Housing Instability Following Felony Conviction and Incarceration: Disentangling Being Marked from Being Locked Up

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Running head: Housing instability following felony conviction and incarceration

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Abstract

Objectives: I examine housing instability among individuals with a felony conviction but no incarceration history relative to formerly incarcerated individuals as a means of separating the effect of felon status from that of incarceration per se—a distinction often neglected in prior research. I consider mechanisms and whether this relationship varies based on gender, race/ethnicity, time since conviction, and type of offense.

Methods: I use National Longitudinal Survey of Youth 1997 data and restricted comparison group, individual fixed effects, and sibling fixed effects models to examine residential mobility and temporary housing residence during early adulthood.

Results: I find robust evidence that never-incarcerated individuals with felony convictions experience elevated risk of housing instability and residential mobility, even after adjusting for important mediators like financial resources and relationships. The evidence that incarceration has an additional, independent effect on housing instability is weaker, however, suggesting that the association between incarceration and housing instability found in prior studies may largely be driven by conviction status.

Conclusions: These findings reveal that conviction, independent of incarceration, introduces instability into the lives of the 12 million Americans who have been convicted of a felony but never imprisoned. Thus, research that attempts to identify an incarceration effect by comparing outcomes to convicted individuals who receive non-custodial sentences may obscure the important independent effect of conviction. Moreover, these findings highlight that the socioeconomic effects of criminal justice contact are broader than incarceration-focused research indicates. Consequently, reform efforts promoting the use of community corrections over incarceration may do less to reduce the harm of criminal justice contact than expected.

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Significant scholarly and political attention has considered the consequences of mass incarceration in the United States, with researchers and advocates alike pointing to sharply increased incarceration rates over the last five decades and the consequently large population of previously incarcerated individuals as cause for concern (Charles Koch Institute, 2019; National Research Council, 2014). In response, a large research literature has considered whether incarceration may disrupt the life course and lead to subsequent disadvantage across a multitude of domains ranging from health to socioeconomic status to family well-being (Adams, 2018; Massoglia & Pridemore, 2015; National Research Council, 2014; Western et al., 2015). Because incarceration is concentrated among racial minorities and less educated Americans, scholars have argued that exposure to incarceration may not only exacerbate disadvantage in the individual life course but may also contribute to the production and reproduction of inequality at the societal level (National Research Council, 2014; Wakefield & Uggen, 2010; Western & Pettit, 2010).

Our understanding of the consequences of non-custodial interactions with the justice system in the current correctional climate is far less developed, however. While criminological research in the 20th century often dealt with the secondary consequences of arrest and conviction (e.g., Boshier & Johnson, 1974; S. D. Bushway, 1998; Nagin & Waldfogel, 1998; Schwartz & Skolnick, 1962), researchers have primarily focused on incarceration over the past two decades. Recently, however, scholars have drawn attention to the size of the population under community supervision (Phelps, 2013) or with felony conviction records (Shannon et al., 2017), and some recent work has investigated how lower level interactions with the justice system may affect subsequent outcomes. Papers using National Longitudinal Survey of Youth 1997 data, for example, highlight how arrest and conviction may lead to some of the same types of health and economic disadvantages associated with prior incarceration (Bryan, 2020; Maroto & Sykes, 2019; Sugie & Turney, 2017; Warner & Remster, 2021). Other researchers have used audit studies to gauge the effect of arrests and convictions in employment and rental housing markets (Evans, 2016; Evans & Porter, 2015; Leasure & Martin, 2017; Uggen et al., 2014). This paper contributes to the recent literature on consequences of non-custodial justice system interactions by using nationally representative data to examine the relationship between conviction *without incarceration* and housing stability, an outcome that has been much less explored in the research literature on consequences of criminal justice contact but which several scholars suggest is greatly diminished by incarceration (Geller & Curtis, 2011; Herbert et al., 2015; Warner, 2015).

Other papers have considered the experiences of non-incarcerated, convicted individuals but only for the purpose of better identifying the causal effect of incarceration on outcomes like employment and recidivism (Apel & Sweeten, 2010; Green & Winik, 2010; Harding et al., 2017; Loeffler, 2013). Rather than simply using never-incarcerated individuals with felony convictions as a reference group to aid in estimating the effect of imprisonment, however, I consider this group as a treatment group of interest. Comparing individuals who have been convicted of a felony but never incarcerated to both formerly incarcerated individuals *and* never-convicted individuals allows us to disentangle the effect of felon status per se from that of incarceration and all of the intermediary effects that accompany it (e.g., removal from labor market, weakened social ties, health effects). Thus, this paper considers (a) whether conviction may be independently disruptive to housing stability despite being a much less intensive treatment than incarceration and (b) how much of an independent effect incarceration appears to have above and beyond that of conviction.

Moreover, because I rely upon a rich, longitudinal dataset – the National Longitudinal Survey of Youth 1997 – I can consider not just the role of felony conviction versus incarceration but also the mechanisms that link criminal justice contact to housing instability – something that is difficult to do with administrative or experimental data. Using a variety of modeling strategies to account for potential confounders, including individual and sibling fixed effects, I find that never-incarcerated individuals with felony convictions experience elevated risk of housing instability and residential mobility even after adjusting for important mediators like financial resources and relationships. I also find that this relationship appears to be amplified for women.

These findings highlight that the challenges faced by the formerly incarcerated population derive not only from their actual incarceration but also from the fact that they are marked as felons, particularly in this moment when criminal records are so easily searched online. Consequently, research studies that attempt to identify the effect of incarceration by comparing outcomes to convicted individuals who do not receive custodial sentences may obscure the fact that conviction has an important independent effect.

Given that individuals with felony convictions but no history of imprisonment outnumber formerly incarcerated Americans by roughly two-to-one (Shannon et al., 2017), these findings also highlight that the socioeconomic effects of criminal justice contact are much broader than the incarceration-focused research suggests. Consequently, criminal justice reform efforts promoting the use of community supervision in lieu of incarceration may do less to reduce the informal harms of justice system contact than reformers expect. As such, reformers and policymakers concerned about the challenge of social (re)integration following criminal justice system contact ought to expand beyond a narrow focus on prison reentry to consider the challenges faced by the millions of Americans who bear the stigma of felon status but have never passed through a prison gate.

Felony Convictions and the Stigma of a Felony Record

In 2006, the most recent year for which data are available, 1.2 million individuals were convicted of a felony in the United States (Rosenmerkel et al., 2009). While most felony convictions lead to a prison or jail sentence, approximately 30 percent do not (Rosenmerkel et al., 2009). Despite avoiding incarceration, however, these individuals do acquire a felony record (but see Chiricos et al., 2007). Recent estimates put the number of Americans who have been convicted of a felony crime but never served time in prison at 11.7 million, or 5 percent of the adult population – more than double the number of people who have been previously incarcerated (Shannon et al., 2017).[2]

While incarceration marks the most serious form of punishment, prior felony conviction status is associated with a broad range of disadvantages and prohibitions that may follow individuals for many years and that appear to contribute to the more severe stigma attached to felony versus misdemeanor conviction history (Uggen et al., 2014). Individuals with felony convictions, particularly for drug crimes, can be denied access to a wide variety of rights and benefits, ranging from voting and jury service to postsecondary education assistance (American Bar Association, 2009; GAO, 2005; Uggen et al., 2006). They may also be denied housing, employment, and occupational licenses in most states due to their conviction records (Legal Action Center, 2004; Love et al., 2018). Moreover, the easy accessibility of criminal background checks (S. Bushway et al., 2007; Lageson, 2016) means gatekeepers in both the housing and labor markets can and do discriminate on "criminal history" broadly, not just prior incarceration (Holzer et al., 2007; Uggen et al., 2014).

Although these 12 million Americans with felony records but no incarceration history are likely to experience significant repercussions, their experiences have received far less attention

[2] Calculated from Shannon et al. (2017) by subtracting 2010 "Total in Prison or on Parole" estimated count in Table 1 from the 2010 "Total Felons" estimated count in Table 2.

than those of formerly incarcerated individuals (National Research Council, 2014). When they have been included in studies it has generally been either as part of a monolithic "felon" category that overlooks incarceration history (e.g., Leasure & Martin, 2017; Uggen & Manza, 2002) or as a comparison group for individuals receiving custodial sentences for the purpose of estimating a causal incarceration effect (Apel & Sweeten, 2010; Green & Winik, 2010; Harding et al., 2017; Loeffler, 2013).

At the same time, studies of post-incarceration outcomes have often attributed the negative effects they find at least in part to the stigma of felon status (e.g., Pager, 2007) but been unable to fully disentangle the effect of *felon status* in its own right from the intermediate (e.g., loss of work experience) or the potentially enduring (e.g., anxiety in crowds) effects of *incarceration*. Only two prior studies have attempted to isolate the effect of "felon" status per se (Chiricos et al., 2007; Waldfogel, 1994). Using administrative records on convicted individuals and clever designs, these studies find compelling evidence that acquiring felon status and the stigma of conviction have important detrimental effects, independent of the effects of incarceration, on subsequent recidivism, employment, and income. Studies that rely on administrative data, however, are necessarily limited in their ability to explore mediating and confounding factors, as well as in the generalizability of their findings. The NLSY97 data, on the other hand, allow me to employ a rich set of covariates and a variety of modelling strategies to consider felony stigma and other mechanisms in a nationally representative sample. Moreover, NLSY97 data allow me to consider an outcome less easily measured in administrative data: housing instability.

While discussions about the stigma of a criminal record often focus on the labor market, housing offers an opportune context to explore this topic because it, too, is a market with gatekeepers and high potential for stigma and discrimination to play a role, but there are fewer legal prohibitions[3] limiting options than in the employment market, where occupational licensure restrictions summarily exclude individuals with felony convictions from many lines of work. Moreover, housing has been a relatively understudied outcome in the research literature on collateral consequences of criminal justice contact. I review this literature below before entering into a fuller discussion of potential mechanisms linking felony conviction to housing instability.

Housing Challenges Following Criminal Justice System Exit

Prior research finds that incarceration is associated with increased housing instability, particularly in terms of number of residential moves, and decreased neighborhood quality, primarily for whites (Geller & Curtis, 2011; Harding et al., 2013; Massoglia et al., 2013; Warner, 2015, 2016). While selection bias threatens causal inference for this population, papers relying on individual fixed effects models provide compelling evidence that the relationship between incarceration and residential mobility, as well as neighborhood attainment, may be causal (Massoglia et al., 2013; Warner, 2015). These papers do not, however, explore how much of this apparently causal effect may result from felon status as opposed to incarceration itself.

The literature on housing instability as an outcome is relatively sparse, but a variety of studies suggest that housing instability may be predictive of other outcomes relevant to individuals' quality of life and opportunities. Among the formerly imprisoned population, homelessness and greater residential mobility following release are associated with higher risk of rearrest and reincarceration (Lutze et al., 2014; Metraux & Culhane, 2004; Steiner et al., 2015). Health-focused research, on the other hand, has linked housing instability to poorer health care

[3] Some individuals may face residential geographic constraints as part of their probation or parole terms, and public housing authorities are required to deny applicants who are on the lifetime sex offender registry in any state or have been convicted of manufacturing methamphetamines on public housing property (Curtis et al., 2013).

and contraceptive access (Clark et al., 2021; Kushel et al., 2006; Reid et al., 2008), lower birthweight among pregnant mothers (Carrion et al., 2015), and greater incidence of depression and generalized anxiety disorder among women (Suglia et al., 2011).

Moreover, housing instability is an important form of social exclusion (Foster & Hagan, 2007; Lee et al., 2010) that may hinder individuals' ability to achieve stability more generally. Edin and Shaefer (2015, p. 55) offer qualitative evidence that 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 gain and maintain access to resources like cash assistance, food stamps, and even internet access at the local library (Desmond, 2016, pp. 63, 216; Edin & Shaefer, 2015, p. 100). Additionally, unstable housing may hinder the ability of justice-system-involved individuals to foster and maintain the pro-social family relationships that are crucial to desistance (Laub & Sampson, 2001, 2006). For example, Western and Smith (2018) find that unstable housing following incarceration is associated with less contact with children.

Despite the importance of housing for individual opportunity and stability, federal law permits public housing authorities and private landlords to reject prospective tenants based on their criminal history. At their discretion, public housing authorities may reject applicants with felony convictions who apply for subsidized units or vouchers, and many housing authorities do so (Curtis et al., 2013; Tran-Leung, 2015). Moreover, in many cities, individuals already living in subsidized housing 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 applicants about their criminal history and run criminal records checks when deciding whether to rent to a prospective tenant. Prior research suggests that they routinely do so, often turning away applicants who reveal felony records (Delgado, 2005; Evans & Porter, 2015; Helfgott, 1997; Leasure & Martin, 2017; Rosen et al., 2021; 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 (Geller & Curtis, 2011; Harding et al., 2013; Warner, 2015), but this group also faces barriers in the form of strained relationships, poor employment history, lack of financial resources, and substance abuse or mental health issues that may have been exacerbated by their incarceration, all of which are likely to affect their ability to find and maintain stable housing (Harding et al., 2019; Petersilia, 2003; Western, 2018). Because the physical removal from one's community entailed by incarceration affects individuals in so many ways and these mediators are difficult to wholly account for in observational data, 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 community removal itself.

A focus on individuals who have been convicted of a felony *but never incarcerated*, however, allows us to disentangle the effect of felon status – and the stigma and ensuing discrimination it is likely to entail – from all of the bundled intermediary effects of incarceration itself, even in the absence of robust measures of mediating factors. If formerly convicted individuals who have never been incarcerated have housing experiences that differ little from those of observably similar never-convicted individuals, then we can assume that it is not the mark of a felony record that increases housing instability among the formerly incarcerated but something about the actual experience of incarceration itself. If, alternatively, formerly-convicted-but-never-incarcerated individuals experience significantly greater housing instability than observably similar never-convicted individuals, then this will provide evidence for an independent effect of felon *status*. In this case we can also test whether incarceration appears to have an additional additive effect above and beyond that of felony conviction. If housing instability does not differ significantly between formerly incarcerated individuals and formerly convicted individuals who have *not* been incarcerated, then that would suggest that the association between incarceration and housing instability observed in prior work is predominately driven by felon status. Thus, in comparing the housing instability experiences of formerly-convicted-but-never-incarcerated individuals to those of formerly incarcerated individuals, this analysis attempts to tease apart the effect of being *marked* by felony conviction from the effect of being *locked up*.

Moreover, the rich longitudinal nature of the NLYS97 data allows for the examination of alternative mechanisms that might affect the ability of justice-system-involved individuals, regardless of their incarceration history, to achieve stable housing. For example, Harris' work makes clear that the financial sanctions imposed by the criminal justice system may severely impact individuals' financial resources (2016), potentially hindering their ability maintain a stable residence even in the absence of incarceration. Additionally, formerly-convicted-but-never-incarcerated individuals are not physically removed from the labor market like incarcerated individuals, but they may face disruptions to employment resulting from activities mandated by probation orders. If they also experience labor market discrimination due to their felony convictions, then their income is likely to be suppressed as a result.

Thus, I also consider other potentially important mechanisms that may alter the ability of individuals with felony convictions to secure and maintain stable housing, regardless of their incarceration history. Namely, I examine the role of skills and work experience, financial resources, relationships, and family background and privilege. Other scholars (e.g., Turney et al., 2012; Turney & Wildeman, 2013) have engaged in similar analyses in trying to understand why prior incarceration is linked to unfavorable outcomes, but by incorporating the experiences of formerly-convicted-but-never-incarcerated individuals into my analysis I am able to add a more formal test of felony stigma and ensuing discrimination. Once I account for these other mechanisms and potential confounders, as discussed below, remaining differences in housing instability between formerly-convicted-but-never-incarcerated individuals and never-convicted individuals would provide firmer 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 (NLSY97), 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.[4] From 1997 to 2011, NLSY97 surveys were conducted annually; as of 2013 data collection is biennial. The most recent survey for which data are available is 2017, at which point sample members were 32-38 years old; 75 percent of original sample members participated.

NLSY97 includes extensive self-reported data on arrests, convictions, and incarceration spells since age 12, which allow me to construct incarceration and conviction histories for all

[4] The NLSY97 includes a nationally representative sample of 6,748 respondents and a supplemental oversample of 2,236 Hispanic and non-Hispanic black respondents.

respondents. To encourage honest reporting by respondents, data on criminal justice contact, criminal behavior, and other sensitive topics are collected through computer-assisted self-interview (Bureau of Labor Statistics, 2019). In each survey wave NLSY97 also collects considerable information about respondents' current housing situation and residential moves that occurred between interview waves. I use information about the type of housing unit each respondent is living in at each survey wave and the number of times the respondent has moved since the last survey wave to gauge respondents' housing stability.

Because NLSY97 respondents are still relatively young as of the most recent survey wave, some may still be in the midst of their criminal careers as of 2017. Criminal offending usually peaks in the late teens, however, and most respondents should have aged out of offending by their late 20s (Hirschi & Gottfredson, 1983; Loeber & Farrington, 2014). Moreover, although respondents were just 32-38 in 2017, the median time since last arrest that led to a felony conviction was 11.8 years, and the median time since release from last incarceration was 7.5 years.

I examine housing instability experiences in the 96,144 person-year observations in which NLSY97 respondents (N=8,705) were age 20 and older. Because the early 20s are a volatile life course period during which housing instability is particularly common (Arnett, 2000; Benetsky et al., 2015; Goldscheider & Goldscheider, 1994), I have also run models on all person-years in which respondents are 25 and older. All results are consistent with those presented in the main analyses and are available upon request.

Housing Instability Outcome Variables

I assess respondent's housing instability based on two time-varying measures: *number of residential moves* since their last interview and an indicator variable set equal to one if the respondent's current dwelling at the time of each interview is some sort of *temporary housing*.

At each survey wave, respondents report the number of different addresses at which they have lived for more than one month 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. Findings with regard to residential mobility are also consistent if I instead use a dichotomous between-wave residential mobility indicator variable as my outcome. The *temporary housing* variable identifies respondents who are currently residing in a hotel, motel, rooming house, boarding house, shelter, hospital, group home or treatment center, or on the street at the time of each survey. Given that residence in a group home or treatment center might be court-mandated or the result of a probation or parole technical violation, I also run models on a version of the temporary housing variable that excludes group home and treatment center residence. Results are consistent with those presented in the main text and tables.

Incarceration & Felony Conviction History Measures

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, research using NLSY79 data to examine consequences of criminal justice system contact, including Warner's research on post-incarceration residential mobility (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. The more detailed NLSY97 data, however, allow the identification of not just formerly incarcerated individuals – including those 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 sales) 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 broad assumptions about the categories of crime that most often qualify as felonies. I code assault, robbery, burglary, theft[5], and drug sales as felonies, excluding destruction of property, "other property crimes"[6], drug possession[7], major traffic offenses, public order offenses[8], parole and probation violations, and the "other offense" catchall category. This is a conservative approach, as the broad crime categories I bundle together as felonies will capture some misdemeanor offenses (e.g., misdemeanor theft). Because legal collateral consequences (e.g., occupational licensure restrictions) rarely accompany misdemeanor convictions (Love et al., 2018) and misdemeanor offenses are less likely to bear the same level of stigma as felony convictions, however, the potential inclusion of misdemeanor convictions in the felony conviction indicator variable is likely to bias the coefficient towards zero. What I necessarily sacrifice in precision about exact charges and sentencing by virtue of using the NSLY97 data I make up for with rich, longitudinal information on both confounding and mediating factors.

In order to gauge the effect of felony conviction independent of incarceration, I create an indicator variable identifying respondents who have a *previous felony conviction* at each survey wave, which I then pair with three additional dummy variables identifying respondents who (1)

[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."
[7] Drug possession is the most serious charge in 14.6% of felony convictions in state courts (Rosenmerkel et al., 2009), therefore I have also run models with drug possession coded as a felony conviction. Results of these models are consistent with those shown here and are available upon request.
[8] While the public order category can include felonies because weapons offenses fall into the public order offenses category, weapons convictions without an accompanying more serious felony conviction make up only 3 percent of all felony convictions in state courts (Rosenmerkel et al., 2009) and 8 percent of federal felony convictions (Schmitt & Jones, 2017).

also have a *previous felony incarceration*, (2) have been *previously incarcerated without a felony conviction*[9], and/or (3) are *currently incarcerated* in any given survey year[10]. The non-felony incarceration dummy variable captures respondents who have only been incarcerated for pretrial detention or misdemeanor offenses, which typically result in shorter jail stays as opposed to the longer prison sentences that typically accompany felony convictions. The inclusion of the *currently incarcerated* indicator ensures that the *previously incarcerated* variable identifies individuals who have completed their incarceration spells. As Figure 1 shows, 10.7 percent of NLSY97 respondents have ever been convicted of a felony by 2017, and four in ten of those respondents (4.2 percent) have never been incarcerated.

[INSERT FIGURE 1 HERE]

I do not separate out non-felony convictions in my models, instead letting respondents who have other convictions fall into the reference category, given my interest in thinking about how felony conviction stigma disadvantages individuals relative to anyone who does not bear that status, even if they have a criminal record of some sort. Thus, when I refer to "never-convicted individuals" in the remainder of the paper, what I am truly referring to is respondents who do not have a *felony* conviction.[11]

Analytic Approach

Because criminal conviction and incarceration are subject to selection that is likely related to housing instability, I employ a variety of strategies to account for confounding factors:

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[9] I have also run models in which I combine felony and non-felony incarcerations in a single *previously incarcerated* variable. Results from these models are substantively consistent with those presented below.

[10] Because currently incarcerated respondents are by definition residing in a jail or prison, not one of the forms of temporary housing I consider, the *currently incarcerated* variable is perfectly collinear with the temporary housing outcome and, therefore, falls out of models of temporary housing residence.

[11] In models that pull respondents with other convictions out into a separate category, I see that coefficients on *previous felony conviction* and *previous felony incarceration* increase slightly. Standard errors are little changed, and all results remain statistically significant. Results available upon request.

controlling for observed characteristics, restricting the sample only to respondents with more serious criminal involvement, individual fixed effects, and sibling fixed effects. I view these models as dealing with different primary threats to causal inference and consider robustness of findings across these differing modelling approaches to be suggestive of a relationship between felony conviction and housing instability that is likely causal. I also conduct a wide variety of supplementary analyses to assess the robustness of the results.

Across all models I use Poisson regression to predict number of residential moves since the last interview date (Eq. (1)) and logistic regression to predict current residence at each survey wave in some sort of temporary housing (Eq. (2)).[12] These models take the following general form,

$$\begin{aligned}\ln(Number\ of\ Moves\ Since\ Last\ Interview_{it}) \\ &= \beta_0 + \beta_1 Previous\ Felony\ Conviction_{it} \\ &+ \beta_2 Previous\ Felony\ Incarceration_{it} \\ &+ \beta_3 Previous\ Non\ Felony\ Incarceration_{it} + X_{it}\delta + v_i \\ &+ ln(Years\ Since\ Last\ Interview_{it})\end{aligned} \tag{1}$$

$$\begin{aligned}\ln\left(\frac{P_{temporary\ housing_{it}}}{1 - P_{temporary\ housing_{it}}}\right) \\ &= \beta_0 + \beta_1 Previous\ Felony\ Conviction_{it} \\ &+ \beta_2 Previous\ Felony\ Incarceration_{it} \\ &+ \beta_3 Previous\ Non\ Felony\ Incarceration_{it} + X_{it}\delta + v_i\end{aligned} \tag{2}$$

where the log of number of residential moves since the last interview and log odds of residence in temporary housing for each respondent *i* at each time period *t* is estimated based on time-varying measures of felony conviction and incarceration history and the set of individual-level time-varying covariates described below (X_{it}). All models also include either a respondent-level

[12] Results are consistent if I instead use zero-inflated Poisson and Firth penalized logistic regression models, respectively. While the number of moves variable is slightly overdispersed, negative binomial models produce nearly identical coefficients and standard errors as Poisson models. All results available upon request.

random effect (v_i), incorporated to account for repeated observation of respondents across survey waves, or a respondent- or sibling-level fixed effect as described below. The Poisson model of number of residential moves since last interview also includes an offset ($ln(Years\ Since\ Last\ Interview_{it})$) to account for differences in the exact length of time between interviews across respondents. Because NLSY97 uses cluster sampling, I cluster standard errors at the primary sampling unit level in all models (Bureau of Labor Statistics, 2021).

The inclusion of the time-varying *previous felony incarceration* and *previous non felony incarceration* variables in these models ensures that the coefficient on *previous felony conviction* (β_1) corresponds to the average difference in housing outcomes between never-convicted respondents (the reference group) and respondents who have a felony conviction but who have not (yet) been incarcerated as of each survey wave. The coefficient on *previous felony incarceration* (β_2), on the other hand, should reflect the intermediary, difficult to quantify effects of actual incarceration (e.g., removal from labor market, weakened social ties, health effects of confinement). If incarceration affects housing instability solely through the intermediary effects of community removal and confinement, then there should be few differences in housing instability between respondents with a felony conviction but no history of incarceration to date and never-convicted respondents once observable characteristics are controlled, and even less so when potentially confounding unobservable characteristics are accounted for in the high crime comparison group, individual fixed effect, and sibling fixed effect models described below. If, however, we see that formerly convicted respondents who have not (yet) been incarcerated experience significantly greater levels of housing instability even after these differences are accounted for, this will suggest that felony conviction status and the mark of a criminal record independently affect housing stability. A non-significant β_2 coefficient in this case would suggest that the association between felony incarceration and housing instability is primarily driven by felony conviction status.

Covariate Adjustment

In all models I control for basic demographic characteristics that relate to probability of having been incarcerated or convicted or of experiencing housing instability: age, gender, and race/ethnicity. Age is captured by a fully flexible set of dummy variables, with 25 as the reference.[13] Gender is an indicator variable set equal to one if the respondent is female. Race and ethnicity are captured in four discrete categories: white non-Hispanic (reference), black non-Hispanic, Hispanic, and other.

The second model adds controls for characteristics that are unlikely to have been affected by incarceration or conviction but that may confound the relationship between criminal justice contact and housing instability, including 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. Because 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 control for number of times the respondent moved between ages 12 and 16 (inclusive). I also account for respondent's household structure in 1997, parents' education, and parents' net worth in 1997. Household structure is measured as a four-category variable: lived with both biological parents (reference), lived with

[13] Controlling for age as a set of dummy variables avoids making assumptions about the functional form of the relationship between age, felony conviction risk, and housing instability risk. We know these relationships are highly age-graded and nonlinear, but the exact shape of the relationship between age and criminal activity, at least, differs across datasets. Moreover, by accounting for age as a set of dummy variables – essentially using age fixed effects – coefficients are estimated by comparing outcomes among respondents of the same age, which is recommended as best practice in attempting to estimate causal effects of criminal justice system contact using observational data (D. S. Nagin et al., 2009).

one biological parent and one stepparent, lived with one biological parent only, and some other living arrangement. Parents' education is coded as the highest degree completed by either of the respondent's resident parents (biological, step, adoptive, or foster) as reported in the 1997 parent interview: no diploma or degree (reference), high school diploma, some college, college degree, or graduate or professional degree. Parents' net worth, reported in the 1997 parent interview, is adjusted for inflation to 2016 dollars. Finally, I include respondent's age-adjusted percentile score on the Armed Services Vocational Aptitude Battery test, which the NLSY97 administered in the first two survey waves, as a rough measure of respondent's cognitive achievement.

Thus, this second model aims to reduce concerns about selection bias by accounting for some of the factors that help predict which individuals select into criminal justice system contact (Kirk & Wakefield, 2018). In doing so, the results from this model provide a more plausible upper bound on the relationship between felony conviction and housing instability. The third model incorporates a full set of covariates meant to account both for mediating characteristics and other potential time-varying confounders, including educational attainment, marital status, parenthood, employment, income, assets, and urban residence. I also include a scale measure of respondent's mental health in 2000, the only year in which such a measure is available, given the strong connection between mental health, the criminal justice system, and housing instability. Scores range from 5 to 20, with higher scores indicating more positive mental health and lower scores indicating more emotional problems.

Respondent education is recorded as the highest degree completed using the same five category coding applied to parents' education. I include an indicator to identify current students. Marital status is a dummy variable set equal to one if the respondent is currently married on the interview date. Parenthood is captured by an indicator variable set equal to one if the respondent has at least one biological child residing in her household at the survey date. I account for labor force attachment with a measure of the number of weeks the respondent was employed, including self-employment, in the last calendar year. I account for respondents' time-varying financial resources with measures of total wages and salary in the prior year, gift income received from family and friends in the prior calendar year[14], and respondents' net worth.[15] All financial resource variables are measured in dollars and adjusted for inflation to 2016 dollars. Because policing – and, therefore, risk of felony conviction – and housing opportunities (e.g., number of accessible shelter beds, density of alternative residential properties, etc.) may differ across urban versus rural spaces, I also include a dummy variable that identifies whether respondents are living in a Census-designated urban area at each survey date.

By controlling for these various potential intervening mechanisms, I attempt to both further reduce confounding and provide a lower bound for the potential role of felony conviction stigma and accompanying housing market discrimination. If felony conviction only affects housing stability via its effect on probability of achieving stable employment, entering into and/or maintaining a stable union, and financial resources, then any differences in housing stability between never-convicted and previously convicted respondents that remained in the second model should disappear in this model. If, alternatively, significant differences in housing stability remain

[14] Some respondents reported exact dollar amounts for gift income, while others reported estimated amounts using seven ordinal response categories ranging from "$1-500" to "more than $10,000." To adjust these values for inflation, I assign the midpoint of the reported range as the gift income amount in a given year, then adjust values to 2016 dollars. NLSY stopped asking respondents about gift income in 2013, so 2013, 2015, and 2017 values are imputed based on reported gift income amounts received in previous years along with all other covariates used in the full model. Models that exclude gift income or exclude observations from 2013-2017 in order to avoid the use of imputed gift income values produce results consistent with those shown in main tables.

[15] NLSY97 collects data on the net worth of respondents and, if applicable, their spouse/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 and debts that respondents report their spouses/partners do not share with them, then multiply impute net worth values for years in which assets are not collected.

after the inclusion of these covariates, then this will provide evidence for the role of felon stigma and discrimination as a mechanism contributing to greater housing instability among both never-incarcerated individuals with felony records *and* previously incarcerated individuals.

I multiply impute missing values on control variables by producing 10 imputed datasets with the chained equations method in Stata MI commands, which fills in missing values in multiple variables iteratively using a sequence of univariate imputation models with fully conditional specifications (Allison, 2001; White et al., 2011).[16] Model results produced with multiply imputed datasets are consistent with those produced using casewise deletion.[17]

High Crime Comparison Group Model

A primary threat to causal inference is that unaccounted for behavioral differences, particularly in criminal activity, could confound the relationship between conviction and housing instability. Indeed, as Figure 2 shows, ever-incarcerated and ever-convicted-but-never-incarcerated NLSY97 respondents report similar criminal activity levels during adolescence, but their criminal activity distributions are distinctively left-skewed relative to never-convicted respondents'.

[INSERT FIGURE 2 HERE]

While NLSY97 collects self-reported data on a range of criminal behaviors (e.g., assault, drug sales, theft) from all respondents between 1998 and 2003, these questions are only asked of respondents who report having previously been arrested and a small subsample of other

[16] I use OLS regression to fill in missing values on continuous variables, logistic regression for binary variables, ordinal logistic regression for ordinal variables, multinomial logit for nominal variables (e.g., household structure at age 14), and Poisson regression for count variables.
[17] Standard errors are larger in the casewise deletion models because of smaller sample size, but the coefficients on *previous felony conviction* remain highly statistically significant across outcomes. The coefficients on *previous felony incarceration*, however, are no longer statistically significant in the full controls, sibling fixed effects, and high crime restricted comparison group models for either outcome when using casewise deletion.

randomly selected respondents starting in 2004.[18] Thus, I am unable to control for time-varying criminal activity across all years, but in an attempt to account for unobservable behavioral differences that could confound the relationships of interest, I run models taking the same form as those shown in Eq. (1) and Eq. (2) but restricting the comparison group to a high-crime subsample of respondents based on self-reported criminal activity before 2004. Specifically, I drop all never-convicted respondents with below median age-adjusted early adult criminal activity. 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).

Individual Fixed Effects Model

To further account for unobserved confounders, I also run models in which the individual random effect (v_i) shown in Eq. (1) and Eq. (2) is replaced with an individual level fixed effect. Unobserved time-invariant characteristics are captured in the individual fixed effects, thus reducing concerns about fixed family background, behavioral, or genetic traits that might confound the relationship of interest. Fixed effect models rely on within-unit variation in the variables of interest to estimate coefficients, however, meaning that individuals who do not vary on a measure during the observation period do not contribute to the parameter estimate. Thus, the individual fixed effect model produces a *felony conviction* coefficient based only on respondents who avoid first conviction until after age 20.[19] Because respondents who manage to avoid their

[18] Questions about gun carrying behavior are asked of all respondents 1998-2011, and questions about marijuana and hard drug use are asked of all respondents 1998-2011 and in 2015. Controlling for these variables in the years available produces results consistent with those in the "full controls" model.
[19] Thirty-six percent of all NLSY97 respondents who are ever convicted of a felony by 2017 report their first conviction by age 20.

first conviction until a later age may differ in important ways from those who experience earlier conviction, the results from the individual fixed effects model may not be generalizable to the general ever-convicted population. This potential sacrifice in generalizability is offset, however, by the much stronger causal test of the relationship between felony conviction and housing instability provided by the fixed effect model.

<u>Sibling Fixed Effects Model</u>

In an additional attempt to control for some of the unobserved characteristics that may confound the relationship between felony conviction and housing instability while still leveraging information from respondents who are first convicted at a young age, I also run sibling fixed effect models in which the respondent-level random effects included in Eq. (1) and Eq. (2) are replaced with family-level fixed effects (w_k).

$$\begin{aligned}\ln(&Number\ of\ Moves\ Since\ Last\ Interview_{it})\\ &= \beta_0 + \beta_1 Previous\ Felony\ Conviction_{it}\\ &+ \beta_2 Previous\ Felony\ Incarceration_{it}\\ &+ \beta_3 Previous\ Non\ Felony\ Incarceration_{it} + X_{it}\delta + w_k\\ &+ ln(Years\ Since\ Last\ Interview_{it})\end{aligned} \tag{3}$$

$$\begin{aligned}\ln&\left(\frac{P_{temporary\ housing\ it}}{1 - P_{temporary\ housing\ it}}\right)\\ &= \beta_0 + \beta_1 Previous\ Felony\ Conviction_{it}\\ &+ \beta_2 Previous\ Felony\ Incarceration_{it}\\ &+ \beta_3 Previous\ Non\ Felony\ Incarceration_{it} + X_{it}\delta + w_k\end{aligned} \tag{4}$$

Biological sibling fixed effects can account for some of the most important unobserved characteristics that could confound the relationship between felony conviction and housing instability while still using observations from respondents first convicted of a felony before age 20 to estimate the *previous felony conviction* coefficient (Moffitt & Beckley, 2015; Motz et al., 2020; Pingault et al., 2018). Because NLSY97 sampled at the household level, enrolling all household residents aged 12 to 16, 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 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 survey questions. Thus, with sibling fixed effects, coefficient estimates reflect differences in housing instability for siblings with differing in time-varying criminal justice contact, controlling for individual-level covariates.

Because I use multiple observation years for each sibling, siblings need not have differing criminal justice contact histories in all years to contribute to the estimation of the coefficients on *felony conviction* and *felony incarceration*; they need only differ in at least one year. Thus, for siblings who start and end with the same criminal justice contact history (e.g., both have no convictions at age 20 and both have been convicted by age 30), if one sibling is first convicted of a felony at age 21 while the other is not convicted until age 28, their observations from ages 21 to 27 will factor into the estimation of the β_1 coefficient.[20]

Results

Table 1 displays weighted descriptive statistics for all outcome and control variables, broken out by criminal justice contact history to date: previously incarcerated for a felony, previously convicted of a felony but never (yet) incarcerated, and never (yet) convicted or

[20] Because I cluster standard errors at the primary sampling unit (PSU) level, I am unable to cluster standard errors at the individual level in sibling fixed effect models to account for repeated observations of individuals. Results are consistent with those shown in the main table, however, when I cluster standard errors at the individual rather than PSU level in sibling fixed effect models.

incarcerated.[21] The top panel of the table (Panel A) displays values for time-varying variables at the person-year level, and the bottom panel (Panel B) shows cumulative values by 2017 for outcome variables, values for time invariant control variables, and 2016 values for time-varying employment and monetary variables at the person level.

In any given year (Panel A), each form of housing instability is relatively rare, but over the full observation period (Panel B) both are more common, particularly for formerly convicted and, especially, formerly incarcerated individuals.[22] That formerly convicted respondents fall between the full sample and formerly incarcerated respondents on each of these outcomes makes sense given their relative privilege compared to formerly incarcerated respondents. Their racial composition is more similar to that of never-convicted respondents, and they are more educated, more likely to have lived with both parents in adolescence, and more likely to be married than formerly incarcerated respondents. They also have more educated, wealthier parents than formerly incarcerated respondents and higher cognitive test scores, employment levels, labor income, and assets. Respondents with a felony conviction but no incarceration history (yet), are also more likely to be female and more likely to be co-resident parents than formerly incarcerated respondents. Where formerly incarcerated and formerly convicted respondents are remarkably similar is in the amount of residential mobility they experienced during adolescence (2.5 moves on average), self-reported criminal activity in adolescence and early adulthood (65th and 64th percentile, respectively), and mental health index score (14.8 and 14.9, respectively).

[INSERT TABLE 1 HERE]

[21] I apply custom longitudinal sampling weights (https://www.nlsinfo.org/weights/nlsy97) when calculating the descriptive statistics shown in Figure 1 and Table 1, but I do not weight regression models. Results of weighted models are consistent with those presented in the main text and available upon request.
[22] The prevalence of temporary housing residence I observe in the NLSY97 (15.2% by 2017 among respondents who have been incarcerated for a felony) is in keeping with rates of homelessness observed among formerly incarcerated individuals in other studies (Metraux et al., 2007; Remster, 2021).

Tables 2 and 3 display results from models predicting number of moves since the last interview and log odds of temporary housing residence, respectively. In each table column one shows results from the simplest model that controls only for basic demographic characteristics, column two shows results from the pre-treatment controls model, column three displays results from the model using the full slate of control variables, column four displays results from the high crime restricted comparison group model, column five shows individual fixed effect model results, and column six displays biological sibling fixed effect model results. Thus, results shown in the left-most columns correspond to the strongest causal tests of the relationship between felony conviction (or incarceration) and housing instability. With the exception of race and gender, control variable coefficients are not shown in Tables 2 and 3 but can be seen in Tables 4 and 5.

The coefficient on *previous felony conviction* in these models indicates the average difference in housing outcomes, all else held equal, between individuals with felony records but no history of incarceration thus far and individuals with no prior felonies or history of incarceration. The coefficient on *previous felony incarceration*, on the other hand, represents the average difference in outcomes between former felons who have and have not been incarcerated. Finally, the coefficient on *non-felony incarceration* represents the average difference in housing instability outcomes between individuals who have previously been incarcerated but never been convicted of a felony (e.g., those who have either experienced pretrial detention or been incarcerated for misdemeanor offenses) and individuals with no prior felonies or history of incarceration.

[INSERT TABLE 2 HERE]

When controlling only for race/ethnicity, gender, and age (Model 1, Table 2), residential mobility is about 30 percent higher among respondents with a felony conviction but no history of incarceration relative to never-convicted individuals (Incidence Rate Ration (IRR) = $e^{0.260}$ = 1.297). Part of this difference appears attributable to differences in family background and cognitive test scores (Model 2), but residential mobility remains 21 percent higher (IRR = $e^{0.191}$ = 1.210), on average, among formerly convicted individuals without any history of incarceration (yet) compared to never-incarcerated individuals even when mediating factors like educational attainment, labor force attachment, financial resources, marital and parent status, and mental health index score are taken into account (Model 3).

Restricting comparison to respondents who were at or above the median on age-adjusted, self-reported criminal activity in their teens and early 20s (Model 4), further reduces the coefficient on *previous felony conviction*, but the difference between previously convicted and never (yet) convicted individuals remains positive and statistically significant even among respondents with similar histories of delinquency in their youth. The difference in residential mobility between never-incarcerated individuals with and without a felony conviction is even larger in the two fixed effect models (Models 4 and 5), which provide a stricter causal test. The coefficients on *previous felony conviction* in these two models indicate that felony conviction is associated with significantly higher residential mobility even when comparisons are restricted within person or within biological sibling pairs ($IRR_{\text{individual FE}} = e^{0.169} = 1.184$; $IRR_{\text{sibling FE}} = e^{0.154} = 1.166$). Although the exact coefficient sizes vary across models, the *previous felony conviction* coefficients in Models 4-6 are not significantly different from each other.

The picture regarding the relationship between incarceration and residential mobility, however, is less clear than that between conviction and mobility. The *previous felony* *incarceration* and *non-felony incarceration* coefficients in Models 1-4, which control only for observables, are positive and statistically significant, suggesting that incarceration in its own right is independently associated with greater residential mobility. Fittingly, prior felony incarceration is associated with greater residential mobility than prior non-felony incarceration. In Model 4, for example, previous felony incarceration is associated with 0.262 higher log expected number of moves over the course of a year (0.131 + 0.131) compared to a 0.161 higher log expected count for individuals with only a non-felony incarceration history relative to never-convicted individuals.

However, in the individual fixed effects model (Model 5), which accounts for time-invariant person-level confounders like fixed behavioral predispositions, neither form of incarceration is significant any longer. This finding suggests that it is gaining felon status, not serving time behind bars, that is most predictive of higher residential mobility for the population of people who interact with the criminal justice system. The sibling fixed effects model (Model 6) likewise indicates that felony incarceration is not associated with additional increases to residential mobility beyond those that stem from felony conviction. Unlike in the individual fixed effects model, however, non-felony incarceration is associated with significantly greater residential mobility when comparisons are made between sibling pairs.

Results from logit models predicting temporary housing residence (Table 3) are substantively similar to the residential mobility findings in Table 2. Prior felony conviction without incarceration is again associated with significantly greater housing instability, this time in terms of temporary housing residence, while the relationship between incarceration and temporary housing risk is, again, less clear. The individual fixed effects model results indicate that one's odds of temporary housing residence are more than twice as high after initial felony conviction than they were before ($OR_{\text{individual FE}} = e^{0.816} = 2.261$), and the sibling fixed effects model results suggest that formerly convicted individuals have multiple times higher odds of temporary housing residence than their biological siblings, even when differences in achieved characteristics like employment, financial resources, and relationship status are accounted for ($OR_{\text{sibling FE}} = e^{1.791} = 5.995$). Both felony incarceration and non-felony incarceration are associated with significantly greater log odds of temporary housing residence in the individual fixed effect model (Model 5), but neither incarceration coefficient is statistically significant in the sibling fixed effects model (Model 6).

[INSERT TABLE 3 HERE]

It is worth noting that small differences in unobserved characteristics between sibling pairs may generate bias in sibling fixed effect models (Angrist & Pischke, 2009, p. 226; Frisell, 2021), which – in combination with the greatly reduced sample size and larger standard errors[23] in the temporary housing models – could explain the non-significant coefficients on *previous felony incarceration* and *non-felony incarceration*, as well as the larger coefficient on *previous felony conviction*, in Model 6 relative to the other models of temporary housing.

Still, the non-significance of the previous incarceration coefficients in this model in combination with the insignificant incarceration coefficients in Table 2 calls into question the strength of the independent relationship between incarceration per se and housing instability. The consistency of findings with regard to previous felony conviction across models and outcomes, on the other hand, suggests that the relationship between conviction and housing instability is not wholly attributable to underlying differences in criminal proclivity, unobserved individual-level

[23] Because fixed effects models require within unit variation to produce estimates, respondents who never live in temporary housing and respondents from families in which no sibling ever reports living in temporary housing at or after age 20 drop out of the individual and sibling fixed effect models, respectively.

time-invariant confounders, or unobserved family-level confounders. As such, these results should provide some confidence that the relationship between prior felony conviction *without* incarceration and housing instability identified here is not purely spurious.

Mechanisms

While the consistency of the relationship between felony conviction (without incarceration) and housing instability across various modelling strategies that tackle different primary threats to causal inference suggests that this relationship may be causal in nature, it is important to reiterate that Models 3-6 include many mediating factors that may themselves have been affected by felony conviction or incarceration (e.g., financial resources, labor market attachment). In Tables 4 and 5, I examine the role of various mechanisms in linking felony conviction history to housing instability. These tables display coefficients for models that introduce covariates one mechanism group at a time. Because the individual and sibling fixed effect models do not allow me to directly examine the role of covariates that are fixed within person or within biological sibling pair, respectively, I use the high crime comparison group model (Model 4) as the basis for the mechanisms analysis.

The first column in Tables 4 and 5 contains only demographic control variables. I then add covariates in groups according to the mechanisms they represent: family background characteristics, skills and work experience, financial resources, and relationships and behavior.[24] Adding these covariate groups separately allows me to examine whether any particular mechanism appears to be especially important for explaining the baseline differences in housing

[24] I include the urban residence control in the relationships and behavior mechanism group. When I pull out urban residence separately it does little to explain the relationship between prior felony conviction or incarceration and housing instability; its inclusion reduces the coefficient on previous felony conviction by about 2 percent and increases the coefficient on *previous felony incarceration* by 4.6 percent across both outcomes.

instability observed among previously convicted and formerly incarcerated individuals relative to never-convicted individuals. Because I use the high crime comparison group for this analysis, the coefficients in column six ("Full Controls") correspond exactly to those shown in column four of Tables 2 and 3.

[INSERT TABLE 4 HERE]

[INSERT TABLE 5 HERE]

The coefficients in Tables 4 and 5 indicate that each mechanism group plays some role in reducing the magnitude of the *previous felony conviction* and *previous felony incarceration* coefficients in models of residential mobility, but which mechanisms matter most varies based on which treatment and which outcome we consider. Family background plays a particularly large role in explaining the relationship between felony conviction history and residential mobility (reducing the coefficient by 20 percent) and, to a lesser extent, temporary housing risk (reducing the coefficient by 17 percent). But family background does little to explain the relationship between prior felony incarceration and housing instability. Accounting for family background characteristics reduces the *previous felony incarceration* coefficient by only 7 percent in Table 5 (temporary housing) and by just 0.6 percent in Table 4 (residential mobility). Family background plays a much larger explanatory role in understanding the baseline relationship between non-felony incarceration history and housing instability, however. The inclusion of family background characteristics reduces the coefficient on *non-felony incarceration* by 11 percent in the model of residential mobility (Table 4) and 16 percent in the model of temporary housing (Table 5).

Skills and work appear to play a large role in helping to understand the relationship between criminal justice contact and temporary housing risk (Table 5) – reducing the coefficients on *previous felony conviction*, *previous felony incarceration*, and *non-felony incarceration* by 22-28 percent – but essentially no role in explaining the relationship between criminal justice contact and residential mobility (Table 4). In fact, accounting for differences in skills and labor force attachment actually increases the coefficients on *previous felony conviction* and *non-felony incarceration* in the residential mobility model.

Accounting for financial resources is particularly helpful in understanding the relationship between prior felony incarceration and housing instability. The financial resources measures explain more of the relationship between felony incarceration history and both residential mobility and temporary housing risk than any other mechanism group, reducing the coefficient on *previous felony incarceration* by 12 percent and 26 percent, respectively. Financial resources also help to account for about 15 percent of the baseline relationship between prior felony conviction and temporary housing and 20 percent of the relationship between non-felony incarceration and temporary housing, but they are less helpful in explaining the relationship between these forms of criminal justice contact and residential mobility (7 percent and 11 percent, respectively).

Finally, the relationships and behavior measures are among the least explanatory for both housing instability outcomes. They are able to explain about 11 percent of the baseline association between *previous felony incarceration* and residential mobility and 9 percent of the association between felony incarceration and temporary housing risk, but they reduce the coefficients on *previous felony conviction* and *non-felony incarceration* by less than six percent for each outcome. The relatively low explanatory power for the relationships and behavior covariates may in part be due to the fact that the comparison group in Tables 4 and 5 is restricted to individuals with high self-reported criminal activity in their youth. As such, behavioral differences between individuals with and without formal criminal justice system contact may be relatively small in these models.

Robustness Checks

Despite the consistency of the results across models, a primary threat to causal inference using the NLSY97 data is the lack of time-varying measures of criminal behavior, which may be especially likely to confound the relationship between felony conviction and subsequent housing instability. While NLSY97 stopped collecting self-reported data on criminal behaviors from all respondents in 2003, they have collected two measures of substance use in almost every year: whether the respondent has had any alcohol in the last year and whether the respondent has used any hard drugs in the last year.[25] Although time-varying measures of current criminal activity would be ideal, substance use is at least plausibly relevant to risk of interaction with the criminal justice system and housing instability risk. Thus, I have run models that incorporate these two variables on the person years in which they are available (all but 2013 and 2017). When I do so I find consistent results with those reported in the main tables. (Results available upon request.)

To further address the concern that unobserved behavioral characteristics may confound the relationships of interest by explaining both one's likelihood of being unstably housed and one's likelihood of being sanctioned by the criminal justice system, I directly test whether individuals who will *eventually* be convicted of a felony or incarcerated have differing housing stability trajectories even before their first conviction or incarceration than individuals who will never be convicted. The individual fixed effect model handles this concern by relying upon

[25] The hard drug use question text reads: "Excluding marijuana and alcohol, in the last twelve months, have you used any drugs like cocaine, crack, heroin, or crystal meth, or any other substance not prescribed by a doctor, in order to get high or to achieve an altered state?" NLSY97 does not ask respondents these questions in 2013 or 2017, which is why I do not include these variables in my main tables.

within person variation to estimate coefficients, but it does not allow us to directly examine whether housing instability is already higher prior to initial criminal justice system contact for the population who will be exposed because these potential baseline differences are captured by the individual fixed effects terms. Thus, to directly test this question, I re-run the high crime comparison group and sibling fixed effect models with the addition of indicator variables identifying respondents who will ever be convicted of a felony or ever incarcerated by the last survey wave. In these models, significant coefficients on either of those variables would indicate pre-existing baseline differences in housing instability between individuals who will eventually have formal justice system contact and those who will not. As such, these models can be thought of as a placebo test – if we are attributing differences in housing instability between the treatment and control group to the effect of felony conviction status, then we should not see differences in housing instability prior to initial conviction, conditional on controls.

The results from these models confirm that, conditional on covariates, there are not significant baseline differences in either form of housing instability between individuals who will eventually be convicted of a felony and those who are never convicted (Tables S1 and S2). The *will ever be incarcerated* coefficient in the high crime comparison group model of residential mobility does suggest that individuals who will eventually be incarcerated have greater residential mobility even before their first incarceration compared to individuals who will not be incarcerated, but this difference is not statistically significant in the sibling fixed effects model, which is better able to account for potential confounders, nor are there baseline differences in log odds of temporary housing residence between individuals who will eventually be incarcerated and those who will not. Thus, the relationships of interest do not appear to be driven by behavioral differences that affect one's likelihood of being unstably housed even before initial criminal justice system sanctioning.

I have also run models that account for length of time since conviction and since release from incarceration. These models suggest that the relationship between felony incarceration and residential mobility, to the extent there is one, may attenuate with time but the relationship between felony conviction without incarceration and residential mobility does not (Table S3). This finding is in keeping with the idea that, while the effect of community removal via incarceration may attenuate with time since release, the effect of being *marked* as a felon is likely to decay much more slowly, on a time frame not observable in the NLSY97 data.

Models of temporary housing residence, on the other hand, suggest that this particular form of housing instability may be shorter lived. The high crime comparison group and individual fixed effect models both suggest that the relationship between felony conviction and temporary housing, as well as the relationship between felony incarceration and temporary housing, wanes with time, though the sibling fixed effect model does not suggest as much (Table S4). Coefficients from the individual fixed effects model suggest that the relationship between felony incarceration and temporary housing residence attenuates more quickly, with the negative relationship between felony incarceration and temporary housing residence attenuating in about 6 years (1.100/-0.179), on average, compared to about 14 years for felony conviction (.946/-0.0664).

I also examined whether housing instability outcomes differ by type of crime for which the respondent was convicted (i.e., drug, violent, property). Results from these models indicate that property crime, and to a lesser extent violent crime, convictions are significantly associated with both forms of housing instability, while drug convictions are not associated with greater housing instability. (See Tables S5 and S6.)

Given differences in criminal justice system enforcement, housing vacancy rates, and availability of shelter beds across places in the United States, I also test for geographic variation in the relationship between felony conviction and housing instability across Census geographic regions[26] and across urban versus rural locations. When I do so, I do not find significant differences in the relationship between felony conviction or incarceration and either measure of housing instability based on the region or urbanicity of respondents' residence at each survey wave. Results available upon request.

To account for the possibility that housing instability following conviction or incarceration could be attributable to strained relationships – strained either by incarceration itself or by having exhausted family and partners' patience while passing through the court system – I also ran models predicting log odds of living with adult family members or with a romantic partner. I find no evidence that either prior conviction or incarceration is associated with lower log odds of living with family members or romantic partners. In fact, once all covariates are added, previously convicted individuals and individuals with a prior non-felony incarceration are marginally *more* likely to live with a romantic partner than observably similar never-convicted and never-incarcerated individuals. This may be evidence of formerly convicted individuals needing to rely more heavily on romantic partners for housing because of the difficulty of getting a lease in their own name with a conviction record.

Because the labor market stigma of incarceration appears to vary by race (Pager, 2003; Pager et al., 2009), I also examined interactions between race/ethnicity and criminal justice history. Supplementary Tables S7 and S8 report race-interacted versions of the full controls and

[26] The four Census regions include Northeast, North Central, South, and Midwest.

high crime comparison group models.[27] I find no evidence that the relationship between felony conviction and either housing outcome differs significantly by race or ethnicity. It does appear, however, that the relationship between prior felony incarceration and temporary housing risk may be greater for white individuals than for black or other race individuals based on the full controls model, but these differences do not hold in the high crime comparison group model (Table S8).

Finally, gender-interacted models suggest that the relationship between justice system contact and temporary housing is amplified for women, with women experiencing a significantly higher risk of temporary housing residence after felony conviction and non-felony incarceration than observably similar men.[28] (See Supplementary Tables S9 and S10.) This difference does not appear to be driven by gendered differences co-residence with own children. In models that include three-way interactions between gender, parenthood, and conviction/incarceration history, the higher probability of temporary housing – as well as greater residential mobility – among formerly convicted and incarcerated women relative to men appears to be driven by women without children. That differences in child co-residence do not explain the 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 et al., 2015; Laub & Sampson, 2001).

These findings, combined with Desmond's finding that women are at a higher risk of eviction than men (2016), suggest that continued exploration of the gendered dynamics of

[27] I am unable to run race-interacted sibling and individual fixed effect models because of lack of within unit variation on race/ethnicity.

[28] Same-sex sibling fixed effect models and single-sex individual fixed effect models reveal a similar finding: the magnitude of the relationship between felony incarceration and both forms of housing instability, especially temporary housing, appears to be larger for women than for men. (Results available upon request.)

housing instability would be worthwhile. Moreover, 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 given the relatively limited understanding we have of collateral consequences of justice system involvement for women due to their lower incarceration rates and the shortage of studies that examine the effects of conviction without incarceration.

Conclusion

Across multiple modelling strategies that approach the problem of unobserved confounding from different angles, I find robust evidence that felony conviction appears to increase housing instability – as measured by number of residential moves in the last year and residence in temporary housing – even when conviction is not accompanied by incarceration. The evidence that incarceration has an additional, independent negative effect on housing instability, however, is less clear in these models. These findings, thus, complicate those of prior studies focused on housing instability following incarceration by suggesting that the association found in those studies may largely be driven by the fact of having been *marked* as a felon, rather than by the experience of incarceration itself.

While prior studies have often talked about the importance of being marked with felon status, researchers have often conflated felon status and incarceration in the operationalization of their models, rather than pulling the two conditions apart. Thus, this paper makes an important contribution by disentangling the concept of felon status from that of incarceration effects. By doing so and finding that much of what passed for an incarceration effect in prior studies (e.g., Warner, 2015) is perhaps instead a conviction effect, this paper can also help solve a puzzle in the prior literature. Studies attempting to identify a causal effect of incarceration by using individual fixed effect models and observational data often find large negative associations between incarceration and a variety of measures of subsequent wellbeing (Kirk & Wakefield, 2018), while research that attempts to identify the causal effect of incarceration by comparing incarcerated individuals to comparable never-incarcerated but also convicted individuals finds minimal evidence of an incarceration effect (Loeffler & Nagin, 2022; Petrich et al., 2021). My findings suggest that the reason for these disparate conclusions may be that many of the challenges experienced by formerly incarcerated Americans derive from their felon status, not necessarily from the incarceration itself.

I also 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 employment and financial resources, (2) family-level unobserved differences that could confound the relationship between likelihood of felony conviction and housing instability, and (3) baseline behavioral differences 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, the remaining significant differences in housing instability among formerly-convicted-but-never-incarcerated individuals that I observe might plausibly be attributed to discriminatory behavior by housing market gatekeepers. These findings, thus, complement those of audit studies that find localized evidence of housing discrimination against prospective applicants with felony records (Evans, 2016; Evans & Porter, 2015; Furst & Evans, 2017; Leasure & Martin, 2017).

While audit studies are able to rule out omitted variable bias by virtue of random assignment of test conditions, an advantage of the observational data used in this paper is the ability to test for differences by race/ethnicity and across geographies, which audit studies have not yet accomplished. Furthermore, a common question asked of experimental studies that test for discrimination at the entry stage to the labor or housing market (e.g., callbacks in response to an initial inquiry) is how informative findings of disparities at this stage of the process are for understanding the final outcomes about which we typically care (e.g., getting a job, successfully renting an apartment) as opposed to the search time required to achieve those end goals. An advantage of an observational study like this one is the ability to step back and investigate whether the felony conviction stigma that experimental studies identify as problematic at the entry stage actually translates into meaningful differences in population level outcomes. I find that felony conviction, with or without incarceration, does indeed appear to translate into higher levels of housing instability in a nationally representative sample.

The NLSY97 data also allow me to examine the contribution of mechanisms other than stigma to the relationship between felony conviction and housing instability. In doing so, I find that family background and skills and work are particularly helpful at explaining why we see higher levels of housing instability among individuals with conviction records. Finally, by using observational data, I can directly compare the "effect size" of felony conviction to that of incarceration – something that experimental studies have yet to do. In doing so, I find that the difference in housing instability between individuals with felony convictions only and those with a felony incarceration history is a difference of degree, not of kind. Formerly incarcerated individuals and formerly-convicted-but-never-yet-incarcerated individuals are more likely to live in temporary housing and move more often than observably similar never-convicted individuals. Such differences persist even within individual fixed effect models and when comparisons are restricted to biological sibling pairs. Other researchers have observed greater residential mobility among formerly incarcerated individuals (Harding et al., 2013; Warner, 2015), but the finding of higher residential mobility, as well as greater temporary housing risk, among individuals with a felony conviction but no incarceration history is a new contribution to the literature.

Limitations

Despite the strength of the results under a variety of specifications, the threat of omitted variable bias poses significant challenges for identifying causal effects in observational data like the NLSY97, especially in the case of a treatment like criminal justice contact where selection bias is of particular concern. While I am able to account for many of the characteristics most likely to confound the relationship between criminal justice system contact and subsequent outcomes – like race, age, and gender (D. S. Nagin et al., 2009), as well as things like parental resources and own financial resources that are particularly relevant in the case of housing instability – I lack consistent time-varying measures of criminal behavior.

In the absence of such measures, I attempt to account for behavioral differences that may confound the results in several ways. To the extent that criminal activity is correlated with relationship status and labor market attachment, as life course criminology suggests (Laub & Sampson, 2006; Sampson & Laub, 1993), then the covariate adjustments should lessen concerns about this source of potential confounding. The high crime restricted comparison group model goes further by limiting comparison of outcomes to never-convicted respondents who should be more behaviorally similar to respondents who are convicted of a felony, and the individual fixed effects model helps to account for time-invariant behavioral differences at the individual level that could confound results. I also run models that include two time-varying measures of substance use available in almost every survey wave, finding consistent results with those reported in the main tables. While none of these models alone can meet all assumptions necessary to identify a singular causal estimate, the consistency of substantive findings across models lends confidence that the observed differences are not simply an artifact of unobserved confounding.

An additional limitation of this analysis is that felony conviction is not observed in the data outright; instead I must infer it based on offense type. Because felony thresholds and sentencing guidelines vary from state to state, there is bound to be measurement error in this variable. Quasi-experimental studies that use random judge assignment and follow the housing trajectories of individuals who are and are not convicted of felonies may be useful in getting around concerns of behavioral confounding and measurement error, though such studies would likely be limited in how long and how accurately they could observe housing trajectories following adjudication, particularly for individuals not convicted of felonies and therefore not subject to the extended scrutiny of community supervision.

A final limitation is that I am unable to account for how local housing market context may shape the relationship between felony conviction and housing instability. Unfortunately, to my knowledge, no comprehensive database of local or state-level renter protection laws currently exists. Compiling such information and testing how the relationships explored in this paper vary across spaces with varying strength of renter (or landlord) protections would be a useful extension of this paper and could illuminate whether and when policies appear to be protective for the population of formerly convicted Americans or whether such policies may instead put such individuals at greater risk of being unstably housed.

Discussion

Prior studies of post-incarceration housing instability are useful for highlighting an important barrier to successful reentry, but they are unable to adjudicate between the relative role of removal from one's community (i.e., incarceration) versus the role of discrimination and prohibitions against individuals marked by criminal records (i.e., felon status) in increasing housing instability amongst previously incarcerated individuals. The robust evidence – including from individual fixed effect and sibling fixed effect models – that individuals with felony convictions but no incarceration history experience significantly elevated rates of housing instability suggests an independent and sizeable role of felony stigma in hampering social integration and stability both for formerly incarcerated Americans and for the millions of never-incarcerated Americans with a felony record. Thus, these findings make clear that building a better reentry program will not solve the problem of social integration following criminal justice contact, because it is not only those returning from correctional facilities who are subject to the destabilizing force of criminal justice system interaction.

By considering whether felony conviction even without incarceration has the potential to disrupt and destabilize the normal life course, this paper also adds to the burgeoning literature exploring the consequences of sub-imprisonment exposure to the criminal justice system (Bryan, 2020; Maroto & Sykes, 2019; Sugie & Turney, 2017). Given that individuals with felony convictions greatly outnumber formerly incarcerated individuals in the U.S. (Shannon et al., 2017), these findings suggest that the research literature to date has not yet made a full accounting of the costs of criminal justice system interactions, particularly for women who are far more likely to receive non-custodial sentences for felony convictions (Rosenmerkel et al., 2009).

Finally, these findings have important implications for understanding 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 United States, a variety of bipartisan criminal justice reform coalitions have emerged in recent years (e.g., Coalition for Public Safety). These efforts have largely focused on reducing the size of the criminal justice system via shorter sentences and/or greater use of community corrections. While such reforms would help reduce the number of people incarcerated at any given point in time, they would not alter the number who pass through and are marked with felon status by the justice system, nor would they alter the penalties and stigma those individuals face after conviction. Thus, altering the distribution of criminal sentences without providing greater support to increase stability for individuals who interact with the justice system is likely only to reduce the financial costs of the prison system, not the harms faced by the millions of Americans marked by justice system contact. The socioeconomic inequalities generated by criminal justice system contact are not likely to be eliminated by simply reducing incarceration but, rather, by reducing the extent of stigma and legally sanctioned marginalization that flow from criminal conviction.

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TABLES

Table 1. Descriptive Statistics for Full Sample and by Criminal Justice Contact, NLSY97

		Criminal Justice Contact		
	Full Sample	Previously Incarcerated (Felony)	Previously Convicted, Never (Yet) Incarcerated	Never (Yet) Convicted or Incarcerated
Panel A: Person-Year Level				
Outcome variables				
Number of moves in last year (mean)	0.48	0.65	0.57	0.47
std. dev.	0.69	0.84	0.76	0.68
median	0.44	0.46	0.22	0.00
max	20	8	9	20
Temporary housing	0.6%	3.2%	1.2%	0.4%
Control variables				
Currently incarcerated	1.0%	18.2%	--	--
Age (mean)	26.4	28.0	26.6	26.3
std. dev.	4.5	4.5	4.5	4.5
Highest degree completed				
None	9.8%	30.2%	23.9%	7.7%
High school diploma or GED	61.8%	65.9%	64.7%	61.3%
Some college/Associate's	6.2%	2.0%	5.4%	6.5%
Bachelor's	17.9%	1.5%	4.6%	19.8%
Graduate or professional degree	4.3%	0.4%	1.4%	4.7%
Current student	18.9%	4.8%	10.6%	20.3%
Married	30.0%	16.9%	22.1%	31.3%
Co-resident parent	35.8%	32.8%	40.2%	35.7%
Weeks employed last calendar year (mean)	37.6	25.5	34.5	38.5
std. dev.	19.6	21.8	20.8	19.2
Labor income last year (mean)	$25,734	$13,086	$23,568	$26,598
std. dev.	$28,150	$19,294	$28,183	$28,481
Gift income last year (mean)	$388	$292	$285	$399
std. dev.	$1,925	$1,112	$2,425	$1,935
Assets (mean)	$44,645	$18,772	$35,953	$46,860
std. dev.	$103,798	$68,636	$95,110	$105,925
Urban	78.5%	75.0%	83.0%	78.7%
Person-years	96,144	4,449	3,969	87,726
Panel B: Person Level				
Outcome variables (cumulative by 2017)				
Number of moves since turning 20 (mean)	6.67	9.43	7.77	6.32
std. dev.	4.30	6.01	4.42	3.99
median	6	8	7	6
max	40	40	30	39

Temporary housing since turning 20	4.2%	15.2%	7.9%	3.0%
<u>Control variables</u>				
Female	48.7%	22.4%	36.6%	52.4%
Race/Ethnicity				
White	66.7%	57.0%	66.3%	67.7%
Black	15.4%	27.0%	16.4%	14.3%
Hispanic	12.8%	13.1%	13.1%	12.8%
Other	5.1%	2.9%	4.2%	5.2%
Number of residences lived in from ages 12-16 (mean)	1.95	2.53	2.54	1.86
std. dev.	1.53	1.94	2.22	1.42
Household structure, 1997				
Lived with both biological parents	52.5%	27.6%	36.0%	55.7%
Lived with one biological parent, one step	14.7%	23.0%	16.4%	14.0%
Lived with one biological parent only	27.9%	39.9%	40.7%	26.0%
No biological parents present	5.0%	9.6%	6.9%	4.3%
Parents' education (highest degree)				
None	13.2%	24.7%	16.7%	12.0%
High school diploma or GED	30.3%	40.0%	31.3%	29.1%
Some college/Associate's	26.9%	18.6%	27.2%	27.6%
Bachelor's	15.5%	10.6%	12.9%	16.2%
Graduate or professional degree	14.1%	6.1%	11.9%	15.1%
Parents' net worth in 1997 (mean)	$108,934	$53,936	$71,559	$115,992
std. dev.	$150,336	$125,918	$116,138	$153,129
Armed Services Vocational Aptitude Battery (ASVAB) percentile (mean)	59.5	47.8	52.7	61.0
std. dev.	32.5	36.8	34.8	31.6
Weeks employed 2016 (mean)	40.1	28.8	37.1	41.2
std. dev.	19.7	23.3	21.0	19.1
Labor income 2016 (mean)	$42,775	$20,552	$35,338	$45,332
std. dev.	$43,681	$27,070	$41,667	$44,592
Gift income 2016 (mean)	$949	$938	$892	$956
std. dev.	$1,155	$1,151	$1,122	$1,159
Assets 2017 (mean)	$123,097	$44,093	$107,863	$130,871
std. dev.	$179,674	$117,472	$172,316	$182,814
At least one biological sibling also in sample	40.9%	44.7%	37.4%	40.9%
Number of biological siblings in sample, if any (mean)	1.18	1.29	1.19	1.17
std. dev.	0.43	0.58	0.43	0.41
Self-reported pre-2004 criminal activity percentile (mean)	50.9	64.2	63.0	48.7
std. dev.	18.0	18.1	16.3	17.1
Mental health index score (2000)	15.3	14.8	14.9	15.4
std. dev.	2.5	2.9	3.0	2.4
Respondents	8,705	645	367	7,693

Note: Weighted values. Descriptive statistics refer to person-years in which respondents were 20 years or older. All dollar value variables have been adjusted for inflation to 2016 values.

Table 2. Poisson Regression Predicting Number of Moves in Last Year

	(1) Demographic Controls	(2) Pre-Treatment Controls	(3) Full Controls	(4) High Crime Comparison Group	(5) Individual Fixed Effects	(6) Sibling Fixed Effects
Previous felony conviction	0.260***	0.216***	0.191***	0.131***	0.169***	0.154*
	(0.0328)	(0.0320)	(0.0317)	(0.0317)	(0.0509)	(0.0610)
Previous felony incarceration	0.151***	0.154***	0.124**	0.131**	0.0489	0.0710
	(0.0423)	(0.0425)	(0.0419)	(0.0422)	(0.0518)	(0.0809)
Non-felony incarceration	0.265***	0.238***	0.212***	0.161***	0.0324	0.148**
	(0.0318)	(0.0322)	(0.0330)	(0.0332)	(0.0476)	(0.0552)
Currently incarcerated	-0.00496	-0.0117	-0.0297	-0.0462	-0.0186	-0.0751
	(0.0409)	(0.0401)	(0.0411)	(0.0421)	(0.0414)	(0.0563)
Race/ethnicity						
Black	-0.0908***	-0.0990***	-0.138***	-0.136***		
	(0.0254)	(0.0229)	(0.0237)	(0.0290)		
Hispanic	-0.169***	-0.137***	-0.166***	-0.141***		
	(0.0323)	(0.0314)	(0.0316)	(0.0329)		
Other	-0.0449	-0.0476	-0.0807*	-0.0775†		
	(0.0363)	(0.0363)	(0.0359)	(0.0466)		
Female	0.102***	0.0908***	0.0815***	0.109***		0.0829**
	(0.0147)	(0.0138)	(0.0139)	(0.0203)		(0.0287)
Pre-treatment controls	No	Yes	Yes	Yes	Yes	Yes
Full controls	No	No	Yes	Yes	Yes	Yes
Observations (person-years)	95,976	95,976	95,976	44,726	92,659	40,280
Respondents	8,704	8,704	8,704	4,044	8,176	
Biological sibling sets						1,731

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. Individual random effects are included in all but individual and sibling fixed effects models. Age is controlled for with a set of dummy variables. Pre-treatment controls include household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, and ASVAB percentile score. Full controls include highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score.

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$ (two-tailed tests)

Table 3. Logistic Regression Predicting Current Residence in Temporary Housing

	(1) Demographic Controls	(2) Pre-Treatment Controls	(3) Full Controls	(4) High Crime Comparison Group	(5) Individual Fixed Effects	(6) Sibling Fixed Effects
Previous felony conviction	1.314***	1.099***	0.651**	0.533*	0.816*	1.791***
	(0.244)	(0.242)	(0.238)	(0.264)	(0.405)	(0.520)
Previous felony incarceration	0.926***	0.868**	0.503*	0.568*	0.773*	-0.186
	(0.274)	(0.271)	(0.250)	(0.244)	(0.386)	(0.475)
Non-felony incarceration	1.652***	1.410***	0.859***	0.735**	1.215**	0.274
	(0.276)	(0.277)	(0.257)	(0.278)	(0.438)	(0.564)
Race/ethnicity						
Black	0.447**	0.137	-0.205	-0.251		
	(0.163)	(0.166)	(0.165)	(0.206)		
Hispanic	0.205	0.112	-0.0358	0.144		
	(0.177)	(0.180)	(0.175)	(0.218)		
Other	0.674*	0.605*	0.388	0.289		
	(0.320)	(0.302)	(0.275)	(0.323)		
Female	0.141	0.0602	0.204	0.472*		0.496
	(0.144)	(0.144)	(0.147)	(0.187)		(0.375)
Pre-treatment controls	No	Yes	Yes	Yes	Yes	Yes
Full controls	No	No	Yes	Yes	Yes	Yes
Observations (person-years)	96,128	96,128	96,128	44,819	4,591	3,623
Respondents	8,705	8,705	8,705	4,044	391	
Biological sibling sets						139

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. Individual random effects are included in all but individual and sibling fixed effects models. Age is controlled for with a set of dummy variables. Pre-treatment controls include household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, and ASVAB percentile score. Full controls include highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score.

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table 4. Poisson Regression Predicting Number of Moves in Last Year, with Mechanisms Groups

	Demographic Controls	Family Background	Skills & Work	Financial Resources	Relationships & Behavior	Full Controls
Previous felony conviction	0.168***	0.135***	0.172***	0.157***	0.161***	0.131***
	(0.0325)	(0.0320)	(0.0329)	(0.0316)	(0.0322)	(0.0317)
Previous felony incarceration	0.163***	0.162***	0.160***	0.144***	0.145***	0.131**
	(0.0423)	(0.0423)	(0.0421)	(0.0423)	(0.0427)	(0.0422)
Non-felony incarceration	0.192***	0.171***	0.195***	0.171***	0.187***	0.161***
	(0.0321)	(0.0328)	(0.0319)	(0.0322)	(0.0328)	(0.0332)
Currently incarcerated	-0.0168	-0.0215	-0.0323	-0.0221	-0.0245	-0.0462
	(0.0419)	(0.0410)	(0.0420)	(0.0422)	(0.0425)	(0.0421)
Race/ethnicity						
Black	-0.0821**	-0.107***	-0.0760**	-0.106***	-0.0985***	-0.136***
	(0.0274)	(0.0284)	(0.0273)	(0.0277)	(0.0280)	(0.0290)
Hispanic	-0.131***	-0.123***	-0.120***	-0.143***	-0.145***	-0.141***
	(0.0332)	(0.0323)	(0.0354)	(0.0330)	(0.0338)	(0.0329)
Other	-0.0493	-0.0535	-0.0513	-0.0575	-0.0688	-0.0775†
	(0.0471)	(0.0466)	(0.0449)	(0.0485)	(0.0485)	(0.0466)
Female	0.145***	0.126***	0.143***	0.130***	0.139***	0.109***
	(0.0205)	(0.0197)	(0.0204)	(0.0210)	(0.0211)	(0.0203)
Household structure, 1997						
One biological parent, one stepparent		0.0915**				0.0927**
		(0.0308)				(0.0312)
One biological parent only		0.0822**				0.0782**
		(0.0255)				(0.0252)
No biological parents present		0.150***				0.154***
		(0.0413)				(0.0414)
Parents' education (highest degree)						
High school diploma/GED		0.0129				0.00547
		(0.0338)				(0.0343)
Some college/Associate's		0.0591†				0.0389
		(0.0327)				(0.0345)
Bachelor's		0.110**				0.0697
		(0.0422)				(0.0429)
Graduate or professional degree		0.175***				0.118*
		(0.0453)				(0.0477)
Parents' net worth, 1997 (ten thousands)		-0.000982				-0.00113
		(0.000894)				(0.000883)
Number of residences ages 12-16		0.0684***				0.0663***
		(0.00808)				(0.00797)

ASVAB percentile score			0.0738*			0.0490
			(0.0309)			(0.0314)
Highest degree completed						
High school diploma/GED			0.00952			0.0462
			(0.0285)			(0.0288)
Some college/Associate's			-0.0169			0.0315
			(0.0503)			(0.0479)
Bachelor's			0.104**			0.175***
			(0.0346)			(0.0353)
Graduate or professional degree			0.0522			0.155*
			(0.0717)			(0.0695)
Current student			-0.0920***			-0.0960***
			(0.0237)			(0.0229)
Weeks employed last year			-0.00151***			-0.00113**
			(0.000352)			(0.000363)
Labor income last year (thousands)				-0.000513		-0.000344
				(0.000381)		(0.000392)
Gift income last year (thousands)				0.00451		0.00534
				(0.00325)		(0.00331)
Assets (ten thousands)				-0.00765***		-0.00691***
				(0.00110)		(0.00111)
Married					-0.0754***	-0.0550**
					(0.0203)	(0.0206)
Parent					-0.0994***	-0.0929***
					(0.0208)	(0.0199)
Urban					0.125***	0.110***
					(0.0252)	(0.0240)
Mental health index (2000)					-0.0204***	-0.0157***
					(0.00347)	(0.00347)
Observations (person-years)	44,726	44,726	44,726	44,726	44,726	44,726
Respondents	4,044	4,044	4,044	4,044	4,044	4,044

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. Individual random effects are included in all models. Age is controlled for with a set of dummy variables.
*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table 5. Logistic Regression Predicting Current Residence in Temporary Housing, with Mechanisms Groups

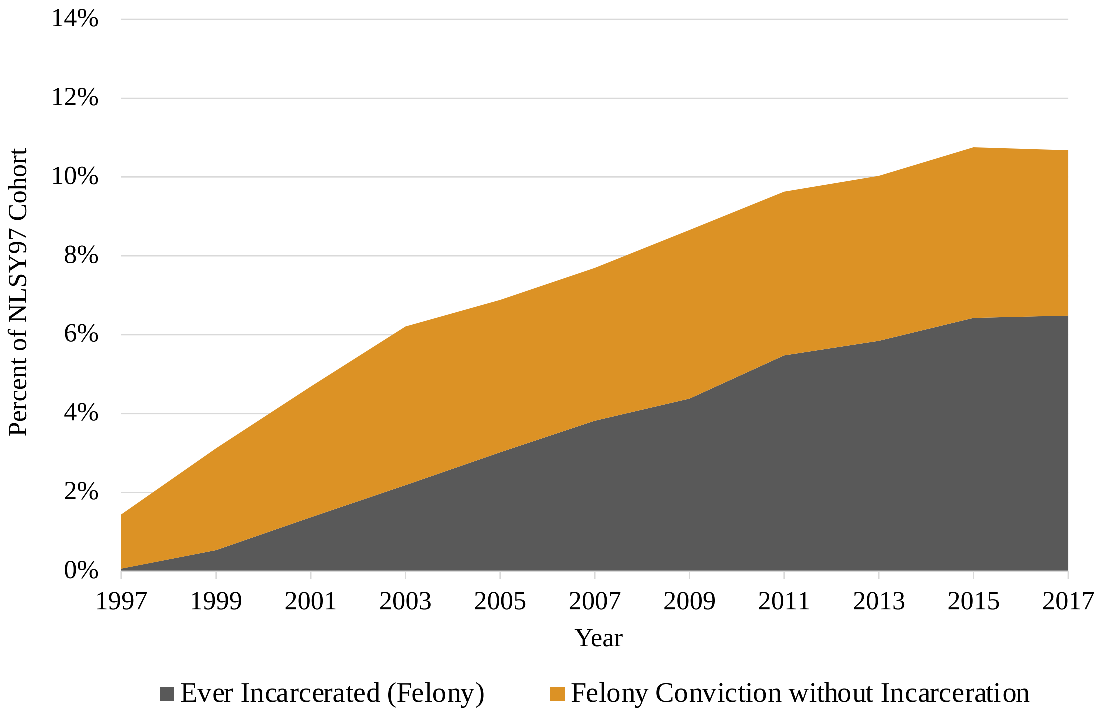
	Demographic Controls	Family Background	Skills & Work	Financial Resources	Relationships & Behavior	Full Controls
Previous felony conviction	1.030***	0.855**	0.738**	0.873***	0.970***	0.533*
	(0.268)	(0.268)	(0.260)	(0.263)	(0.266)	(0.264)
Previous felony incarceration	0.977***	0.905***	0.762**	0.724**	0.891***	0.568*
	(0.262)	(0.263)	(0.252)	(0.245)	(0.257)	(0.244)
Non-felony incarceration	1.391***	1.172***	1.015***	1.118***	1.338***	0.735**
	(0.283)	(0.290)	(0.277)	(0.275)	(0.276)	(0.278)
Race/ethnicity						
Black	0.371†	0.0546	0.111	0.118	0.297	-0.251
	(0.201)	(0.205)	(0.203)	(0.203)	(0.196)	(0.206)
Hispanic	0.368†	0.279	0.219	0.289	0.334	0.144
	(0.219)	(0.224)	(0.213)	(0.211)	(0.217)	(0.218)
Other	0.612	0.507	0.569	0.510	0.472	0.289
	(0.375)	(0.353)	(0.349)	(0.363)	(0.357)	(0.323)
Female	0.442*	0.311†	0.408*	0.256	0.569**	0.472*
	(0.179)	(0.176)	(0.176)	(0.179)	(0.184)	(0.187)
Household structure, 1997						
One biological parent, one stepparent		0.979***				0.798**
		(0.269)				(0.258)
One biological parent only		0.916***				0.736**
		(0.239)				(0.224)
No biological parents present		1.683***				1.315***
		(0.299)				(0.283)
Parents' education (highest degree)						
High school diploma/GED		-0.130				-0.0480
		(0.250)				(0.239)
Some college/Associate's		0.0664				0.113
		(0.243)				(0.235)
Bachelor's		-0.0233				0.110
		(0.353)				(0.334)
Graduate or professional degree		0.0900				0.221
		(0.347)				(0.340)
Parents' net worth, 1997 (ten thousands)		-0.00377				-0.00161
		(0.00725)				(0.00715)
Number of residences ages 12-16		0.0978*				0.0575
		(0.0428)				(0.0401)
ASVAB percentile score			0.275			0.160
			(0.233)			(0.222)

Highest degree completed						
High school diploma/GED			-0.365†			-0.262
			(0.189)			(0.187)
Some college/Associate's			-0.581†			-0.384
			(0.340)			(0.318)
Bachelor's			-1.239**			-0.980*
			(0.405)			(0.392)
Graduate or professional degree			-1.758*			-1.299†
			(0.745)			(0.785)
Current student			-0.965**			-1.052**
			(0.328)			(0.331)
Weeks employed last year			-0.0178***			-0.0132***
			(0.00317)			(0.00358)
Labor income last year (thousands)				-0.0211***		-0.0101
				(0.00616)		(0.00634)
Gift income last year (thousands)				-0.0146		-0.00378
				(0.0538)		(0.0545)
Assets (ten thousands)				-0.0295**		-0.0242*
				(0.0113)		(0.0117)
Married					-0.443*	-0.269
					(0.212)	(0.211)
Parent					-1.129***	-1.216***
					(0.207)	(0.207)
Urban					0.742**	0.781**
					(0.277)	(0.275)
Mental health index (2000)					-0.0951**	-0.0569†
					(0.0321)	(0.0296)
Observations (person-years)	44,819	44,819	44,819	44,819	44,819	44,819
Respondents	4,044	4,044	4,044	4,044	4,044	4,044

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. Individual random effects are included in all models. Age is controlled for with a set of dummy variables.
*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

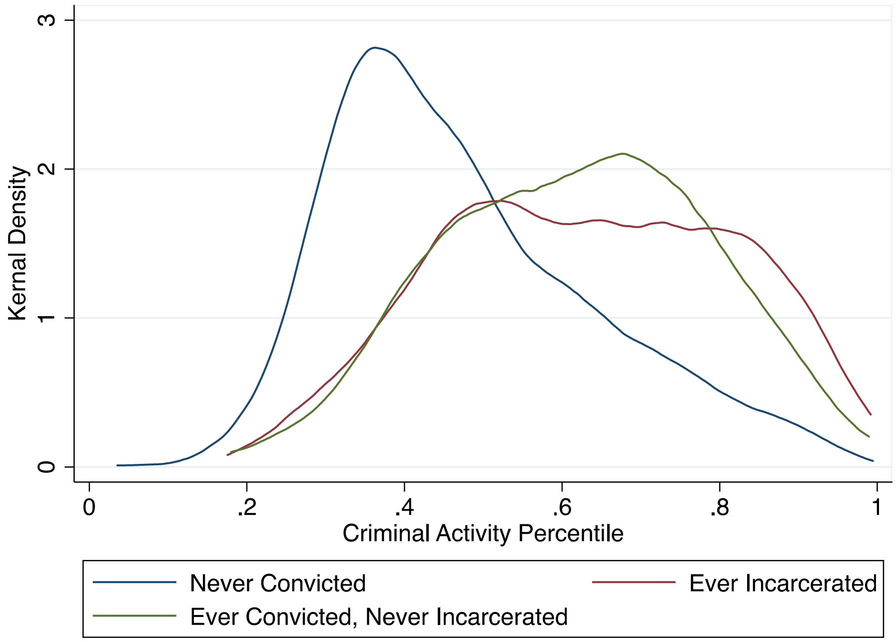
FIGURES

Figure 1. Criminal Justice Contact, NLSY97



Note: Weighted percentages.

Figure 2. Self-Reported Criminal Activity Distributions by Criminal Justice Contact, NLSY97



Note: This figure displays the distribution of age-adjusted percentile scores for self-reported criminal activity across the 1997-2003 survey years by criminal justice contact (by 2017). Criminal activity percentile scores are based on respondents' 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.

SUPPLEMENTARY TABLES

Table S1. Testing for Pre-Treatment Housing Instability Differences: Poisson Regression Predicting Number of Moves in Last Year

	High Crime Comparison Group	Sibling Fixed Effects
Will ever be felony convicted$_i$	-0.0345	0.112
	(0.0501)	(0.0684)
Previous felony conviction$_{it}$	0.106**	0.272***
	(0.0374)	(0.0507)
Will ever be incarcerated$_i$	0.132*	0.0910
	(0.0540)	(0.0709)
Previous felony incarceration$_{it}$	0.188***	0.127†
	(0.0477)	(0.0700)
Non-felony incarceration$_{it}$	0.149***	0.207***
	(0.0339)	(0.0522)
Currently incarcerated	-0.0506	-0.0543
	(0.0421)	(0.0541)
Race/ethnicity		
Black	-0.139***	
	(0.0287)	
Hispanic	-0.141***	
	(0.0327)	
Other	-0.0763†	
	(0.0462)	
Female	0.114***	0.109***
	(0.0203)	(0.0246)
Observations (person-years)	44,726	40,280
Respondents	4,044	
Biological sibling sets		1,731

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table S2. Testing for Pre-Treatment Housing Instability Differences: Logistic Regression Model of Temporary Housing

	High Crime Comparison Group	Sibling Fixed Effects
Will ever be felony convicted$_i$	0.295	0.365
	(0.457)	(0.833)
Previous felony conviction$_{it}$	0.603*	1.119**
	(0.296)	(0.358)
Will ever be incarcerated$_i$	-0.0807	-0.221
	(0.414)	(0.751)
Previous felony incarceration$_{it}$	0.545*	0.220
	(0.274)	(0.377)
Non-felony incarceration$_{it}$	0.719**	0.243
	(0.277)	(0.410)
Race/ethnicity		
Black	-0.252	
	(0.206)	
Hispanic	0.143	
	(0.220)	
Other	0.288	
	(0.321)	
Female	0.476*	0.356
	(0.185)	(0.238)
Observations (person-years)	44,819	40,357
Respondents	4,044	
Biological sibling sets		139

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table S3. Poisson Regression Predicting Number of Moves in Last Year with Time Since Last Conviction or Incarceration

	High Crime Comparison Group	Individual Fixed Effects	Sibling Fixed Effects
Previous felony conviction	0.146***	0.160**	0.183**
	(0.0390)	(0.0528)	(0.0690)
Years since last felony conviction	-0.00207	0.00290	-0.00373
	(0.00372)	(0.00419)	(0.00546)
Previous felony incarceration	0.161***	0.0701	0.119
	(0.0466)	(0.0542)	(0.0826)
Years since last incarceration release	-0.0106†	-0.00923	-0.0167*
	(0.00563)	(0.00691)	(0.00809)
Non-felony incarceration	0.162***	0.0319	0.152**
	(0.0332)	(0.0475)	(0.0551)
Currently incarcerated	-0.0707	-0.0290	-0.116*
	(0.0447)	(0.0426)	(0.0585)
Race/ethnicity			
Black	-0.135***		
	(0.0289)		
Hispanic	-0.141***		
	(0.0328)		
Other	-0.0770†		
	(0.0465)		
Female	0.108***		0.0840**
	(0.0203)		(0.0287)
Observations (person-years)	44,726	92,659	40,280
Respondents	4,044	8,176	
Biological sibling sets			1,731

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$ (two-tailed tests)

Table S4. Logistic Regression Predicting Current Residence in Temporary Housing with Time Since Last Conviction or Incarceration

	High Crime Comparison Group	Individual Fixed Effects	Sibling Fixed Effects
Previous felony conviction	1.081***	0.946*	2.185**
	(0.320)	(0.421)	(0.701)
Years since last felony conviction	-0.0795**	-0.0664†	-0.0540
	(0.0295)	(0.0359)	(0.0517)
Previous felony incarceration	0.566*	1.100**	-0.126
	(0.270)	(0.357)	(0.576)
Years since last incarceration release	-0.0684	-0.179**	-0.0937
	(0.0436)	(0.0615)	(0.0716)
Non-felony incarceration	0.706*	1.118*	0.235
	(0.277)	(0.437)	(0.569)
Race/ethnicity			
Black	-0.246		
	(0.205)		
Hispanic	0.127		
	(0.218)		
Other	0.301		
	(0.320)		
Female	0.447*		0.496
	(0.188)		(0.376)
Observations (person-years)	44,819	4,591	3,623
Respondents	4,044	391	
Biological sibling sets			139

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table S5. Poisson Regression Predicting Number of Moves in Last Year, by Felony Conviction Type

	High Crime Comparison Group	Individual Fixed Effects	Sibling Fixed Effects
Previous violent felony conviction	0.0706*	0.113†	0.0259
	(0.0358)	(0.0620)	(0.0528)
Previous drug felony conviction	-0.0266	0.0507	0.00768
	(0.0553)	(0.0983)	(0.0938)
Previous property crime felony conviction	0.0890**	0.141*	0.110†
	(0.0314)	(0.0656)	(0.0608)
Previous felony incarceration	0.171***	0.0905†	0.131*
	(0.0368)	(0.0521)	(0.0595)
Non-felony incarceration	0.148***	0.0165	0.128*
	(0.0331)	(0.0455)	(0.0552)
Currently incarcerated	-0.0489	-0.0204	-0.0788
	(0.0421)	(0.0476)	(0.0564)
Race/ethnicity			
Black	-0.135***		
	(0.0291)		
Hispanic	-0.141***		
	(0.0331)		
Other	-0.0777†		
	(0.0470)		
Female	0.110***		0.0829**
	(0.0204)		(0.0285)
	(0.00994)		
Constant	-0.715***	-0.474***	-0.738***
	(0.0825)	(0.0394)	(0.185)
Observations (person-years)	44,726	92,659	40,280
Respondents	4,044	8,176	
Biological sibling sets			1,731

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table S6. Logistic Regression Predicting Current Residence in Temporary Housing, by Felony Conviction Type

	High Crime Comparison Group	Individual Fixed Effects	Sibling Fixed Effects
Previous violent felony conviction	0.159	-0.545	0.280
	(0.274)	(0.485)	(0.622)
Previous drug felony conviction	-0.0655	-1.083*	-0.0610
	(0.291)	(0.549)	(0.674)
Previous property crime felony conviction	0.0854	0.500	0.758†
	(0.207)	(0.567)	(0.435)
Previous felony incarceration	0.878***	1.255***	0.806†
	(0.243)	(0.339)	(0.457)
Non-felony incarceration	0.641*	0.794†	0.135
	(0.268)	(0.411)	(0.517)
Race/ethnicity			
Black	-0.255		
	(0.206)		
Hispanic	0.136		
	(0.217)		
Other	0.291		
	(0.323)		
Female	0.463*		0.448
	(0.190)		(0.352)
Observations (person-years)	44,819	4,591	3,623
Respondents	4,044	391	
Biological sibling sets			139

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** p<0.001, ** p<0.01, * p<0.05, † p<0.1 (two-tailed tests)

Table S7. Race-Interacted Poisson Regression Model Predicting Number of Moves in Last Year

	Full Controls	High Crime Comparison Group
Previous felony conviction	0.168***	0.119**
	(0.0461)	(0.0456)
Previously convicted*Black	0.0535	0.0434
	(0.0740)	(0.0772)
Previously convicted*Hispanic	0.0575	0.0148
	(0.0796)	(0.0764)
Previously convicted*Other	-0.143	-0.172
	(0.171)	(0.169)
Previous felony incarceration	0.161**	0.163**
	(0.0593)	(0.0592)
Previously incarcerated*Black	-0.0749	-0.0683
	(0.0962)	(0.0964)
Previously incarcerated*Hispanic	-0.0758	-0.0679
	(0.100)	(0.101)
Previously incarcerated*Other	0.0481	0.0893
	(0.210)	(0.209)
Non-felony incarceration	0.215***	0.170***
	(0.0481)	(0.0476)
Non-felony incarceration*Black	-0.0548	-0.0515
	(0.0707)	(0.0715)
Non-felony incarceration*Hispanic	0.0149	-0.0238
	(0.0892)	(0.0925)
Non-felony incarceration*Other	0.208†	0.225†
	(0.123)	(0.125)
Currently incarcerated	-0.0263	-0.0431
	(0.0413)	(0.0424)
Race/ethnicity		
Black	-0.138***	-0.134***
	(0.0250)	(0.0317)
Hispanic	-0.168***	-0.135***
	(0.0331)	(0.0365)
Other	-0.0797*	-0.0750
	(0.0379)	(0.0489)
Female	0.0815***	0.109***
	(0.0140)	(0.0204)
Observations (person-years)	95,976	44,726
Respondents	8,704	4,044

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. Individual random effects are included in both models to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.
*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$ (two-tailed tests)

Table S8. Race-Interacted Logistic Regression Predicting Temporary Housing Residence

	Full Controls	High Crime Comparison Group
Previous felony conviction	0.409	0.0961†
	(0.370)	(0.0499)
Previously convicted*Black	0.137	0.0439
	(0.552)	(0.0841)
Previously convicted*Hispanic	0.660	0.0725
	(0.568)	(0.0871)
Previously convicted*Other	1.009	-0.209
	(1.035)	(0.197)
Previous felony incarceration	1.115**	0.184**
	(0.388)	(0.0630)
Previously incarcerated*Black	-1.034†	-0.0537
	(0.586)	(0.0985)
Previously incarcerated*Hispanic	-0.571	-0.108
	(0.572)	(0.106)
Previously incarcerated*Other	-2.142†	-0.118
	(1.134)	(0.209)
Non-felony incarceration	1.076**	0.121*
	(0.392)	(0.0502)
Non-felony incarceration*Black	-0.696	0.0314
	(0.627)	(0.0728)
Non-felony incarceration*Hispanic	-0.183	0.0214
	(0.609)	(0.0934)
Non-felony incarceration*Other	0.300	0.226†
	(0.842)	(0.127)
Race/ethnicity		
Black	-0.0122	-0.171***
	(0.179)	(0.0345)
Hispanic	-0.0829	-0.150***
	(0.186)	(0.0384)
Other	0.395	-0.0682
	(0.350)	(0.0565)
Female	0.201	0.0932***
	(0.149)	(0.0216)
Observations (person-years)	96,128	44,819
Respondents	8,705	4,044

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. Individual random effects are included in both models to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.
*** *p*<0.001, ** *p*<0.01, * *p*<0.05, † *p*<0.1 (two-tailed tests)

Table S9. Gender-Interacted Poisson Regression Model Predicting Number of Moves in Last Year

	High Crime Comparison Group	Sibling Fixed Effects
Previous felony conviction	0.116***	0.158*
	(0.0332)	(0.0652)
Previous felony conviction*Female	0.0376	-0.0103
	(0.0675)	(0.127)
Previous felony incarceration	0.126**	0.0570
	(0.0459)	(0.0839)
Previous felony incarceration*Female	0.0393	0.0598
	(0.0905)	(0.146)
Non-felony incarceration	0.150***	0.148*
	(0.0357)	(0.0615)
Non-felony incarceration*Female	0.0380	-0.00182
	(0.0696)	(0.113)
Currently incarcerated	-0.0431	-0.0719
	(0.0422)	(0.0557)
Race/ethnicity		
Black	-0.134***	
	(0.0288)	
Hispanic	-0.140***	
	(0.0329)	
Other	-0.0779†	
	(0.0462)	
Female	0.0979***	0.0812**
	(0.0220)	(0.0295)
Observations (person-years)	44,726	40,280
Respondents	4,044	
Biological sibling sets		1,731

Note: Coefficients shown are log expected count. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents' net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$ (two-tailed tests)

Table S10. Gender-Interacted Logistic Regression Model of Temporary Housing

	High Crime Comparison Group	Sibling Fixed Effects
Previous felony conviction	-0.0389	1.617*
	(0.350)	(0.647)
Previously convicted*Female	1.222**	0.131
	(0.446)	(0.945)
Previous felony incarceration	0.908**	-0.0717
	(0.335)	(0.549)
Previously incarcerated*Female	-0.591	-0.265
	(0.462)	(0.945)
Non-felony incarceration	0.345	-0.0280
	(0.339)	(0.717)
Non-felony incarceration*Female	1.099*	0.885
	(0.459)	(0.934)
Race/ethnicity		
Black	-0.244	
	(0.209)	
Hispanic	0.138	
	(0.222)	
Other	0.307	
	(0.326)	
Female	0.0961	0.436
	(0.235)	(0.390)
Observations (person-years)	44,819	3,623
Respondents	4,044	
Biological sibling sets		139

Note: Coefficients shown are log odds. Standard errors (shown in parentheses) are clustered at the Primary Sampling Unit level. The high crime comparison group model includes individual random effects to account for repeated observation of respondents across survey waves. Covariates not shown: age, household structure in 1997, parents' education level, parents’ net worth in 1997, number of residences between ages 12-16, ASVAB percentile score, highest degree completed, current student status, marital status, parent status, number of weeks worked last year, labor income last year, gift income last year, assets, urban residence, and mental health index score. Age is controlled for with a set of dummy variables.
*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$ (two-tailed tests)