conviction without incarceration, and all of these intermediate aspects of the incarceration experience are likely to affect individuals in ways detrimental to their ability to maintain stable housing (e.g., weakened social ties, diminished financial resources and employment history, potential emotional and physical trauma caused by confinement).

The effect of felony conviction *without incarceration*, on the other hand, is likely to be primarily due to the stigma of having a criminal record and the legal discrimination and prohibition from programs and benefits that accompany felony conviction status. Therefore, by comparing the housing experiences of individuals who fall into this category with the housing experiences of formerly-incarcerated individuals, we can start to think more systematically about what it means to be *marked* as a felon versus what it means to have been *locked up* in modern America. Although formerly-incarcerated individuals will face both of these issues, these two experiences are most definitely not the same. However, prior research has often treated them as such. This chapter makes an

important contribution, therefore, by providing a methodological approach to pull apart this conceptual difference.

Moreover, I propose that, having accounted for (1) the impact of potential labor market discrimination (e.g., Pager 2007) and financial sanctions (e.g., Harris 2016) by controlling for income and assets, (2) behavioral differences (i.e., hard drug use) that may affect willingness of family or friends to live with an individual and/or an individual's ability to seek out and maintain independent residence, and, especially, (3) family-level unobserved differences that could confound the relationship between likelihood of felony conviction and housing instability, the remaining significant differences in housing instability among those who have been convicted of a felony but never incarcerated might plausibly be attributed to discriminatory behavior by gatekeepers in the housing market. That is, the results I present above may reflect the decision of public housing and private landlord to turn away applicants with felony records. It is, obviously, impossible to fully substantiate this claim without evidence of landlord behavior, but I plan to pursue this possibility in future experimental work. Additionally, data collection on the frequency with which employment and housing applications specifically ask about criminal history and, when they do, how often they specify *felony* conviction would be very helpful for understanding the scope of this potential mechanism.

For now, what these findings make clear is that the difference in housing instability experiences between individuals with felony convictions only and those with a history of incarceration is a difference of degree, not of kind. Both formerly-incarcerated individuals and those with felony convictions but no incarceration history are more likely to live in temporary housing, are more likely to perceive themselves as having no place of

permanent residence, and move more often than individuals without convictions in their past, even when comparisons are restricted to biological sibling pairs and behaviorally similar individuals who have never been convicted. The finding of greater residential mobility among formerly-incarcerated individuals is consistent with previous research on post-incarceration housing patterns (Harding et al. 2013; Warner 2015), but the finding of higher residential mobility among individuals with a felony conviction but no incarceration history is a new contribution to the literature.

Conclusion

Prior studies of housing instability following incarceration are useful for pointing out an important barrier to successful reentry, but they are unable to adjudicate between the relative role of incarceration and removal from one's community versus the role of discrimination and prohibitions against individuals marked by criminal records in increasing housing instability amongst previously-incarcerated individuals. Focusing on individuals with felony convictions but no history of incarceration allows us to set aside many other competing explanations for the relationship between incarceration and housing instability and focus in more specifically on the role of being marked as a felon. The robust evidence that individuals who have never been incarcerated but who are marked as convicted felons experience significant housing instability similar in magnitude to that experienced by former inmates – even when financial resources and behavior are accounted for and they are compared to their own biological siblings – provides important initial evidence of the independent and sizeable role of felony stigma in hampering social integration and stability.

More importantly, by considering whether felony conviction without incarceration also has the potential to disrupt and destabilize the normal life course,

specifically in the important realm of housing, this chapter expands our understanding of breadth of collateral consequences that follow criminal justice system involvement. Given that former felons greatly outnumber former prisoners in the U.S. (Shannon et al. 2017), these findings suggest that the research literature to date has not come close to making a full accounting of the costs of criminal justice system interactions. Future research that explores other potential collateral consequences of felony conviction independent of incarceration would be helpful in providing a fuller portrait of the costs of American criminal justice system expansion.

Relatedly, the finding that conviction may independently affect housing stability suggests that the problem of social integration – the concern at the core of reentry-focused programs and efforts – is much larger than we often think. Returning prisoners can often turn to reentry organizations to help them establish housing and employment following release, but to my knowledge no such programs exist to promote stability among never-incarcerated individuals who are dealing with the labor and housing market effects of former felon status. Therefore, these findings make clear that building a better reentry program will not fully solve the problem of social integration following criminal justice contact, because it is not just reentering former prisoners who are subject to the destabilizing force of being processed by the criminal justice system.

Finally, the findings from this chapter have important implications for thinking about the limits of current criminal justice reform efforts. In light of the significant disadvantage and marginalization that previously incarcerated and convicted individuals face in the U.S., a variety of bipartisan criminal justice reform coalitions have emerged in recent years (Charles Koch Institute n.d.; #cut50 n.d.). These reform efforts have largely focused on reducing the size of the criminal justice system via shorter sentences and/or

greater use of community corrections. Such reforms would help reduce the number of people in prison or jail at any given point in time, but they would not alter the number of people who pass through and are marked as former felons by the criminal justice system, nor would they alter the penalties and stigma those individuals face after conviction and/or release. Thus, these findings highlight that altering the distribution of criminal sentence without providing greater support to increase stability for Americans who have passed through the criminal justice system is likely only to reduce the cost of the prison system, not the harms faced by those who have been marked by it.

3. Seeking Support or Avoiding Institutions?: Social Safety Net Usage after Incarceration

The dramatic growth in incarceration rates over the past four decades (Kaeble et al. 2015; National Research Council 2014c) coupled with the fact that nearly all prisoners are eventually released back to their communities (Carson and Golinelli 2013; Nellis 2017) has produced a large and growing population of formerly-incarcerated individuals in the U.S. in recent decades. According to recent estimates, there were approximately 5.8 million former prisoners in the U.S. population in 2010, up from less than 1 million in 1980 (Glaze and Bonczar 2011; Shannon et al. 2017). These former prisoners represent more than two percent of the total adult U.S. population and nearly eight percent of the adult African American population (Shannon et al. 2017).¹

Prior research demonstrates that formerly-incarcerated individuals face severe financial hardship in the few years immediately following release (Harding et al. 2014; Western et al. 2015) and suggests that they are likely to experience continued hardship as time goes on. Prior incarceration has been linked to lower employment levels, reduced earnings, labor market discrimination, and lower asset levels (Holzer 2009; National

¹ Former prisoner totals and population shares calculated using Shannon et al.'s estimate of "formerly in prison or on parole" population in 2010 plus Glaze and Bonczar's 2010 year-end count of current parolees.

Research Council 2014c; Pager 2003; Pager et al. 2009; Turney and Schneider 2016). As a result, formerly-incarcerated individuals are likely to have greater need for social safety net programs, like the Supplemental Nutritional Assistance Program (formerly known as “Food Stamps”), that could smooth consumption and provide some financial stability. However, other research suggests that individuals who have had contact with the criminal justice system have lower levels of trust in government and avoid interactions with institutions that keep formal records (Brayne 2014; Weaver and Lerman 2010). Thus, despite need, formerly-incarcerated adults may fail to utilize social safety net resources, particularly those that require in-person interactions with government offices, as part of a larger pattern of system avoidance behavior.

In the midst of these sharply rising incarceration rates, the structure of the American social safety net has been shifting. Public assistance programs available to all who met financial need requirements (“entitlement” programs) have dwindled and been transformed into or supplanted by work-based income support programs over the past three decades (Edin and Shaefer 2015; Hahn et al. 2017; Haskins 2012). Thus, formerly-incarcerated individuals may be increasingly shut out of access to safety net programs as a result of their more tenuous employment histories and the employment discrimination they are likely to face in the formal labor market. For these same reasons, formerly-incarcerated individuals are also less likely to be able to take advantage of social insurance programs like Unemployment Insurance (UI) or Disability (SSDI), eligibility for which is based on prior work history in the formal labor market.

In this chapter I consider these hypotheses, as well as provide a descriptive analysis of the extent to which formerly-incarcerated Americans draw on the social safety net. Although prior work suggests that formerly-incarcerated individuals may have

greater need for the income-smoothing benefits of the social safety net as a result of their more precarious labor market attachment, we currently have no estimates of the extent to which formerly-incarcerated adults actually engage social safety net resources. Previously-incarcerated individuals are likely to receive assistance from reentry programs shortly after their release, but these programs focus on providing short-term supports during the challenging transition back to life on the outside. Prior work on employment discrimination, as well as my findings in Chapter 2, suggests these individuals may continue to face challenges even after the reentry period due to the stigma of the criminal conviction status they bear. This paper will provide us with important baseline knowledge of the extent to which America's social safety net is supporting individuals who have come out of prisons and jails. Identifying disproportionate use – or lack thereof – of safety net programs by formerly-incarcerated individuals will illuminate the extent to which government programs are alleviating or perpetuating the inequality generated by America's criminal justice system.

I use data from the National Longitudinal Survey of Youth 1979 (NLSY79) for this analysis, examining patterns of receipt across six safety net programs that vary both in their eligibility structure (i.e., means-tested assistance vs. social insurance) and the amount of in-person interaction and administrative burden they require. I consider two social insurance programs – unemployment insurance (UI) and disability (SSDI) – and four non-contributory means-tested programs: Aid to Families with Dependent Children (AFDC)/Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI)², food stamps/Supplemental Nutritional Assistance Program (SNAP), and

² Although SSI is administered by the Social Security Administration, it is a non-contributory cash assistance program designed to benefit aged, blind and disabled individuals with little to no income.

the Earned Income Tax Credit (EITC). These programs range from requiring periodic, in-person meetings to maintain eligibility (e.g., AFDC/TANF, SSI, disability) to requiring nothing more than sending in a tax return (EITC).

I first examine average differences in benefit receipt across programs for formerly-incarcerated individuals compared to never-incarcerated individuals to get a general sense of how often formerly-incarcerated individuals are benefitting from the social safety net. But individuals who will ever be incarcerated are likely to differ from those who will never be incarcerated in a number of ways that may relate to likelihood of safety net dependency, and, as I note above, incarceration itself is likely to affect both need and eligibility for safety net programs via its intermediary effects on health, employment, relationships, and earnings (National Research Council 2014c). Thus, after presenting these simple descriptive results, I present results from models that condition upon eligibility-related characteristics to estimate whether formerly-incarcerated individuals appear to utilize these safety net programs differently than individuals with similar levels of need and eligibility-related characteristics who have never been incarcerated. It is these models, which will essentially estimate differences in program uptake between formerly-incarcerated and never-incarcerated individuals, that will allow me to test the system avoidance hypothesis. If system avoidance behaviors do in fact take priority over financial need among the formerly incarcerated, then the participation gap between observably similar formerly-incarcerated and never-incarcerated individuals should be larger for administratively burdensome benefit programs like AFDC/TANF and SSI than for programs like EITC and unemployment insurance, benefits for which can often be collected without any in-person interaction.

Additionally, I examine trends in benefit receipt before and after 1996, the year in which eligibility guidelines for cash welfare and food stamp assistance were changed to add work requirements and impose restrictions on receipt for convicted drug offenders. By exploring these dynamics and determining the extent to which formerly-incarcerated Americans make use of the social safety net, this paper will provide first insight into whether the safety net is serving the needs of this growing population or whether this is another realm in which formerly-incarcerated individuals are marginalized and excluded from American society and the benefits afforded to other citizens (Uggen et al. 2006).

The Changing Nature of the American Social Safety Net

Many means-tested entitlement programs – meaning that benefits are available to all who qualified based on need – have been transformed into or replaced by programs incumbent upon work-based eligibility, as best illustrated by the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which replaced the Aid to Dependent Families with Children (AFDC) entitlement program (commonly known as “welfare”) with the Temporary Assistance for Needy Families (TANF) block grant program, which added restrictions on eligibility for felony drug offenders and time limits for federally-funded benefits, as well as work requirements for eligibility.³ While the Supplemental Nutritional Assistance Program (SNAP), formerly known as “food stamps,” remains an entitlement program that provides benefits to all who meet

³ Although AFDC and TANF are usually thought of as serving single mothers, men were eligible to receive AFDC and are eligible to receive TANF if they meet eligibility guidelines – typically, having sufficiently low income and being the single custodial parent of a child (Chambers 1995; Urban Institute 2018). In the NLSY79, 12 percent of all AFDC/TANF recipients are men, and 63 percent of formerly-incarcerated AFDC/TANF recipients are men. Many of the male AFDC/TANF recipients in the NLSY79 are married or have a partner, but 20 percent report neither a spouse nor a partner.

eligibility requirements, PRWORA likewise introduced work requirements and imposed restrictions on eligibility for individuals convicted of felony drug offences for SNAP.

As politicians worked to restructure these public assistance entitlement programs at the end of the 20th century, political support and funding for the Earned Income Tax Credit (EITC) grew markedly. The EITC, which supplements taxable wages for low- to moderate-income families and individuals, surpassed AFDC/TANF in the amount of federal dollars dedicated to the program in 1996, and it is now the second largest means-tested public assistance program behind SNAP (Office of Management and Budget 2017). Because it functions as a refundable tax credit, it is only available to individuals who file a tax return.

What all of this means is that the American social safety net has become increasingly predicated on work-based eligibility (in the formal labor market) over the past few decades, at the same time that the number of formerly-incarcerated individuals – who are removed from the labor market while incarcerated and then likely to face employment discrimination upon reentry – has grown. Thus, the shift to work-based eligibility, as well as restrictions on receipt of federally-funded benefits by former drug offenders, for public assistance programs is likely to hinder formerly-incarcerated individuals' ability to utilize these programs in times of need.

Formerly-incarcerated individuals may also be less able to collect benefits from contributory social insurance programs, like unemployment and disability insurance, as a result of spotty work histories and low participation in the formal labor market (Visher et al. 2011). While TANF and SNAP work requirements can sometimes be fulfilled through time spent in educational or approved unpaid activities, both eligibility and benefit levels for unemployment insurance and disability are determined by prior

employment in formal labor market jobs that pay into these systems. As a result, formerly-incarcerated individuals may be less able to collect these social insurance benefits and, even when they do, may tend to receive smaller benefit amounts than other beneficiaries.

The following analyses will illuminate the extent to which formerly-incarcerated Americans participate in each of the six safety net programs I have noted. Because their work histories and other characteristics are likely to differ in important ways from those of other potential beneficiaries, I take into account eligibility-related criteria, thus predicting probability of benefit receipt if we were to equalize eligibility-related characteristics between formerly-incarcerated individuals and the other NLSY79 respondents. I also consider whether the AFDC/TANF and SNAP program changes that went into effect with the passage of PRWORA in 1996 affect probability of receipt by comparing these outcomes prior to and after 1996.

System Avoidance and Histories of Surveillance

In addition to the possibility that formerly-incarcerated individuals are *less able* to collect safety net benefits than financially-similar never-incarcerated individuals, there is also the possibility that formerly-incarcerated individuals may be *less willing* to participate in some safety net programs. In a 2014 article, Brayne argues that individuals who have had contact with the criminal justice system are likely to engage in “system avoidance” – that is, avoidance of record-keeping and surveilling institutions, including financial, medical and labor market institutions.

There are multiple mechanisms that may drive such behavior. One may be the desire to avoid apprehension by avoiding record-keeping institutions. For example, Goffman (2014) describes how young men with active warrants in Philadelphia avoid

settings, like hospitals, where they may be at greater risk of reporting to and/or apprehension by criminal justice authorities (e.g., police). Alternatively, criminal justice system involvement may increase distrust in surveilling institutions, thereby encouraging formerly-incarcerated individuals to avoid them when possible. Weaver and Lerman (2010) have found that criminal justice system contact and, especially, prior incarceration are associated with significantly lower political participation and trust in government.

Formerly-incarcerated individuals may avoid participation in safety net programs for either of these reasons. They may fear that a government benefits office is a setting in which they may be more likely to either encounter law enforcement or where they may have to provide identifying information, like current address of residence, that could be shared with law enforcement. Continued receipt of some benefits, like AFDC/TANF, typically requires periodic visits to such offices. Alternatively, formerly-incarcerated individuals may simply never apply for such benefits out of a general distrust of government institutions and programs.

If formerly-incarcerated individuals do engage in system avoidance behavior in the realm of social safety net program, then we might expect them to selectively participate in programs based on the level of administrative burden and in-person interaction required for eligibility determination and benefit receipt. Thus, the system avoidance hypothesis suggests that former inmates should more readily utilize programs with fewer administrative barriers. In that case, I expect the greatest differences in benefit receipt between those who have and have not been incarcerated for disability, SSI and AFDC/TANF receipt – all of which require sometimes substantial in-person interactions and recordkeeping to determine both initial and continued eligibility.

Accordingly, we should expect to see a moderate difference based on incarceration history in unemployment insurance receipt and little to no difference in less administratively burdensome programs like food stamps/SNAP and, especially, EITC.

Although Brayne does not discuss racial dynamics in her discussion of system avoidance, there is reason to believe that system avoidance behaviors may vary along racial lines, particularly with regard to avoidance of surveilling government institutions and, especially, public assistance programs. There is a long history of discriminatory and disparate surveillance of black citizens by American social welfare institutions that continues to inform views of the social welfare state (Gordy 2011). For example, use of “midnight raids” to surveil women receiving AFDC and enforce “man in the house” rules, which prevented women from receiving benefits if an able-bodied man was found to be present in the household, in the mid-20th century represented an extreme invasion of privacy that disproportionately focused on black women with the purpose of removing them from welfare rolls and deterring participation in the program (Reich 1963; Soss, Fording, and Schram 2011:87).

Additionally, the stark racial disparities in criminal justice contact at every level – ranging from police stops to incarceration – may further exacerbate legal cynicism and system avoidance among nonwhite, and especially black, former inmates who may feel they are likely to be subject to greater scrutiny by all government institutions (Bonczar 2003; Heath 2014; Pierson et al. 2017). Thus, black formerly-incarcerated individuals may be much more likely to engage in system avoidance behaviors, particularly with respect to public assistance programs, than formerly-incarcerated whites. I test this possibility by examining black-white racial differences in safety net program participation, looking specifically for varying patterns in participation between more

administratively burdensome programs (e.g., TANF, SSI) and less administratively burdensome programs (e.g., SNAP, EITC) by race.

Data & Methods

The National Longitudinal Survey of Youth 1979 (NLSY79) has collected detailed annual and biennial data on household structure, employment, and sources of income – including receipt of safety net benefits – from a nationally-representative cohort of 12,868 men and women since they were first interviewed in 1979 at ages 14-22. Members of the NLSY79 cohort were 50-58 years old in the most recent survey wave (2014). Sample members were interviewed annually from 1979 through 1994 and have been interviewed biennially since, with the response rate remaining close to 80 percent (Bureau of Labor Statistics n.d.).

NLSY79 collects a rich array of data on sources of income in each year, including income received from various social safety net programs. Data on food stamp/SNAP, AFDC/TANF, unemployment insurance and SSI income are collected from all respondents in all years. Information on EITC receipt is collected starting in 2000, and disability insurance income is collected starting in 2002. Therefore, sample sizes are smaller when disability and EITC receipt are the outcomes of interest.

While NLSY79 does not collect particularly detailed data on criminal history or incarceration spells across survey waves, I am able to identify respondents who were incarcerated in a prison or jail at the time of each survey. Although respondents who served time between survey waves are not identified, Western (2002) finds that incarceration rates created from the NLSY79 closely match national imprisonment rates for young men using data from the Bureau of Justice Statistics. Therefore, the NLSY79 data should reasonably identify respondents who have ever been incarcerated. I account

for incarceration history in all models by including a dummy variable indicating whether the respondent has ever been incarcerated in any previous survey wave (*Previously Incarcerated_{it}*) and another indicating incarceration at the previous survey wave (*Jail_{it-1}*). Because benefit receipt variables refer to receipt in the calendar year preceding the current survey year (e.g., 1990 for the 1991 survey, 2012 for the 2014 survey), these variables capture incarceration at or before benefit receipt. Likewise, all other time-varying control variables are lagged to reflect their values in the year preceding benefit receipt.

In the following analyses I first provide simple descriptive results of the proportion of formerly-incarcerated individuals who receive benefits from each of the six safety net programs relative to the proportion of never-incarcerated individuals who receive these benefits. These results will provide us with baseline information on whether formerly-incarcerated individuals appear to benefit from these social safety net programs disproportionately, on average, compared to the never-incarcerated population. I then present results from a series of logit models that predict any receipt for each benefit in question, conditional on eligibility-related covariates.⁴ The logit models use the following general form:

$$\ln\left(\frac{P_{any\ receipt_{it}}}{1 - P_{any\ receipt_{it}}}\right) = \beta_0 + \beta_1 Previously\ Incarcerated_{it} + \beta_2 Jail_{it-1} + \beta_3 Age_{it} + \beta_4 Black_i + \beta_5 Hispanic_i + \beta_6 Other\ Race_i + \beta_7 Female_i + \beta_8 Education_{it-1} + \beta_9 Region_{it-1} + \beta_{10} X_{it} + \delta Year_t$$

⁴ Unfortunately, NLSY79 only collects data on amount (successfully) received from each of these programs, so I am unable identify whether respondents who did not report income from these programs applied or filed a claim.

Education level in each year is measured in four categories reflecting attainment: less than high school, a high school diploma, some college, and a four-year college degree or higher, with high school completion as the reference category. *Region* of residence is another time-varying categorical variable divided into Northeast, North Central, South, and West, with South as the reference group. X_{it} is a vector of program-specific eligibility-related covariates, as described below.

In addition to the general logit model described above, I also run individual fixed effect models which include the same controls as the main model, aside from race and gender, which do not vary within person. I multiply impute missing values for education, region of residence, and eligibility-related covariates. Additionally, I employ year fixed effects and cluster errors at the individual level throughout. All dollar value variables are adjusted for inflation to 2014 dollars using the Consumer Price Index.

Eligibility-Related Covariates

AFDC/TANF

The model of welfare cash assistance receipt incorporates measures of the *number of children* in the respondent's household in the previous survey year, his or her *marital status* at the prior survey (indicator set equal to one if married), *household income* (labor income from the respondent plus his/her spouse or partner) in the past year, and *number of weeks worked* by the respondent in the past calendar year. Because many states have asset tests for TANF eligibility, I also include a measure of *non-retirement financial assets* in the prior period, as these asset limits typically apply to liquid assets, excluding vehicles and tax-preferred retirement savings accounts (Prosperity Now 2017).

SSI

The model of Supplemental Security Income receipt includes measures of *disability* status in the prior survey year, number of *weeks out of the labor force* in the past calendar year, number of *weeks worked* in the past calendar year, respondent's *labor income* in the past two years, and *non-retirement financial assets* in the prior period, as SSI is, like AFDC/TANF, a means-tested program. The disability measure is a dummy variable set equal to one if the respondent reported having a disability that either prevented her from working or limited the type of work she could reasonably do.

Disability

The model used to estimate disability benefit receipt is nearly identical to those used to estimate SSI receipt, but because disability insurance is not means-tested I drop the measures of non-retirement financial assets. I still include measures of labor income in the last two years, as eligibility and benefit level are related to prior earnings since disability is a contributory social insurance program.

Unemployment Insurance

In the unemployment receipt model, I control for a variety of work history measures related to UI eligibility. These include *percent of weeks worked since last interview*, which accounts for all weeks between survey waves even when data collection becomes biennial, and measures of labor force participation in the past calendar year (for which benefit receipt is reported): number of *weeks unemployed*, number of *weeks out of the labor force*, and number of *weeks in the active duty military*. Additionally, because unemployment insurance is only available to individuals in the formal labor market who meet minimum employment requirements, I have also created a variable

indicating whether the respondent had worked at a “traditional” job more than 10 hours per week for at least 10 weeks during the prior year.⁵ I include two lagged versions of this measure (*Likely UI eligible job_{it-1}* and *Likely UI eligible job_{it-2}*) as proxies to indicate whether the respondent is likely to have worked at any job that would have paid into unemployment insurance on their behalf in the two survey waves preceding benefit receipt. I also include a categorical variable identifying the *employment sector* – not currently working, private, government, self-employed/family business, or military – of the respondent’s primary job at the last survey wave (with private sector as the reference group), an indicator identifying *disability* status in the prior survey year, and respondent’s *labor income* in the past two years.

Food Stamps/SNAP

In addition to the standard set of covariates described above, the SNAP receipt model includes measures of *household income* (respondent plus spouse/partner labor income) in the past year and *household size* in the prior survey year, which are used to determine eligibility and benefit levels. Additionally, because PRWORA introduced work requirements for receipt in 1996, I also include a measure of the *number of weeks worked* by the respondent in the past calendar year. Finally, I include *non-retirement financial assets* in the prior period, as many states use asset tests in determining SNAP eligibility.

⁵ NLSY79 respondents were asked if the current jobs they reported were “odd jobs” or were “traditional” or “regular” jobs (wording changed over the years). Those who responded that they worked in “odd jobs” are included in the above measure if they indicated that the “odd job” they identified working for offered them benefits (e.g., health insurance, paid time off) and they reported working at that job for over 10 hours a week for at least 10 weeks. Unpaid and self-employed jobs are excluded from this measure.

EITC

Finally, when modelling EITC receipt, I incorporate measures of *number of children* in the respondent's household in the previous survey year, *marital status* at the prior survey, and *household labor income* (respondent plus spouse/partner) in the year preceding EITC receipt.

Model Alterations for Additional Hypothesis Testing

The above models will allow me to examine the extent to which formerly-incarcerated individuals draw on social safety net programs, independent of their eligibility-related characteristics. By comparing probability of receipt across programs I can see how benefit receipt differs for social insurance versus public assistance programs, as well as whether formerly-incarcerated respondents appear to exhibit system avoidance behavior by less frequently utilizing programs like AFDC/TANF and SSI that require greater levels of in-person interaction and entail more reporting and surveillance.

In order to test whether the work requirements and eligibility restrictions for felony drug offenders imposed on TANF and SNAP in 1996 with the passage of PRWORA altered probability of receipt, I run versions of the models described above with the year fixed effects replaced by two dummy variables that identify years before 1996 and years after 1996 – 1996 serves as the reference year. I interact the *post-1996* dummy variable with *Previously Incarcerated* to test whether the PRWORA reforms differentially affected receipt for formerly-incarcerated individuals. Because 1996 also saw the passage of the Contract with America Advancement Act, which terminated SSI and disability benefits for recipients whose primary impairment was drug addiction or alcoholism, I run these pre- vs. post-1996 models for AFDC/TANF, food stamps/SNAP, and SSI

receipt. Because NLSY79 does not begin collecting data on disability income until 2002, I am unable to compare disability receipt before and after 1996.

Additionally, to test whether system avoidance behaviors differ by race, I also run a set of models that include interactions between race and prior incarceration history. Results of these models and those above are provided and described in the following section.

Results

Table 3.1 shows the proportion of respondents receiving safety net benefits, average benefit amount among those receiving any, and descriptive statistics for control variables by incarceration history. The top portion of the table represents averages across all person-years for time-varying variables, including safety net benefit receipt. The bottom portion of the table presents averages of time-invariant characteristics among respondents who participated in 2014, the most recent survey wave.

Before adjusting for individual characteristics and program eligibility, we see that formerly-incarcerated individuals have higher participation rates in all safety net programs with the exception of unemployment insurance. Difference in benefit receipt rates appears to be greatest for SSI, for which formerly-incarcerated individuals' receipt rates are nearly four times those of never-incarcerated individuals (9.3 versus 2.4 percent, respectively), and food stamps, which formerly-incarcerated respondents receive more than twice as often (14.9 versus 5.8 percent, respectively). This greater participation in safety net programs is perhaps unsurprising when we consider average differences in other characteristics between formerly-incarcerated and never-incarcerated NLSY79 respondents.

Table 3.1. Descriptive Statistics across NLSY79 Person-Years

	Previously Incarcerated	
	No	Yes
Person-Year Level		
<u>Program Participation</u>		
<i>AFDC/TANF</i>		
Any received	2.8%	3.6%
Person-years (1980-2014)	229,342	7,955
Amount (if received)	\$7,301 (9,304)	\$7,099 (6,765)
<i>Supplemental Security Income (SSI)</i>		
Any received	2.4%	9.3%
Person-years (1980-2014)	228,986	7,897
Amount (if received)	\$7,440 (8,797)	\$7,805 (7,203)
<i>Disability Insurance</i>		
Any received	5.4%	6.5%
Person-years (2002-2014)	49,542	2,979
Amount (if received)	\$2,220 (4,108)	\$1,104 (1,670)
<i>Unemployment Insurance (UI)</i>		
Any received	4.8%	4.3%
Person-years (1980-2014)	229,268	8,013
Amount (if received)	\$5,239 (7,253)	\$5,580 (5,018)
<i>Food Stamps/SNAP</i>		
Any received	5.8%	14.9%
Person-years (1980-2014)	228,863	7,859
Amount (if received)	\$3,001 (4,520)	\$2,313 (2,249)
<i>Earned Income Tax Credit (EITC)</i>		
Any received	6.8%	8.6%
Person-years (2000-2014)	56,025	3,333
Amount (if received)	\$1,700 (1,654)	\$1,727 (2,281)
<u>Standard Control Variables</u>		
Age	31.8	36.7
Education		
Less than high school	19.2%	40.1%
High school or GED	38.6%	44.9%
Some college	23.3%	13.6%
College graduate or more	18.9%	1.5%
In Jail	--	28.9%
Region		
Northeast	19.0%	13.6%
North Central	28.8%	17.3%
South	34.7%	47.0%
West	17.5%	22.2%
<u>Eligibility-Related Variables</u>		
Personal labor income	\$30,341 (28,398)	\$13,929 (19,927)
Household labor income	\$51,784 (45,185)	\$19,238 (29,050)

Table 3.1. (Continued)

Married	47.9%	21.5%
Number of children (in household)	0.83	0.49
Household size	3.18	2.79
Disabled	7.6%	18.9%
Work History		
% weeks worked since last interview	74.0%	47.5%
Weeks worked in past year	38.11	24.40
Weeks unemployed in past year	2.50	5.54
Weeks out of labor force in past year	10.29	21.66
Weeks active duty military in past year	0.90	0.08
Worked in a likely UI eligible job in past year	73.7%	53.7%
Employment Sector (current job)		
Not currently employed	13.8%	31.8%
Private	66.5%	53.7%
Government	10.8%	4.7%
Self-employed or family business	7.2%	9.7%
Military	1.8%	0.1%
Non-retirement account financial assets	\$47,342 (101,677)	\$7,899 (41,875)
	<i>Person-years</i>	
	229,963	8,060
Person-Level (2014)		
Female	53.7%	12.1%
Male	46.3%	87.9%
Race		
White	63.6%	37.1%
Black	14.7%	38.5%
Hispanic	6.4%	10.7%
Other	14.4%	13.0%
	<i>N (2014)</i>	
	6,653	418

Note: Weighted values. Standard deviations in parentheses. All dollar value variables are adjusted for inflation to 2014 values.

Both personal and household labor income among formerly-incarcerated individuals are less than half of the average income for never-incarcerated individuals (\$14,000 compared to \$30,000 and \$19,000 compared to \$52,000, respectively). Relatedly, formerly-incarcerated individuals spend over twice as much time unemployed (5.5 versus 2.5 weeks per year) or out of the labor force (22 versus 10 weeks) each year as never-incarcerated individuals. Likewise, they work about 36 percent fewer weeks in any given period of time as never-incarcerated individuals (24 versus 38 weeks in the past year, for example). Formerly-incarcerated individuals are also less likely to report that they worked 10 hours or more per week for at least 10 weeks at any regular job within the

past year (54 percent compared to 74 percent) than never-incarcerated respondents. They also own considerably less in non-retirement financial assets than never-incarcerated individuals: just \$8,000, on average, compared to an average of \$47,000 among never-incarcerated individuals.

Formerly-incarcerated individuals also report much higher disability rates (19 percent versus 8 percent) and lower marital rates (22 percent versus 48 percent). Unsurprisingly, given differential incarceration rates by race, gender, education and region, formerly-incarcerated NLSY79 respondents are disproportionately black (39 percent versus 15 percent), male (88 versus 46 percent), and Southern (47 versus 35 percent), and less likely to have attended at least some college (15 versus 42 percent) than never-incarcerated individuals.

Table 3.2 displays results from the logit models predicting receipt of any income from each of the six safety net programs. Coefficients represent log-odds of having received any income from the relevant program, conditional on race, age, gender, education, region and eligibility-related covariates. The programs are arranged in the table from roughly the most administratively burdensome and surveilling (AFDC/TANF, SSI and disability) on the left to the least (EITC) on the right-hand side. If system avoidance were strongly influencing social safety net usage, we might expect to see that the coefficients on *Previously Incarcerated*, which tell us the difference in log-odds of benefit receipt between formerly-incarcerated and never-incarcerated respondents holding all other variables in the model constant, would be (more) negative on the left and less negative or positive for the programs in the right-side columns. This is not the case, however. Instead, the results in Table 3.2 reveal that, all else equal, formerly-incarcerated individuals are significantly more likely to receive means-tested public

Table 3-2. Logit Models Predicting Any Receipt of Safety Net Benefits with Year Fixed Effects, by Program

	(1)	(2)	(3)	(4)	(5)	(6)
	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Previously Incarcerated	0.43** (0.15)	0.24* (0.10)	-0.40*** (0.12)	-0.28** (0.10)	0.31*** (0.08)	0.05 (0.11)
In Jail (lagged)	-1.81*** (0.21)	-1.70*** (0.14)	-1.57*** (0.38)	-1.55*** (0.26)	-1.76*** (0.11)	-0.38 (0.24)
Age	0.05*** (0.01)	0.06*** (0.01)	0.04** (0.01)	0.06*** (0.01)	0.13*** (0.01)	-0.04*** (0.01)
Female	1.51*** (0.07)	-0.03 (0.06)	-0.19** (0.06)	-0.25*** (0.03)	1.06*** (0.04)	0.53*** (0.06)
Race						
Black	0.46*** (0.06)	0.80*** (0.06)	0.02 (0.07)	-0.21*** (0.04)	0.47*** (0.05)	0.28*** (0.07)
Hispanic	0.01 (0.08)	0.27*** (0.08)	-0.21* (0.09)	-0.03 (0.05)	0.28*** (0.06)	0.00 (0.08)
Other	0.07 (0.10)	0.20* (0.09)	0.14 (0.10)	0.09† (0.06)	0.16* (0.07)	0.15 (0.10)
Education						
Less than high school	-0.12* (0.06)	0.31*** (0.06)	-0.24** (0.09)	-0.37*** (0.05)	0.29*** (0.04)	-0.07 (0.07)
Some college	-0.58*** (0.07)	-0.37*** (0.07)	0.10 (0.07)	-0.36*** (0.04)	-0.66*** (0.05)	-0.03 (0.07)
B.A. or higher	-1.53*** (0.18)	-0.99*** (0.12)	-0.36*** (0.10)	-0.87*** (0.06)	-1.72*** (0.11)	-0.68*** (0.11)
Region						
Northeast	0.33*** (0.08)	0.96*** (0.07)	-0.22* (0.09)	0.20*** (0.05)	0.16** (0.05)	-0.50*** (0.09)
North Central	1.12*** (0.06)	0.78*** (0.06)	-0.14† (0.08)	0.32*** (0.04)	0.53*** (0.05)	-0.15* (0.07)
West	0.99*** (0.07)	0.37*** (0.07)	-0.09 (0.08)	0.42*** (0.04)	0.09 (0.05)	-0.16† (0.08)
Eligibility-Related Characteristics						
Married (lagged)	-0.97*** (0.07)					-0.38*** (0.06)
Number of children (lagged)	0.79*** (0.03)					0.49*** (0.02)
Household size (lagged)					0.00 (0.01)	
Disabled (lagged)		1.24*** (0.04)	1.46*** (0.06)	-0.27*** (0.05)		

Table 3.2. (Continued)	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Non-retirement account financial assets (lagged)	-0.00 [†] (0.00)	-0.00 (0.00)			-0.00 ^{***} (0.00)	
Personal labor income		-0.03 ^{***} (0.00)	-0.01 ^{***} (0.00)	-0.02 ^{***} (0.00)		
Lagged personal labor income		-0.02 ^{***} (0.00)	0.01 ^{***} (0.00)	0.02 ^{***} (0.00)		
Household labor income	-0.06 ^{***} (0.00)				-0.05 ^{***} (0.00)	
Lagged household labor income						-0.01 ^{***} (0.00)
Work History						
% weeks worked since last interview				-0.73 ^{***} (0.08)		
Weeks worked in past year	-0.03 ^{***} (0.00)	-0.02 ^{***} (0.00)	-0.00 (0.00)		-0.02 ^{***} (0.00)	
Weeks unemployed in past year				0.06 ^{***} (0.00)		
Weeks out of labor force in past year		0.01 ^{***} (0.00)	0.02 ^{***} (0.00)	-0.01 ^{***} (0.00)		
Weeks active duty military in past year				-0.03 ^{***} (0.00)		
Likely UI eligible job at last survey				0.36 ^{***} (0.04)		
Lagged likely UI eligible job				0.45 ^{***} (0.04)		
Employment Sector (lagged)				-1.49 ^{***} (0.06)		
Not currently employed				-0.77 ^{***} (0.06)		
Government				-1.29 ^{***} (0.08)		
Self-employed or family business				1.23 ^{***} (0.13)		
Military						
	<i>Observations (person-years)</i>	237,256	236,842	52,512	236,681	12,153

*** p<0.001, ** p<0.01, * p<0.05, † p<0.10
Note: Coefficients represent log odds. Standard errors are clustered at the individual level. Year fixed effects are included in all models but not displayed above. Financial covariates (income, assets) are measured in thousands and adjusted for inflation to 2014 values. Variables noted as lagged above are lagged one period.

assistance benefits, including those with higher levels of surveillance (AFDC/TANF and SSI), than never-incarcerated individuals on average, but less likely to receive benefits from contributory social insurance programs (disability and unemployment).

Rather than supporting the system avoidance hypothesis, these results suggest that financial need may outweigh concerns over surveillance among individuals who have been incarcerated. Moreover, the lower levels of disability and unemployment insurance receipt among formerly-incarcerated individuals, conditional on eligibility-related characteristics, suggest that employment-based social insurance programs may not serve previously-incarcerated individuals as well as other Americans, given more unstable employment histories among the formerly incarcerated, as shown in Table 3.1.

This gap may also indicate that the covariates I have included in these models do not completely capture relevant differences in work history between formerly-incarcerated and never-incarcerated respondents – this is probably particularly true with regard to identifying prior employment and earnings accrued at employers that withhold Social Security taxes and pay into state disability insurance programs. Given prior findings that formerly-incarcerated individuals were more likely to be earning wages from informal work than from legal, traditional employers eight months after release (Visher et al. 2011), it is extremely likely that the prior earnings and weeks worked variables included in these models do not only reflect wages and time spent at employers that pay into these two social insurance programs on employees behalf, making their predictive value weaker.

With that said, what these models can tell us is that, conditional on having the same level of earnings and number of weeks in and out of the labor force – from or at any type of job – formerly-incarcerated individuals are less likely to collect social

insurance program benefits than never-incarcerated individuals, despite the fact that they are more likely than never-incarcerated individuals to collect means-tested safety net benefits. Thus, these lower log-odds of disability and unemployment receipt likely do not represent system avoidance behavior, but rather a diminished ability to collect benefits that are only available to those with established work histories in the formal labor market – a market that formerly-incarcerated individuals are often shut out of both by incarceration itself and by the stigma and discrimination likely to follow from it.

EITC is the only program for which log odds of receipt do not differ significantly by incarceration history. This may reflect the fact that, relative to the other programs examined here, eligibility is very straightforward, application is easy, and potential applicants are heavily encouraged to apply for the program both by the federal government and, especially, by tax preparation services, which offer to do most of the work and sometimes even allow the applicant to receive cash that day, rather than waiting to receive a refund check in the mail, in exchange for taking a cut of the applicant's benefit. Therefore, uptake is likely to be high for EITC across the board.

Table 3.3 shows results from the individual fixed effects models, which account for potential confounding due to unobserved fixed individual-level characteristics that may systematically vary between formerly-incarcerated and never-incarcerated individuals, such as intrinsic motivation. As in the main models displayed in Table 3.2, the individual fixed effects models reveal that log odds of AFDC/TANF, SSI, and food stamp receipt are significantly higher after incarceration, suggesting that need trumps system avoidance for this population. Differences in unemployment insurance and disability insurance receipt are no longer statistically significant in the individual fixed effects model, which may reflect unobserved differences in the general type and stability

Table 3.3. Individual Fixed Effect Logit Models Predicting Any Receipt of Safety Net Benefits, by Program

	(1)	(2)	(3)	(4)	(5)	(6)
	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Previously Incarcerated	0.75*** (0.15)	0.28** (0.11)	-1.03 (0.91)	-0.01 (0.13)	0.42*** (0.08)	0.51 (0.41)
In Jail (lagged)	-1.85*** (0.20)	-1.53*** (0.15)	-0.44 (0.45)	-1.59*** (0.26)	-1.65*** (0.10)	-0.72** (0.25)
Age	-0.14** (0.05)	-0.05 (0.04)	-0.04 (0.08)	-0.07† (0.04)	-0.05 (0.03)	0.07 (0.05)
Education						
Less than high school	-0.60*** (0.07)	-0.33*** (0.06)	0.22 (0.23)	-0.74*** (0.06)	-0.55*** (0.04)	0.06 (0.17)
Some college	-0.04 (0.09)	-0.12 (0.08)	0.01 (0.24)	-0.14* (0.07)	-0.10† (0.06)	0.02 (0.16)
B.A. or higher	-0.32 (0.21)	0.25† (0.14)	0.18 (0.28)	0.27** (0.10)	-0.24† (0.12)	-0.30 (0.23)
Region						
Northeast	0.36* (0.16)	0.59*** (0.11)	0.53 (0.36)	0.09 (0.10)	-0.01 (0.08)	-0.33 (0.30)
North Central	0.95*** (0.13)	0.72*** (0.11)	-0.03 (0.29)	0.16† (0.09)	0.50*** (0.08)	0.21 (0.20)
West	0.83*** (0.15)	0.38** (0.12)	0.12 (0.31)	0.23* (0.09)	0.17* (0.08)	-0.54* (0.24)
Eligibility-Related Characteristics						
Married (lagged)	-0.69*** (0.05)					-0.30*** (0.07)
Number of children (lagged)	0.55*** (0.02)					0.25*** (0.03)
Household size (lagged)					-0.07*** (0.01)	
Disabled (lagged)		0.70*** (0.04)	0.44*** (0.09)	-0.30*** (0.05)		
Non-retirement account financial assets (lagged)	-0.00 (0.00)	0.00 (0.00)			-0.00† (0.00)	
Personal labor income		-0.02*** (0.00)	-0.01*** (0.00)	-0.02*** (0.00)		
Lagged personal labor income		-0.01*** (0.00)	0.00 (0.00)	0.02*** (0.00)		
Household labor income	-0.05*** (0.00)				-0.05*** (0.00)	

	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Table 3.3. (Continued)						
Lagged household labor income						
Work History						
% weeks worked since last interview				-0.94*** (0.07)		-0.01*** (0.00)
Weeks worked in past year	-0.03*** (0.00)	-0.01*** (0.00)	-0.00 (0.00)		-0.02*** (0.00)	
Weeks unemployed in past year				0.06*** (0.00)		
Weeks out of labor force in past year		0.01*** (0.00)	0.02*** (0.00)	-0.01*** (0.00)		
Weeks active duty military in past year				-0.05*** (0.00)		
Likely UI eligible job at last survey				0.28*** (0.04)		
Lagged likely UI eligible job				0.36*** (0.03)		
Employment Sector (lagged)						
Not currently employed				-1.38*** (0.06)		
Government				-0.72*** (0.06)		
Self-employed or family business				-1.07*** (0.08)		
Military				1.52*** (0.17)		
	<i>Observations (person-years)</i>	46,170	10,007	100,508	85,516	16,296
	<i>Individuals</i>	2,206	1,548	5,096	4,192	2,287

*** p<0.001, ** p<0.01, * p<0.05, † p<0.10

Note: Coefficients represent log odds. Individual and year fixed effects are included in all models but not displayed above. Financial covariates (income, assets) are measured in thousands and adjusted for inflation to 2014 values. Variables noted as lagged above are lagged one period.

of employment among individuals who will ever be incarcerated compared to never-incarcerated individuals.

Table 3.4 displays results from the logit models of benefit receipt that use dummy variables to compare log odds of receipt before and after the 1996 reforms that altered eligibility guidelines for AFDC/TANF, food stamp/SNAP, and SSI. In these models, *Previously Incarcerated* is interacted with the dummy variable identifying *post-1996*, so the coefficient on *Previously Incarcerated* reflects the log odds of receipt for formerly-incarcerated individuals relative to never-incarcerated individuals prior to 1996.

Table 3.4. Logit Models Predicting Any Receipt of Safety Net Benefits Pre- and Post-1996 Reforms, by Program

	(1) AFDC/TANF	(2) SSI	(3) Food Stamps
Pre-1996	0.51*** (0.08)	-0.05 (0.07)	0.65*** (0.05)
Post-1996	-2.12*** (0.10)	-0.60*** (0.07)	-1.92*** (0.06)
Ever Incarcerated	0.38* (0.17)	0.43*** (0.12)	0.13 (0.11)
Ever Incarcerated*Post-1996	0.45 [†] (0.24)	-0.21 (0.14)	0.48*** (0.12)
In Jail (lagged)	-1.64*** (0.23)	-1.70*** (0.14)	-1.53*** (0.12)
Age	0.03*** (0.00)	0.06*** (0.00)	0.12*** (0.00)
Female	1.49*** (0.06)	0.01 (0.05)	1.08*** (0.04)
Race			
Black	0.49*** (0.06)	0.83*** (0.06)	0.52*** (0.05)
Hispanic	0.04 (0.08)	0.31*** (0.08)	0.33*** (0.06)
Other	0.10 (0.09)	0.22* (0.09)	0.21** (0.07)
Education			
Less than high school	-0.24*** (0.06)	0.24*** (0.06)	0.18*** (0.04)
Some college	-0.56*** (0.07)	-0.38*** (0.07)	-0.67*** (0.05)
B.A. or higher	-1.47*** (0.19)	-0.98*** (0.12)	-1.72*** (0.11)
Region			
Northeast	0.34*** (0.08)	0.96*** (0.07)	0.16** (0.05)
North Central	1.12*** (0.06)	0.79*** (0.06)	0.53*** (0.05)

held at their means, the predicted probability of food stamp (SNAP) receipt dropped by .0392 among formerly-incarcerated individuals after 1996, compared to a drop of .036 among never-incarcerated individuals. Likewise, formerly-incarcerated individuals saw a drop of .006 in predicted probability of AFDC/TANF receipt following the 1996 reforms, whereas otherwise similar never-incarcerated respondents (covariates held at means) saw a drop of .004 in predicted probability of receipt.

Table 3.5 displays results from logit models of benefit receipt like those in Table 2, but with the inclusion of race and incarceration history interaction terms. Results from these models, therefore, allow us to see whether there are racial differences in system avoidance behavior or utilization of safety net benefits by program structure (i.e., social insurance versus means-tested assistance). The race-interacted models reveal that the public-assistance-seeking behavior observed in Table 3.2 – i.e., statistically significantly higher log odds of SNAP, SSI and AFDC/TANF receipt among formerly-incarcerated individuals – is limited to whites. In fact, the statistically significant, larger (in absolute terms), negative coefficients on the *Previously Incarcerated*Black* interaction term in these three models indicate not only that prior incarceration is *not* associated with higher log odds of receiving benefits from these public assistance programs for black sample members, but that prior incarceration may actually be associated with *lower* probability of receiving public assistance benefits for blacks. Moreover, the fact that the gap between the positive coefficient on the *Previously Incarcerated* term and the negative coefficient on the *Previously Incarcerated*Black* is largest for AFDC/TANF, followed by SSI, then SNAP is the first potential evidence of system avoidance behavior in these results.

Table 3-5. Logit Models Predicting Any Receipt of Safety Net Benefits with Race Interactions and Year Fixed Effects, by Program

	(1)	(2)	(3)	(4)	(5)	(6)
	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Ever Incarcerated	1.16*** (0.24)	0.99*** (0.16)	-0.45† (0.25)	-0.31 (0.21)	1.28*** (0.16)	0.41 (0.29)
Ever Incarcerated*Black	-1.62*** (0.31)	-1.16*** (0.20)	-0.01 (0.29)	-0.21 (0.25)	-1.45*** (0.18)	-0.52† (0.31)
Ever Incarcerated*Hispanic	0.09 (0.29)	-0.59* (0.25)	0.23 (0.33)	0.63* (0.27)	-0.77*** (0.21)	-0.25 (0.36)
Ever Incarcerated*Other race	-0.41 (0.47)	0.13 (0.28)	0.26 (0.42)	-0.35 (0.39)	-0.64* (0.27)	-0.30 (0.54)
In Jail (lagged)	-1.79*** (0.21)	-1.61*** (0.14)	-1.57*** (0.38)	-1.54*** (0.25)	-1.68*** (0.10)	-0.34 (0.24)
Age	0.05*** (0.01)	0.06*** (0.01)	0.04** (0.01)	0.06*** (0.01)	0.14*** (0.01)	-0.04*** (0.01)
Female	1.50*** (0.07)	-0.03 (0.06)	-0.19** (0.06)	-0.25*** (0.03)	1.06*** (0.04)	0.53*** (0.06)
Race						
Black	0.50*** (0.06)	0.87*** (0.06)	0.02 (0.08)	-0.20*** (0.04)	0.53*** (0.05)	0.30*** (0.07)
Hispanic	-0.02 (0.09)	0.30*** (0.08)	-0.22* (0.09)	-0.06 (0.05)	0.29*** (0.06)	-0.00 (0.09)
Other	0.06 (0.10)	0.18† (0.10)	0.13 (0.11)	0.10† (0.06)	0.17* (0.07)	0.15 (0.11)
Education						
Less than high school	-0.11† (0.06)	0.30*** (0.06)	-0.25** (0.09)	-0.38*** (0.05)	0.29*** (0.04)	-0.07 (0.07)
Some college	-0.58*** (0.07)	-0.37*** (0.07)	0.10 (0.07)	-0.36*** (0.04)	-0.66*** (0.05)	-0.03 (0.07)
B.A. or higher	-1.52*** (0.18)	-0.99*** (0.12)	-0.37*** (0.10)	-0.87*** (0.06)	-1.72*** (0.11)	-0.68*** (0.11)
Region						
Northeast	0.34*** (0.08)	0.98*** (0.07)	-0.22* (0.09)	0.20*** (0.05)	0.17** (0.05)	-0.50*** (0.09)
North Central	1.13*** (0.06)	0.79*** (0.06)	-0.13† (0.08)	0.31*** (0.04)	0.54*** (0.05)	-0.15* (0.07)
West	1.00*** (0.07)	0.37*** (0.07)	-0.08 (0.08)	0.42*** (0.04)	0.10† (0.05)	-0.14† (0.08)
<u>Eligibility-Related Characteristics</u>						
Married (lagged)	-0.97*** (0.07)					-0.38*** (0.06)
Number of children (lagged)	0.79*** (0.03)					0.49*** (0.02)

	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Table 3.5. (Continued)						
Household size (lagged)					0.00 (0.01)	
Disabled (lagged)		1.24*** (0.04)	1.46*** (0.06)	-0.27*** (0.05)		
Non-retirement account financial assets (lagged)	-0.00† (0.00)	-0.00 (0.00)			-0.00*** (0.00)	
Personal labor income		-0.03*** (0.00)	-0.01*** (0.00)	-0.02*** (0.00)		
Lagged personal labor income		-0.02*** (0.00)	0.01*** (0.00)	0.02*** (0.00)		
Household labor income	-0.06*** (0.00)				-0.05*** (0.00)	
Lagged household labor income						-0.01*** (0.00)
Work History						
% weeks worked since last interview				-0.73*** (0.08)		
Weeks worked in past year	-0.03*** (0.00)	-0.02*** (0.00)	-0.00 (0.00)		-0.02*** (0.00)	
Weeks unemployed in past year				0.06*** (0.00)		
Weeks out of labor force in past year		0.01*** (0.00)	0.02*** (0.00)	-0.01*** (0.00)		
Weeks active duty military in past year				-0.03*** (0.00)		
Likely UI eligible job at last survey				0.37*** (0.04)		
Lagged likely UI eligible job				0.45*** (0.04)		
Employment Sector (lagged)						
Not currently employed				-1.49*** (0.06)		
Government				-0.76*** (0.06)		
Self-employed or family business				-1.29*** (0.08)		
Military				1.22*** (0.13)		
	<i>Observations (person-years)</i>	235,622	52,221	223,033	235,051	12,094

***, p<0.001, **, p<0.01, * p<0.05, † p<0.10

Note: Coefficients represent log odds. Standard errors are clustered at the individual level. Year fixed effects are included in all models but not displayed above. Financial covariates (income, assets) are measured in thousands and adjusted for inflation to 2014 values. Variables noted as lagged above are lagged one period.

In order to test the robustness of this potential evidence of system avoidance by black former inmates, I also ran single race, fully-interacted models for black and white respondents (see Appendix Tables A3.1 and A3.2). When I examine results from the fully-interacted race model, however, there is no statistically significant difference between formerly-incarcerated and never-incarcerated black respondents in log odds of receiving benefits from any of these three programs, conditional on eligibility-related criteria (Appendix Table A3.1). Prior incarceration is still associated with significantly lower log-odds of receiving disability and unemployment benefits, however, although the *p*-value on the *Previously Incarcerated* coefficient in the unemployment receipt model of unemployment is only marginally significant because of larger standard errors. Therefore, the earlier observed disadvantage with regard to receipt of social insurance benefits appears to hold up in the single race model, but prior incarceration does not appear to be associated with either system avoidance or assistance-seeking behaviors among black sample members.

Thus, what the single race models make clear is that both blacks and whites appear to be hampered in their ability to take advantage of the benefits of social insurance programs following incarceration, even when observable differences in employment and earnings history are taken into account. But, unlike blacks, formerly-incarcerated whites appear empowered to seek out support from means-tested public assistance programs even more so than observably similar never-incarcerated whites.

Robustness Checks

Because NLSY79 collects data on the amount of benefits received, I also ran a series of models using the same covariates described above to predict amount of benefit received conditional on any receipt. There are no clear patterns by incarceration history

when it comes to amount of benefit among recipients, which is unsurprising given that most of the theoretically interesting differences should emerge in probability of any receipt rather than in specific amount received. These results are shown in Appendix Table A3.3.

Because the vast majority of formerly-incarcerated persons in both the population as a whole and the NLSY79 sample are men, I also ran the same set of primary models only on NLSY79 male respondents. The findings reported above hold true when the sample is restricted only to male respondents, except that the magnitude of the difference in SSI receipt is smaller, making the coefficient no longer statistically significant, though it is still positive. Results from the men only models can be seen in Appendix Table A3.4.

In a further attempt to control for eligibility and estimate program take-up by incarceration history, I have also run a set of models that restrict the sample to a subset of respondents who are more likely to be eligible for each program in each year. For EITC, this restriction is simple, as NLSY79 has already estimated eligibility for each respondent in each survey year based on his or her family size and self-reported household income. For disability insurance and SSI receipt, I drop all respondents who never report a work-limiting disability in any survey year from these models, although this restriction eliminates 20 percent of actual disability recipients and 26 percent of SSI recipients who never report a work-limiting disability to an NLSY79 interviewer.

For means-tested programs that use household-level income tests – AFDC/TANF and food stamps/SNAP – I limit inclusion in the eligibility-restricted models to respondents who reported a household income below \$40,000 in the relevant survey year. For SSI, which is determined based on individual income, I limit restrict the model

to respondents who reported \$30,000 or less in personal earned income. These income restrictions drop 5 percent of actual SSI recipients, 4 percent of food stamp/SNAP recipients, and 3 percent of AFDC/TANF recipients.

Because asset data are not collected in every survey year, I use the available asset values to multiply impute asset values for years in which these data are not collected. As a result, I cannot use asset level in the year of benefit receipt to restrict the sample for the eligibility-restricted models of means-tested benefit receipt, because many of the benefit years rely upon multiply imputed asset values.⁷ A benefit of multiple imputation is that multiple values are imputed for each missing value to better account for the error in the imputed values (relative to single imputation), but that also means that an individual's asset level in a year for which values have been imputed may be above the cutoff in some imputations and below the cutoff in others. Rather than arbitrarily deciding which imputation values to prioritize over others for the purpose of selecting an estimation sample, I instead restrict inclusion in the eligibility-based models of means-tested benefits using respondent-reported asset levels from the years in which NLSY collected asset data. Thus, I limit the models of means-tested programs (AFDC/TANF, SSI, and food stamps) to respondents who never reported more than \$20,000 in financial assets (in 2014 dollars) in any of the years in which asset data were collected. The asset restriction winds up excluding 14 percent of AFDC/TANF recipients, 16 percent of SSI recipients, and 15 percent of food stamp recipients.

Finally, for the model of unemployment insurance receipt, I drop respondents who reported working 100 percent of weeks since the last interview, having zero weeks of

⁷ NLSY79 collected asset data from 1985 to 2012, inclusive, with the exception of the 1991, 2002, 2006, and 2010 survey years.

unemployment in the last calendar year, and zero weeks out of the labor force in the last calendar year. This restriction eliminates 9 percent of actual UI recipients.

The results from the eligibility-restricted models are shown in Appendix Table A3.5. Differences in safety net benefit receipt by incarceration history are remarkably similar to those reported in the main models in Table 3.2, with the exception of SSI receipt. When I impose the disability-reporting, income limits, and asset limits described above, there is no longer a statistically significant difference in log odds of SSI receipt between formerly-incarcerated and never-incarcerated individuals. It is worth noting however, that the data individuals provide to survey interviewers may not perfectly align with the data they provide to case workers who are determining their eligibility for a benefit. Thus, 30 percent of person-years in which respondents actually received SSI income are dropped from the eligibility-restricted model by the income, asset, and disability exclusions I apply.

Finally, some readers may fear that behavioral differences unaccounted for in the preceding models could confound the relationship between incarceration history and benefit receipt. In an attempt to account for this possibility, I have run an additional series of models that incorporate controls for potentially confounding behavioral characteristics or restrict the comparison group to other respondents who displayed delinquent or criminal behavior in early adulthood but were never observed in prison or jail. I view these models as a sensitivity analysis, which allows me to compare the stability of the estimated relationship between prior incarceration and safety net benefit receipt across models as the reference group changes.

The first such set of models adds to the primary models described above controls for delinquency in late adolescence/young adulthood that serve as proxies for behavioral

characteristics that may confound the results: whether the respondent reported using any drug other than marijuana more than 2 times in the prior year, whether the respondent had ever been sentenced to time in juvenile detention, and a delinquency index z-score reflecting the frequency with which the respondent reported engaging in 20 different delinquent and illegal activities in the prior year.⁸ All of these measures were collected in 1980, when respondents were 15-23 years old. The other sensitivity analysis models use the same equations as the primary models but restrict the sample so that the comparison group is limited to respondents who: (1) reported using hard drugs in 1980, (2) had ever been charged with a crime other than a minor traffic offense in 1980, or (3) had ever been convicted of a crime other than a minor traffic offense in 1980.⁹

For the sake of parsimony, I report only the coefficients and standard errors on the *Previously Incarcerated* dummy variable across these models in Appendix Table A3.6. (Full result tables are available upon request.) Although *p*-values sometimes vary in these models because of differing sample sizes and, consequently, standard errors, the substantive findings reported above hold true across these comparison groups and with the inclusion of behavioral control variables. Even when behavioral differences are

⁸ The delinquency index score reflects respondent's reported frequency of: running away from home, skipping school, drinking alcohol underage, intentionally damaging property, fighting at school, shoplifting, stealing other's belongings, using force to take something from a person, hitting or threatening to hit someone, attacking someone with intent to hurt or kill, smoking marijuana, using any other drugs to get high, selling marijuana, selling hard drugs, conning someone, stealing a car, breaking and entering, selling or holding stolen goods, and aiding in a gambling operation. Frequency of engagement in each activity within the last year is measured as follows: 0 = Never, 1 = Once, 2 = Twice, 3 = 3 to 5 times, 4 = 6 to 10 times, 5 = 11 to 50 times, 6 = more than 50 times. Each respondent's responses to the 20 activity frequency questions is summed, and this measure is then standardized across all respondents to create a z-score with mean of 0 and standard deviation of 1.

⁹ Formerly-incarcerated respondents reported more drug use, much higher levels of delinquency, and more criminal charges and convictions in 1980 than respondents who were never interviewed in prison or jail. As a result, sample sizes drop dramatically in when I impose comparison group restrictions as a means of adjusting for these underlying group differences.

accounted for in the ways described above, prior incarceration is associated with significantly higher log odds of means-tested public assistance receipt, lower log odds of social insurance benefit receipt, and no significant difference in the log odds of EITC receipt. Furthermore, the racial differences described in Table 3.5 also hold up when behavioral controls are added and when the comparison group is restricted as described above.

Discussion

Despite the compelling theoretical arguments supporting the system avoidance hypothesis, I find no evidence of system avoidance behaviors among the formerly incarcerated – individuals who have experienced the most extreme form of criminal justice system contact considered by Brayne, Lerman, and Weaver – when the system under consideration is the American social safety net. Instead, I find evidence of *assistance-seeking* behavior, even from heavily surveilling programs like AFDC/TANF and SSI, among the formerly incarcerated. However, this assistance-seeking behavior is limited to whites, which raises the question of why formerly-incarcerated whites disproportionately take advantage of this benefit of citizenship. This is not a question that can be answered with these data but may be one worth pursuing in qualitative work.

Additionally, given that prior incarceration does not appear to affect public assistance usage among blacks, conditional on eligibility-related characteristics, it would be interesting for future work to explore why. Is this indicative of more general system avoidance and distrust of these programs among black Americans in general, regardless of criminal justice system contact? Or do formerly-incarcerated blacks tend to return to households that were already using public assistance to begin with, such that avoiding this particular system seems less imperative?

Moreover, this finding that formerly-incarcerated whites appear to engage in assistance-seeking behavior while formerly-incarcerated black individuals do not also raises the question of whether the negative effects of criminal justice contact on political participation and trust in government that Weaver and Lerman (2010) observe – and perhaps even the system avoidance behaviors that Brayne (2014) observed in non-benefit contexts – might be driven primarily by black respondents, who make up a disproportionately large share of those who have had criminal justice system contact. It may be worth replicating these authors’ work and allowing for race interactions to test this possibility, given the stark racial differences in assistance-seeking behavior that I observe among formerly-incarcerated individuals in the NLSY79 data. If my findings replicate in these other datasets, it would be well worth investigating how and why it is that whites’ interactions with other institutions following release from criminal justice institutions differ so starkly from those of formerly-incarcerated black Americans.

While my results suggest that incarceration might actually spur participation in means-tested public assistance programs for whites, I also find that participation in these programs declined, not just for formerly-incarcerated individuals but for everyone, following the 1996 reforms to AFDC/TANF, SSI and SNAP.¹⁰ Finally, I find evidence that social insurance programs fail to serve formerly-incarcerated individuals in the same way that they serve never-incarcerated individuals. Regardless of race, formerly-incarcerated individuals are not benefiting from social insurance programs in the same

¹⁰ In future versions of this paper, I plan to incorporate analyses of the NLSY97 cohort, which came of age entirely after the implementation of these 1996 reforms. This will allow me to make cohort comparisons of benefit receipt during early adulthood (18 to 36) for young adults who came of age during the ramp up in incarceration rates in the 1980s (NLSY79 cohort) compared to young adults who came of age as the American carceral system was reaching its maximum size in the early 2000s (NLSY97 cohort). Thus, with these two cohorts, I should be able to start disentangling age versus year effects.

way that never-incarcerated individuals are, even after observed differences in labor market participation and disability are taken into account.

It is important to note that these findings do not reflect the full causal effect of incarceration on safety net benefit receipt, because I control for differences in work history and other eligibility-related covariates, like family size, that are themselves likely to be directly affected by incarceration. As a result, the models could more accurately be viewed as estimating the effect of having formerly been incarcerated (i.e., being marked as a convicted criminal) on probability of benefit receipt *if* we set aside the indirect effect of incarceration via employment levels – that is, if we assume equal employment, marital status, assets, etc. Prior research makes clear that incarceration does indeed appear to affect all of these characteristics, therefore the coefficients reported above only speak to the direct effect of being formerly incarcerated, rather than the full effect (both direct and indirect) of incarceration on benefit receipt. An interesting future direction would be to disaggregate direct and indirect effects of incarceration on safety net benefit receipt. Because the incarceration data in NLSY79 are imprecise, it may not be the best dataset to use for such an analysis, however, the NLSY 1997 cohort survey contains much more detailed data on criminal justice contact, so it may be a promising dataset with which to pursue such an analysis.

Limitations

As with any survey data, the NSLY79 data used for these analyses are likely to suffer from some degree of reporting error. In particular, individuals may not recall with great accuracy the exact amount of income they received from various sources in the past year. However, to the extent that individuals recall receiving any income from each of these safety net programs – which is probably particularly likely with regard to benefits,

like AFDC/TANF and disability, that are more burdensome to apply for – the results are unlikely to be affected by recollection bias.

It is also possible that social desirability bias may come into play with regard to reporting receipt of sometimes stigmatized public assistance benefits – particularly AFDC/TANF during the timeframe over which these data were collected – such that some individuals may not acknowledge receipt of public assistance benefits to avoid embarrassment. Hopefully the fact that the NLSY surveys have collected so much data about so many topics in the respondents' lives, ranging from relationship formation and dissolution to health conditions, at each survey wave will reduce the likelihood that respondents become embarrassed about reporting public assistance income in particular. But, to the extent that social desirability bias may come into play, it would only prove problematic for my results if the extent of such bias differed dramatically between formerly-incarcerated and never-incarcerated individuals.

It is possible that the assistance-seeking behavior among formerly-incarcerated whites could be an artifact of social desirability bias among never-incarcerated white respondents who are less willing to admit receiving public assistance benefits. Perhaps white respondents who have already been interviewed while incarcerated would not feel the same level of embarrassment about admitting such receipt, however, and so their receipt of such benefits appears to be higher than that of never-incarcerated whites when it does not truly in fact differ. This possibility would be difficult to assess in these data but may be possible with an alternate dataset that uses a more private, self-administered survey instrument to collect data on public assistance receipt.

Because survey enumerators mark the type of residence – which may include jail or prison – at each survey wave, there should be no error with regard to classification of

respondents marked as formerly incarcerated in these data. However, it is likely that some respondents who served relatively short sentences are misidentified as never incarcerated in my analyses because they were never observed in a correctional facility at any survey wave. Because these respondents would be inappropriately grouped with the never-incarcerated respondents, this potential source of error would bias results toward zero, making coefficients smaller than they truly ought to be.

Another limitation of the current analysis is that eligibility criteria (e.g., asset limits and restrictions on individuals with drug convictions) and benefit levels for these programs vary year to year and, for some, state to state. For federally-operated programs (i.e., SSI, disability, EITC), eligibility criteria and benefit levels, conditional on eligibility and means, are consistent across states within years. Therefore, the inclusion of year fixed effects in the main models should account for any year-to-year variation in the benefit cap or eligibility criteria for these programs. For state-operated programs (e.g., TANF, SNAP, unemployment insurance), the year fixed effects will only pick up national-level differences in federal guidelines, with regard to TANF and SNAP, and economic conditions, with regard to unemployment insurance. Therefore, I plan to use state-year eligibility guidelines compiled in the Urban Institute's TRIM3 simulation database to more accurately calculate each respondent's year-by-year eligibility for each of these programs in a future version of this paper. Doing so will allow me to predict benefit receipt conditional on simulated eligibility, rather than conditional on eligibility-related covariates, as I have done in this chapter.

Conclusion

Formerly-incarcerated individuals are often marginalized and shut out of the full rights of citizenship (National Research Council 2014c; Uggen et al. 2006; Wakefield and

Uggen 2010), but my findings suggest that they are not wholly shut out of the American social safety net. I find that formerly-incarcerated individuals are no less likely to participate in means-tested public assistance programs, conditional on eligibility-related criteria, than never-incarcerated individuals, and formerly-incarcerated whites are even more likely than their never-incarcerated counterparts to participate in public assistance programs. However, the findings also indicate that formerly-incarcerated individuals may be significantly hindered in their ability to draw on social insurance programs, though results from the individual fixed effects model suggest this difference may be attributable to unobserved differences in the types of individuals who experience incarceration, perhaps reflecting their lower probability of employment in the formal labor market even prior to incarceration.

I also find that the 1996 reforms to welfare cash assistance (AFDC/TANF) and food stamps (SNAP), which imposed work requirements and eligibility restrictions for individuals convicted of felony drug offenses, appear to have reduced receipt of benefits from these programs not just for formerly-incarcerated individuals but for everyone. Thus, a preexisting problem with the safety net's ability to aid formerly-incarcerated Americans – i.e., social insurance programs are primarily useful for those with stable employment histories in the formal labor market, which formerly-incarcerated individuals often lack – appears to have been exacerbated by the passage of PRWORA. Moreover, current proposals to add work requirements for Medicaid eligibility (Williams 2018), could mean that formerly-incarcerated individuals will be increasingly shut out of a work-based social safety net. As the next chapter reveals, this cohort of formerly-incarcerated individuals already finds itself in extremely precarious financial conditions. Without significant reforms to safety net program eligibility and, perhaps, anti-

discrimination employment law, it is difficult to see how this pattern can be shifted going forward.

In the next chapter, I more fully consider the financial stability and trajectories of these formerly-incarcerated NLSY79 respondents, assessing among other things how much transfer income, like that received from the programs examined in this chapter, contributes to their total income packages.

4. Total Income Trajectories over the Life Course

Post-Incarceration

For criminologists, the life course is strongly normalizing. As individuals age and attain the traditional markers of adulthood (i.e., steady work, a stable union), they are drawn into predictable, prosocial roles. The web of informal social control that comes along with these roles encourages desistance from crime for offenders, most of whom can be expected to achieve some level of social and economic security (Laub and Sampson 2001, 2006; Sampson and Laub 1993).

While involvement in crime declines with age, it is unclear whether the consequences of criminal sanctions similarly recede. The stigma and disruption caused by incarceration may, like crime, eventually give way to the normalizing force of the life course. But prior research demonstrates that achievement of the stable, prosocial roles that could act as turning points for criminally-involved individuals (i.e., employee, spouse) is made less likely by incarceration (Apel and Sweeten 2010; Huebner 2005; Lopoo and Western 2005). Thus, incarceration may directly prevent or delay individuals from attaining the prosocial roles that traditionally mark passage through adulthood, precluding formerly-incarcerated individuals from ever rejoining their peers on the normal life course trajectory.

This dissertation has focused on the socioeconomic consequences of incarceration and other criminal justice involvement. In this chapter, I consider long-term income trajectories in a cohort of formerly-incarcerated men, examining how their income trajectories compare to those of their never-incarcerated peers and whether these formerly-incarcerated men are able to eventually attain normal-looking income packages. The key hypothesis is that the income path for men who have been incarcerated is enduringly different from that of observably similar men who have not been incarcerated.

Background

A diverse literature has identified incarceration as an event that leads to a variety of social disruptions in the lives of those who experience it. Prior research links incarceration to poor health outcomes, relationship dissolution, and, especially, employment difficulties, among other disadvantages (National Research Council 2014c). All of these consequences of incarceration are likely to, in turn, affect overall income and financial wellbeing over the life course, but the previous research literature has only examined the financial stability of formerly-incarcerated individuals either over a short time period or with limited indicators of economic wellbeing. Several studies of recently-released prisoners have examined financial wellbeing from a holistic perspective – examining earned income, as well as public benefits receipt and support from family – but these studies only follow former prisoners over a relatively short time span, one to three years, following release (Harding et al. 2014; Western et al. 2015). To my knowledge, studies using longitudinal data to examine financial circumstances of former prisoners have only examined earned income (Western 2002) or, more recently, asset ownership (Turney and Schneider 2016).

Thus, little is known about sources of income beyond personal earnings, how income levels and patterns change over the years following incarceration, and how long the income shocks of incarceration last. This chapter uses National Longitudinal Survey of Youth 1979 (NLSY79) data to provide a descriptive analysis of the financial wellbeing of formerly-incarcerated men as they navigate the remainder of their lives. I consider both the typical income trajectories of formerly-incarcerated men through their mid-50s, as well as variation within this group. In particular, I use cluster analysis to identify common life course income trajectory patterns among this cohort of formerly-incarcerated men and investigate what characteristics help to predict a traditional-looking income trajectory.

Incarceration and Employment

Many researchers have examined the effect of incarceration on subsequent employment and earnings, typically finding that employment levels and earnings drop after incarceration (Holzer 2009; National Research Council 2014a; Western et al. 2001). Lower employment appears to be the result of both labor market discrimination by employers (Holzer et al. 2007; Pager et al. 2009; Uggen et al. 2014), as well as discouragement and lower labor force participation by formerly-incarcerated individuals (Apel and Sweeten 2010).

In addition to lower levels of employment, prior research also suggests that earnings drop as a result of lower wages following incarceration (Apel and Sweeten 2010; Western 2002). All of this evidence, thus, points toward lower expected earned income following incarceration. However, less stable employment among the formerly incarcerated may also mean that this population has less access to transfer income from social insurance programs, like disability and unemployment insurance. Alternatively,

lower earned income may allow formerly-incarcerated men to meet eligibility requirements for means-tested public assistance programs more often than their never-incarcerated counterparts. This chapter will allow for all of these possibilities by examining the total income packages, including transfer income, of formerly-incarcerated men. Thus, by examining all sources of income, not just earned income, this analysis will provide a fuller picture than prior studies of how the various collateral effects of incarceration add up to shape the total financial wellbeing of formerly-incarcerated men.

Incarceration and Relationships

Previous studies using the NLSY79 have found that incarceration is associated with diminished probability of marriage, as well as higher rates of separation or divorce (Huebner 2005, 2007; Lopoo and Western 2005). Moreover, ethnographic work by Goffman (2009) highlights how involvement with the criminal justice system may promote unpredictable behavior among young men, which can destabilize their romantic relationships. Thus, lower probability of partnership and marriage suggests that formerly-incarcerated men may fare worse than their never-incarcerated counterparts not just in terms of own earned income, but also in terms of their ability to access spouse or partner income. Moreover, given that strong, stable romantic partnerships encourage desistance from crime (Laub and Sampson 2001), the detrimental effects of criminal justice system involvement on the maintenance and formation of such relationships may further diminish formerly-incarcerated men's total income by preventing them from experiencing the sort of turning points that could put them on a path toward more stable, higher income employment (Laub and Sampson 1993).

Incarceration and Health

Another sizeable stream of research on the effects of incarceration focuses on the health of formerly-incarcerated individuals. This research literature demonstrates both the higher incidence of health problems among this population prior to incarceration, as well as increased health problems following incarceration (National Research Council 2014b). The studies on post-incarceration health find that prior incarceration is associated with both poorer mental health (Schnittker et al. 2012; Turney et al. 2012) and diminished physical health (Massoglia 2008a, 2008b; Schnittker and John 2007).

Poorer health among formerly-incarcerated individuals may, thus, translate into lower earnings via lessened labor market participation as a result of greater prevalence of disability. However, it is also worth noting that higher incidence of health problems and, potentially, disability could mean that formerly-incarcerated men are more likely to qualify for public assistance programs, like Supplemental Security Income, that are targeted towards people with work-limiting disabilities. As a result, differences in health by incarceration history could allow formerly-incarcerated men greater access to transfer income than the general population, which could offset some of the decreases in earned income that generally follow incarceration.

Chapter Plan

The rest of this chapter will explore how the various consequences of incarceration add up by examining total income through midlife following release. Like Harding et al. (2014) and Western et al. (2015), I consider additional sources of income beyond only earned income in order to provide fuller picture of financial stability, but by utilizing longitudinal data from the NLSY79, I am able to examine income and economic

wellbeing over a much longer post-release time period than these prior studies of stability in the few years following release.

First, I describe the NLSY79 data and the sources of income I examine in greater detail. Then I will describe how income levels and composition of total income packages vary across time, across total income quartiles, and by incarceration history. Next, I explore the extent to which incarceration marks an income shock for formerly-incarcerated men as opposed to a continuation of already lower income trajectories prior to incarceration. I then examine how long it takes for these income shocks to wear off across different types of income. Finally, I consider variation in income trajectories within the formerly incarcerated population, using cluster analysis to identify different income trajectory patterns within this group. I then use multinomial logistic regression to identify the characteristics that best predict the type of trajectory former prisoners experience, including which formerly-incarcerated men are most likely to attain stable total income packages and which continue to struggle in the decades after incarceration. I conclude with a discussion of the implications of these descriptive findings for policy and for how both scholars and practitioners think about “reentry” and social reintegration following incarceration.

Data

This chapter uses data from the National Longitudinal Survey of Youth 1979 (NLSY79) cohort, members of which were 50-58 years old in 2014, the most recently released survey year. The 12,868 members of the NLSY79 sample have reported detailed data on employment, income, educational attainment, family formation, health, and more since they were first interviewed in 1979 at ages 14-22. Respondents were interviewed annually from 1979 through 1994 and have been interviewed biennially

since. The response rate in 2014 was 77 percent, with over half (52 percent) of surviving sample members having completed all 26 survey rounds (Bureau of Labor Statistics n.d.).

At each survey, NLSY79 records the respondent's current residence type, including residence in correctional facilities. I use this question to identify respondents who will ever be observed incarcerated in any wave, who are currently incarcerated, and who have previously been observed in prison or jail. I also create an approximate measure of time since release based on the number of years since the respondent was last interviewed in a prison or jail facility. While respondents who served time between survey waves cannot be identified using the current residence type question, Western (2002) finds that incarceration rates created from the NLSY79 closely match national imprisonment rates for young men from the Bureau of Justice Statistics. Moreover, given the dearth of survey data that more accurately capture criminal justice contact *and* observe respondents both prior to and after such contact, I consider this an acceptable shortcoming of the NLSY79, particularly in light of its long window of observation and nationally-representative sample.

As in the population as a whole, the male incarceration rate among NLSY79 respondents is far greater than the female incarceration rate. By 2014, approximately 11 percent of male NLSY79 respondents were ever interviewed in prison or jail, compared to just 1 percent of female respondents. (See Figure A4.1 in the Appendix.) Because there are so few identifiable formerly-incarcerated women in the NLSY79, I restrict the following analyses to male respondents only.

I consider four types of income: personal earned income, spouse/partner income, transfer income, and other income. *Personal earned income* is a combination of

respondent's self-reported income from wages, salary, and tips in the past calendar year, as well as their earnings from any farms or businesses that they own. *Spouse/partner income* includes wage and salary income, farm and business earnings, unemployment insurance income, and the value of any child support benefits received by the respondent's spouse or cohabiting partner in the prior calendar year. *Transfer income* reflects income from all government transfers, including cash assistance, food stamps, veteran's benefits, disability, Supplemental Security Income, respondent unemployment benefits, respondent child support and alimony benefits, and the Earned Income Tax Credit. Finally, *other income*, captures inheritances and cash gifts from family, as well as miscellaneous income reported in the "other" category by respondents. I have adjusted all values to 2014 dollars using the Consumer Price Index.

Below, I present figures displaying average income levels across type, age, and incarceration history, as well as composition of total income packages by total income quartile and incarceration history. Next, in order to better understand how income patterns differ both before and after incarceration for men who will ever be incarcerated relative to never-incarcerated men, I use regression and individual fixed effect models of log income and income type as a share of total income to provide descriptive differences based on incarceration history for each of the four income categories outlined above.¹ Because both exposure to incarceration (Pettit and Western 2004) and earnings (Grotsky and Pager 2001) vary greatly by race, I next present results from regression models that adjust for race. Race is captured by a series of dummy variables (white,

¹ One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income. Results are substantively similar if zeros are instead replaced with 100.

black, Hispanic, and other²), with white as the reference category. These models should provide some insight into how average income gaps might change if we could equalize racial differences between the ever incarcerated and the never incarcerated.

As I note above, incarceration affects employment levels, relationships, and health, all of which are likely to in turn affect income levels. But individuals who will eventually be incarcerated also tend to have lower levels of employment, lower probability of marriage, and worse health, on average, than never-incarcerated individuals even before they are incarcerated. (See the coefficients on *will ever be incarcerated* in Appendix Tables A4.1-A4.3.)³ Therefore, I also run a set of regression models that accounts for these important mechanisms by adjusting for differences in employment, marital status, and disability, as these are primary channels through which incarceration is likely to affect income. Thus, this model should provide a lower-bound on the estimated relationship between incarceration and subsequent income if we could equalize these important mediating characteristics. I also account for education level in this model, as education is an important dimension along which exposure to incarceration varies and is strongly related to earnings (Bureau of Labor Statistics 2017; Pettit and Western 2004).

² The “other” race category includes respondents who did not self-identify as either Hispanic, non-Hispanic black, or non-Hispanic white. Non-Hispanic white is the reference category.

³ Appendix tables A4.1 displays results from regression and individual fixed effects models predicting number of weeks worked in the past calendar year. Tables A4.2 and 4.3 display log odds from logit and fixed effects models predicting marriage and disability. These models reveal that, relative to never-incarcerated men, male NLSY79 cohort members who will ever be observed incarcerated have significantly lower employment and log odds of marriage, as well as significantly higher log odds of disability, prior to incarceration, and that these disparities increase following incarceration. These patterns hold true even after adjusting for differential exposure to incarceration and employment levels by race and education and inclusion of individual fixed effects.

I account for employment levels with a continuous measure of number of weeks worked in the past calendar year. Marital status is captured by a time-varying indicator variable set equal to one if the respondent reported being married at each survey date. Disability status is measured with a dummy variable set equal to one if the respondent indicated that he had a disability that either prevented him from working or limited the type of work he could reasonably do at each survey date. Education is measured as a time-varying four-category ordinal variable reflecting attainment at of each survey date: less than high school, a high school diploma or GED, some college, or a four-year college degree or more, with high school completion as the reference category.

Findings

Table 4.1 displays descriptive statistics by respondent incarceration history. In keeping with findings that minority and less educated men are disproportionately exposed to the criminal justice system and incarceration in particular (Pew Charitable Trusts 2010), ever-incarcerated and formerly-incarcerated respondents have much lower levels of educational attainment (the vast majority have no more than high school or a GED) than never-incarcerated men (54 percent of whom attained more than a high school diploma or GED by 2014). They are also disproportionately nonwhite (62 percent compared to just 33 percent of never-incarcerated respondents). Table 4.1 also displays differences in labor force attachment, marital status, and health in keeping with those described above among former prisoners. Both prior to and following incarceration, ever-incarcerated NLSY79 respondents report lower marriage rates, fewer weeks worked in the past calendar year and, especially after incarceration, higher disability rates, with 44 percent of formerly-incarcerated men in 2014 self-identifying as having an employment-limiting disability.

Table 4.1. Descriptive Statistics by Criminal Justice Contact

Person-Year Level	Never Incarcerated	Not Yet Incarcerated	Previously Incarcerated
Highest degree completed			
None	18.8%	59.7%	38.7%
High school diploma or GED	39.3%	31.6%	46.7%
Some college/Associate's	21.9%	8.4%	13.9%
Graduate or professional degree	5.5%	0.0%	0.2%
Married	46.4%	19.1%	21.3%
Disabled	6.3%	7.5%	19.4%
Weeks worked in past calendar year	40.6	27.9	24.4
Total Income -- <i>mean</i>	\$58,433	\$19,137	\$21,034
<i>median</i>	\$43,197	\$12,064	\$9,212
Personal Income -- <i>mean</i>	\$44,135	\$15,274	\$15,121
<i>median</i>	\$34,091	\$8,639	\$3,279
Spouse Income, if married -- <i>mean</i>	\$23,118	\$9,227	\$14,177
<i>median</i>	\$17,241	\$2,011	\$548
Transfer Income -- <i>mean</i>	\$690	\$863	\$1,394
<i>median</i>	\$0	\$0	\$0
Other Income -- <i>mean</i>	\$2,253	\$421	\$787
<i>median</i>	\$0	\$0	\$0
	<i>Person-years</i>		
	109,968	5,124	7,921
Person Level	Never Incarcerated	Ever Incarcerated	
Race/Ethnicity			
White	66.6%	38.2%	
Black	12.0%	38.4%	
Hispanic	6.0%	10.3%	
Other	15.4%	13.1%	
Highest degree completed (2014)			
None	6.6%	22.7%	
High school diploma or GED	39.5%	56.7%	
Some college/Associate's	23.0%	18.2%	
Bachelor's	20.0%	2.1%	
Graduate or professional degree	10.9%	0.3%	
Married (2014)	65.2%	22.6%	
Disabled (2014)	17.5%	44.4%	
Weeks worked in past calendar year (2014)	43.0	22.1	
Total Income (2014) -- <i>mean</i>	\$106,169	\$21,760	
<i>median</i>	\$73,171	\$9,329	
Personal Income (2014) -- <i>mean</i>	\$75,944	\$14,457	
<i>median</i>	\$50,813	\$0	
Spouse Income, if married (2014) -- <i>mean</i>	\$31,269	\$18,159	
<i>median</i>	\$21,341	\$2,744	
Transfer Income (2014) -- <i>mean</i>	\$836	\$1,919	
<i>median</i>	\$0	\$0	
Other Income (2014) -- <i>mean</i>	\$9,062	\$1,290	
<i>median</i>	\$0	\$0	
	<i>N</i>		
	5,735	616	

Note: Weighted values. Men only. All dollar value variables have been adjusted for inflation to 2014 values.

Table 4.1 also displays stark differences in both mean and median income values by incarceration history. Both across person-years and in 2014, ever-incarcerated and formerly-incarcerated respondents report mean total income levels 65 to 80 percent below those of never-incarcerated respondents. The total income gap is even greater at the median and later in life, with formerly-incarcerated respondents reporting median total income of just \$9,300 in 2014, compared to \$73,000 among the never incarcerated. The same trends hold true with regard to personal income, which makes up the bulk of total income. While spouse/partner income is lower among the ever incarcerated than among the never incarcerated, the gap is smaller at the mean, with ever-incarcerated respondents reporting spouse income 40 to 60 percent that of never-incarcerated respondents. The spouse/partner income gap is particularly great at the median, however: married formerly-incarcerated respondents reporting a median spouse income of just \$550 compared to \$17,000 among never-incarcerated married respondents.

Where ever-incarcerated respondents appear to have an edge is in transfer income. Formerly- and ever-incarcerated NLSY79 respondents report much higher levels of transfer income receipt than never-incarcerated respondents both across years and in 2014. Transfer income levels are low, however, with ever-incarcerated respondents reporting \$1,900, on average, in transfer income in 2014 compared to \$840 among the never incarcerated. It is worth noting that even among the formerly incarcerated fewer than half receive *any* income from transfer programs in any given year (the median amount of transfer income is \$0 across all groups). Finally, ever- and formerly-incarcerated respondents are also disadvantaged in “other” income levels relative to never-incarcerated respondents. But, as with transfer income, the value of other income

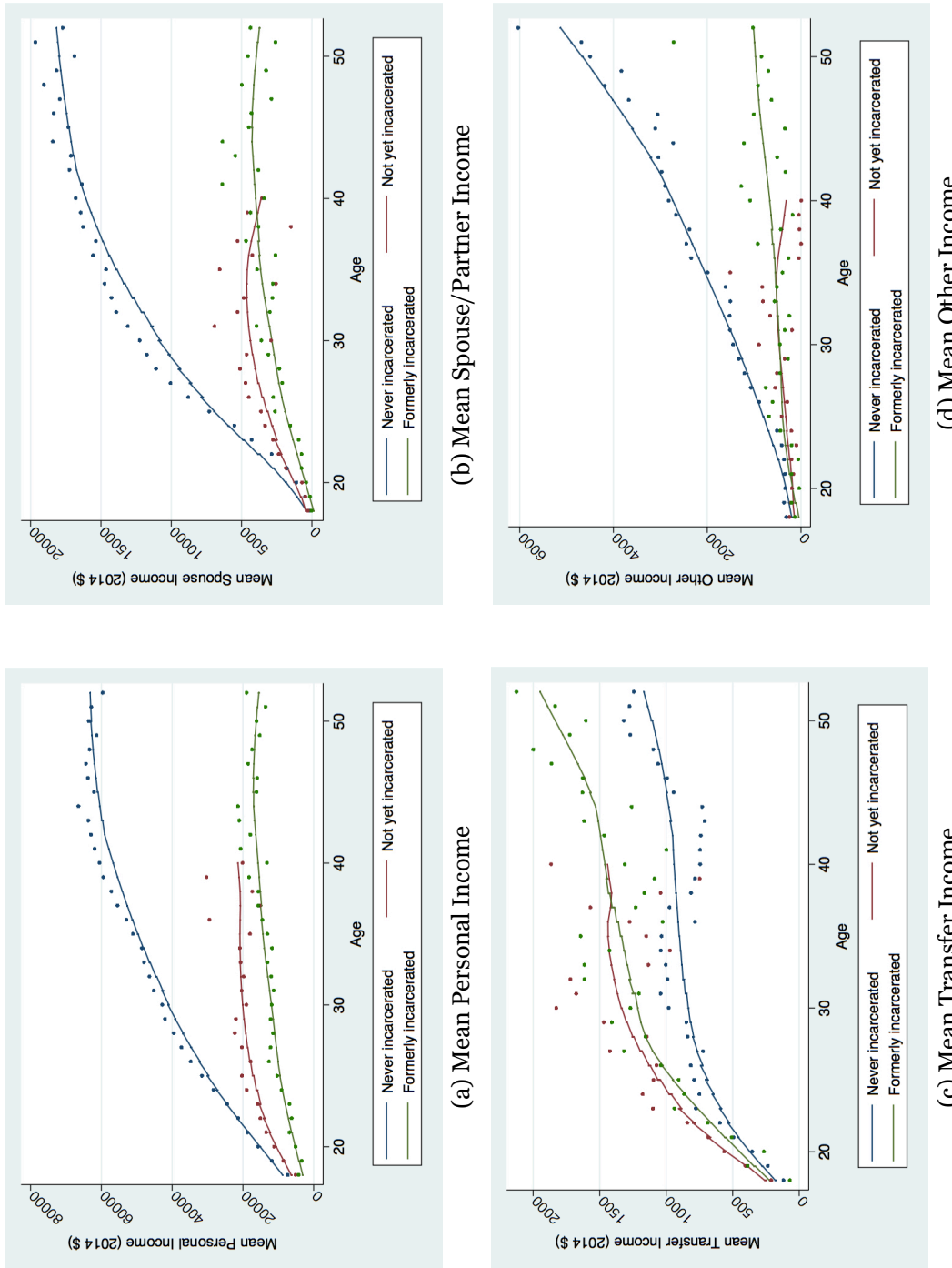
is small compared to that of personal and spouse/partner income, and the median amount received in any year is \$0 for all groups.

Table 4.1 provides a good overview of the general differences between the never-incarcerated and ever-incarcerated men in the NLSY79, but it does not provide a sense of what income trajectories look like for these groups across the life course. Therefore, Figure 4.1 displays mean income levels by age and incarceration history for the four types of income described above. While levels of personal earned income, spouse/partner income, and other income vary widely, the patterns by incarceration history and age are similar: all respondents start at a similar level in their late teens and early 20s, but income levels grow dramatically through the following decades for never-incarcerated respondents (before levelling out at about age 40 for earned income and spouse income), while income growth is smaller and then flat after about age 30 for respondents who will eventually be incarcerated or already have been incarcerated.

While average personal income among never-incarcerated respondents grows to over \$60,000 in the 40s and 50s, earned income tops out at only about \$20,000 among both formerly- and not-yet-incarcerated respondents.⁴ Likewise, average spouse income tops out at about \$5,000 per year among ever-incarcerated respondents, but reaches more than three times as much among never-incarcerated respondents in their 40s and 50s. Respondents who have been or ever will be incarcerated do report higher average transfer income than never-incarcerated respondents throughout their lives after age 20, and the gap appears to grow with age, but it is much smaller than the gap in other types of income. Moreover, the level of income received from transfers is far below that of any

⁴ Average income levels are not displayed after age 40 for the not-yet-incarcerated group, as only 6 percent of NLSY79 male respondents who will ever be incarcerated are first observed in prison or jail after age 40.

Figure 4.1. Mean Income Across the Life Course by Type and Incarceration History



Note: Dots display average income at each age for each group. Lines are lowess-smoothed. Average income levels are only displayed through age 40 for the not-yet-incarcerated group because only 6 percent of ever-incarcerated respondents are first observed in prison or jail after age 40.

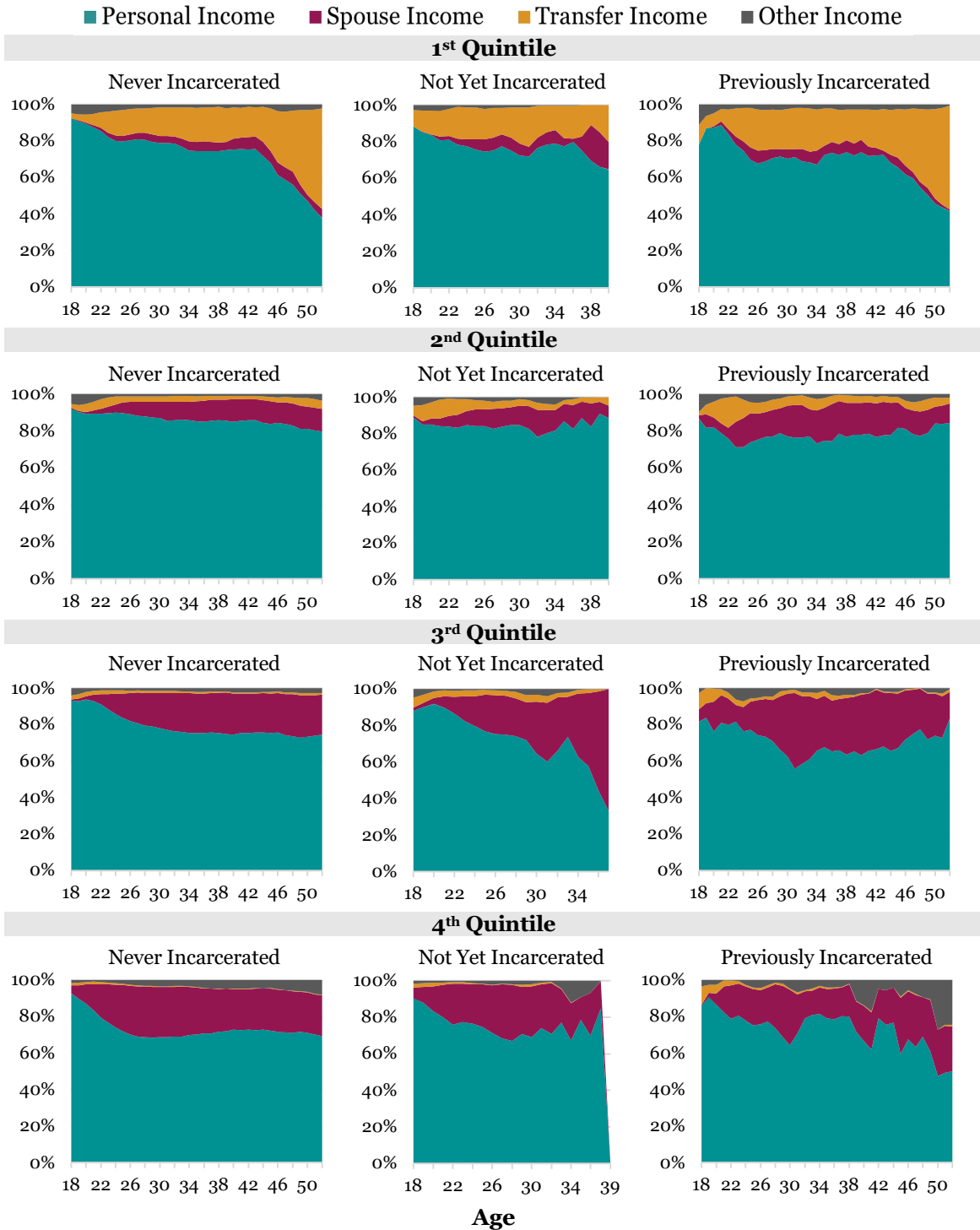
other type of income – even at the peak in the late 50s, formerly-incarcerated respondents receive only about \$2,000 annually in transfer income compared to about \$1,000 among never-incarcerated respondents, which is far less than the average gap in other sources of income at this age.

Given that average income levels are much lower among the formerly or ever incarcerated, Figure 4.2 displays the share of total income comprised by each of these four income sources within income quartiles. This presentation should make it easier to see whether there are stark differences in composition of total income packages based on incarceration history once we restrict comparison to other respondents with similar total income levels. Note that, because only 6 percent of NLSY79 male respondents who will ever be incarcerated are first incarcerated after age 40, the “not yet incarcerated” panels in the middle of Figure 4.2 only extend to age 40.

Across total income quintiles and incarceration history, earned income represents the lion’s share of total income. Spouse income, on the other hand, appears to contribute proportionately more to total income at higher total income levels, while transfer income makes up a sizeable share of total income among bottom quartile members, particularly in the late 40s and early 50s. Both of these patterns hold true regardless of incarceration history. It appears that spouse’s income may comprise a slightly larger share of total income among the formerly incarcerated in the second and third quintiles, but the differences are not stark.

Thus, based on Figures 4.1 and 4.2, it appears that the primary difference in the total income packages of never-incarcerated men compared to those of men who will be or have already been incarcerated is in *level* of income, rather than composition of income. To more clearly test whether this is true, I run OLS regression models predicting

Figure 4.2. Sources of Income by Total Income Quintile, Incarceration History, and Age



Note: Graphs display 3-year moving averages of income by age. Income levels only displayed through age 40 for the not-yet-incarcerated group because only 6 percent of ever-incarcerated respondents are first observed in prison or jail after age 40.

log of income and share of total income for each of the four income components. These models account for incarceration history with a dummy variable set equal to one to identify respondents who will ever be observed in prison or jail – testing whether baseline differences in income levels exist even prior to incarceration – and two additional dummy variables that identify person-years in which respondents are currently incarcerated and those following incarceration, so that we can see the extent to which income falls (or potentially rises) *after* incarceration.

Tables 4.2-4.5 present results from these models. In each table, the first column represents the baseline difference in log income between these four groups. The coefficient on *will ever be incarcerated* represents the average difference in log income between not-yet-incarcerated and never-incarcerated respondents (the reference category), conditional on age. Average log income differences between not-yet-incarcerated and previously-incarcerated respondents are reflected by the *previously incarcerated* coefficients. Thus, the total income difference between never-incarcerated and previously-incarcerated respondents is the sum of the *will ever be incarcerated* and *previously incarcerated* coefficients. The *currently incarcerated* coefficient represents average income difference between currently-incarcerated and not-yet-incarcerated respondents.

The second column in Tables 4.2-4.5 displays differences in log income between these groups after differences in race and ethnicity are controlled for, and the third column displays remaining differences when the relevant mediating characteristics described above (i.e., education, marital status, disability status, weeks worked in past

Table 4.2. Personal Income Before and After Incarceration

	Log Personal Income			Personal Income as a Share of Total Income		
	OLS	OLS	Fixed Effects	OLS	OLS	Fixed Effects
Will ever be incarcerated	-1.336*** (0.108)	-0.963*** (0.109)	-0.425*** (0.080)	-0.028** (0.009)	-0.033*** (0.009)	-0.012 (0.008)
Previously incarcerated	-1.849*** (0.152)	-1.893*** (0.151)	-0.821*** (0.096)	-0.055*** (0.014)	-0.054*** (0.014)	-0.040*** (0.006)
Currently incarcerated	-2.363*** (0.158)	-2.288*** (0.157)	-1.460*** (0.107)	-0.021 (0.017)	-0.021 (0.017)	-0.004 (0.008)
Race/ethnicity						
Black		-1.204*** (0.066)	-0.658*** (0.047)		0.013** (0.005)	0.036*** (0.004)
Hispanic		-0.625*** (0.069)	-0.295*** (0.046)		0.013* (0.005)	0.021*** (0.005)
Other		-0.367*** (0.075)	-0.166** (0.055)		-0.017** (0.006)	-0.010 (0.006)
Highest degree completed						
High school diploma or GED		0.847*** (0.047)	0.848*** (0.054)		0.028*** (0.005)	0.013*** (0.004)
Some college/Associate's		0.848*** (0.054)	0.848*** (0.054)		0.023*** (0.005)	0.003 (0.005)
Bachelor's		1.174*** (0.061)	1.174*** (0.061)		0.009 (0.006)	-0.005 (0.006)
Weeks worked in past year		0.082*** (0.001)	0.082*** (0.001)		0.005*** (0.000)	0.005*** (4.99e-05)
Disabled		-2.164*** (0.070)	-2.164*** (0.070)		-0.209*** (0.007)	-0.152*** (0.003)
Constant	3.376*** (0.152)	3.769*** (0.151)	3.609*** (0.149)	1.00*** (0.002)	1.00*** (0.003)	1.001*** (0.016)
Observations (<i>person-years</i>)	122,831	122,831	122,831	113,130	113,130	113,130
Respondents		6,351	6,351			6,340

*** p<0.001, ** p<0.01, * p<0.05

Note: Age fixed effects included in all models. Standard errors are clustered at the individual level. One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income.

year) are taken into account.⁵ The fourth and fifth columns display coefficients from individual fixed effect models that compare income levels within individual before and after incarceration; mediators are included in the fifth column. I employ the same modelling approach in the second set of columns, which display results from regressions predicting share of total income comprised by each income source. All models include age dummy variables and, with the exception of the individual fixed effect models, clustered standard errors to account for repeated observation of individual respondents.⁶

The values displayed in Table 4.2 demonstrate that men who will be or who have ever been incarcerated have lower personal income, on average, than never-incarcerated men, and these average personal differences are not entirely attributable to differences in race, education, weeks worked, and disability status. Differences in these characteristics do appear to explain about half of the baseline log personal income gap between not-yet-incarcerated and never-incarcerated men – the *will ever be incarcerated* coefficient in column one indicates that men who will ever be incarcerated earn 74 percent less, on average, than never-incarcerated men, but once covariates are held constant, the size of this gap drops to 35 percent. Race accounts for part of the difference in log personal income between not-yet-incarcerated and never-incarcerated men, but only reduces the income gap to 62 percent (column two).

⁵ Only relevant mediators are controlled. Thus, the personal income model includes weeks worked in past calendar year and disability status, but not marital status. The spouse/partner income model includes only marital status. Because many of the transfer income questions ask jointly about receipt of benefits by the respondent and/or his partner, I control for marital status in transfer income models, in addition to weeks worked and disability status. Because the sources of other income are unclear, I control for weeks worked, disability, and marital status. All models that include mediators also include educational attainment as a covariate.

⁶ Coefficients are nearly identical when age dummies are replaced with a quadratic measure of age.

Furthermore, the coefficients on *previously incarcerated* indicate that men who will ever be incarcerated see their earnings further reduced by more than 80 percent following incarceration (84 percent in the column one pooled estimate, 88 percent in the column four fixed effect estimate). The mediating characteristics help account for some of this gap, but personal earned income is still about 56 to 67 percent lower following incarceration for men who will ever be incarcerated even when post-incarceration differences in employment and disability are taken into account.

The second set of columns in Table 4.2, displaying results from models predicting the share of total income comprised by personal income, indicate that income composition does vary significantly, if not drastically, by incarceration history. Personal income makes up a significantly smaller share of total income both prior to and following incarceration for ever-incarcerated men relative to never-incarcerated men. The pooled model (column six) indicates that earned income comprises about 8 percentage points less of total income while the fixed effects model (column nine) indicates that earned income makes up 4 percentage points less of total income for previously-incarcerated men relative to never-incarcerated men. Conditioning on race does little to explain the difference in personal income as a share of total income between ever-incarcerated men and never-incarcerated men (column seven), but differences in the relative size of this income component both before and after incarceration appear to be entirely attributable to differences in education, weeks worked, and disability (column eight). If these characteristics were equalized, the share of total income comprised of personal income would be nearly identical between never-incarcerated and formerly-incarcerated men.

Table 4.3. Spouse Income Before and After Incarceration

	Log Spouse Income			Spouse Income as a Share of Total Income		
	OLS	OLS	Fixed Effects	OLS	OLS	Fixed Effects
Will ever be incarcerated	-0.861*** (0.096)	-0.431*** (0.098)	0.0568 (0.079)	-0.016** (0.005)	-0.006 (0.001)	0.012* (0.005)
Previously incarcerated	-1.626*** (0.141)	-1.682*** (0.137)	-0.455*** (0.106)	-0.044*** (0.008)	-0.046*** (0.008)	-0.006 (0.007)
Currently incarcerated	-0.677*** (0.118)	-0.596*** (0.116)	-0.284** (0.096)	-0.018 (0.010)	-0.016 (0.010)	-0.004 (0.008)
Race/ethnicity						
Black		-1.32*** (0.081)	-0.242*** (0.053)		-0.030*** (0.004)	0.007* (0.003)
Hispanic		-0.734*** (0.094)	-0.433*** (0.070)		-0.019*** (0.004)	-0.012** (0.004)
Other		-0.0711 (0.112)	-0.097 (0.079)		0.008 (0.005)	0.005 (0.004)
Highest degree completed						
High school diploma or GED			0.363*** (0.062)		0.007* (0.003)	0.002 (0.003)
Some college/Associate's			0.558*** (0.069)		0.015*** (0.004)	0.008* (0.003)
Bachelor's			0.850*** (0.091)		0.014** (0.005)	0.007 (0.004)
Married			5.91*** (0.053)		0.220*** (0.003)	0.221*** (0.002)
Constant	0.111*** (0.018)	0.507*** (0.045)	0.137*** (0.028)	0.002** (0.001)	0.008*** (0.002)	-0.002 (0.001)
Observations (<i>person-years</i>)	123,012	123,012	123,012	113,161	113,161	113,161
Respondents			6,351			6,340

*** p<0.001, ** p<0.01, * p<0.05

Note: Age fixed effects included in all models. Standard errors are clustered at the individual level. One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income.

Table 4.3 displays results from models predicting log spouse income and spouse income as a share of total income. The coefficients in the first and sixth columns show that, like with personal income, log spouse income and share of total income comprised by spouse income are lower, on average, for men who have been or ever will be incarcerated relative to never-incarcerated men.⁷ While race accounts for some of the difference in log spouse income between not-yet-incarcerated and never-incarcerated men (and all of the difference in spouse income as a share of total income), it does not help to explain the difference in spouse income (or spouse income as a share of total) following incarceration among ever-incarcerated men. Accounting for lower marriage rates among ever-incarcerated men (see Table A4.1 in the Appendix), as well as educational differences, does explain the difference in spouse income between not-yet-incarcerated and never-incarcerated men, but it only partially explains average differences among ever-incarcerated men following incarceration. Even when marriage rates and education levels are held constant, log spouse income is somewhere between 37 and 42 percent lower following incarceration than it was before. While differences in marital status and education cannot explain away spousal income gaps, they do appear to explain differences by incarceration history in share of total income comprised by spouse income.

In keeping with prior studies that suggest formerly-incarcerated men often rely upon public assistance after release (Harding et al. 2014; Visher et al. 2011; Western et al. 2015) and my own Chapter 2 findings, which indicate that formerly-incarcerated men more often receive public assistance benefits than never- incarcerated men, the Table 4.4

⁷ Results are very similar in models that exclude the 1,640 (26 percent) never-married respondents.

Table 4.4. Transfer Income Before and After Incarceration

	Log Transfer Income			Transfer Income as a Share of Total Income		
	OLS	OLS	Fixed Effects	OLS	OLS	Fixed Effects
Will ever be incarcerated	0.447*** (0.081)	0.413*** (0.082)	0.114 (0.075)	0.051*** (0.007)	0.040*** (0.007)	0.010 (0.006)
Previously incarcerated	0.549*** (0.118)	0.554*** (0.119)	0.030 (0.101)	0.097*** (0.013)	0.099*** (0.013)	0.042*** (0.009)
Currently incarcerated	-1.293*** (0.103)	-1.298*** (0.104)	-1.467*** (0.092)	-1.054*** (0.070)	-0.090*** (0.013)	-0.109*** (0.011)
Race/ethnicity						
Black	0.117* (0.046)	0.117* (0.046)	-0.135*** (0.037)	0.035*** (0.004)	0.035*** (0.004)	0.007** (0.003)
Hispanic	0.188*** (0.053)	0.188*** (0.053)	-0.034 (0.043)	0.022*** (0.004)	0.022*** (0.004)	0.007* (0.003)
Other	0.152* (0.062)	0.152* (0.062)	0.019 (0.050)	0.011** (0.004)	0.011** (0.004)	0.002 (0.003)
Highest degree completed						
High school diploma or GED	-0.342*** (0.050)	-0.342*** (0.050)	-0.342*** (0.050)	-0.043*** (0.004)	-0.043*** (0.004)	-0.009** (0.003)
Some college/Associate's	-0.735*** (0.053)	-0.735*** (0.053)	-0.735*** (0.053)	-0.059*** (0.004)	-0.059*** (0.004)	-0.017*** (0.003)
Bachelor's	-1.284*** (0.054)	-1.284*** (0.054)	-1.284*** (0.054)	-0.072*** (0.004)	-0.072*** (0.004)	-0.019*** (0.004)
Married	0.258*** (0.032)	0.258*** (0.032)	0.258*** (0.032)	-0.004*** (9.9e-05)	-0.004*** (9.9e-05)	-0.004*** (0.004)
Weeks worked in past year	-0.029*** (0.001)	-0.029*** (0.001)	-0.029*** (0.001)	0.216*** (0.008)	0.216*** (0.008)	0.149*** (0.002)
Disabled	2.078*** (0.068)	2.078*** (0.068)	2.078*** (0.068)	-0.023*** (0.002)	-0.023*** (0.002)	-0.011*** (0.001)
Constant	-0.058*** (0.012)	-0.133*** (0.022)	-0.096*** (0.028)	-0.006*** (0.001)	-0.016*** (0.002)	0.008 (0.011)
Observations (<i>person-years</i>)	123,009	123,009	123,009	113,161	113,161	113,161
Respondents			6,351			6,340

*** p<0.001, ** p<0.01, * p<0.05

Note: Age fixed effects included in all models. Standard errors are clustered at the individual level. One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income.

results indicate that men who will ever be incarcerated rely more heavily on transfer income than never-incarcerated men. Both transfer income levels and share of total income accounted for by transfer income are significantly higher among the formerly-incarcerated. But Table 4.4 also reveals that log transfer income and share of total income composed of transfer income are higher among ever-incarcerated men than among never-incarcerated men even before incarceration.

When differences in mediating characteristics are taken into account (columns three and five), however, differences in transfer income levels between not-yet-incarcerated and never-incarcerated men, as well as differences between not-yet-incarcerated and formerly-incarcerated men, are no longer significant. Thus, it appears that the difference in both pre- and post-incarceration transfer income between the ever-incarcerated and the never incarcerated are almost entirely mediated by differences in education, employment, disability, and marital status. The same is not true with regard to share of total income comprised by transfer income, however. Even when these mediating characteristics, race, and age are held constant, transfer income accounts for a significantly larger share of total income (about 4 to 5 percentage points) among the formerly-incarcerated than among the not-yet-incarcerated.

The coefficients in Table 4.5 indicate that NLSY79 respondents who will be incarcerated report less in other income on average than never-incarcerated respondents, and formerly-incarcerated respondents report less in other income than not-yet-incarcerated respondents. Both differences are largely related to differences in race and mediating characteristics, but significant differences remain even after race, education, employment, disability, and marital status are taken into account. There is

Table 4.5. Other Income Before and After Incarceration

	Log Other Income			Other Income as a Share of Total Income					
	OLS	OLS	Fixed Effects	OLS	OLS	OLS	OLS	Fixed Effects	Fixed Effects
Will ever be incarcerated	-1.068*** (0.052)	-0.544*** (0.055)	-0.120* (0.049)	-0.007* (0.003)	-0.001 (0.003)	0.001 (0.003)	0.001 (0.003)	0.007* (0.003)	0.004 (0.003)
Previously incarcerated	-0.550*** (0.064)	-0.620*** (0.066)	-0.238*** (0.060)	-0.544*** (0.057)	-0.307*** (0.057)	0.002 (0.003)	0.001 (0.003)	0.007* (0.003)	-0.004 (0.003)
Currently incarcerated	0.410*** (0.072)	0.503*** (0.075)	0.526*** (0.073)	0.326*** (0.066)	0.317*** (0.066)	0.127*** (0.013)	0.118*** (0.012)	0.100*** (0.004)	0.094*** (0.004)
Race/ethnicity									
Black	-1.634*** (0.053)	-1.271*** (0.046)		-0.017*** (0.001)	-0.020*** (0.001)				
Hispanic	-1.344*** (0.064)	-0.948*** (0.055)		-0.015*** (0.002)	-0.012*** (0.001)				
Other	-0.543*** (0.088)	-0.354*** (0.074)		-0.003 (0.002)	-0.001 (0.002)				
Highest degree completed									
High school diploma or GED	0.699*** (0.037)	0.699*** (0.037)		0.172*** (0.038)	0.172*** (0.038)				
Some college/Associate's	1.588*** (0.053)	1.588*** (0.053)		0.637*** (0.047)	0.637*** (0.047)				
Bachelor's	3.096*** (0.080)	3.096*** (0.080)		1.450*** (0.058)	1.450*** (0.058)				
Married	0.003*** (0.001)	0.003*** (0.001)		-0.002** (0.000)	-0.002** (0.000)				
Weeks worked in past year	-0.068 (0.048)	-0.068 (0.048)		0.088** (0.033)	0.088** (0.033)				
Disabled	0.509*** (0.035)	0.509*** (0.035)		0.457*** (0.021)	0.457*** (0.021)				
Constant	0.154*** (0.023)	0.779*** (0.046)	0.548*** (0.038)	0.198 (0.113)	0.177 (0.113)	0.003 (0.002)	0.008*** (0.002)	0.007*** (0.002)	-0.002 (0.007)
Observations (person-years)	122,718	122,718	122,718	122,718	122,718	112,916	112,916	112,916	112,916
Respondents				6,351	6,351			6,340	6,340

*** p<0.001, ** p<0.01, * p<0.05
 Note: Age fixed effects included in all models. Standard errors are clustered at the individual level. One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income.

little evidence that the proportion of total income comprised of other income differs based on incarceration history, however.

Thus, the findings in Tables 4.2-4.5 indicate that incarceration is a shock that appears to lead to lower personal, spousal, and other income. We may wonder, however, whether these are lasting shocks that will persist over time, or whether formerly-incarcerated men eventually close the income gap after enough time has passed. I examine this question by running a set of models predicting log income that account for length of time since the respondent was last observed in a correctional facility. The results of these models are displayed in Table 4.6 and 4.7. These tables display results from two models predicting each type of log income: first, a pooled regression model, then an individual fixed effects model. The first eight columns display results for each type of log income discussed above, while the last two columns display results for log total income. In Table 4.6 I control only for respondent race/ethnicity and age, using a quadratic measure of age.⁸ In Table 4.7 I add the full controls included in Tables 4.2-4.5 to get a sense for how these mechanisms shape the duration of income shocks.

Table 4.6 shows that some types of income recover – and some even increase – following incarceration, while others do not. The coefficients in the pooled regression model of log personal income (column one) indicate that, on average, the earned income shock experienced by formerly-incarcerated men relative to not-yet-incarcerated men (*previously incarcerated*) takes approximately 44 years to wear off (*years since last incarceration*), and it would take about 63 years post-release for formerly-incarcerated men to match the average annual log income of same-aged never-incarcerated men

⁸ I use quadratic age in Tables 4.6 and 4.7, rather than age fixed effects, because age and years since incarceration would otherwise be collinear, as respondents get one year older and one year farther from incarceration each year.

Table 4.6. Income Before and After Incarceration by Type with Time Since Release Measure

	Log Personal Income		Log Spouse Income		Log Transfer Income		Log Other Income		Log Total Income	
	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects
Will ever be incarcerated	-1.015*** (0.111)		-0.437*** (0.098)		0.382*** (0.083)		-0.558*** (0.054)		-0.825*** (0.092)	
Previously incarcerated	-2.294*** (0.169)	-2.191*** (0.078)	-1.730*** (0.143)	-2.003*** (0.102)	0.137 (0.123)	0.061 (0.073)	-0.603*** (0.062)	-0.561*** (0.069)	-1.991*** (0.143)	-1.965*** (0.067)
Years since last incarceration	0.053** (0.016)	0.011† (0.006)	0.007 (0.015)	-0.013† (0.008)	0.054*** (0.012)	0.053*** (0.006)	-0.002 (0.007)	0.004 (0.005)	0.083*** (0.011)	0.047*** (0.005)
Currently incarcerated	-1.833*** (0.158)	-1.434*** (0.086)	-0.528*** (0.123)	-0.518*** (0.111)	-0.843*** (0.105)	-0.673*** (0.080)	-0.517*** (0.072)	0.379*** (0.075)	-1.859*** (0.140)	-1.513*** (0.073)
Race/ethnicity										
Black	-1.221*** (0.066)		-1.320*** (0.081)		0.109* (0.046)		-1.637*** (0.053)		-1.116*** (0.050)	
Hispanic	-0.656*** (0.069)		-0.738*** (0.094)		0.171** (0.053)		-1.349*** (0.064)		-0.593*** (0.052)	
Other	-0.359*** (0.075)		-0.070 (0.112)		0.156* (0.062)		-0.544*** (0.088)		-0.258*** (0.057)	
Age	0.555*** (0.009)	0.527*** (0.005)	0.678*** (0.012)	0.668*** (0.007)	0.097*** (0.007)	0.094*** (0.005)	0.271*** (0.007)	0.242*** (0.005)	0.564*** (0.008)	0.540*** (0.005)
Age ²	-0.008*** (0.000)	-0.007*** (0.000)	-0.008*** (0.000)	-0.008*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.004*** (0.000)	-0.003*** (0.000)	-0.007*** (0.000)	-0.007*** (0.000)
Constant	0.495*** (0.150)	0.387*** (0.085)	-8.430*** (0.173)	-8.743*** (0.110)	-0.814*** (0.100)	-0.675*** (0.078)	-1.970*** (0.104)	-2.172*** (0.075)	0.404** (0.131)	0.294*** (0.072)
Observations (<i>person-years</i>)	122,831	122,831	123,012	123,012	123,009	123,009	122,718	122,718	123,012	123,012
Respondents	6,351	6,351	6,351	6,351	6,351	6,351	6,351	6,351	6,351	6,351

*** p<0.001, ** p<0.01, * p<0.05, †p<0.1

Note: Standard errors are clustered at the individual level. One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income.

(*previously incarcerated + will ever be incarcerated*). The fixed effect model of personal income, on the other hand, suggests that the earned income shock experienced by formerly-incarcerated never fully wanes in their lifetime. The coefficient on *years since last incarceration* is only marginally significant and indicates that it would take about 194 years, on average, for the post-release income shock to fully fade out.

Table 4.6 reveals that transfer income, on the other hand, is not just higher following incarceration (see Table 4.4), but that it increases, on average, with time since release among the formerly-incarcerated. The shocks to other income and spousal income observed in Tables 4.3 and 4.5 do not appear to wane with time, however.⁹ In fact, the fixed effects model suggests that average spouse income drops after incarceration and then declines even further with time since release (though this decrease with time is only marginally significant). Summing across these various income sources to total income, total income does eventually recover to levels similar to those among not-yet-incarcerated and never-incarcerated men of the same race and age, but it takes between 24 and 42 years to do so – i.e., most of adulthood, given that the median and mean age of last release is 34 among this cohort.

The average differences reported in Table 4.6 do not account for the fact that employment and marriage rates tend to be significantly diminished by incarceration, while disability is significantly increased by the experience of incarceration. To get a sense of how these important mediating characteristics influence income recovery post-release, I also run the same set of models with employment, disability, marital status, and education included as covariates. These results, displayed in Table 4.7, demonstrate

⁹ Results are substantively similar when the log spouse income models are run only on ever-married respondents.

Table 4-7. Income Before and After Incarceration by Type with Time Since Release and Mediating Characteristics

	Log Personal Income		Log Spouse Income		Log Transfer Income		Log Other Income		Log Total Income	
	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects
Will ever be incarcerated	-0.371*** (0.078)	0.090 (0.078)	0.143 [†] (0.076)	-0.118* (0.049)	-0.332*** (0.079)	-0.118* (0.049)	-0.332*** (0.079)	-0.118* (0.049)	-0.332*** (0.079)	-0.118* (0.049)
Previously incarcerated	-0.958*** (0.113)	-1.079*** (0.068)	-0.298** (0.117)	-0.177* (0.072)	-0.299*** (0.069)	-0.191*** (0.061)	-0.299*** (0.069)	-0.191*** (0.061)	-1.054*** (0.116)	-1.143*** (0.063)
Years since last incarceration	0.933*** (0.009)	0.006 (0.005)	0.046*** (0.009)	0.040*** (0.005)	0.002 (0.005)	-0.005 (0.006)	0.002 (0.005)	0.002 (0.005)	0.060*** (0.008)	0.033*** (0.005)
Currently incarcerated	-1.143*** (0.113)	-0.853*** (0.074)	-1.031*** (0.102)	-0.816*** (0.078)	0.381*** (0.075)	0.516*** (0.073)	0.381*** (0.075)	0.516*** (0.073)	-1.424*** (0.121)	-1.145*** (0.068)
Race/ethnicity										
Black	-0.553*** (0.046)	-0.185*** (0.052)	-0.123*** (0.037)	-1.261*** (0.046)	-0.600*** (0.041)	-1.261*** (0.046)	-0.600*** (0.041)	-1.261*** (0.046)	-0.600*** (0.041)	-0.600*** (0.041)
Hispanic	-0.269*** (0.044)	-0.424*** (0.070)	-0.012 (0.043)	-0.943*** (0.055)	-0.288*** (0.040)	-0.943*** (0.055)	-0.288*** (0.040)	-0.943*** (0.055)	-0.288*** (0.040)	-0.288*** (0.040)
Other	-0.159** (0.054)	-0.091 (0.078)	0.050 (0.051)	-0.356*** (0.073)	-0.118* (0.049)	-0.356*** (0.073)	-0.118* (0.049)	-0.356*** (0.073)	-0.118* (0.049)	-0.118* (0.049)
Highest degree completed										
High school diploma or GED	1.033*** (0.042)	0.835*** (0.033)	-0.046 (0.045)	0.688*** (0.035)	0.374*** (0.032)	0.688*** (0.035)	0.374*** (0.032)	0.688*** (0.035)	0.964*** (0.039)	1.070*** (0.030)
Some college/Associate's	1.067*** (0.049)	0.650*** (0.042)	-0.440*** (0.048)	1.577*** (0.050)	0.864*** (0.042)	1.577*** (0.050)	0.864*** (0.042)	1.577*** (0.050)	1.013*** (0.045)	0.967*** (0.040)
Bachelor's	1.287*** (0.057)	1.140*** (0.053)	-1.015*** (0.051)	3.087*** (0.079)	1.679*** (0.054)	3.087*** (0.079)	1.679*** (0.054)	3.087*** (0.079)	1.220*** (0.052)	1.427*** (0.050)
Married	0.657*** (0.032)	0.269*** (0.021)	0.272*** (0.032)	0.511*** (0.035)	0.470*** (0.021)	0.511*** (0.035)	0.470*** (0.021)	0.511*** (0.035)	1.011*** (0.028)	0.687*** (0.019)
Weeks worked in past year	-2.062*** (0.068)	-1.392*** (0.033)	2.151*** (0.068)	-0.039 (0.048)	0.127*** (0.033)	-0.039 (0.048)	0.127*** (0.033)	-0.039 (0.048)	-0.165*** (0.049)	0.068* (0.030)
Disabled	0.082*** (0.001)	0.084*** (0.000)	-0.027*** (0.001)	0.007*** (0.001)	-0.024*** (0.001)	0.004*** (0.001)	0.000 (0.000)	0.000 (0.000)	0.050*** (0.001)	0.051*** (0.000)
Age	0.029** (0.010)	0.073*** (0.006)	0.246*** (0.009)	0.061*** (0.008)	0.126*** (0.006)	0.061*** (0.008)	0.126*** (0.006)	0.061*** (0.008)	0.169*** (0.009)	0.175*** (0.005)
Age ²	-0.001*** (0.000)	-0.001*** (0.000)	-0.003*** (0.000)	-0.001*** (0.000)	-0.002*** (0.000)	-0.001*** (0.000)	-0.002*** (0.000)	-0.001*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)
Constant	5.120*** (0.125)	4.333*** (0.082)	-2.119*** (0.116)	0.162 (0.104)	-0.980*** (0.086)	0.162 (0.104)	-0.980*** (0.086)	0.162 (0.104)	4.008*** (0.119)	3.633*** (0.076)
Observations (person-years)	122,831	122,831	123,009	122,718	123,009	122,718	123,009	122,718	123,012	123,012
Respondents	6,351	6,351	6,351	6,351	6,351	6,351	6,351	6,351	6,351	6,351

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1

Note: Standard errors are clustered at the individual level. One dollar is added to all income variables before taking the natural log so that respondents with zero income are included in models of log income.

that the duration of these income shocks is not greatly mediated by employment, disability, marital status, and education. Conditioning on these covariates only slightly reduces the average length of time it takes for the personal income and total income shocks to wear off by one-third or less. If we could somehow equalize weeks worked, marriage rates, disability, and education across groups, it would still take between 18 (pooled regression estimate) and 35 years (fixed effect estimate) for the post-incarceration total income shock to fully recede.¹⁰

However, this estimate of 18 to 35 years (or 24 to 42 years based on Table 4.6) only tells us how long it takes, on average, for post-incarceration *annual* total income to return to its pre-incarceration level. In those intervening years, formerly-incarcerated men are likely to accrue substantial deficits in lifetime income. If we consider, for example, a young man first incarcerated at age 26 (the median age at first incarceration among men in the NLSY79 cohort) and released at age 27¹¹ who reported \$20,000 in total income at age 25 (the median income among 25-year-old not-yet-incarcerated respondents), the fixed effect model in Table 4.6 predicts that by age 55 he will have accrued \$418,000 less in total income over the last 30 years than if he had never been incarcerated. Alternatively, the OLS regression model predicts that that same man will

¹⁰ While it had appeared in Table 4.4 that transfer income did not change significantly following incarceration, the Table 4.7 coefficients indicate that formerly-incarcerated men do experience a significant drop in transfer income in the immediate aftermath of incarceration (of about 16 to 22 percent) compared to otherwise observably similar men, but transfer income recovers relatively quickly, matching levels among otherwise similar not-yet-incarcerated men after about 4 to 6 years. Table 4.7 also indicates that spouse income among the formerly incarcerated drops somewhat after incarceration relative to levels among observably similar men, and then further declines with time.

¹¹ The median number of years ever-incarcerated, male NLSY79 respondents were observed in prison or jail is two, but 1 year is the modal category.

have accrued \$250,000 less in total income from ages 26 to 55 than a counterfactual not-yet-incarcerated man and \$445,000 less than a never-incarcerated man.

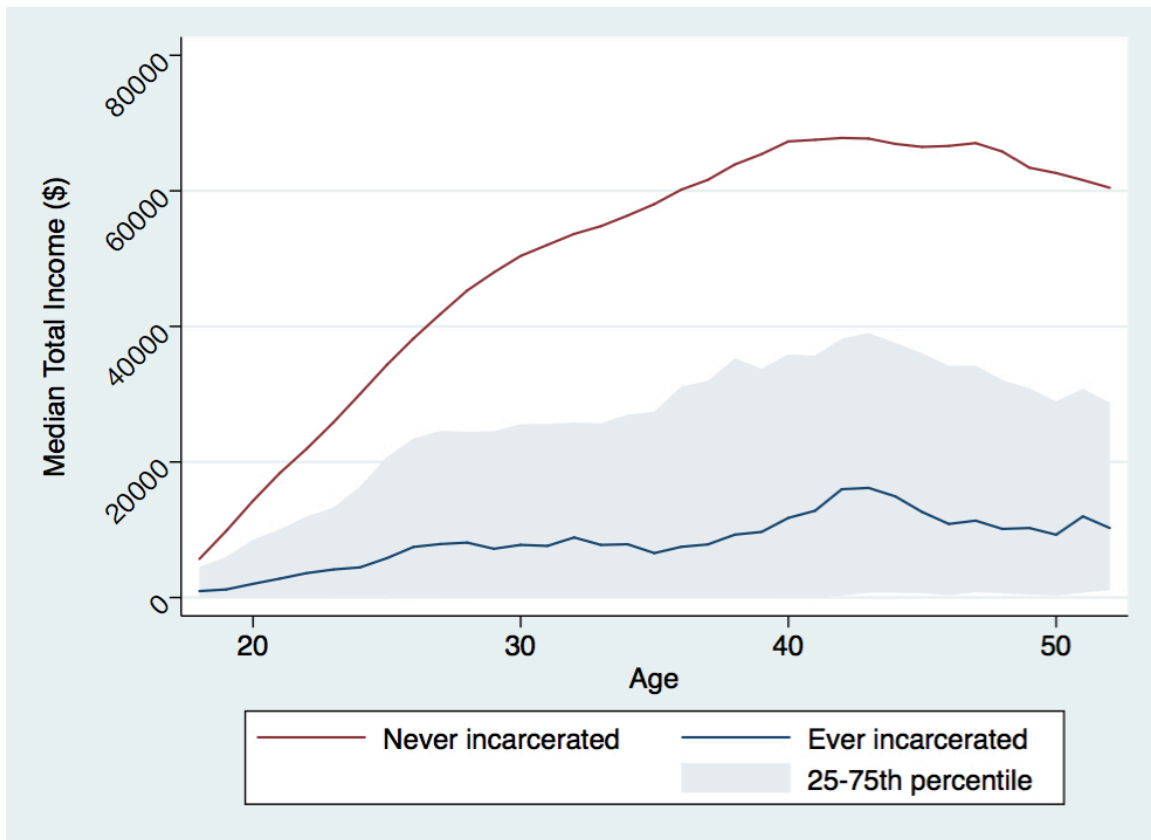
If, alternatively, we compared this same man to another man with the same level of education, who worked the same number of weeks each year, and who had the same annual marital and disability status but who had not been incarcerated, the results in Table 4.7 indicate that the accumulated income gap would be smaller, but not trivial. The fixed effect model of total income in Table 4.7 predicts that the accumulated income gap between ages 26 and 55 would drop by only about one-third, to \$279,000. The OLS model in Table 4.7 is somewhat more optimistic, indicating that the accumulated income gap would drop by more than half if these traits could all be equalized, but still it predicts that this same man would accumulate \$192,000 less in total income over this period than an observably similar never-incarcerated man (but only \$33,000 less than a hypothetical, otherwise similar not-yet-incarcerated man).

Income Trajectory Heterogeneity

The prior findings have considered the mean income trajectories of formerly-incarcerated men relative to never-incarcerated men. I now consider variation within the group of NLSY79 respondents who are ever observed in prison or jail. There is less variation in all types of income, other than transfer income, among ever-incarcerated men than there is among never-incarcerated men (see Figure A4.2 in the Appendix), but, as Figure 4.3 shows, there is still a sizeable degree of variation in personal earned income within the formerly-incarcerated respondent group. The shaded area in Figure 4.3 represents the middle two quartiles of the personal income distribution for formerly-incarcerated men, and the blue line displays median earned income at each age for this group. While median income and 25th percentile income remain more or less flat over

time, income increases with age for formerly-incarcerated men at the 75th percentile of the distribution in a pattern similar to that seen among the never-incarcerated respondents.

Figure 4.3. Median Total Income by Age and Incarceration History



Note: Median, 25th and 75th percentiles represent 3-year moving averages by age.

In order to investigate heterogeneity in lifetime income trajectories within the ever-incarcerated group and determine whether some formerly-incarcerated men have income trajectories that look more similar to those of the never-incarcerated group, I use cluster analysis to identify different income trajectory patterns within the ever-incarcerated group. Using Stata's `kmedians` partition-clustering command, I have identified four income trajectory groups based on income levels at age 30, 40, and 50 for

each of the four income types analyzed above. This clustering approach sorts observations – ever-incarcerated individuals, in this case – into groups of the most similar observations using Euclidean distance from the median values of the sorting variables (i.e., levels of each income type at age 30, 40, and 50) of each group. Observations are sorted and re-assigned until group assignments are stable.

Figure 4.4 displays lowess-smoothed income by age plots for each income type across the four clusters. Group 1 – the modal category, containing 42 percent of ever-incarcerated male NLSY79 respondents – has exceptionally low levels of both income and growth across all ages and types, with the exception of transfer income, which grows steadily with age. Groups 2 and 4, each of which represent about 20 percent of ever-incarcerated respondents, are similar in their trajectories across income type, except that Group 4 members experience a relatively sharp decrease in earned income in later adulthood (40s through 50s), whereas members of Group 2 maintain income gains accrued in early adulthood, holding steady through the 30s and even seeing a slight uptick in average personal income in the 40s and 50s, on average.

Group 3, comprising 19 percent of the ever-incarcerated respondents, appears to have markedly different income trajectories across type compared to the other clusters. For members of Group 3, average income grows with age for all sources other than transfer income, and earned income grows dramatically through midlife, reaching approximately \$50,000 annually in the late 40s and 50s – more than twice that of Group 1, 2 and 4 members at the same age.

In fact, both the income trajectories and levels of Group 3 members look remarkably similar to those of the full sample, which are shown in the last column of Figure 4.5. Members of Group 3 top out at a slightly lower level of personal income in

Figure 4.4. Clusters of Lifetime Income Trajectories Among the Ever Incarcerated by Cluster, Type, and Age

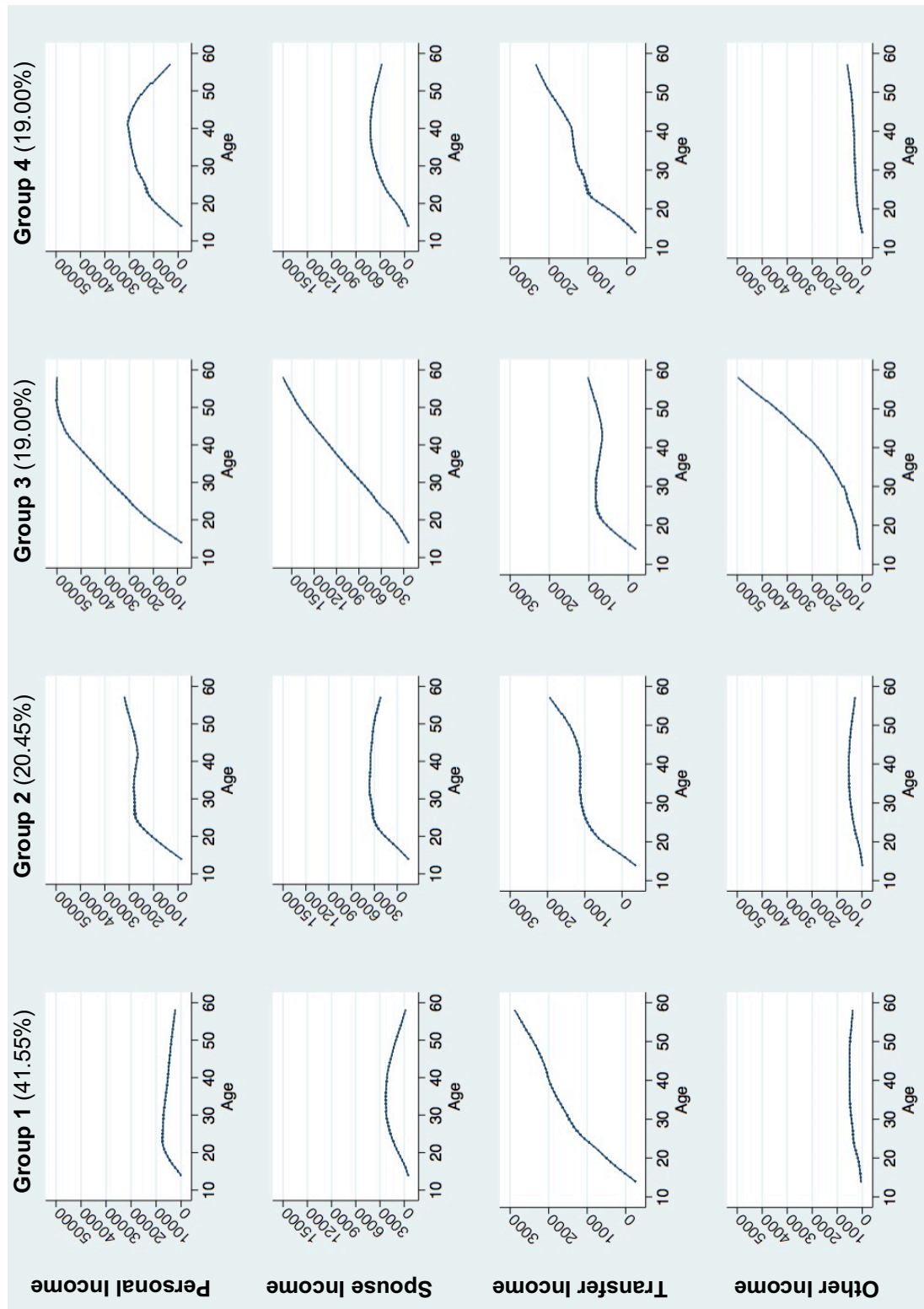
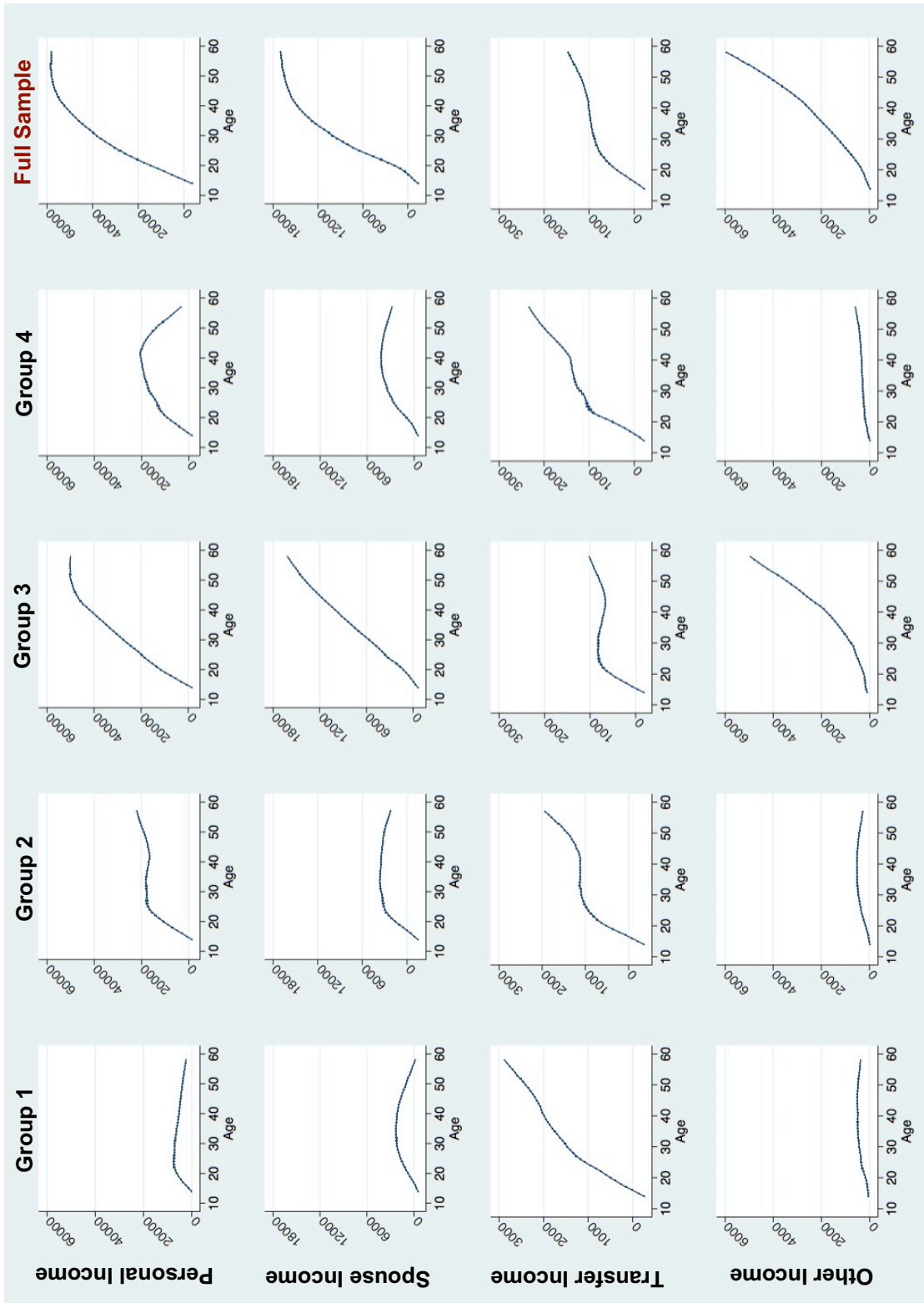


Figure 4.5. Comparison of Ever Incarcerated Income Trajectory Clusters to Full Sample Income Trajectory



their 50s than the full sample (about \$50,000 compared to \$60,000), but otherwise have nearly identical income trajectories across all types. Thus, while Group 1 seems representative of the disadvantage and marginality we come to expect among the formerly-incarcerated based on prior research, Group 3 members appear to represent an unusual subset of ever-incarcerated respondents for whom incarceration does not seem to prevent them from achieving a more traditional income trajectory.

In order to provide some insight into what types of individuals make up each of these four clusters, Table 4.8 displays descriptive statistics by cluster for ever-incarcerated respondents. Perhaps unsurprisingly, Group 3 appears to contain the most advantaged of the ever-incarcerated NLSY79 respondents. They have the highest education levels, shortest average number of years incarcerated, lowest disability rate, and highest average number of weeks worked in the past calendar year. They are disproportionately white (33 percent) relative to the other three groups, and their average income far exceeds that of the other three groups for all but transfer income. However, while Group 3 members do have fewer average years incarcerated relative to other cluster members, they are not first incarcerated at later ages.¹²

On the other end of the spectrum, Group 1, the modal cluster, is the most disadvantaged on every measure. They have the earliest average age of first incarceration (26) and longest average number of years observed in a correctional facility (5.2). They are also mostly black (59 percent), and fewer than half (47 percent) completed high school. They report the fewest average weeks worked in the past year, the lowest marriage rates, and disability rates twice as high as those among the other three clusters.

¹² The difference in average number of years observed incarcerated between Group 1 and Group 3 members is statistically significant, but the difference in age at first incarceration is not.

Table 4.8. Descriptive Statistics by Ever Incarcerated Lifetime Income Trajectory Cluster

Person Level	Group 1	Group 2	Group 3	Group 4
Age at first incarceration	26.0	30.1	26.5	27.0
Years incarcerated	5.2	3.2	2.5	3.1
Race/Ethnicity				
White	16.4%	16.7%	32.8%	23.7%
Black	59.0%	50.0%	40.5%	48.3%
Hispanic	18.4%	26.2%	14.7%	17.8%
Other	6.3%	7.1%	12.1%	10.2%
Highest degree completed at age 25				
None	53.0%	46.4%	36.0%	39.0%
High school diploma or GED	38.3%	46.4%	49.6%	52.5%
Some college/Associate's	8.8%	7.2%	10.8%	7.6%
Bachelor's or more	0.0%	0.0%	3.6%	0.9%
Ever married	46.1%	69.0%	67.2%	63.6%
Age at first marriage	31.4	31.4	33.3	33.2
	<i>N</i>	256	126	116
		126	116	118
Person-Year Level	Group 1	Group 2	Group 3	Group 4
Married	11.8%	20.4%	30.4%	20.5%
Disabled	20.9%	8.7%	5.7%	9.4%
Weeks worked in past calendar year	17.4	30.4	32.9	26.9
Total Income -- <i>mean</i>	\$9,677	\$20,898	\$33,660	\$18,862
<i>median</i>	\$2,865	\$15,267	\$22,779	\$12,185
Personal Income -- <i>mean</i>	\$6,237	\$16,495	\$26,451	\$14,689
<i>median</i>	\$323	\$12,023	\$19,316	\$8,537
Spouse Income, if married -- <i>mean</i>	\$8,890	\$11,114	\$15,601	\$9,556
<i>median</i>	\$0	\$1,471	\$3,174	\$0
Transfer Income -- <i>mean</i>	\$1,362	\$1,008	\$730	\$1,063
<i>median</i>	\$0	\$0	\$0	\$0
Other Income -- <i>mean</i>	\$390	\$400	\$983	\$265
<i>median</i>	\$0	\$0	\$0	\$0
	<i>Person-years</i>	5,566	2,781	2,234
		2,464		

Note: All dollar value variables have been adjusted for inflation to 2014 values.

They also report exceptionally low income – less than \$10,000, on average. Their only income advantage compared to other groups is in transfer income (\$1,400 annually, on average), and even then the median amount of transfer income received by this group is \$0.

Groups 2 and 4 fall in the mid-range between Group 1 and Group 3. They are the two most closely matched groups in terms of income averages, but Group 2 is the slightly more advantaged of the two in terms of marriage rates, disability, weeks worked, and age at first incarceration, though a larger proportion of Group 4 members have completed high school than Group 2 members. Hispanics are overrepresented in Group 2, making up over one-quarter of all Group 2 members, compared to less than one-fifth of all other groups.

Because many of the characteristics in Table 4.8 are highly correlated (e.g., race and education level, etc.), I also use a multinomial logit model to predict cluster group membership to better understand which characteristics best predict membership in the relatively privileged Group 3 as opposed to the modal and financially-marginal Group 1. Group 1 is the reference category, so the log odds coefficients displayed in Table 4.8 represent the log odds of being sorted into Group 2, 3 or 4 rather than Group 1.

The results from the multinomial logit regression indicate that, all else equal, black and Hispanic respondents are significantly less likely than whites (the reference category) to make it into Group 3 rather than Group 1. Marital status, disability, and average number of weeks worked annually are also predictive of membership in Group 3 (instead of Group 1), with more years of marriage and more weeks worked per year positively associated with Group 3 membership, while years of disability is negatively associated with Group 3 membership. Respondents who are first incarcerated at an early

Table 4.9. Results from Multinomial Logit Model Predicting Lifetime Income Trajectory Cluster Membership (Reference Group=Cluster 1)

	Group 2	Group 3	Group 4
Age at first incarceration	0.0186 (0.0185)	-0.0561* (0.0219)	-0.0318 (0.0196)
Years incarcerated	-0.0390 (0.0449)	-0.188** (0.0571)	-0.163*** (0.0480)
Race/ethnicity			
Black	-0.122 (0.373)	-0.735* (0.372)	-0.448 (0.347)
Hispanic	0.0104 (0.422)	-1.414** (0.471)	-0.646 (0.416)
Other	0.206 (0.575)	0.0290 (0.536)	0.116 (0.515)
Highest degree completed, age 25			
High school diploma or GED	0.182 (0.274)	0.583† (0.310)	0.577* (0.274)
Some college/Associate's	-0.128 (0.511)	0.630 (0.514)	0.248 (0.496)
Bachelor's	-0.419 (27,519)	21.18 (11,333)	20.66 (11,333)
Average weeks worked per year	0.0973*** (0.0150)	0.0863*** (0.0158)	0.0552*** (0.0141)
Age at first marriage	0.0132 (0.00907)	0.0145 (0.0103)	0.0183* (0.00908)
Years married	0.00670 (0.0346)	0.0756* (0.0352)	0.0197 (0.0347)
Years disabled	-0.141** (0.0440)	-0.299*** (0.0623)	-0.164*** (0.0419)
Parents' highest degree completed			
High school diploma or GED	0.305 (0.304)	-0.0235 (0.336)	-0.129 (0.306)
Some college/Associate's	-0.0271 (0.553)	0.471 (0.528)	0.206 (0.490)
Bachelor's	0.200 (0.743)	0.262 (0.761)	-0.523 (0.813)
More than college	-14.66 (1,211)	-1.911† (1.147)	-15.56 (674.2)
Household structure in 1979			
Living with one biological parent, one stepparent	0.270 (0.398)	0.240 (0.442)	-0.148 (0.412)
Living with one biological parent only	0.683* (0.313)	0.966** (0.341)	0.560† (0.307)
No biological parents present	0.438 (0.480)	-0.186 (0.646)	0.328 (0.468)
Constant	-3.586*** (0.782)	-0.700 (0.798)	-0.417 (0.710)
	<i>Observations (person-years)</i>		
	605	605	605

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1

age and/or incarcerated for a greater number of years are significantly less likely to end up in Group 3 rather than Group 1. Number of weeks worked and disability status are similarly predictive of ending up in Group 2 or 4, rather than Group 1, again highlighting the extreme marginality of Group 1 members with regard to disability status and employment levels.

In order to account for the possibility that ever-incarcerated men from more privileged backgrounds are less likely to end up with the disadvantaged income trajectories that characterize Group 1, I also include two measures of family background in this model: parent's education level and household structure at age 14. Parents' education is measured as highest grade level completed by the respondent's residential biological parent(s), categorized as less than high school (reference), exactly a high school diploma, some college education, a four-year college degree, or more than a four-year degree. Household structure at age 14 is measured using four mutually-exclusive categories: respondent lived with both biological parents (reference), respondent lived with one biological parent and one stepparent, respondent lived with one biological parent and no stepparent, or respondent did not live with any biological parent. Family background, independent of race and own achieved characteristics, does not appear to be strongly associated with income trajectory among ever-incarcerated men, though.¹³

This analysis gives us some insight into what type of formerly-incarcerated men experience more advantaged income trajectories and levels, but it cannot tell us why or how exactly their paths diverge. As the results in Table 4.9 reveal, higher levels of employment and marriage are associated with significantly higher log-odds of having a

¹³ Surprisingly, men who lived with only one biological parent instead of two at age 14 are less likely to be sorted into Group 1 than any of the other categories, but I have no a prior theoretical hypothesis for why this would be the case.

more normative income trajectory as opposed to the very disadvantaged income trajectory that is modal among the formerly incarcerated. It may be that, the stable employment and relationships that help individuals to desist from crime also help them to attain a more traditional life-course trajectory in general regardless of criminal justice system interactions. However, because the results presented here are only correlational, future work that investigates the processes through which approximately 20 percent of formerly-incarcerated men manage to avoid the extremely low income levels and challenges experienced by most of the formerly incarcerated could be extremely beneficial in illuminating the keys, and barriers, to successful social reintegration following incarceration.

Conclusion

In the focus on the reentry period and labor market outcomes that dominates most of the literature on the economic wellbeing of former prisoners, we have failed to make a full accounting of the financial stability of formerly-incarcerated Americans as they navigate the remainder of their lives. As a result, we lack information on how long the financial instability identified during the reentry period persists, whether formerly-incarcerated men are able to substitute other forms of income for lost earnings, and how the many social disruptions that flow from incarceration combine to affect income levels throughout the life course.

Employment, which has been the focus of a sizeable portion of the research literature, is an important part of this picture – indeed, earned income comprises the vast majority of total income for formerly-incarcerated men – but it is not the only contributor to income and economic wellbeing. By examining all sources of income over many years post-incarceration, the findings in this chapter reveal that formerly-

incarcerated men are penalized not only in their earned income, but in all types of income following incarceration. Men in the NLSY79 cohort experienced significant drops in every type of income other than transfer income following incarceration, making clear that the economic disadvantage experienced by formerly-incarcerated men is even greater than we may be led to believe by focusing our attention on employment and earnings alone. These men are not substituting other forms of income for lost wages following incarceration – rather, they appear to be managing to survive on extremely low levels of income, on average. Moreover, as Table 4.6 shows, men who will eventually be incarcerated already face lower earnings, spouse income, and other income levels even prior to their incarceration. Thus, incarceration appears to have the effect of making already lower incomes even lower.

While some forms of income recover after incarceration, others do not. Fixed effect models indicate that spouse income, for example, gets even worse with time after incarceration, perhaps reflecting formerly-incarcerated men's low value in the marriage market. On the whole, recovery to pre-incarceration total income levels is slow, taking somewhere between 24 and 42 years, on average. Thus, a key finding from these descriptive analyses is that the financial marginality previously documented during the reentry period (Harding et al. 2014; Western et al. 2015) persists in one way or another for many years. Furthermore, in the intervening years between incarceration and full recovery to pre-incarceration income levels, these men lose out on hundreds of thousands of dollars in accumulated lifetime income.

However, while the findings presented in this chapter reveal that the majority of formerly-incarcerated men experience extreme financial marginality in terms of their income trajectories and levels, a minority manage to achieve a normal-looking income

trajectory across the life course. These men appear to be disproportionately white and married relative to other formerly-incarcerated men. They also report much lower rates of disability and are employed at greater levels. How exactly these men manage to avoid the extremely low levels of income, employment, and marriage that are the norm among formerly-incarcerated men is not clear from these findings. But the fact that approximately one-fifth of former prisoners experience a traditional-looking income trajectory following incarceration is a noteworthy finding that merits further investigation.

It is important to reiterate, though, that these men are the exception to the rule. The vast majority of formerly-incarcerated men in this sample have extremely low levels of income post-incarceration that demonstrate little growth over their adult years. While the median total income in 2014 for never-incarcerated male NLSY79 respondents was \$73,000 (and the mean was \$106,000), the median total income for formerly-incarcerated respondents was just \$9,300 (the mean was \$22,000). Moreover, close to one-fifth (17 percent) of formerly-incarcerated respondents reported zero dollars in total income in 2014. In fact, formerly-incarcerated respondents report zero income in 19 percent of all post-incarceration person-years. How exactly these men are managing to survive is unclear. It may be that they have income they are not reporting or not properly recalling, but it seems unlikely that all of these men are truly financially stable. Thus, future work that can illuminate the housing situations and survival strategies of these men living in extreme financial marginality following incarceration would be worthwhile.

On the whole, formerly-incarcerated men appear to be dealing with levels of social and economic instability in their 50s that are normally associated with a much earlier stage in the life course. The average level of personal earned income and spousal

income reported by formerly-incarcerated men in their 50s is on par with levels reported by never-incarcerated men in their mid- to late-20s. Meanwhile, among the group with the most disadvantaged income trajectories, income levels in the 50s (for all types but transfer) are similar to those among never-incarcerated men in their late teens. Likewise, marriage rates in the 50s among formerly-incarcerated men are similar to those reported by never-incarcerated men in their very early 20s, while employment levels (i.e., number of weeks worked in the last year) among formerly-incarcerated men in their 50s are on par with those reported by never-incarcerated men in their late teens. Thus, prior incarceration appears to be associated with an extreme departure from normal progression through the life course. The extent to which incarceration is the cause, rather than a symptom, of this departure is unclear from these findings, but is worthy of future investigation.

In documenting the long-lasting, extremely low average income levels of formerly-incarcerated men, this chapter provides further evidence of the extreme marginality that characterizes most formerly-incarcerated Americans, whom Uggen, Manza, and Thompson (2006) rightfully describe as a caste. Not only are their rights of citizenship limited, their employment prospects restricted, and their marriage prospects diminished, but – partially as a result of all of these things – they tend to live in extreme financial precarity. This finding has important implications for how we think about “reentry” following incarceration. Reentry programs and policies tend to focus on providing support and increasing stability over a relatively short period of time, often with the goal of reducing recidivism in the first five years following release. But the enduring financial marginality of the formerly-incarcerated men described in this

chapter suggests that perhaps we should be thinking about the problem of social reintegration following release on a much longer time horizon.

Moreover, as Chapter 3 points out, social safety net programs, which already provide a small proportion of total income for formerly-incarcerated men over the time period analyzed here, are likely to become less and less helpful for these men in future years because of their increasing reliance on work-based eligibility determination. The extreme financial marginality of formerly-incarcerated men would be greatly benefited by a social safety net that supports stable employment, provides income support, and does not permanently penalize men for prior offenses (i.e., felony drug crimes), but whether the political will exists to reshape the safety net in these ways is doubtful, particularly at this particular moment in time.

5. Conclusion

This dissertation makes clear that the problem of social integration after criminal justice contact is much larger than typically conceptualized. Both service providers and much of the existing research literature primarily focus on outcomes over the first few years following release from prison or jail as a critical period for formerly justice-involved adults to begin to re-integrate into society and establish their self-sufficiency (Harding et al. 2014; National Research Council 2008; Western et al. 2015). But what my findings make clear is that the challenges to stability and self-sufficiency that typically mark the reentry period extend far beyond the few years following release from prison and the formerly-incarcerated population. Financial insecurity follows formerly-incarcerated men across the life course, and the social safety net does not fully fill the gaps in lost earnings. Moreover, the challenges of being marked as a felon and the instability that follows apply to far more than the 4.9 million formerly-incarcerated Americans upon whom most research focuses – they also extend to the approximately 12 million Americans with felony convictions who have never been imprisoned.

In Chapter 2, I used NLSY97 data to compare the housing experiences of individuals with a felony conviction but no history of incarceration to those of formerly-

incarcerated individuals, with the goal of disentangling the effect of being *marked* as a felon from the effect of having actually been locked up. Prior research has often conflated these two experiences when considering outcomes among the formerly incarcerated, making it impossible to discern the relative contribution of each mechanism to the instability frequently observed among this population. I found that felony conviction appears to increase housing instability – as measured by number of residential moves in the last year, likelihood of living in temporary housing, and likelihood of having no permanent residence – even when conviction is not accompanied by a custodial sentence. The magnitude of the relationship between prior incarceration and housing instability is generally greater than the magnitude of the relationship between felony conviction without incarceration and housing instability, which is unsurprising, as incarceration is a multifaceted treatment – involving removal from one’s community, employment disruption, and confinement, in addition to the criminal record stigma likely to follow release – compared to conviction without incarceration. But the fact that formerly-convicted individuals with no history of incarceration are more likely to experience housing instability than otherwise similar never-convicted individuals, even when comparisons are restricted to biological siblings and behaviorally-similar individuals, suggests that felon *status*, not just the experience of incarceration per se, has important implications for individual stability and self-sufficiency. Moreover, given that formerly convicted individuals with no history of imprisonment outnumber former prisoners by more than two-to-one in the U.S. (Shannon et al. 2017), these findings suggest that the research literature to date has not come close to making a full accounting of the costs of criminal justice system interactions.

Chapter 3 considered participation in social safety net programs by the formerly incarcerated with the goal of better understanding (1) how formerly-incarcerated individuals interact with other state institutions following release, (2) whether they exhibit system avoidance behaviors in doing so, and (3) whether there is evidence of differential participation in such programs based on program structure and race. Using data from the NLSY79 cohort, I found no evidence that formerly-incarcerated individuals engage in system avoidance behaviors by participating more often in less administratively burdensome programs (e.g., food stamps, EITC) and less often in more surveilling programs (e.g., SSI, AFDC/TANF). Instead, I found evidence of *assistance-seeking* behavior among white former prisoners – formerly-incarcerated whites are more likely to receive means-tested public assistance benefits, even from heavily surveilling programs like SSI, than their otherwise observably similar never-incarcerated counterparts. However, I also found that, regardless of race, formerly-incarcerated individuals are less likely to receive social insurance benefits (i.e., disability and unemployment insurance) than never-incarcerated individuals, even after observable differences in labor market participation and disability are taken into account. Finally, I found that the 1996 reforms to welfare cash assistance (AFDC/TANF) and food stamps (SNAP), which imposed work requirements and eligibility restrictions for individuals convicted of felony drug offenses, appear to have reduced receipt of benefits from these programs, not just for formerly-incarcerated individuals but for everyone.

Given that formerly-incarcerated individuals often have unstable work histories and a lower probability of employment in the formal labor market (Visher et al. 2011), the addition of work requirements to public assistance programs – and, perhaps, Medicaid, if current proposals succeed (Williams 2018) – likely means that formerly-

incarcerated individuals will be increasingly shut out of a work-based social safety net. Whether this change to program eligibility requirements will significantly alter the ability of formerly-incarcerated individuals to achieve economic stability is unclear, but it is a question I plan to pursue in future work looking at the NLSY97 cohort, which came of age entirely after the implementation of these 1996 reforms.

Finally, Chapter 4 demonstrated that the vast majority of formerly-incarcerated men appear to live in an extended state of financial marginality. They have income levels far below those of the average adult male in their cohort that remain extremely low over the life course. Men who will ever be incarcerated are already less likely to be married, more likely to be disabled, work fewer weeks per year, and have lower income, on average, than never-incarcerated men even before incarceration, but all of these disadvantages are exacerbated following incarceration. On average, the total income shock that these men experience following incarceration takes between 20 and 35 years to wear off, and some types of income, like spouse/partner income, never seem to recover. These average experiences, however, mask a sizeable degree of variation in income trajectories among the formerly incarcerated. While the most common income trajectory pattern for this group is characterized by exceptionally low income of all types, with the exception of transfer income, and stagnant growth, about one-in-five formerly-incarcerated men do appear to attain a normal-looking income trajectory that closely mirrors that of never-incarcerated men. On the other end of the spectrum, however, another one-fifth of formerly-incarcerated men report zero income *of any type* in any given year. Race appears to be somewhat predictive of the type of income trajectory formerly-incarcerated men experience, with white men more likely to attain traditional-looking income trajectories after incarceration and black men disproportionately likely

to have persistently low income across the life course following incarceration, but race is by no means wholly determinative of post-incarceration income trajectories. Marriage and higher employment levels are also strongly associated with experiencing a more advantaged income trajectory post-incarceration, but it is unclear from my descriptive analyses whether formerly-incarcerated men who manage to achieve higher levels of employment and higher income are more often married simply because they are more marriageable (Wilson 2012) or whether marriage itself served as a stabilizing turning point for these men, encouraging them to seek out stable employment and, thus, earnings (Laub and Sampson 1993). Thus, future work that seeks to disentangle the causal ordering of these experiences for formerly-incarcerated men who manage to achieve a traditional life course income trajectory could be very useful in illuminating the processes that best promote the full social and economic reintegration of formerly-incarcerated men.

The findings in this dissertation have important implications for how we – both the scholarly community and the polity – account for the full costs of the criminal justice system, whom we target stability-promoting “reentry” type services to, and how long we offer assistance to those individuals. As the results presented make clear, the community in need of stability-promoting support following criminal justice system contact extends far beyond the formerly incarcerated, and the length of time over which this support may be necessary is far longer than typically conceived of (Petersilia 2003).

Whether or not we will make appreciable strides in extending this type of support is another question, however. In recent years, a number of bipartisan criminal justice reform efforts have emerged with the goal of shrinking the size of the incarcerated population in America and reducing the legal collateral consequences that hinder the

social reintegration of former offenders (e.g., Charles Koch Institute n.d.; #cut50 n.d.). Likewise, public support for criminal justice reform is currently strong (American Civil Liberties Union 2017). But the tides may be turning, as evinced by the success of Donald Trump's 2016 presidential campaign, which often relied upon the law-and-order rhetoric that preceded the rise of mass incarceration in the 1960s and 70s (National Research Council 2014c). Early in the new administration, Attorney General Jeff Sessions made clear that the Trump era Department of Justice would abandon previous Attorney General Eric Holder's progressive policies and return to the harsh prosecutorial policies that characterized the 1990s (Horwitz and Zapotosky 2017). And at this current moment in time, President Trump is campaigning for aggressive mandatory minimum sentences – even going to far as to suggest use of the death penalty – for convicted opioids dealers (Haberman, Goodnough, and Seelye 2018; The Associated Press 2018).

Even if the Trump administration's rhetoric fails to shift public sentiment on criminal justice reform and reform efforts continue at the state-level, however, criminal justice reform will only accomplish so much if change comes solely in the form of shorter average sentences and greater diversion toward community-based corrections (i.e., probation in lieu of custodial sentences). As the findings from Chapter 2 show, former felon status, independent of actual incarceration, appears to undermine individual stability, and while the harms of incarceration per se appear to diminish with time, the stigma of felony conviction does not similarly wear off. Thus, simply reducing the size of the currently incarcerated population will not eliminate the hurdles to self-sufficiency faced by individuals who have been marked by the criminal justice system.

Therefore, if we want to truly lessen the collateral harms inflicted by the American criminal justice system, then we need to work toward reducing the size of the

criminal justice system as a whole – not just the size of prisons and jails – and create programs and policies that help individuals achieve stability and self-sufficiency after they exit the system. This can and should take the form of fewer limits on public assistance receipt for formerly-convicted individuals, forgiving work requirements for public benefit programs, the elimination of public housing policies that exclude formerly-convicted individuals and threaten the benefits of individuals who co-reside with or briefly host someone with a felony conviction, the reduction of occupational licensure restrictions and other legal sanctions that diminish the employment prospects of former felons, and greater work supports and income support for all people exiting the criminal justice system.

With regard to the research agenda for scholars of inequality and criminal justice, we need to devote more attention to understanding how various types of criminal justice contact beyond incarceration affect individual opportunities and how they affect individuals not just in the short term, but how they may restructure the individual life course for many years to come. As noted by Kirk and Wakefield (2018), the scholarly community also needs to develop a much more robust understanding of the intervening mechanisms that link criminal justice contact to diminished individual outcomes, particularly if we wish to meaningfully contribute to larger conversations about creating a better criminal justice system.

Appendix

Table A3.1. Logit Models Predicting Any Receipt of Safety Net Benefits with Year Fixed Effects by Program, Black Respondents Only

	(1) AFDC/TANF	(2) SSI	(3) Disability	(4) Unemployment	(5) Food Stamps	(6) EITC
Ever Incarcerated	0.01 (0.24)	0.02 (0.14)	-0.43* (0.17)	-0.27† (0.14)	0.08 (0.12)	0.04 (0.14)
In Jail (lagged)	-1.84*** (0.41)	-1.89*** (0.19)	-1.44** (0.44)	-1.33*** (0.33)	-1.60*** (0.15)	-0.21 (0.30)
Age	0.04** (0.02)	0.08*** (0.02)	0.03 (0.02)	0.04** (0.01)	0.15*** (0.01)	-0.04* (0.02)
Female	2.17*** (0.12)	0.22** (0.08)	-0.20† (0.10)	-0.09 (0.06)	1.58*** (0.07)	0.80*** (0.09)
Education						
Less than high school	-0.31*** (0.09)	0.31*** (0.08)	-0.23† (0.14)	-0.57*** (0.10)	0.18** (0.07)	-0.19 (0.12)
Some college	-0.39*** (0.10)	-0.34*** (0.10)	0.17 (0.12)	-0.25*** (0.07)	-0.56*** (0.08)	-0.04 (0.10)
B.A. or higher	-0.69** (0.24)	-0.39* (0.19)	0.02 (0.17)	-0.37*** (0.10)	-1.23*** (0.15)	-0.50** (0.18)
Region						
Northeast	0.22† (0.12)	0.97*** (0.10)	-0.39** (0.15)	0.11 (0.08)	0.25** (0.08)	-0.53*** (0.13)
North Central	1.06*** (0.09)	0.94*** (0.09)	-0.42** (0.14)	0.23** (0.08)	0.78*** (0.07)	-0.24* (0.10)
West	1.04*** (0.14)	0.28* (0.12)	-0.23 (0.17)	0.15 (0.10)	0.16 (0.11)	-0.52** (0.19)
Eligibility-Related Characteristics						
Married (lagged)	-1.21*** (0.13)					-0.33*** (0.09)
Number of children (lagged)	0.78*** (0.04)					0.46*** (0.04)
Household size (lagged)					-0.03** (0.01)	
Disabled (lagged)		1.10*** (0.06)	1.33*** (0.09)	-0.36*** (0.10)		
Non-retirement account financial assets (lagged)	-0.00 (0.00)	0.00 (0.00)			-0.00 (0.00)	
Personal labor income		-0.03*** (0.00)	-0.01*** (0.00)	-0.02*** (0.00)		
Lagged personal labor income		-0.02*** (0.00)	0.01** (0.00)	0.01*** (0.00)		

Table A3.2. Logit Models Predicting Any Receipt of Safety Net Benefits with Year Fixed Effects by Program, White Respondents Only

	(1)	(2)	(3)	(4)	(5)	(6)
	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Ever Incarcerated	0.87** (0.26)	0.71*** (0.18)	-0.43 (0.27)	-0.46† (0.24)	1.12*** (0.18)	0.24 (0.32)
In Jail (lagged)	-1.69*** (0.41)	-1.52*** (0.34)	-1.34 (1.07)	-1.84* (0.91)	-2.00*** (0.24)	-0.49 (0.71)
Age	0.07** (0.02)	0.06*** (0.02)	0.04 (0.02)	0.06*** (0.01)	0.18*** (0.02)	-0.01 (0.02)
Female	1.13*** (0.11)	-0.38*** (0.10)	-0.05 (0.12)	-0.40*** (0.05)	0.70*** (0.07)	0.28* (0.11)
Education						
Less than high school	0.21† (0.12)	0.40*** (0.11)	-0.40* (0.18)	-0.46*** (0.08)	0.48*** (0.08)	0.44** (0.15)
Some college	-0.82*** (0.15)	-0.42*** (0.12)	0.06 (0.13)	-0.41*** (0.06)	-0.89*** (0.09)	0.02 (0.13)
B.A. or higher	-2.19*** (0.31)	-1.43*** (0.21)	-0.70*** (0.16)	-0.98*** (0.08)	-2.15*** (0.19)	-0.77*** (0.18)
Region						
Northeast	0.31† (0.19)	0.91*** (0.13)	-0.07 (0.15)	0.36*** (0.07)	0.09 (0.11)	-0.22 (0.16)
North Central	1.36*** (0.13)	0.66*** (0.11)	-0.03 (0.13)	0.41*** (0.07)	0.47*** (0.08)	-0.01 (0.12)
West	1.18*** (0.14)	0.56*** (0.13)	-0.07 (0.15)	0.42*** (0.07)	0.20* (0.09)	-0.00 (0.14)
Eligibility-Related Characteristics						
Married (lagged)	-0.80*** (0.12)					-0.57*** (0.11)
Number of children (lagged)	0.90*** (0.05)					0.56*** (0.05)
Household size (lagged)					0.03† (0.02)	
Disabled (lagged)		1.32*** (0.08)	1.58*** (0.10)	-0.16† (0.09)		
Non-retirement account financial assets (lagged)	-0.00* (0.00)	-0.00 (0.00)			-0.00* (0.00)	
Personal labor income		-0.03*** (0.00)	-0.01*** (0.00)	-0.02*** (0.00)		
Lagged personal labor income		-0.02*** (0.00)	0.01** (0.00)	0.01*** (0.00)		

Table A3-3. Regression Models Predicting Benefit Amount Conditional on Any Receipt with Year Fixed Effects, by Program

	(1)	(2)	(3)	(4)	(5)	(6)
	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Ever Incarcerated	433.08 (677.44)	-680.08* (325.42)	-485.75** (185.37)	-314.32 (270.11)	-303.51* (136.34)	170.42 (262.52)
In Jail (lagged)	-1,441.21 (899.85)	-956.17 (696.98)	238.85 (644.88)	-1,286.36 (1,055.94)	-897.21*** (194.50)	39.87 (439.23)
Age	78.38 (52.15)	70.22 (49.64)	21.21 (32.94)	109.44** (38.85)	74.48*** (22.24)	-18.33 (15.22)
Female	-1,136.90** (362.83)	445.95* (223.02)	-35.44 (153.89)	-111.77 (253.38)	286.29** (109.00)	115.27 (82.87)
Race						
Black	773.63** (284.27)	-141.80 (266.16)	-336.03† (171.59)	-137.13 (162.86)	293.75** (94.93)	134.03 (88.89)
Hispanic	577.50† (323.26)	649.11† (389.43)	-63.08 (227.73)	405.61 (417.57)	406.01** (133.32)	409.09*** (123.61)
Other	1,140.64** (426.78)	86.26 (376.78)	359.00 (292.01)	157.98 (206.05)	160.66 (127.11)	349.68** (131.17)
Education						
Less than high school	59.36 (227.11)	-53.53 (214.79)	-114.00 (188.45)	-307.83† (165.07)	346.34*** (90.03)	168.42† (101.59)
Some college	110.97 (314.85)	-73.95 (368.33)	217.90 (182.81)	302.25 (271.93)	-330.34*** (91.66)	-149.13† (87.46)
B.A. or higher	-1,169.23* (464.93)	1,914.89 (1,275.91)	745.89* (291.97)	-10.62 (222.17)	-580.77*** (152.66)	-279.79 (218.33)
Region						
Northeast	4,336.41*** (418.99)	1,281.29*** (306.84)	218.97 (223.91)	1,790.94*** (365.02)	-455.77*** (128.19)	41.82 (190.81)
North Central	2,533.72*** (240.04)	310.02 (257.26)	49.57 (190.95)	655.56*** (139.24)	-487.14*** (105.89)	-83.08 (89.34)
West	5,451.75*** (317.44)	1,472.49*** (331.43)	177.24 (230.26)	105.23 (213.76)	-863.85*** (121.05)	-260.16* (101.50)
Eligibility-Related Characteristics						
Married (lagged)	-985.57*** (292.46)					-31.69 (79.62)
Number of children (lagged)	1,048.08*** (96.77)					285.39*** (31.53)
Household size (lagged)					343.90*** (26.90)	

Table A3.3. (Continued)

Disabled (lagged)	115.41 (250.47)	7.49 (227.87)	-34.06 (286.15)	
Non-retirement account financial assets (lagged)	-0.21 (1.22)		-0.20 (0.50)	
Personal labor income	16.42 (13.81)	-5.04 (4.27)	-21.24*** (6.35)	
Lagged personal labor income	4.44 (10.98)	11.25** (4.28)	64.44*** (6.71)	
Household labor income				-24.11*** (2.89)
Lagged household labor income				1.51 (1.99)
Work History				
% weeks worked since last interview				
Weeks worked in past year	-54.84*** (7.50)	6.00 (9.27)	463.80 (815.43)	-10.01*** (1.96)
Weeks unemployed in past year			148.88*** (22.43)	
Weeks out of labor force in past year		21.74* (9.88)	79.15*** (13.65)	
Weeks active duty military in past year			-82.09*** (13.38)	
Likely UI eligible job at last survey			-399.20 (498.96)	
Lagged likely UI eligible job			514.67*** (122.22)	
Employment Sector (lagged)				
Not currently employed				-182.93 (505.95)
Government				86.99 (394.25)
Self-employed or family business				935.25* (411.87)
Military				202.78 (574.56)
<i>Observations (person-years)</i>				10,220
				8441
				3,108
				20,514
				3,638

*** p<0.001, ** p<0.01, * p<0.05, † p<0.10

Note: Standard errors are clustered at the individual level. Year fixed effects are included in all models but not displayed above. Financial covariates (income, assets) are measured in thousands and adjusted for inflation to 2014 values. Variables noted as lagged above are lagged one period.

Table A3.4. Logit Models Predicting Any Receipt of Safety Net Benefits with Year Fixed Effects by Program, Male Respondents Only

	(1)	(2)	(3)	(4)	(5)	(6)
	AFDC/TANF	SSI	Disability	Unemployment	Food Stamps	EITC
Ever Incarcerated	0.69*** (0.16)	0.14 (0.11)	-0.48*** (0.13)	-0.20 [†] (0.11)	0.48*** (0.08)	0.13 (0.12)
In Jail (lagged)	-1.23*** (0.22)	-1.70*** (0.15)	-1.72*** (0.40)	-1.66*** (0.26)	-1.49*** (0.11)	-0.32 (0.26)
Age	-0.01 (0.02)	0.08*** (0.02)	0.06*** (0.02)	0.07*** (0.01)	0.13*** (0.01)	-0.06** (0.02)
Race						
Black	-0.15 (0.14)	0.35*** (0.10)	0.05 (0.11)	-0.36*** (0.06)	-0.20** (0.08)	0.02 (0.11)
Hispanic	0.02 (0.15)	-0.01 (0.13)	-0.14 (0.13)	-0.07 (0.06)	0.11 (0.09)	-0.07 (0.13)
Other	0.19 (0.18)	0.17 (0.14)	0.32* (0.15)	0.02 (0.08)	0.20 [†] (0.10)	0.05 (0.17)
Education						
Less than high school	-0.05 (0.12)	0.21* (0.09)	-0.19 (0.12)	-0.41*** (0.06)	0.32*** (0.07)	-0.12 (0.11)
Some college	-0.78*** (0.16)	-0.40*** (0.11)	0.08 (0.11)	-0.39*** (0.05)	-0.48*** (0.09)	-0.15 (0.12)
B.A. or higher	-1.99*** (0.41)	-0.89*** (0.19)	-0.22 (0.14)	-1.11*** (0.08)	-1.38*** (0.19)	-0.85*** (0.22)
Region						
Northeast	0.47** (0.18)	1.15*** (0.12)	-0.23 [†] (0.13)	0.20** (0.06)	-0.00 (0.09)	-0.49*** (0.14)
North Central	1.29*** (0.14)	1.25*** (0.11)	-0.07 (0.11)	0.46*** (0.06)	0.45*** (0.07)	-0.08 (0.11)
West	0.96*** (0.15)	0.72*** (0.12)	0.01 (0.11)	0.37*** (0.06)	-0.07 (0.09)	-0.13 (0.12)
<u>Eligibility-Related Characteristics</u>						
Married (lagged)	1.12*** (0.14)					0.11 (0.10)
Number of children (lagged)	0.57*** (0.04)					0.46*** (0.04)
Household size (lagged)					0.03* (0.01)	
Disabled (lagged)		1.25*** (0.07)	1.34*** (0.09)	-0.34*** (0.08)		

Table A3.4. (Continued)

Non-retirement account financial assets (lagged)	-0.00 (0.00)	-0.00 (0.00)	-0.01*** (0.00)	-0.02*** (0.00)	-0.00* (0.00)
Personal labor income			-0.01*** (0.00)	-0.02*** (0.00)	
Lagged personal labor income			0.01*** (0.00)	0.01*** (0.00)	
Household labor income					-0.05*** (0.00)
Lagged household labor income		-0.06*** (0.00)			-0.01*** (0.00)
Work History					
% weeks worked since last interview				-0.75*** (0.10)	
Weeks worked in past year		-0.01*** (0.00)	-0.00 (0.00)		-0.01*** (0.00)
Weeks unemployed in past year				0.05*** (0.00)	
Weeks out of labor force in past year			0.03*** (0.00)		
Weeks active duty military in past year			0.00† (0.00)	-0.04*** (0.00)	
Likely UI eligible job at last survey				0.28*** (0.05)	
Lagged likely UI eligible job				0.38*** (0.05)	
Employment Sector (lagged)					
Not currently employed				-1.40*** (0.09)	
Government				-0.95*** (0.08)	
Self-employed or family business				-1.28*** (0.10)	
Military				0.98*** (0.15)	
	<i>Observations (person-years)</i>	116,973	116,758	25,454	116,686
					4,783

*** p<0.001, ** p<0.01, * p<0.05, † p<0.10

Note: Coefficients represent log odds. Standard errors are clustered at the individual level. Year fixed effects are included in all models but not displayed above. Financial covariates (income, assets) are measured in thousands and adjusted for inflation to 2014 values. Variables noted as lagged above are lagged one period.

Table A3-5. Logit Models Predicting Any Receipt of Safety Net Benefits with Year Fixed Effects by Program, Eligibility Restricted Samples

	(1) AFDC/TANF	(2) SSI	(3) Disability	(4) Unemployment	(5) Food Stamps	(6) ETTC
Ever Incarcerated	0.52*** (0.16)	0.05 (0.11)	-0.39** (0.12)	-0.30** (0.10)	0.23** (0.09)	0.05 (0.11)
In Jail (lagged)	-1.52*** (0.22)	-1.82*** (0.17)	-1.51*** (0.38)	-1.34*** (0.25)	-1.64*** (0.11)	-0.38 (0.24)
Age	0.02 (0.01)	0.04** (0.01)	0.05** (0.01)	0.07*** (0.01)	0.12*** (0.01)	-0.04*** (0.01)
Female	1.65*** (0.07)	-0.09 (0.07)	-0.31*** (0.07)	-0.22*** (0.03)	1.12*** (0.05)	0.53*** (0.06)
Race						
Black	0.32*** (0.07)	0.62*** (0.08)	-0.05 (0.08)	-0.21*** (0.04)	0.23*** (0.05)	0.28*** (0.07)
Hispanic	-0.07 (0.09)	0.23* (0.10)	-0.21* (0.10)	0.01 (0.05)	0.15* (0.07)	0.00 (0.08)
Other	0.10 (0.10)	0.21† (0.11)	0.12 (0.12)	0.15** (0.05)	0.15† (0.08)	0.15 (0.10)
Education						
Less than high school	-0.08 (0.06)	0.13† (0.07)	-0.24** (0.09)	-0.40*** (0.04)	0.24*** (0.05)	-0.07 (0.07)
Some college	-0.43*** (0.08)	-0.14 (0.09)	0.21* (0.08)	-0.38*** (0.04)	-0.50*** (0.06)	-0.03 (0.07)
B.A. or higher	-1.07*** (0.22)	-0.46* (0.20)	-0.12 (0.12)	-0.92*** (0.06)	-1.37*** (0.16)	-0.68*** (0.11)
Region						
Northeast	0.49*** (0.09)	0.96*** (0.09)	-0.29** (0.10)	0.18*** (0.05)	0.18** (0.06)	-0.50*** (0.09)
North Central	1.14*** (0.07)	0.76*** (0.08)	-0.16† (0.09)	0.34*** (0.04)	0.56*** (0.05)	-0.15* (0.07)
West	1.03*** (0.08)	0.33*** (0.09)	-0.12 (0.09)	0.36*** (0.04)	0.09 (0.06)	-0.16† (0.08)
<u>Eligibility-Related Characteristics</u>						
Married (lagged)	-0.56*** (0.07)					-0.38*** (0.06)
Number of children (lagged)	0.81*** (0.03)					0.49*** (0.02)
Household size (lagged)					0.01 (0.01)	

Table A3.5. (Continued)

Disabled (lagged)	1.01*** (0.05)	1.18*** (0.06)	-0.29*** (0.05)	
Non-retirement account financial assets (lagged)	0.00 (0.00)		-0.01 (0.00)	-0.00 (0.00)
Personal labor income	-0.05*** (0.00)	-0.01*** (0.00)		
Lagged personal labor income	-0.03*** (0.00)	0.01*** (0.00)	0.02*** (0.00)	
Household labor income				-0.04*** (0.00)
Lagged household labor income	-0.09*** (0.00)			
Work History				
% weeks worked since last interview			0.04 (0.06)	
Weeks worked in past year	-0.01*** (0.00)	-0.00 (0.00)		-0.02*** (0.00)
Weeks unemployed in past year			0.05*** (0.00)	
Weeks out of labor force in past year	0.01*** (0.00)	0.02*** (0.00)	-0.02*** (0.00)	
Weeks active duty military in past year			-0.03*** (0.00)	
Likely UI eligible job at last survey			0.56*** (0.04)	
Lagged likely UI eligible job			0.51*** (0.03)	
Employment Sector (lagged)				
Not currently employed			-0.96*** (0.06)	
Government			-0.52*** (0.06)	
Self-employed or family business			-1.31*** (0.09)	
Military			1.10*** (0.14)	
	<i>Observations (person-years)</i>	95,310	59,374	27,697
		126,232	94,821	12,153

*** p<0.001, ** p<0.01, * p<0.05, † p<0.10

Note: Standard errors are clustered at the individual level. Year fixed effects are included in all models but not displayed above. Financial covariates (income, assets) are measured in thousands and adjusted for inflation to 2014 values. Variables noted as lagged above are lagged one period.

Table A3.6. Estimated Differences in Log Odds of Safety Net Benefit Receipt between Formerly-Incarcerated and Never-Incarcerated Individuals

	Standard Models (Table 2)	+ Behavioral Controls	Comparison Group Restricted to:		
			Hard Drug Users (1980)	Charged with Crime (1980)	Convicted of Crime (1980)
AFDC/TANF (1979-2014)	0.43**	0.43**	0.29	0.38*	0.35*
std. error	(0.15)	(0.16)	(0.19)	(0.17)	(0.17)
N	237,256	220,017	31,801	27,747	18,673
SSI (1979-2014)	0.24*	0.09	0.38*	0.25†	0.43**
std. error	(0.10)	(0.11)	(0.15)	(0.13)	(0.15)
N	237,256	219,681	31,722	27,666	18,608
Disability (2002-2014)	-0.40***	-0.42**	-0.20	-0.39*	-0.42*
std. error	(0.12)	(0.13)	(0.17)	(0.16)	(0.18)
N	52,512	48,744	7,774	6,597	4,826
Unemployment Insurance (1979-2014)	-0.28**	-0.30**	-0.32**	-0.22†	-0.21
std. error	(0.10)	(0.11)	(0.12)	(0.12)	(0.14)
N	224,555	208,414	30,412	26,471	17,968
Food Stamps/SNAP (1979-2014)	0.31***	0.18†	0.29**	0.29**	0.32**
std. error	(0.08)	(0.09)	(0.11)	(0.10)	(0.11)
N	236,681	219,527	31,674	27,594	18,560
EITC (2000-2012)	0.05	0.01	0.12	0.05	0.20
std. error	(0.11)	(0.12)	(0.19)	(0.16)	(0.19)
N	12,153	11,169	1,736	1,653	1,178
Controls					
Drug user (1980)	No	Yes	No	No	No
Ever been in juvenile detention (1980)	No	Yes	No	No	No
Delinquency index score (1980)	No	Yes	No	No	No
Comparison Group					
Drug user (1980)	No	No	Yes	No	No
Charged with crime (1980)	No	No	No	Yes	No
Convicted of crime (1980)	No	No	No	No	Yes

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1

Notes: Coefficients and standard errors reported above are those for the *Previously Incarcerated* dummy variable in all regressions. Coefficients represent log odds. Standard errors are clustered at the individual level. Year fixed effects are included in all models.

Figure A4.1. Incarceration History within the NLSY79 Sample by Gender and Year

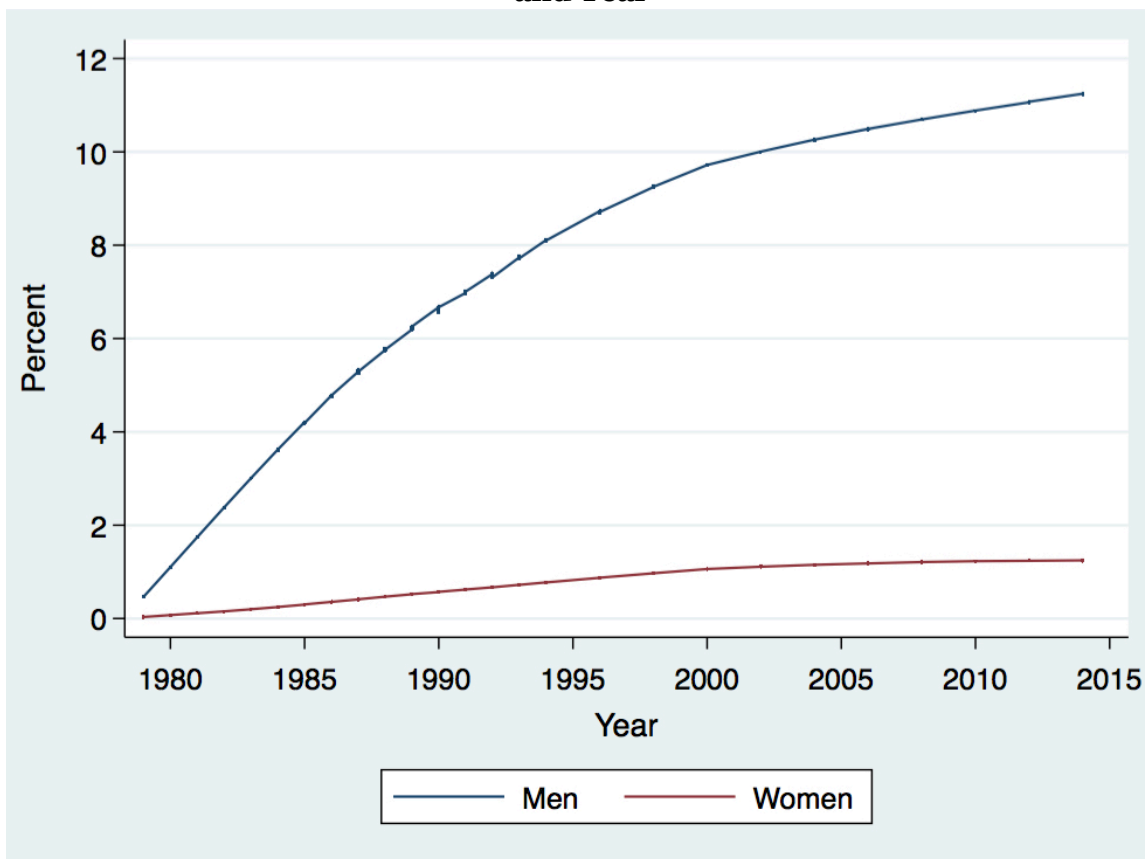


Table A4.1. Differences in Weeks Worked in Past Calendar Year by Incarceration History

	OLS	OLS	Fixed Effects
Will ever be incarcerated	-5.415*** (0.569)	-3.097*** (0.575)	
Previously incarcerated	-9.732*** (0.808)	-9.589*** (0.803)	-9.263*** (0.350)
Currently incarcerated	-12.29*** (0.738)	-11.85*** (0.745)	-8.294*** (0.406)
Race/ethnicity			
Black		-4.888*** (0.343)	
Hispanic		-1.033** (0.380)	
Other		-0.816* (0.408)	
Highest degree completed			
High school diploma or GED		2.070*** (0.396)	0.952*** (0.236)
Some college/Associate's		2.469*** (0.440)	2.786*** (0.295)
Bachelor's		5.404*** (0.452)	8.932*** (0.358)
Constant	0.699*** (0.111)	1.843*** (0.183)	0.754 (0.675)
<i>Observations (person-years)</i>	122,901	121,114	121,114
<i>Respondents</i>			6,351

*** p<0.001, ** p<0.01, * p<0.05

Note: Age fixed effects included in all models. Standard errors are clustered at the individual level.

Table A4.2. Differences in Log Odds of Marriage by Incarceration History

	Logit	Logit	Fixed Effects
Will ever be incarcerated	-0.805*** (0.103)	-0.500*** (0.106)	
Previously incarcerated	-0.606*** (0.126)	-0.617*** (0.128)	-1.654*** (0.0868)
Currently incarcerated	-0.816*** (0.131)	-0.811*** (0.133)	-0.735*** (0.107)
Race/ethnicity			
Black		-0.855*** (0.0511)	
Hispanic		-0.119* (0.0571)	
Other		0.104 (0.0626)	
Highest degree completed			
High school diploma or GED		0.160** (0.0549)	-0.0728 (0.0716)
Some college/Associate's		-0.0137 (0.0620)	-0.220* (0.0925)
Bachelor's		0.281*** (0.0683)	1.097*** (0.117)
Constant	0.179 (0.288)	0.329 (0.300)	
<i>Observations (person-years)</i>	121,079	119,292	92,309
<i>Respondents</i>			4,471

*** p<0.001, ** p<0.01, * p<0.05

Note: Age fixed effects included in all models. Standard errors are clustered at the individual level.

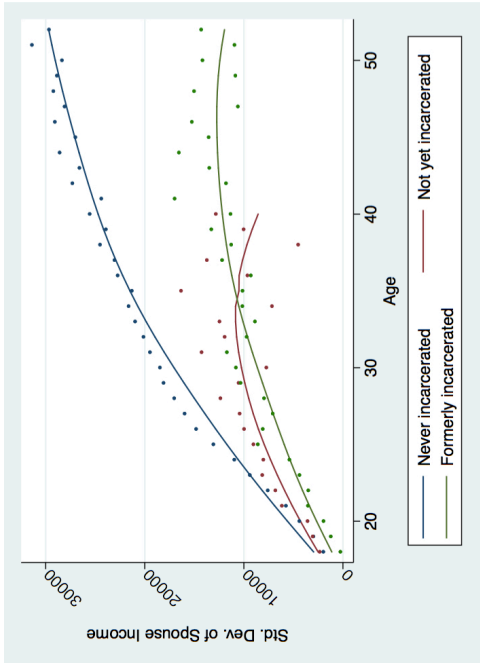
Table A4.3. Differences in Log Odds of Disability by Incarceration History

	Logit	Logit	Fixed Effects
Will ever be incarcerated	0.424*** (0.113)	0.155 (0.119)	
Previously incarcerated	0.552*** (0.125)	0.463*** (0.128)	0.391*** (0.0978)
Currently incarcerated	-0.437*** (0.121)	-0.405** (0.127)	-0.428*** (0.103)
Race/ethnicity			
Black		0.157* (0.0722)	
Hispanic		0.00326 (0.0867)	
Other		-0.0347 (0.101)	
Highest degree completed			
High school diploma or GED		-0.534*** (0.0697)	-0.0965 (0.0764)
Some college/Associate's		-0.780*** (0.0900)	-0.479*** (0.101)
Bachelor's		-1.614*** (0.121)	-1.449*** (0.137)
Constant	-2.881*** (0.197)	-2.874*** (0.200)	
<i>Observations (person-years)</i>	122,843	121,058	50,554
<i>Respondents</i>			2,379

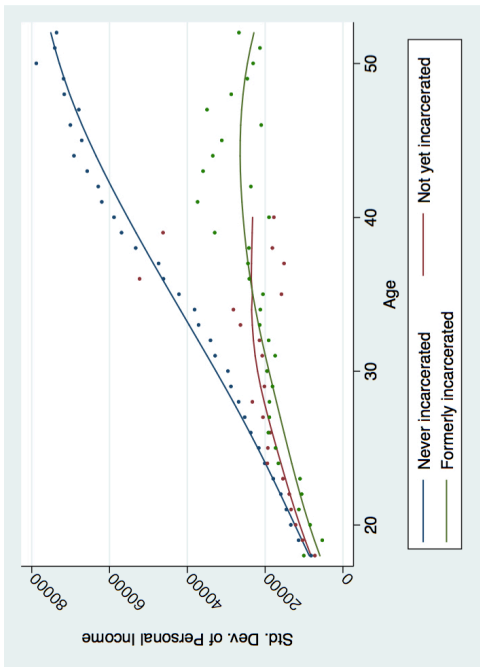
*** p<0.001, ** p<0.01, * p<0.05

Note: Age fixed effects included in all models. Standard errors are clustered at the individual level.

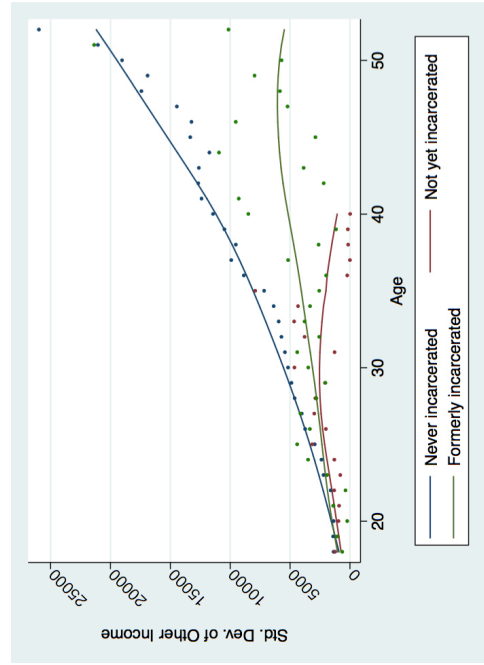
Figure A4.2. Standard Deviation of Income by Type, Incarceration History, and Age



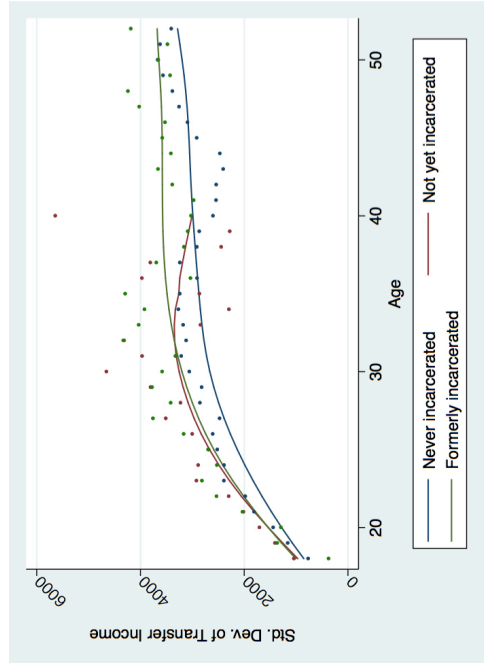
(a) Personal Income



(b) Spouse/Partner Income



(c) Transfer Income



(d) Other Income

Note: Dots display variance of income at each age for each group. Lines are lowess-smoothed. Income variance is only displayed through age 40 for the not-yet-incarcerated group because only 6 percent of ever-incarcerated respondents are first observed in prison or jail after age 40.

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