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Homeownership Experiences Following Criminal Justice Contact

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Abstract

Recent work has highlighted the significance of incarceration for wealth accrual and black-white gaps in homeownership, but the monetary sanctions and disruptions to employment that often accompany even low-level criminal justice contact may also have important consequences for individual homeownership and racial disparities in homeownership. Using data from the National Longitudinal Survey of Youth 1997, this paper considers the potential of a broad variety of criminal justice system interactions to shape homeownership experiences among young adults. Using a variety of models to address concerns of unobserved confounding, I investigate how arrest, criminal charges, conviction, and incarceration relate to (1) probability of homeownership, (2) age of entry into first homeownership, and (3) homeownership duration. Results indicate that, like incarceration, these lower level forms of criminal justice contact are independently associated with lower levels of homeownership, delayed entry into homeownership, and shorter duration of homeownership among respondents who succeed in becoming homeowners. Given the importance of homeownership for individual wealth accumulation and racial wealth gaps, as well as sizeable racial disparities in criminal justice contact in the U.S., these findings illuminate a potentially important pathway through which racial disparities in socioeconomic wellbeing are reinforced.

Introduction

A notable amount of both scholarly and political attention has been devoted to the consequences of mass incarceration in the United States in recent years. Researchers and advocates alike have pointed to the sharp increase in national incarceration rates over the last four decades and the consequently large population of former prisoners as cause for concern (Charles Koch Institute 2019; National Research Council 2014). According to recent estimates, there were 5 million former prisoners in the American population in 2010, up from a historic average of about 1 million throughout most of the 20th century (Shannon et al. 2017).

A sizeable research literature reveals that the consequences of incarceration do not stop at the prison gate. Incarceration appears to lead to greater disadvantage and marginalization in individuals' lives along almost every dimension, from health to socioeconomic well-being, and extending even to their children's well-being (Adams 2018; Bryan 2017; Massoglia and Pridemore 2015; Western et al. 2015). Much of this research suggests that the link between incarceration and these various forms of marginalization and disadvantage is not driven purely by selection into incarceration but is causal. Moreover, in addition to exacerbating disadvantage in the individual life course, the fact that incarceration is unequally distributed in the population – concentrated among racial minorities and the less educated – has led researchers to highlight incarceration as a driver of both the production and reproduction of poverty and inequality in American society (National Research Council 2014; Wakefield and Uggen 2010; Western and Pettit 2010). Recent work by Schneider and Turney (2015), for example, suggests that incarceration contributes to the black-white homeownership gap in aggregate, which, in turn, has important implications for black-white wealth disparities given that homeownership is an important contributor to individual wealth (Killewald and Bryan 2016).

The research literature to date has largely overlooked the implications of the broader criminal justice system, though. Incarceration marks the most intensive form of criminal justice system contact, but the reach of criminal justice extends far beyond prisons and jails, touching the millions of Americans who are arrested, charged, and convicted of crimes in any given year. While 2.2 million adults were incarcerated in American prisons and jails in 2016, an additional 4.5 million were under community supervision (i.e., on parole or probation), and 10.6 million were arrested over the course of 2016 (Federal Bureau of Investigation n.d.; Kaeble and Cowhig 2018).

Recent research highlights how these less intensive but far more common forms of criminal justice contact affect individual mental health (Sugie and Turney 2017), but relatively little is known about how these experiences shape socioeconomic wellbeing or opportunity (but see Maroto and Sykes 2019). Using nationally-representative data from the National Longitudinal Survey of Youth 1997 (NLSY97), I explore the consequences of these broad varieties of criminal justice contact for individual socioeconomic wellbeing by examining how individual histories of arrest, criminal charges, conviction, and incarceration relate to homeownership, age of entry into first homeownership, and duration of homeownership. I find robust evidence that not just incarceration but also arrest, criminal charges, and conviction are associated with lower probability of homeownership, later entry into homeownership, and shortened duration of homeownership. In the conclusion I discuss the implications of these findings for racial disparities in homeownership and wealth.

Background

Homeownership in the United States

Principal residence makes up the largest share of household wealth – more than 60 percent of total assets – for the vast majority of Americans (Wolff 2016). Moreover, homeownership is a wealth-generating status. Recent estimates indicate that each year of homeownership increases wealth in mid-life by about $6,800, on average (Killewald and Bryan 2016). Homeownership may also facilitate entry into other wealth-generating investments like entrepreneurial activity (Adelino, Schoar, and Severino 2015; Black, de Meza, and Jeffreys 1996). Additionally, homeownership is associated with a variety of other benefits, including better health outcomes for homeowners and higher cognitive scores and educational achievement for the children of homeowners (Dietz and Haurin 2003).

However, racial disparities in homeownership are vast (Kuebler and Rugh 2013; U.S. Census Bureau 2018), and asset returns to homeownership are racially-graded, with white homeowners receiving annual wealth returns to homeownership twice as large as those received by black homeowners and 60 percent larger than those received by Hispanic homeowners (Killewald and Bryan 2016). Thus, racial disparities in homeownership and the returns to homeownership are major contributors to the black-white and Hispanic-white wealth gaps in the United States (Killewald and Bryan 2016).

Exposure to Criminal Justice System Contact

A vast literature has documented the dramatic growth in the scale of incarceration in the United States, racial disparities in this growth, and the often deleterious consequences of incarceration for individuals, families, and communities (National Research Council 2014). But relatively little is known about the lower-level but more common forms of criminal justice

contact that precede incarceration. While just 5 percent of white men and 27 percent of black men have been imprisoned at some point in time by their early-30s (Western and Pettit 2010), more than one-third (38 percent) of white men and half (49 percent) of black men have already been arrested at least once by age 23 (Brame et al. 2014). Despite the markedly higher prevalence of arrest and conviction (Lerman and Weaver 2014) relative to incarceration, researchers have paid far greater attention to the implications of incarceration, rather than of arrest or conviction, for individual wellbeing (Sugie and Turney 2017).

This prior research has linked incarceration to labor market discrimination (Pager 2003; Pager, Western, and Bonikowski 2009), lower wages (Apel and Sweeten 2010; Western 2002), decreased employment levels (Holzer 2009), diminished earnings (Western, Kling, and Weiman 2001), and very low upward economic mobility (Pew Charitable Trusts 2010). Another vein of research connects prior incarceration to poorer mental health (Schnittker, Massoglia, and Uggen 2012; Turney, Wildeman, and Schnittker 2012), diminished physical health (Massoglia 2008a, 2008b; Schnittker and John 2007), and poorer health behaviors (Porter 2014) among formerly incarcerated adults. While health and employment-related outcomes have received the bulk of scholarly attention in the area of individual-level consequences of incarceration, additional research suggests that incarceration leads to subsequent relationship dissolution (Lopoo and Western 2005; Turney and Wildeman 2013), housing instability (Geller and Curtis 2011; Harding, Morenoff, and Herbert 2013; Warner 2015), and diminished civic participation (Lerman and Weaver 2014; Weaver and Lerman 2010).

More recent work has explored the relationship between incarceration, wealth, and homeownership. Although individuals who will eventually be incarcerated typically have lower asset levels that the general population even prior to their incarceration (Zaw, Hamilton, and Darity 2016), these studies find that asset levels, as well as ownership of assets (i.e., homes, bank

accounts, vehicles), decrease markedly following incarceration (Maroto 2015; Turney and Schneider 2016). Turney and Schneider (2016) find that recent incarceration is associated with lower likelihood of owning a vehicle or having a bank account, while Maroto (2015) finds that the probability of homeownership decreases by 28 percentage points and individual net worth decreases by $42,000, on average, following incarceration.

Moreover, the wealth effects of incarceration appear to spill over to family members and close relations as well. Research indicates that the incarceration of a household member is associated with decreases in both assets and debts (Sykes and Maroto 2016), while the incarceration of a romantic partner is associated with decreased asset ownership among women who share children with recently incarcerated men (Turney and Schneider 2016). Recent work by Bruce Western (2018) that highlights the extensive amount of financial and in-kind support that family members (who maintain relationships with formerly-incarcerated individuals) provide in the wake of incarceration helps to explain why incarceration may be detrimental to the assets of not just incarcerated individuals but also their close relations.

Most of the prior research on the consequences of criminal justice contact relies upon nationally-representative survey datasets (e.g., the National Longitudinal Survey of Youth 1979, the Fragile Families and Child Wellbeing Study), however, that have collected information only on incarceration. As a result, the literature on consequences of other forms of justice system contact is far more limited. But there is evidence that arrest decreases probability of high school completion and may have minor labor market implications in early adulthood (Bushway 1998; Sweeten 2006). Moreover, recent studies using the same dataset I utilize here (NLSY97) find that arrest, independent of conviction or incarceration, is associated with deleterious mental health outcomes, as well as asset and debt declines in early adulthood (Maroto and Sykes 2019; Sugie and Turney 2017). In fact, Sugie and Turney (2017) find that accounting for earlier

criminal justice contact in the form of arrest may explain a large proportion of the negative relationship between incarceration and mental health observed in prior studies. And while Maroto and Sykes (2019) find that incarceration is more disruptive to the wealth profiles of young adults than arrest, they find that arrest also has independent direct and indirect effects on both assets and debt.

This study, thus, contributes to the existing literature on the consequences of criminal justice contact for wealth by taking advantage of the more granular information about justice system interactions available in the NLSY97 to consider how a broader set of criminal justice system encounters relate to subsequent homeownership experiences. Moreover, where previous studies have considered the implications of incarceration for current homeownership, I extend the literature by considering novel outcomes that characterize trajectories of homeownership careers (i.e., age of first entry into homeownership and duration of homeownership).

Potential Pathways from Criminal Justice Contact to Homeownership Outcomes

The most immediate way in which criminal justice contact is likely to influence homeownership outcomes is through its potential effect on financial resources. While the prior research literature has primarily considered the implications of incarceration and/or felon status for employment outcomes, the ease with which potential employers can access criminal background checks that include even arrest records means that lower-level justice contact may also affect an individual's ability to gain employment (Lageson 2016; Uggen et al. 2014). Moreover, time lost to arrest processing, pretrial detention, court appearances, and/or probation and parole meetings may cause justice-system-involved individuals to miss out on work shifts or even lose their jobs entirely. The direct financial costs of criminal justice system interaction in the form of fines, fees, restitution orders, and other financial obligations to the court system and

other criminal justice agencies (Harris 2016; Harris, Evans, and Beckett 2010) are also likely to affect an individual's ability to save enough to enter homeownership, as well as to be able to maintain mortgage payments on already purchased homes.

Prior research on the consequences of incarceration also highlights the potential of criminal justice contact to destabilize and dissolve romantic partner relationships (Lopoo and Western 2005; Turney 2015). Research on the relationship implications of lower-level justice system contact is limited, but ethnographic work by Goffman (2009) highlights how justice system involvement may promote unpredictable behavior among young men, which can destabilize their romantic relationships. Thus, the potentially deleterious consequences of criminal justice system contact for relationship formation and maintenance may inhibit justice-system-involved individuals' ability to pool financial resources with a partner in order to enter into homeownership or to maintain monthly mortgage payments.

Alternatively, it is possible that criminal justice contact could incentivize entry into homeownership, conditional on financial resources, given that landlords may legally discriminate against prospective tenants based on prior criminal history (Delgado 2005; Helfgott 1997; Leasure and Martin 2017; Thacher 2008). Thus, homeownership may be especially attractive to individuals with criminal histories as a means of avoiding the scrutiny and restricted options available to them in the rental market.

Data & Methods

I use data from the National Longitudinal Survey of Youth 1997 (NLSY97), which has collected detailed information on employment, education, assets, 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.[1] From 1997 to 2011, the NLSY97 surveys were conducted annually; as of 2013 data collection is biennial. The most recent survey year for which data are available is 2015, at which point sample members were 30-36 years old[2]; 79 percent of original sample members participated (Bureau of Labor Statistics 2019d).

Although NLSY97 respondents are still relatively young as of the most recent survey year, they are at peak age for entry into homeownership. As of 2018, the median age for first-time homebuyers was 32 (National Association of Realtors 2018). Moreover, the NLSY97 data provide a rare opportunity to assess how criminal justice system contact may be affecting the ability of millennials to enter into and maintain homeownership. Compared to other nationally-representative, longitudinal surveys, the NLSY97 is exceptional for the depth and breadth of information it collects about criminal justice system contact. Whereas other surveys neglect criminal justice encounters or collect data only on incarceration history, the NLSY97 collects self-reported data on all arrests, charges, convictions, and incarceration spells since age 12.[3]

The NLSY97 has collected data on homeownership intermittently since 1998 and at each survey wave since 2007. In 1998 and 1999, respondents who were 18 or older or living independently were asked whether they owned their current place of residence and, if so, when they purchased it. In 2000-2006, respondents were asked about homeownership if they either (1) were newly independent or (2) had turned 18, 20, or 25 years old since their last interview (Bureau of Labor Statistics 2019b). Since 2007, all respondents have been asked about homeownership at each survey. Because of the collection strategy, missingness on

[1] To be eligible for the sample, respondents must have been 12 to 16 years old on December 31, 1996. Because the latest interviews for the first survey round were completed in March through May 1998, some respondents were 17 and 18 years old at the time of their first survey (i.e., "survey year" 1997) (Bureau of Labor Statistics 2019e).
[2] Interviews for the 2015 "survey year" were completed between October 2015 and August 2016.
[3] Minor traffic violations are excluded from NLSY97 data collection on criminal justice contact. Major traffic offenses (e.g., vehicular manslaughter) are included in data collection.

homeownership information is a function primarily of respondent age.[4] Therefore, I include all person-years in which respondents were 18 or older with non-missing homeownership data in the following analyses (N=72,923 person-years covering 8,792 respondents), always controlling for respondent age.[5] I exclude 470 person-years in which respondents were living overseas from the analysis.

Outcome Measures

I first examine *current homeownership* at each survey wave. I consider respondents to be homeowners if they report that they (1) own their current residence independently or jointly with a spouse/partner or (2) continue to own a prior residence, even if they no longer live in it. I do not consider respondents who report living in a residence that is owned entirely by their spouse/partner to be homeowners, as the goal of this paper is to understand how an individual's prior criminal justice contact influences his/her ability to transition into homeownership and accrue the wealth benefits of homeownership.

Next, I consider *age at first homeownership* for the 3,843 respondents who report ever owning a home by 2015. I calculate age at first homeownership based on respondent's age at the first reported date of home purchase.[6] In the rare event that respondents report a date of purchase that is earlier than their 18th birthday, I bottom code age at first homeownership to 18.

[4] Homeownership status is known for 57 percent of all non-missing person-years in the NLSY97. At least 97 percent of respondents asked homeownership questions in any given survey year provided valid responses. Appendix Table A1 displays the proportion of respondents who were asked about homeownership, and the share of those respondents who provided valid responses, at each survey wave.
[5] Some respondents who were living independently prior to age 18 responded to questions about homeownership, but I exclude these person-years (N=1,943) from the analysis.
[6] For respondents who previously reported that their spouse/partner owned the home entirely and then later report that they jointly, or independently, own that same home, I pull purchase date from prior reports of the spouse's/partner's purchase date for that unit, when available.

In cases where respondents do not report the purchase date of their first owned home – e.g., respondents who inherit homes are not asked to report a purchase date – I code age at first homeownership as current age minus the number of years the respondent has lived in the same unit that they now report owning without moving, bottom coded at age 18.[7] Results are consistent if I instead use current age at the first survey date in which the respondent reports owning a home in cases where purchase date of first home is not reported.

Finally, I examine *total years of homeownership to date* at each survey, conditional on age of first home purchase, for respondents who ever own homes. For each reported homeownership spell, I calculate duration of homeownership as the difference between the date the unit was sold, when reported, or the last survey date at which the respondent was observed to own that unit and the respondent-reported purchase date.[8] If respondents do not report a purchase date, I calculate homeownership spell duration as the number of years that the respondent lived in the same unit.[9] If respondents report a date of purchase that is earlier than their 18th birthday and consistent homeownership since, I top code duration to number of years since the respondent turned 18. I then sum up duration of all reported homeownership spells to create the total years of homeownership variable.

Criminal Justice Contact Measures

In the following analyses I use time-varying indicator variables identifying whether respondents report having ever been *arrested*, *charged* with a crime, *convicted*, and/or

[7] Purchase date is missing in 7% of person-years in which respondents are homeowners. Respondents report at each wave whether they have moved residences since the date of their last interview.
[8] As with age at first homeownership, I use spouse's/partner's purchase date for the unit when respondents report jointly or independently owning a home that they originally reported as owned entirely by their spouse/partner.
[9] Because purchase date is rarely missing, results are consistent when I instead calculate homeownership spell start date using the date of interview when homeownership was first reported.

incarcerated by each interview date. I also include an indicator variable identifying whether respondents are *currently incarcerated* (i.e., residing in detention facility at the time of the interview) in any given survey year to ensure that the *previously incarcerated* variable captures prior incarceration.[10]

It is important to note that the measures of criminal justice contact I use are not mutually exclusive. Thus, given that individuals who have been convicted of a crime have necessarily also been charged with one and, typically, arrested as well, the coefficients in the models presented below represent the separate, usually additive, relationships between each of these forms of criminal justice contact and homeownership. I have also run the analyses on the following set of mutually exclusive criminal justice contact variables: *arrested, never charged*; *charged, never convicted*; *convicted, never incarcerated*; *previously incarcerated (and convicted)*; and *ever detained pretrial*. Because the reference group in these models becomes individuals who have never been arrested, the coefficients grow accordingly – particularly for higher levels of criminal justice contact, like incarceration and conviction – but the results are substantively consistent with those presented below. These results are available upon request.

Control Variables

In all models I control for demographic characteristics, individual achieved characteristics, family background characteristics, and contextual characteristics that are likely to affect both probability of criminal justice system interaction and homeownership experiences. I include *age* as a linear term and gender as an indicator variable set equal to one if the respondent is *female*. Respondents' *race and ethnicity* are captured in the following four discrete categories:

[10] In contrast to many surveys, the NLSY has made concerted efforts to continue interviewing sample members even when they are incarcerated, which has contributed to their high retention rates (Bureau of Labor Statistics 2019c, 2019d).

white non-Hispanic, black non-Hispanic, Hispanic, and other. White non-Hispanic is the reference category.

I also include an indicator variable for respondent's *cohabitation* status, as cohabitation may enable cost savings and resource sharing that could bolster an individual's ability to transition into or maintain homeownership. Moreover, stable relationships promote desistance from delinquent behaviors and crime and, therefore, should be associated with lower probability of criminal justice contact (Laub and Sampson 1993, 2001). Only romantic domestic partners or married respondents who currently reside with their spouse are coded as cohabiting. I account for respondents' financial resources and recent employment history by including a measure of total *wages and salary in the prior year*. I also include a measure of respondent's *spouse's or partner's wages and salary in the prior year* for cohabiting individuals. Respondents without a cohabiting partner are coded as having zero spouse/partner income. Both income variables are adjusted for inflation to 2014 dollars.

I further account for respondent's financial resources with a measure of respondent's net worth. NLSY97 collects data on the assets and debts of respondents and, if applicable, their spouse or partner in the first interview during or after the calendar year in which they turn 20, 25, 30, and 35. I subtract out the value of assets and debts that respondents report their spouses or partners do not share with them and adjust values for inflation to 2014 dollars. Because individual asset levels may be endogenous with both criminal justice contact and, especially, homeownership, I only include *net worth at age 20* in the models. However, results are substantively consistent when I instead control for assets with a one-year lagged measure of individual net worth with multiply-imputed values for years in which asset data were not collected.

Given the association between education level and both criminal justice contact and homeownership (Western 2006; Young 2017), I also account for respondent's *highest degree completed* to date among the following five categories: none (reference category), high school diploma or GED, Associate's or some college, Bachelor's, or graduate or professional degree. I also include an indicator variable to identify *current students*.

I attempt to control for respondents' criminal activity and/or proclivity by including indicator variables set equal to one if the respondent reports *ever using marijuana since the last interview*, *ever using hard drugs since the last interview*, or *ever carrying a gun since the last interview*. Ideally, I would include a much fuller set of behavioral controls to account for differences in criminal activity and/or likelihood of entering the criminal justice system. While NLSY97 collects self-reported data on a range of other criminal behaviors (e.g., assault, drug sales, theft) across multiple survey waves, starting in 2004, NLSY97 restricts these questions to respondents who report having previously been arrested and a small subsample of other randomly-selected respondents.[11] Only gun carrying, hard drug usage, and marijuana usage are asked of all respondents at each survey round since 1998. Although this is a limited set of behaviors, these variables should provide some information about the extent to which respondents are engaging in activities that could draw the attention of legal authorities.

In addition to these time-varying individual characteristics, I also include several family background characteristics that could influence both probability of interacting with the criminal

[11] In supplementary analyses I control for respondent behavior with a measure of respondent's average standardized criminal activity score across the 1998 to 2003 survey years, using responses to questions about gun carrying, property destruction, theft, property crimes, assault, marijuana sales, hard drug sales, marijuana usage, and hard drug usage. Because respondents' ages during this period ranged from 12 to 24, I create an age-adjusted standardized score of average self-reported criminal activity over this period, standardizing respondent's behavior within the total distribution of self-reported criminal activity at each age before creating a multi-year average score. Results from models that use this alternative control for criminal behavior are substantively consistent with the main model results reported here.

justice system and likelihood of entering into homeownership in early adulthood. I control for respondent's *household structure in 1997*, measured as a categorical variable containing the following four categories: lived with both biological parents (reference category), lived with one biological parent and one stepparent, lived with one biological parent only, and some other living arrangement. I also include parents' education, coded as the highest degree completed by either of the respondent's resident parents (biological, step, adoptive, or foster) in 1997. To make coding comparable to that used for respondent education, highest grade level completed is translated into highest degree received using standard assumptions about length of time to degree. Parents who reported fewer than 12 years of completed education are coded as having no diploma or degree (reference category), those with exactly 12 years are coded as having a high school diploma, those with 13-15 years are coded as having completed some college, those with exactly 16 years are coded as college graduates, and those reporting more than 16 years are coded as having a graduate or professional degree. Additionally, I also account for parents' net worth in 1997, adjusted for inflation to 2014 dollars.

Finally, because prevalence of criminal justice contact and home costs vary by region and urbanicity, I include time-varying measures of each. Region is captured by indicator variables identifying residence in each of the four Census regions: *Northeast*, *Midwest*, *South* (reference category), and *West*. Finally, I include a variable indicating urban versus *rural* residence, according to Census Bureau standards (Bureau of Labor Statistics 2019a). Respondents whose zip code includes both urban and rural areas are coded as *unknown*. Urban residence is the reference category.[12] I use chained multiple imputation to fill in missing values for control

[12] I also tried models that controlled for respondent residence in a Census-designated Metropolitan Statistical Area (MSA) or Core-Based Statistical Areas (CBSA) but found that adding these measures sometimes caused problems with model convergence and did not markedly alter coefficients or improve model fit.

variables (Acock 2005; Davey, J. Shanahan, and L. Schafer 2001; Little and Rubin 2019). I do not impute missing values for criminal justice contact or outcome (i.e., homeownership) variables.[13]

Analytic Strategy

In considering each of the outcomes noted above, I first run a model that considers incarceration only, modelling the approach typically seen in papers assessing the implications of criminal justice contact for individual outcomes. The second model includes all forms of criminal justice contact noted above. By comparing the coefficient on *previously incarcerated* across these first two models, I can get a sense of how much the prior incarceration variable in Model 1 is picking up some of the partial effects of lower-level criminal justice involvement. Doing so will highlight the extent to which prior research that only considers incarceration may have conflated the direct effect of incarceration with the separate effects of other forms of justice system contact.

Because there may be concerns about whether the covariates I include in the models fully capture selection into criminal justice contact, I next run a difference-in-differences-inspired model (Model 3) in order to test whether homeownership patterns between individuals who will eventually have contact with the criminal justice system and those who will not differ significantly even before initial justice system contact. A traditional difference-in-difference

[13] Multiple imputation by chained equations involves estimating a model for each covariate with at least one missing value to fill in missing values based on values of other covariates. The type of model used to fill in each missing value is determined based on the structure of the variable. For example, ordinal variables, like highest degree completed, are filled in using ordered logit models, while binary variables (e.g., current student status) are imputed using logit models and continuous variables (e.g., net worth at age 20) are imputed by OLS regression. Uncertainty about imputed values is factored into final estimates by creating multiple imputed datasets and combining results from them, taking into account the variance of imputed values across datasets. I use 10 imputed datasets.

model includes a treatment group indicator to identify individuals who will eventually be exposed to the treatment of interest, a time dummy, which identifies observations before versus after exposure to treatment (for the treated group), and an interaction term of these two variables. Because of the inclusion of this interaction term, which denotes how exposure to treatment has changed the outcome trajectories of the treated group, the coefficient on the treatment group indicator variable represents the average difference in outcomes between the treatment group and the control group *prior to treatment*. Thus, this coefficient allows researchers to test the "common trends assumption" – that is, the assumption that, but for exposure to treatment, treatment group members and control group members would have similar outcomes on average – by testing whether the two groups significantly differ from each other before treatment exposure (Angrist and Pischke 2009).

In this case, exposure to "treatment" (i.e., criminal justice contact) does not occur at a single point in time for all respondents who have contact with the criminal justice system. However, by adding "treatment group" variables – that is, indicator variables that identify individuals who will eventually have contact with the criminal justice system – to the model and interacting them with the time-varying criminal justice contact variables I note above (e.g., *previously arrested*), I can attempt to test the "common trends assumption" in a similar way to a traditional difference-in-differences model. As with a traditional difference-in-differences setup, because of the inclusion of the interaction term, the coefficients on the treatment group dummy variables (e.g., *will ever be arrested*) will indicate whether the homeownership experiences of respondents who eventually interact with the criminal justice system differ significantly from those of respondents who never encounter the justice system even prior to first criminal justice contact, conditional on covariates included in the model. Thus, this difference-in-differences-style model allows me to test whether my covariates successfully control for pre-treatment

differences relevant to homeownership outcomes between NLSY97 respondents who will eventually have some form of criminal justice contact by 2015 and those who will not.

At the same time, because of the inclusion of the "treatment group" indicator variables in this difference-in-differences-inspired model, the coefficients on the prior criminal justice contact*treatment group dummy interaction terms in this model represent the post-criminal-justice-exposure difference in homeownership outcomes among respondents in the "treatment" group – e.g., the difference in homeownership following conviction for individuals who will ever be convicted.[14] In this way, the difference-in-differences-inspired model estimates function in a manner somewhat similar to a fixed effects model, in that estimates of the "treatment effect" are based off of variation in homeownership outcomes only among individuals who will be "treated" at some point in time and who may, thus, differ in important unobservable ways from individuals who never interact with the criminal justice system. Yet, because comparisons are not restricted within person – only within treatment group – the difference-in-difference-style model does not drop individuals who never differ in their criminal justice contact over the observation period (e.g., respondents who have already been arrested by age 18) from the model as fixed effects models do.

Finally, I run an individual fixed effect model (Model 4), which addresses concerns of unobserved confounding due to time-invariant, individual-level characteristics (e.g., self-control) by comparing individuals' homeownership patterns following criminal justice system contact with their own homeownership patterns prior to justice system contact. Because fixed effects models rely on within-person comparisons, individuals without any criminal justice system interaction and those whose criminal justice contact does not vary after age 18 (e.g., individuals

[14] The uninteracted time-varying treatment indicators (e.g., *previously arrested*) necessarily drop out of the model because they are perfectly collinear with the treatment group indicator*post-treatment indicator interaction term.

first arrested by age 18 but never charged, convicted, or incarcerated after that) necessarily drop out of these models and do not contribute to the estimation of coefficients. As a result, the results of these models may be less generalizable to the population as a whole, but they represent the strongest test of the relationship between justice system contact and homeownership for individuals who interact with the criminal justice system during early adulthood. Thus, if readers are willing to accept the assumption that there are no time-varying confounding variables not accounted for in this model, then the coefficients produced by the fixed effect models can be interpreted as the causal effects of each form of criminal justice contact on homeownership outcomes.[15]

I use logistic regression for models predicting current homeownership and OLS regression for models of age at first homeownership and total homeownership duration to date. In models predicting current homeownership and duration, which are pooled across years, I cluster standard errors at the individual level to account for repeated observation of respondents. For models of age at first homeownership, I use covariate values from the survey wave prior to homeownership entry to predict age of entry into homeownership. Rather than controlling for age in this model, as age is the outcome of interest, I control for how many years prior to homeownership entry covariates were observed to account for differing amounts of time between

[15] In the case of this analysis, it is possible that in the intervening period between interview dates a respondent may enter into homeownership and subsequently have their first (potential) interaction with the criminal justice system. In such a case, if homeownership affects one's probability of being arrested, charged, etc. then the relationship between homeownership and criminal justice contact may be endogenous. Unfortunately, to my knowledge no researchers have yet addressed this question or the broader question of whether wealth affects criminal justice outcomes independent of the many confounding variables correlated with both wealth and probability of criminal justice contact. A descriptive analysis by Zaw, Hamilton, and Darity (2016) finds that probability of future incarceration is lower among higher wealth individuals, but the authors do not account for any confounding factors other than race.

prior survey response and entry into first homeownership across respondents. Mean number of years between prior survey wave and entry into homeownership is 1.25.

Results

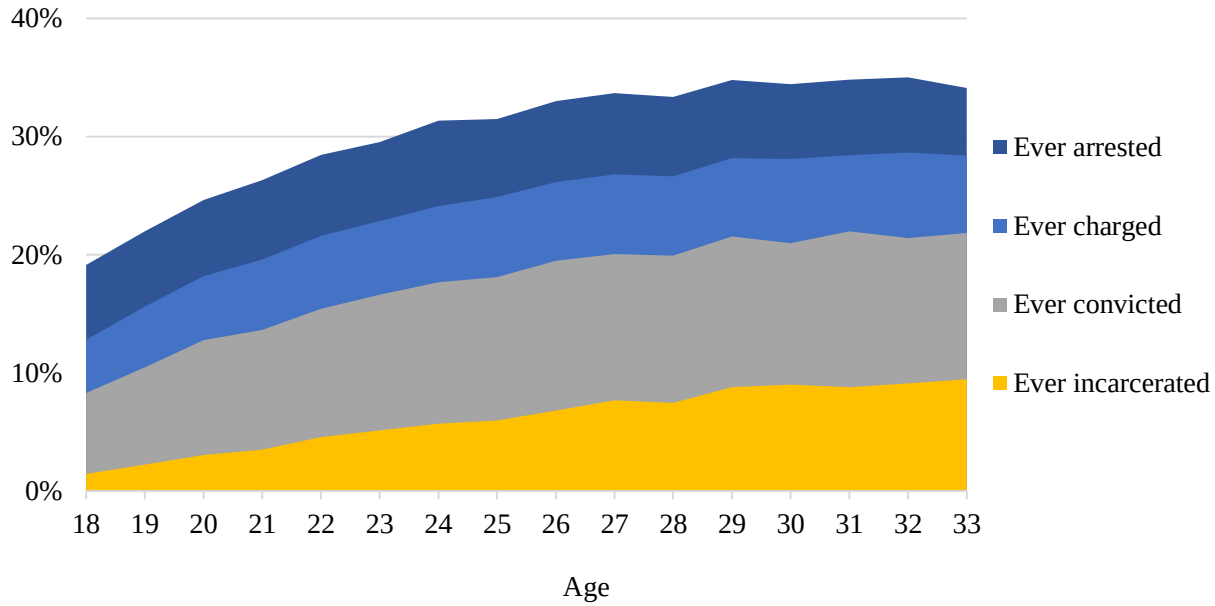
Descriptive Characteristics[16]

Figure 1 displays exposure to criminal justice contact by age for the 8,792 analytic sample members. At age 18, less than two percent of sample members have been incarcerated, but eight percent have been convicted of a crime, 13 percent have ever been charged, and 19 percent have ever been arrested. By age 30, nine percent of sample members have ever been incarcerated, but more than one-third (34 percent) have ever been arrested.[17] Criminal justice contact is not evenly distributed across race/ethnicity, though. By age 30, 40 percent of black (non-Hispanic) sample members report ever having been arrested, compared to just 33 percent of white (non-Hispanic) sample members and 36 percent of Hispanic sample members. The same disparity can be seen across all forms of criminal justice contact, with whites reporting lower rates of contact than blacks and Hispanic sample members falling somewhere between the two (see Table 1).

[16] I apply sample weights for the 8,792 respondents in my analytical sample – i.e., those who ever report valid homeownership data – to the descriptive statistics presented in Figures 1 and 2 and Tables 1 and 2 but not to the models. Results of weighted models are consistent with those presented here and are available upon request.

[17] Figure 1 displays cumulative contact history by age for all sample members who were observed at that age, but because data collection became biennial starting in 2013 (when respondents were 28 to 34 years old), respondents are not observed at every age. Thus, the proportion of sample members who have even been arrested drops from .35 at age 32 to .341 at age 33 in Figure 1 because a slightly different subset of respondents was observed at each of these ages.

Figure 1. Criminal Justice Contact History by Age, NLSY97



Note: Weighted values for analytic sample members (8,792 respondents; 72,923 person-years).

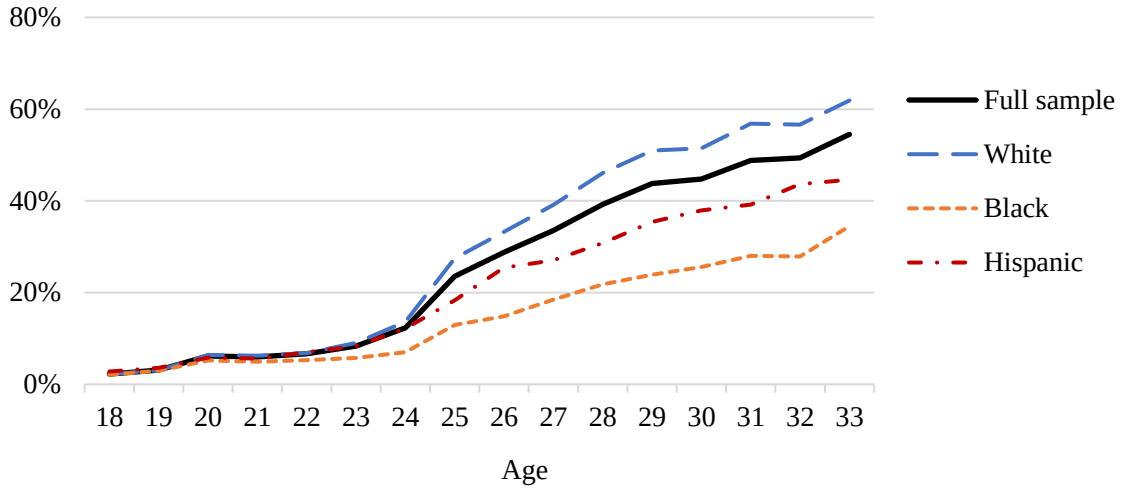
Table 1. Criminal Justice Contact by Age 30, by Race

	Full Sample	White (non-Hispanic)	Black (non-Hispanic)	Hispanic
Ever arrested	34.6%	33.2%	39.6%	35.5%
Ever charged	28.2%	27.8%	30.4%	29.0%
Ever convicted	21.1%	20.7%	23.3%	21.9%
Ever incarcerated	9.1%	8.0%	12.9%	9.7%

Note: Weighted values for analytic sample members (8,792 respondents; 72,923 person-years).

Figure 2 displays the percent of NLSY97 sample members who have ever reported owning a home at any time by age and race. At age 18 homeownership levels are similar across all racial/ethnic groups, but by the early 30s white respondents have taken the lead in homeownership rates. At age 30, 41 percent of white NLSY97 respondents have ever owned a home, compared to 27 percent of Hispanic respondents and only 16 percent of black respondents.

Figure 2. Ever Yet Owned a Home by Race, NLSY97



Note: Weighted values for analytic sample members (8,792 respondents; 72,923 person-years).

Table 2 displays descriptive statistics for all homeownership, criminal justice contact, and control variables described above. The top panel of the table shows percentages, means, and standard deviations for time-varying characteristics, and the bottom panel displays descriptive statistics for fixed characteristics, like race and age at first homeownership. As prior literature suggests, respondents who have interacted with the criminal justice system show higher levels of disadvantage across every outcome. Educational attainment, income, and net worth are all lower among justice-system-involved individuals, particularly ever-incarcerated individuals, relative to the full sample. Respondents with any level of contact with the justice system also come from more disadvantaged families: their parents report lower education levels and lower net worth, and they are far less likely to have lived in two-parent households in adolescence than other NLSY97 sample members. Justice-system-involved individuals also report higher levels of drug use and gun carrying than the full sample members, though these behaviors are still relatively uncommon.

In terms of homeownership outcomes, homeownership in any given year is increasingly less common and total years of homeownership to date decreases with each form of criminal justice contact. A similar pattern holds with regard to average age at first homeownership – with age at first homeownership generally increasing with each additional form of justice system contact – but differences are relatively small. As is apparent from the other covariates in Table 2, however, individuals involved with the justice system differ from non-justice-system-involved individuals in a variety of important ways that may confound any bivariate relationship between justice system contact and homeownership outcomes. Thus, Tables 3-5 display results from multivariate models of the relationship between justice system contact and homeownership outcomes.

Table 2. Descriptive Statistics

	Full Sample	Ever Arrested	Ever Charged	Ever Convicted	Ever Incarcerated
Time-Varying					
Outcome Variables					
Currently own home	22.2%	16.6%	16.3%	15.1%	11.2%
Years of homeownership	1.13	0.91	0.89	0.82	0.77
	(2.78)	(2.65)	(2.55)	(2.55)	(3.02)
Criminal Justice Contact					
Ever arrested	30.6%	100.0%	99.4%	99.6%	99.0%
Ever charged	24.0%	77.9%	100.0%	100.0%	97.6%
Ever convicted	17.7%	57.7%	72.5%	100.0%	98.1%
Ever incarcerated	6.5%	21.0%	26.4%	35.9%	100.0%
Control Variables					
Age	25.7	26.5	26.7	26.9	27.6
	(4.7)	(4.4)	(4.3)	(4.3)	(4.0)
Cohabiting	43.2%	42.7%	42.9%	42.9%	39.4%
Own labor income last year	$23,829	$21,407	$21,441	$20,910	$15,576
	(27,244)	(25,492)	(25,354)	(24,697)	(20,775)
Spouse/partner labor income last year	$29,471	$21,520	$21,287	$20,278	$15,728
	(33,084)	(27,214)	(26,906)	(25,872)	(21,672)

Highest degree completed					
None	13.4%	21.5%	21.6%	22.9%	30.1%
High school	59.9%	63.9%	63.7%	64.3%	64.0%
Associate's, some college	5.6%	4.4%	4.4%	4.4%	3.1%
Bachelor's	16.9%	8.6%	8.8%	7.0%	2.4%
Graduate or professional degree	4.2%	1.6%	1.6%	1.3%	0.4%
Current student	22.3%	13.1%	12.4%	10.9%	6.4%
Ever used marijuana since last interview	20.9%	31.1%	32.8%	33.5%	32.0%
Ever used hard drugs since last interview	5.1%	9.0%	9.8%	10.6%	10.5%
Ever carried gun since last interview	5.3%	7.0%	6.6%	6.8%	6.9%
Region					
Northeast	16.6%	15.5%	15.6%	14.9%	11.6%
Midwest	24.7%	25.2%	26.2%	28.2%	27.6%
South	37.0%	38.2%	37.4%	36.8%	40.1%
West	21.7%	21.1%	20.8%	20.1%	20.7%
Urbanicity					
Rural	21.7%	20.5%	20.9%	22.2%	25.4%
Urban	75.3%	76.8%	76.5%	75.3%	72.0%
Unknown (urban or rural)	3.1%	2.7%	2.6%	2.4%	2.6%
N (person-years)	72,923	22,674	17,571	12,840	4,867
Fixed Characteristics					
Outcome Variables					
Age at 1st homeownership	25.4	25.6	25.6	25.8	25.5
	(3.9)	(4.2)	(4.2)	(4.3)	(4.6)
Control Variables					
Female	48.7%	32.8%	30.8%	28.5%	21.8%
Race/ethnicity					
White (non-Hispanic)	66.7%	63.3%	65.0%	65.5%	58.3%
Black (non-Hispanic)	15.4%	19.2%	18.2%	17.9%	24.2%
Hispanic	12.8%	13.3%	12.9%	12.6%	13.3%
Other (non-Hispanic)	5.1%	4.3%	3.9%	4.0%	4.3%
Individual net worth at 20	$14,155	$11,811	$11,509	$10,548	$8,239
	(32,226)	(26,847)	(26,368)	(24,207)	(17,360)
Household structure, 1997					
Both biological parents present	53.0%	41.1%	41.3%	39.4%	31.3%
One biological parent, one stepparent	14.5%	17.5%	17.8%	18.4%	20.6%
One biological parent only	27.6%	34.9%	34.0%	34.7%	37.9%
Other (no biological parents present)	4.9%	6.5%	7.0%	7.4%	10.3%
Parents' education					
None	13.4%	17.9%	17.6%	18.5%	22.6%
High school	30.3%	32.6%	32.5%	33.8%	39.8%
Associate's, some college	26.7%	25.6%	25.9%	24.8%	21.0%
Bachelor's	15.6%	13.8%	13.3%	13.3%	10.3%
Graduate or professional degree	14.1%	10.2%	10.7%	9.7%	6.3%
Parents' net worth, 1997	$109,910	$88,487	$87,085	$82,542	$68,241
	(150,641)	(140,314)	(138,955)	(132,735)	(133,093)
N (respondents)	8,792	3,088	2,494	1,897	844

Note: Descriptives are based on weighted, nonimputed person-years. Standard deviations in parentheses.

Current Homeownership

Table 3 displays the results from logistic regression models predicting current homeownership at each survey wave conditional on criminal justice contact and the covariates described above. Coefficients are in log-odds form. For ease of interpretation, Figure 3 displays the coefficients on the previous criminal justice contact variables from linear probability models of current homeownership.[18] (Table A2 in the appendix displays all coefficients from the linear probability model version of Table 3.)

The first model replicates the approach used in much of the prior literature on the consequences of criminal justice contact by including incarceration as the only form of justice system contact. In this model, prior incarceration is strongly and negatively associated with

[18] Because marginal predicted probabilities cannot be calculated in the same way for difference-in-difference and fixed effect logit models as for standard logit models, linear probability models (LPMs) offer a more interpretable approach that can be used for all four models in Table 3. The LPM coefficients for Models 1 and 2 are very similar to the marginal predicted probability differences (with all other covariates at their mean values) for Models 1 and 2. For example, the marginal difference in predicted probability for *previously incarcerated* in Model 2 is -.025, while the LPM coefficient on *previously incarcerated* in Model 2 is -.021. Likewise, the predicted probability marginal difference for *previously convicted* in Model 2 is -.022, while the LPM coefficient in Model 2 is -.029.

Table 3. Logistic Regression Models Predicting Current Homeownership

	(1) Incarceration Only	(2) Full Criminal Justice Contact	(3) Difference-in-differences	(4) Fixed Effects
Previously arrested		-0.123		-0.107
		(0.0871)		(0.220)
Will ever be arrested (not yet arrested)			-0.143	
			(0.199)	
Will ever be arrested*Previously arrested			-0.127	
			(0.0911)	
Previously charged		0.0218		-0.724**
		(0.113)		(0.241)
Will ever be charged (not yet charged)			0.231	
			(0.205)	
Will ever be charged*Previously charged			0.0420	
			(0.117)	
Previously convicted		-0.227*		-0.120
		(0.102)		(0.214)
Will ever be convicted (not yet convicted)			-0.234	
			(0.175)	
Will ever be convicted*Previously convicted			-0.236*	
			(0.106)	
Previously incarcerated	-0.497***	-0.260*		-1.009***
	(0.103)	(0.115)		(0.202)
Will ever be incarcerated (not yet incarcerated)			-0.146	
			(0.139)	
Will ever be incarcerated*Previously incarcerated			-0.274*	
			(0.118)	
Currently incarcerated	-1.183***	-1.204***	-1.207***	-0.885*
	(0.312)	(0.311)	(0.311)	(0.386)
Age	0.103***	0.105***	0.104***	0.242***
	(0.00428)	(0.00432)	(0.00437)	(0.00603)
Female	0.00825	-0.0269	-0.0319	
	(0.0460)	(0.0465)	(0.0467)	
Race/ethnicity				
Black (non-Hispanic)	-0.554***	-0.565***	-0.564***	
	(0.0625)	(0.0626)	(0.0626)	
Hispanic	-0.227***	-0.236***	-0.236***	
	(0.0641)	(0.0643)	(0.0642)	
Other (non-Hispanic)	-0.402**	-0.403**	-0.401**	
	(0.139)	(0.138)	(0.138)	
Cohabitating	1.494***	1.495***	1.494***	1.362***
	(0.0419)	(0.0420)	(0.0420)	(0.0495)
Own labor income last year (in 10,000s)	0.176***	0.175***	0.174***	0.148***
	(0.00833)	(0.00833)	(0.00834)	(0.0101)
Spouse/partner labor income last year (in 10,000s)	0.114***	0.114***	0.114***	0.0972***
	(0.00691)	(0.00691)	(0.00691)	(0.00914)
Individual net worth at 20 (in 10,000s)	0.0439***	0.0432***	0.0431***	
	(0.00668)	(0.00668)	(0.00667)	
Highest degree completed				
High school	0.334***	0.306***	0.300***	-0.505***
	(0.0792)	(0.0792)	(0.0795)	(0.117)

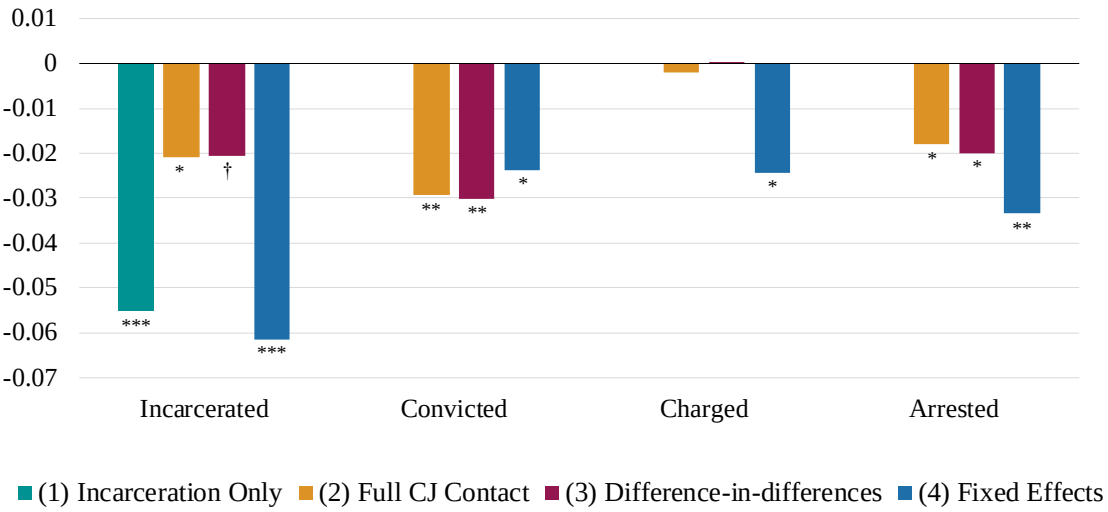
Associate's, some college	0.740***	0.696***	0.689***	-0.212
	(0.106)	(0.106)	(0.106)	(0.175)
Bachelor's	0.647***	0.589***	0.581***	-0.0125
	(0.0958)	(0.0964)	(0.0966)	(0.170)
Graduate or professional degree	0.714***	0.651***	0.645***	0.645**
	(0.121)	(0.122)	(0.122)	(0.237)
Current student	-0.193***	-0.202***	-0.206***	-0.221***
	(0.0460)	(0.0462)	(0.0463)	(0.0602)
Ever used marijuana since last interview	-0.359***	-0.326***	-0.322***	-0.0133
	(0.0529)	(0.0533)	(0.0534)	(0.0692)
Ever used hard drugs since last interview	-0.376***	-0.340***	-0.330***	-0.348**
	(0.0997)	(0.0999)	(0.0997)	(0.123)
Ever carried a gun since last interview	0.445***	0.441***	0.442***	0.270**
	(0.0739)	(0.0741)	(0.0742)	(0.0951)
Household structure in 1997				
One biological parent, one stepparent	-0.203**	-0.193**	-0.190**	
	(0.0651)	(0.0654)	(0.0654)	
One biological parent only	-0.230***	-0.216***	-0.214***	
	(0.0555)	(0.0558)	(0.0558)	
Other (no biological parents present)	-0.471***	-0.460***	-0.456***	
	(0.111)	(0.112)	(0.112)	
Parents' education				
High school	-0.0514	-0.0554	-0.0570	
	(0.0670)	(0.0670)	(0.0670)	
Some college	-0.0757	-0.0776	-0.0808	
	(0.0709)	(0.0709)	(0.0710)	
Bachelor's	-0.140	-0.136	-0.137	
	(0.0860)	(0.0860)	(0.0860)	
Graduate or professional degree	-0.226*	-0.231*	-0.233*	
	(0.0958)	(0.0958)	(0.0958)	
Parents' net worth in 1997 (in 10,000s)	0.00415*	0.00416*	0.00416*	
	(0.00176)	(0.00177)	(0.00177)	
Region				
Northeast	-0.623***	-0.624***	-0.626***	-0.549**
	(0.0687)	(0.0687)	(0.0687)	(0.196)
Midwest	0.183***	0.189***	0.188***	0.140
	(0.0536)	(0.0537)	(0.0537)	(0.150)
West	-0.415***	-0.418***	-0.420***	-0.986***
	(0.0598)	(0.0599)	(0.0599)	(0.148)
Rural	0.669***	0.664***	0.665***	0.441***
	(0.0482)	(0.0483)	(0.0483)	(0.0571)
Unknown (urban or rural)	0.548***	0.543***	0.544***	0.324***
	(0.0792)	(0.0793)	(0.0794)	(0.0977)
Observations (person-years)	72,923	72,923	72,923	33,845
Respondents	8,792	8,792	8,792	3,778

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

Note: Standard errors clustered at the individual level.

probability of current homeownership. The coefficients in Table 3 indicate that, all else held equal, odds of homeownership are 40 percent lower among respondents who have ever been incarcerated than among otherwise similar never-incarcerated respondents (1 - exp[-.497] = .392). Results from the linear probability model, displayed in Figure 3, indicate that the probability of homeownership in any given year is 5.5 percentage points lower among formerly-incarcerated individuals relative to observably similar never-incarcerated individuals.

Figure 3. Difference in Probability of Current Homeownership by Criminal Justice Contact



*** p<0.001, ** p<0.01, * p<0.05, † p<0.1
Note: Coefficients from linear probability model. Standard errors clustered at the individual level.

Because ever-incarcerated individuals have necessarily encountered the criminal justice system at lower levels, however, part of what the coefficient on *previously incarcerated* in Model 1 reflects is the relationship between those lower-level forms of justice system contact and homeownership – though some members of the reference group (never incarcerated) will have also been exposed to these forms of criminal justice contact. Thus, by incorporating the fuller range of criminal justice contacts captured by NLSY97 in Models 2-4, we can get a better

sense of how much each of these less severe forms of contact contributes toward homeownership differentials and how much incarceration independently adds to disparities in homeownership. When I add measures of prior arrest, prior charge, and prior conviction to the model, the coefficient on *previously incarcerated* drops substantially in magnitude (in Models 2 and 3) but remains statistically significant.

The coefficients in Model 2 indicate that conviction and incarceration are each independently associated with significantly lower log odds of homeownership. All else held equal, conviction is associated with a 20 percent decrease in the odds of homeownership (1 - exp[-.227] = .203) – or a 3 percentage point lower probability of being a homeowner – while incarceration is associated with an additional 23 percent decrease in odds of homeownership (1 - exp[-.26] = .229) – or a 2 percentage point lower probability – above and beyond the influence of any conviction that preceded incarceration. Prior arrest is also associated with significantly lower probability of homeownership (2 percentage points lower) in the linear probability model (Figure 3), but the difference is not statistically significant in the logit model (Table 3).

Model 3 uses a difference-in-differences-inspired setup to test whether homeownership patterns among respondents who will ever have contact with the justice system already differ from those of individuals who will never have such contact even before actual exposure to arrest, conviction, etc. In this model, the coefficients on the treatment group dummy variables (e.g., *will ever be arrested*) indicate the extent to which homeownership levels already differ significantly between respondents who will encounter the criminal justice system in the future (but have not yet) and those who report no interactions with the justice system by 2015, conditional on covariates included in the model. The Model 3 results indicate that differences in homeownership levels before criminal justice system contact (for those who will eventually have

it) are not statistically significant, suggesting that the included covariates are successful at capturing important sources of potential confounding.

The coefficients on the interaction terms in this model reflect the average difference in "pre-treatment" versus "post-treatment" homeownership levels for respondents who will ever experience any of these forms of criminal justice contact. As in Model 2, conviction and incarceration are significantly and negatively associated with log odds of homeownership. Homeownership levels decrease by about 3 percentage points, on average, following first conviction, while incarceration is associated with an additional 2 percentage point decrease in probability of homeownership (Figure 3), all else held equal. In the linear probability model (Figure 3), arrest is again associated with significantly lower probability of homeownership (2 percentage points lower), although the difference is not statistically significant in the logit model (Table 3).

Finally, Model 4 in Table 2 displays the results from a fixed effects logit model of current homeownership. Because the fixed effect model relies upon within-person comparisons to estimate the effects of arrest, being charged, conviction, and incarceration on homeownership, unobservable, individual-level fixed characteristics that may confound the relationship between criminal justice contact and homeownership necessarily drop out of this model. The fixed effect model does not, therefore, incorporate the homeownership experiences of never-arrested individuals in estimating any of the coefficients because these individuals have no within-person variation in criminal justice system contact over time.

In the fixed effects logit model (Table 3), the relationship between conviction and homeownership is no longer statistically significant and is about half its size in Models 2 and 3. Instead, having been charged with a crime appears to emerge as the more relevant event in relationship to homeownership among the justice-system-involved individuals included in the

fixed effect model. The coefficient on *previously incarcerated* remains highly statistically significant and is even larger in magnitude than in prior models. The fixed effect coefficient from the linear probability model (Figure 3), indicates that probability of homeownership in any given year drops by about 6 percentage points, on average, following first incarceration.

In the fixed effects linear probability model, the negative association between conviction and homeownership remains statistically significant ($p<0.05$), and the coefficient on *previously arrested* becomes statistically significant ($p<0.01$). (As in the fixed effect logit model, the *previously charged* coefficient is negative and statistically significant.) However, because linear probability models are not designed to fit binary outcome variables as well as logit models, the significant coefficients that emerge only in this model may not be as reliable as those from the logit model (Table 3).

Although the findings from the logit fixed effect model are somewhat inconsistent with those in Models 1-4 and the linear probability models of homeownership, the most important takeaway is that incarceration does not appear to be the only form of criminal justice contact associated with lower probability of homeownership. Instead, conviction, and potentially arrest and being charged, appear to pick up part of the association between incarceration and homeownership observed in the naïve model (Model 1).

Age at First Entry into Homeownership

Table 4 displays results from OLS regression models predicting age at first reported homeownership for respondents who are ever observed to own homes by 2015. Because the outcome is age at first homeownership, positive coefficients in these models indicate a later entry into initial homeownership. Models 1 through 3 mirror those used in Table 3, but there is no fixed effect model because only one person-year (the last survey prior to entry into

homeownership) is used for models of age at first homeownership. In lieu of a fixed effect model, Table 4 includes a final model that is restricted to respondents who report ever having been arrested by 2015. While not as restrictive as a fixed effects model, this model setup should

Table 4. Regression Models Predicting Age at First Homeownership

	(1) Incarceration Only	(2) Full Criminal Justice Contact	(3) Difference-in-differences	(4) Will ever be arrested only
Previously arrested		0.272		1.525***
		(0.200)		(0.309)
Will ever be arrested (not yet arrested)			-0.509	
			(0.369)	
Will ever be arrested*Previously arrested			0.290	
			(0.209)	
Previously charged		0.558*		0.520†
		(0.270)		(0.288)
Will ever be charged (not yet charged)			-0.436	
			(0.418)	
Will ever be charged*Previously charged			0.508†	
			(0.279)	
Previously convicted		0.562*		0.568*
		(0.252)		(0.267)
Will ever be convicted (not yet convicted)			-0.399	
			(0.364)	
Will ever be convicted*Previously convicted			0.507*	
			(0.257)	
Previously incarcerated	2.577***	1.525***		1.505***
	(0.282)	(0.314)		(0.337)
Will ever be incarcerated (not yet incarcerated)			-0.344	
			(0.319)	
Will ever be incarcerated*Previously incarcerated			1.453***	
			(0.315)	
Currently incarcerated	-1.333	-1.355	-1.424	-1.658
	(0.977)	(0.969)	(0.965)	(1.023)
Years between covariate observation and homeownership entry	0.913***	0.921***	0.912***	1.091***
	(0.0365)	(0.0362)	(0.0361)	(0.0977)
Female	-0.402***	-0.267*	-0.311**	-0.0131
	(0.106)	(0.107)	(0.106)	(0.220)
Race/ethnicity				
Black (non-Hispanic)	0.572***	0.611***	0.614***	0.810**
	(0.146)	(0.144)	(0.144)	(0.277)
Hispanic	0.404**	0.438**	0.432**	0.407
	(0.147)	(0.146)	(0.145)	(0.275)
Other (non-Hispanic)	0.156	0.214	0.215	0.512
	(0.274)	(0.272)	(0.271)	(0.632)
Cohabitating	1.253***	1.232***	1.208***	1.139***
	(0.103)	(0.103)	(0.102)	(0.204)
Own labor income last year (in 10,000s)	0.395***	0.394***	0.387***	0.377***
	(0.0234)	(0.0231)	(0.0231)	(0.0490)

Spouse/partner labor income last year (in 10,000s)	0.112***	0.113***	0.110***	0.130**
	(0.0214)	(0.0209)	(0.0207)	(0.0443)
Individual net worth at 20 (in 10,000s)	-0.0444**	-0.0432**	-0.0442**	-0.0731*
	(0.0137)	(0.0136)	(0.0135)	(0.0298)
Highest degree completed				
High school	3.450***	3.451***	3.312***	3.042***
	(0.146)	(0.145)	(0.147)	(0.259)
Associate's, some college	4.805***	4.839***	4.673***	4.010***
	(0.241)	(0.239)	(0.240)	(0.507)
Bachelor's	5.096***	5.178***	5.000***	4.680***
	(0.192)	(0.190)	(0.192)	(0.411)
Graduate or professional degree	6.579***	6.719***	6.539***	5.985***
	(0.290)	(0.288)	(0.289)	(0.856)
Current student	-1.493***	-1.402***	-1.433***	-1.495***
	(0.120)	(0.120)	(0.119)	(0.249)
Ever used marijuana since last interview	-0.274*	-0.400**	-0.290*	-0.651**
	(0.139)	(0.139)	(0.139)	(0.227)
Ever used hard drugs since last interview	-0.0797	-0.219	-0.144	0.130
	(0.273)	(0.272)	(0.271)	(0.388)
Ever carried a gun since last interview	0.120	0.0977	0.121	-0.424
	(0.202)	(0.200)	(0.200)	(0.341)
Household structure in 1997				
One biological parent, one stepparent	0.197	0.134	0.140	0.0342
	(0.151)	(0.150)	(0.149)	(0.279)
One biological parent only	0.411***	0.340**	0.355**	0.581*
	(0.124)	(0.123)	(0.123)	(0.236)
Other (no biological parents present)	0.372	0.301	0.368	-0.142
	(0.258)	(0.256)	(0.255)	(0.437)
Parents' education				
High school	-0.212	-0.184	-0.190	-0.212
	(0.170)	(0.169)	(0.169)	(0.296)
Some college	-0.296†	-0.268	-0.296†	-0.371
	(0.176)	(0.175)	(0.175)	(0.324)
Bachelor's	-0.182	-0.167	-0.163	0.219
	(0.205)	(0.204)	(0.204)	(0.382)
Graduate or professional degree	-0.129	-0.102	-0.119	0.647
	(0.221)	(0.220)	(0.219)	(0.440)
Parents' net worth in 1997 (in 10,000s)	-0.00688	-0.00659	-0.00658	-0.00736
	(0.00440)	(0.00435)	(0.00431)	(0.00866)
Region				
Northeast	0.151	0.162	0.155	0.477
	(0.156)	(0.154)	(0.154)	(0.312)
Midwest	-0.102	-0.116	-0.110	0.139
	(0.125)	(0.124)	(0.123)	(0.242)
West	-0.00347	0.00886	-0.00799	0.351
	(0.137)	(0.136)	(0.136)	(0.273)
Rural	-0.282*	-0.255*	-0.235†	0.00145
	(0.122)	(0.121)	(0.121)	(0.234)
Unknown (urban or rural)	-0.727**	-0.702**	-0.667**	-0.690
	(0.252)	(0.250)	(0.249)	(0.526)
Constant	19.47***	19.16***	19.43***	17.88***
	(0.236)	(0.239)	(0.242)	(0.474)

Observations (respondents)	3,843	3,843	3,843	1,095

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

Note: Models restricted to respondents who are ever observed to own a home by 2015. Standard errors clustered at the individual level.

help allay concerns that individuals who will ever interact with the criminal justice system differ from those who will not in important unobservable ways that could also affect their probability of homeownership.

Because the Table 4 models only include one observation per respondent, the treatment group variables (e.g., *will ever be arrested*) in the difference-in-differences-style model (Model 3) represent the average difference (in years) in age at entry into first homeownership between respondents who will eventually be arrested, for example, but have not yet been before their first homeownership spell and respondents who will never be arrested, conditional on covariates. The coefficients on the interaction terms (e.g., *will ever be arrested*previously arrested*), on the other hand, represent the average difference in age at entry into first homeownership between respondents who have already interacted with the justice system prior to first homeownership and those who will eventually interact with the justice system in the same way (e.g., arrest) but have not yet before first homeownership.

The outcome has changed, but the substantive findings from these models are similar to those from the Table 3 models of current homeownership. Criminal charges, conviction, and incarceration are all independently associated with significantly later entry into first homeownership across all models. In the naïve model (Model 1), incarceration appears to be associated with a 2.6 year delay in entry into homeownership relative to otherwise similar individuals, but the size of this difference drops to roughly 1.5 years once earlier forms of criminal justice contact are taken into account (Models 2-4). Being charged with a crime is associated with about a half year delay in entry into first homeownership, and being convicted is associated with an additional half year delay in entry into homeownership. Finally, while the association between arrest and age at first homeownership is not statistically significant in the full sample (Models 2 and 3), among respondents who will ever be arrested (Model 4), those

who have already been arrested prior to first homeownership become homeowners a full 1.5 years later, on average, than respondents who will eventually be arrested but have not yet been prior to first homeownership.

Years of Homeownership

As the Table 3 models show, criminal justice contact appears to be a barrier to entry into homeownership. But for those justice-system-involved individuals who do manage to cross the threshold into homeownership, their ability to do so appears to be delayed relative to that of observably-similar peers who have not had criminal justice contact (Table 4). The Table 5 models of years of total homeownership to date provide insight into the question of how long justice-system-involved individuals who *do* successfully become homeowners at some point are able to maintain homeownership relative to same-aged peers, even when differences in age at first entry into homeownership are taken into account. On the whole, the Table 5 results indicate that even for the subset of criminal-justice-system-involved individuals who do successfully become homeowners, interactions with the justice system are associated with shorter total duration of homeownership than is experienced by otherwise similar individuals without justice system involvement.

As with models of current homeownership and age at first entry into homeownership, we see that the size of the coefficient on *previously incarcerated* in the naïve model (Model 1) drops markedly once earlier forms of criminal justice contact are added to the model. While Model 1 suggests that prior incarceration is associated with .7 fewer years of homeownership, on average, conditional on covariates and age of first homeownership, the coefficient on *previously incarcerated* drops to roughly half that size (.36 to .41) once earlier forms of criminal justice contact are accounted for. In particular, arrest and conviction each have a consistently significant

Table 5. Regression Models Predicting Total Years of Homeownership To Date

	(1) Incarceration Only	(2) Full Criminal Justice Contact	(3) Difference-in-differences	(4) Fixed Effects
Previously arrested		-0.242**		-0.425***
		(0.0843)		(0.0776)
Will ever be arrested (not yet arrested)			0.159	
			(0.101)	
Will ever be arrested*Previously arrested			-0.221*	
			(0.0906)	
Previously charged		-0.0472		-0.114
		(0.111)		(0.0957)
Will ever be charged (not yet charged)			-0.0622	
			(0.109)	
Will ever be charged*Previously charged			-0.0584	
			(0.116)	
Previously convicted		-0.206*		-0.469***
		(0.0975)		(0.0864)
Will ever be convicted (not yet convicted)			0.0824	
			(0.0831)	
Will ever be convicted*Previously convicted			-0.192†	
			(0.0998)	
Previously incarcerated	-0.720***	-0.364†		-0.413***
	(0.213)	(0.218)		(0.0959)
Will ever be incarcerated (not yet incarcerated)			-0.0941	
			(0.0968)	
Will ever be incarcerated*Previously incarcerated			-0.377†	
			(0.219)	
Currently incarcerated	-0.641*	-0.679*	-0.675*	-0.00182
	(0.290)	(0.291)	(0.291)	(0.167)
Age	0.394***	0.399***	0.400***	0.442***
	(0.00955)	(0.00968)	(0.00967)	(0.00304)
Age at first homeownership	-0.231***	-0.231***	-0.231***	
	(0.00634)	(0.00632)	(0.00633)	
Female	0.0367	-0.00583	0.00208	
	(0.0363)	(0.0364)	(0.0367)	
Race/ethnicity				
Black (non-Hispanic)	-0.0641	-0.0749	-0.0753	
	(0.0550)	(0.0550)	(0.0550)	
Hispanic	0.0201	0.00815	0.00802	
	(0.0613)	(0.0611)	(0.0613)	
Other (non-Hispanic)	0.117	0.102	0.103	
	(0.112)	(0.113)	(0.113)	
Cohabitating	-0.211***	-0.210***	-0.209***	-0.292***
	(0.0515)	(0.0515)	(0.0515)	(0.0258)
Own labor income last year (in 10,000s)	0.00744	0.00593	0.00609	-0.0168***
	(0.00954)	(0.00949)	(0.00949)	(0.00500)
Spouse/partner labor income last year (in 10,000s)	0.0175***	0.0158**	0.0158**	0.0106**
	(0.00498)	(0.00503)	(0.00504)	(0.00390)
Individual net worth at 20 (in 10,000s)	0.00564	0.00511	0.00526	
	(0.00495)	(0.00493)	(0.00494)	
Highest degree completed				
High school	-1.142***	-1.156***	-1.149***	-1.677***
	(0.0650)	(0.0648)	(0.0651)	(0.0312)

Associate's, some college	-1.137***	-1.180***	-1.174***	-1.996***
	(0.125)	(0.126)	(0.126)	(0.0656)
Bachelor's	-1.598***	-1.658***	-1.651***	-2.523***
	(0.0973)	(0.0981)	(0.0985)	(0.0521)
Graduate or professional degree	-1.568***	-1.651***	-1.646***	-2.555***
	(0.130)	(0.132)	(0.132)	(0.0806)
Current student	0.438***	0.401***	0.400***	0.147***
	(0.0344)	(0.0345)	(0.0346)	(0.0274)
Ever used marijuana since last interview	-0.00501	0.0456	0.0391	0.0646*
	(0.0403)	(0.0403)	(0.0404)	(0.0299)
Ever used hard drugs since last interview	-0.103†	-0.0518	-0.0587	0.0398
	(0.0576)	(0.0578)	(0.0580)	(0.0504)
Ever carried a gun since last interview	-0.0361	-0.0334	-0.0344	-0.0197
	(0.0627)	(0.0624)	(0.0626)	(0.0455)
Household structure in 1997				
One biological parent, one stepparent	-0.142**	-0.126*	-0.128*	
	(0.0510)	(0.0508)	(0.0508)	
One biological parent only	-0.0974†	-0.0738	-0.0772	
	(0.0516)	(0.0520)	(0.0521)	
Other (no biological parents present)	-0.305***	-0.285***	-0.287***	
	(0.0797)	(0.0793)	(0.0793)	
Parents' education				
High school	0.131†	0.124†	0.124†	
	(0.0707)	(0.0705)	(0.0705)	
Some college	0.0882	0.0809	0.0817	
	(0.0701)	(0.0698)	(0.0700)	
Bachelor's	0.0648	0.0624	0.0601	
	(0.0691)	(0.0690)	(0.0691)	
Graduate or professional degree	0.0961	0.0820	0.0820	
	(0.0757)	(0.0754)	(0.0756)	
Parents' net worth in 1997 (in 10,000s)	0.00160	0.00159	0.00158	
	(0.00112)	(0.00112)	(0.00112)	
Region				
Northeast	-0.0134	-0.0132	-0.0133	0.226**
	(0.0506)	(0.0504)	(0.0504)	(0.0848)
Midwest	-0.0190	-0.0126	-0.0137	0.0864
	(0.0425)	(0.0425)	(0.0425)	(0.0676)
West	-0.0457	-0.0487	-0.0473	-0.0845
	(0.0568)	(0.0569)	(0.0570)	(0.0702)
Rural	0.183***	0.168***	0.169***	0.0682*
	(0.0425)	(0.0422)	(0.0422)	(0.0290)
Unknown (urban or rural)	-0.201***	-0.209***	-0.208***	-0.203***
	(0.0474)	(0.0474)	(0.0473)	(0.0520)
Constant	-1.272***	-1.271***	-1.314***	-7.294***
	(0.156)	(0.156)	(0.158)	(0.0692)
Observations (person-years)	59,935	59,935	59,935	59,935
Respondents	3,868	3,868	3,868	3,868

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

Note: Models restricted to respondents who are ever observed to own a home by 2015. Standard errors clustered at the individual level.

negative association with homeownership duration, even when differences in age at first entry into homeownership are taken into account.

The Model 2 and 3 coefficients indicate that having been arrested is associated with roughly one-quarter year less of homeownership duration, conditional on age and age of first entry into homeownership, while prior conviction is independently associated with .2 fewer years of homeownership, on average. Model 3, the difference-in-differences model, shows that patterns in homeownership duration prior to criminal justice contact do not differ significantly between individuals who will eventually be involved with the justice system (but have not yet been) and respondents who never have justice system contact, conditional on covariates.

Results from the fixed effects model (Model 4) suggest that, among respondents who ever have some form of contact with the criminal justice system, arrest, conviction, and incarceration are each independently associated with about .4 to .5 fewer years of homeownership, even after accounting for delays in entry into homeownership. Again, because the criminal justice contact variables in these models are not mutually exclusive, these associations are cumulative, meaning that for someone who has been arrested, convicted, and incarcerated all three of these coefficients would apply to their expected total years of homeownership.

Robustness Checks

I have conducted the same analyses presented above on alternative versions of the homeownership measures that exclude continued ownership of a prior residence (in models of current homeownership and total years of homeownership) and/or include spouse/partner sole ownership of current residence (for all models). Results with these alternative versions of the

homeownership measures are substantively similar to those presented here and are available upon request.

I have also run Cox proportional hazard models of entry into homeownership and exit from homeownership using the same covariates included in the models above. The findings from these models are substantively consistent with those reported above. Criminal justice contact – particularly conviction and incarceration – is associated with delayed entry into homeownership and, conditional on becoming a homeowner, earlier exit from homeownership. The results of these hazard models are shown in Table A3 in the appendix.

Finally, I have also run the above models with race, gender, and class (below versus above median parental net worth in 1997) interactions, respectively. I find no evidence that the relationship between criminal justice contact and homeownership outcomes differs by gender but limited evidence that the relationships between some forms of criminal justice contact and some homeownership outcomes differ by race and class.

Although arrest history is not significantly associated with logit models of current homeownership across the full sample, race- and class-interacted models indicate that prior arrest is associated with significantly lower log odds of current homeownership for black respondents and for poorer respondents (i.e., those whose parents had below median net assets in 1997). These patterns hold in both the full criminal justice contact and difference-in-difference models (Models 2 and 3). When individual fixed effects models are run only on black respondents or poorer respondents, however, the relationship between arrest and current homeownership is not statistically significant, mirroring the findings from the fixed effects model (Model 4) in Table 3. The negative relationships between prior conviction and incarceration and current homeownership do not significantly differ by class background.

Race-interacted models also suggest that prior incarceration may be less detrimental for the current homeownership prospects of Hispanic respondents than whites (p <0.1), at least in the full criminal justice contact and difference-in-difference models (Models 2 and 3, respectively). The relationship between prior incarceration and current homeownership is still negative and marginally significant (p <0.1) in the fixed effect model (Model 4) run only on Hispanic respondents, however.

With regard to age at first entry into homeownership, I find some evidence that arrest may be *more* detrimental but incarceration *less* detrimental for Hispanics than for whites, but differences are only marginally significant (p <0.1) in some models (Models 2 and 3 for arrest, and Model 3 only for incarceration). Class-interacted models reveal that having been charged with a crime is only associated with delayed entry into homeownership for poorer respondents. The relationships between conviction and incarceration history and age at first homeownership do not significantly vary by class background, however. Finally, I find no significant class- or race-based differences in the relationship between criminal justice contact history and total years of homeownership. All results from interacted models are available upon request.

Discussion

Across the three outcomes examined in this paper a general pattern emerges: criminal justice contact is associated with lower levels of (current) homeownership, delayed entry into homeownership for those respondents who do make this transition in early adulthood, and shorter duration of homeownership among respondents who succeed in becoming homeowners. A substantial portion of the relationship that would be attributed to incarceration only in a simpler analysis that excludes other forms of criminal justice contact appears to be attributable to a combination of arrest, being charged, and, especially, conviction. However, incarceration has

the strongest relationship (in terms of magnitude) with all homeownership measures, which is fitting given that incarceration marks a far more severe disruption to life than arrest or conviction. That said, the fact that arrest only, without any further justice system contact, is negatively related to homeownership (particularly accrued years of homeownership) is noteworthy given that more than 10 million adults are arrested annually (Federal Bureau of Investigation 2018).

Importantly, arrests, like all forms of criminal justice contact, are not evenly distributed throughout the population. Blacks in particular are arrested at a rate disproportionate to their share of the population and their level of criminal activity (Beckett et al. 2005; Gase et al. 2016). Thus, the disparities observed at every level of criminal justice contact are likely to feed into racial disparities in homeownership and, eventually, wealth accrual over the life course. Because the NLSY97 respondents included in this analysis are still relatively young – and many have yet to enter homeownership – it is difficult to forecast exactly how large an impact these disparities in justice system contact will have on homeownership and wealth disparities in midlife, but the findings of this analysis give cause for concern.

It is highly likely that criminal justice contact may affect not just an individual's ability to enter into or maintain homeownership but also the neighborhood and unit quality available to potential homebuyers. Delving into the question of how criminal justice contact affects unit and neighborhood quality for homebuyers with prior justice system contact is beyond the scope of this paper, but it is well worth investigating given that both factors are likely to have significant implications for the wealth returns to homeownership. Moreover, while I find only limited evidence that race moderates the relationship between criminal justice contact and homeownership, race may be much more likely to affect the quality of units and neighborhoods available to homebuyers with a history of criminal justice system contact.

NLSY97 also contains information on the type of offense for which individuals were charged, convicted, and incarcerated. Thus, future analyses could investigate whether the findings presented here vary by level of offense (i.e., misdemeanor versus felony) or type of offense (e.g., drug charge). Likewise, future research could also delve into the timing of criminal justice contact in relation to homeownership patterns.

Another fruitful direction for future research would be exploring whether homeownership and wealth more broadly affect probability of criminal justice system interaction. There is good reason to suspect that this may be the case – e.g., it may be easier to get a bail bond, thus avoiding pre-trial incarceration, if you can offer a vehicle or other assets as collateral. But to my knowledge only one study has looked at this relationship descriptively, finding that probability of incarceration is lower among higher wealth individuals (Zaw et al. 2016). However, because there are a number of confounding factors likely to influence both wealth and likelihood of interacting with the criminal justice system, a causally-motivated analysis of this question would be an important contribution to the literature.

Although the NLSY97 cohort is still relatively young, the NLSY97 data provide an important opportunity to begin investigating how a fuller range of criminal justice system interactions affect subsequent outcomes and opportunities for the millions of Americans who pass through the justice system every year. Incarceration rates may be declining (Kaeble and Cowhig 2018), but these findings highlight the importance of lower-level forms of criminal justice contact that are not the focus of current criminal justice reform efforts (e.g., Charles Koch Institute n.d.; #cut50 n.d.).

Moreover, these data also afford the opportunity to systematically explore the much-speculated-on housing and homeownership patterns of millennials. While high student loan debt burden is often blamed for lower levels of homeownership among millennials compared to prior

generations (Kitroeff 2018; Thompson 2018), student loan debt is a constraint more relevant for relatively advantaged potential homebuyers with post-secondary degrees.[19] This paper highlights a constraint on potential homeownership for millennials at the other end of the market, for whom student loan debt is likely to be a less relevant factor in the transition into homeownership: the expansive reach of the American criminal justice system.

[19] While higher educated individuals are more likely to be homeowners, 56% of heads of household with a high school diploma and 41% with less than a high school diploma own their homes (Young 2017).

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APPENDIX TABLES

Table A1. Response Rates and Proportion of Sample Answering Homeownership Questions by Survey Year, NLSY97

Survey Year	Num. Participating Respondents	Response Rate from Original Sample	Num. of Respondents Asked Homeownership Questions	% of Participating Respondents Asked Homeownership Questions	% of Eligible Respondents Who Provided Valid Response to Homeownership Questions
1997	8,984	100%	0	0%	
1998	8,386	93%	1,835	22%	99%
1999	8,208	91%	3,105	38%	99%
2000	8,080	90%	4,164	52%	100%
2001	7,882	88%	3,966	50%	97%
2002	7,896	88%	3,945	50%	100%
2003	7,754	86%	2,199	28%	100%
2004	7,502	84%	1,615	22%	99%
2005	7,338	82%	1,512	21%	100%
2006	7,559	84%	1,761	23%	100%
2007	7,418	83%	1,658	100%	100%
2008	7,490	83%	1,697	100%	100%
2009	7,559	84%	1,665	100%	100%
2010	7,479	83%	1,474	100%	100%
2011	7,423	83%	1,639	100%	100%
2013	7,140	79%	3,038	100%	100%
2015	7,103	79%	2,918	100%	99%

Table A2. Linear Probability Models Predicting Current Homeownership

	(1) Incarceration Only	(2) Full Criminal Justice Contact	(3) Difference-in-differences	(4) Fixed Effects
Previously arrested		-0.0181*		-0.0333**
		(0.00887)		(0.0105)
Will ever be arrested (not yet arrested)			-0.0114	
			(0.0119)	
Will ever be arrested*Previously arrested			-0.0200*	
			(0.00962)	
Previously charged		-0.00213		-0.0244*
		(0.0116)		(0.0121)
Will ever be charged (not yet charged)			0.0175	
			(0.0130)	
Will ever be charged*Previously charged			0.000390	
			(0.0124)	
Previously convicted		-0.0292**		-0.0237*
		(0.0107)		(0.0106)
Will ever be convicted (not yet convicted)			-0.00762	
			(0.0106)	
Will ever be convicted*Previously convicted			-0.0303**	
			(0.0113)	
Previously incarcerated	-0.0550***	-0.0209*		-0.0614***
	(0.00893)	(0.0103)		(0.0105)
Will ever be incarcerated (not yet incarcerated)			0.00288	
			(0.00923)	
Will ever be incarcerated*Previously incarcerated			-0.0205†	
			(0.0108)	
Currently incarcerated	0.0203*	0.0171†	0.0171†	0.0242†
	(0.00984)	(0.00984)	(0.00983)	(0.0132)
Age	0.00807***	0.00860***	0.00860***	0.0139***
	(0.000472)	(0.000482)	(0.000486)	(0.000346)
Female	-0.00306	-0.00811	-0.00804	
	(0.00496)	(0.00500)	(0.00506)	
Race/ethnicity				
Black (non-Hispanic)	-0.0538***	-0.0555***	-0.0555***	
	(0.00627)	(0.00629)	(0.00629)	
Hispanic	-0.0303***	-0.0319***	-0.0319***	
	(0.00735)	(0.00736)	(0.00736)	
Other (non-Hispanic)	-0.0473***	-0.0477***	-0.0477***	
	(0.0136)	(0.0136)	(0.0136)	
Cohabitating	0.197***	0.197***	0.197***	0.140***
	(0.00533)	(0.00533)	(0.00533)	(0.00317)
Own labor income last year (in 10,000s)	0.0276***	0.0273***	0.0273***	0.0193***
	(0.00116)	(0.00116)	(0.00116)	(0.000659)
Spouse/partner labor income last year (in 10,000s)	0.0149***	0.0148***	0.0147***	0.00952***
	(0.000742)	(0.000741)	(0.000741)	(0.000541)
Individual net worth at 20 (in 10,000s)	0.00616***	0.00603***	0.00603***	
	(0.000911)	(0.000911)	(0.000911)	
Highest degree completed				
High school	0.00491	0.00102	0.00119	-0.0622***
	(0.00542)	(0.00542)	(0.00546)	(0.00548)

Associate's, some college	0.0657***	0.0594***	0.0596***	-0.0189*
	(0.0134)	(0.0134)	(0.0134)	(0.00959)
Bachelor's	0.0542***	0.0456***	0.0457***	-0.0117
	(0.0104)	(0.0104)	(0.0105)	(0.00846)
Graduate or professional degree	0.0950***	0.0851***	0.0852***	0.0585***
	(0.0170)	(0.0171)	(0.0171)	(0.0118)
Current student	-0.000559	-0.00326	-0.00323	-0.0106**
	(0.00421)	(0.00425)	(0.00426)	(0.00357)
Ever used marijuana since last interview	-0.0375***	-0.0325***	-0.0325***	-0.00798*
	(0.00482)	(0.00485)	(0.00488)	(0.00388)
Ever used hard drugs since last interview	-0.0267***	-0.0215**	-0.0217**	-0.0168**
	(0.00676)	(0.00679)	(0.00678)	(0.00644)
Ever carried a gun since last interview	0.0620***	0.0624***	0.0624***	0.0367***
	(0.00988)	(0.00985)	(0.00985)	(0.00634)
Household structure in 1997				
One biological parent, one stepparent	-0.0281***	-0.0260***	-0.0260***	
	(0.00766)	(0.00766)	(0.00768)	
One biological parent only	-0.0275***	-0.0252***	-0.0251***	
	(0.00613)	(0.00614)	(0.00615)	
Other (no biological parents present)	-0.0491***	-0.0469***	-0.0469***	
	(0.00964)	(0.00967)	(0.00969)	
Parents' education				
High school	-0.00274	-0.00329	-0.00328	
	(0.00692)	(0.00691)	(0.00691)	
Some college	-0.00457	-0.00471	-0.00471	
	(0.00757)	(0.00755)	(0.00756)	
Bachelor's	-0.0113	-0.0112	-0.0111	
	(0.00957)	(0.00956)	(0.00956)	
Graduate or professional degree	-0.0223*	-0.0231*	-0.0231*	
	(0.0108)	(0.0107)	(0.0108)	
Parents' net worth in 1997 (in 10,000s)	0.000650**	0.000652**	0.000652**	
	(0.000218)	(0.000218)	(0.000218)	
Region				
Northeast	-0.0610***	-0.0612***	-0.0611***	-0.0247*
	(0.00663)	(0.00663)	(0.00664)	(0.0101)
Midwest	0.0282***	0.0291***	0.0293***	0.00766
	(0.00670)	(0.00668)	(0.00669)	(0.00920)
West	-0.0452***	-0.0456***	-0.0455***	-0.0699***
	(0.00674)	(0.00674)	(0.00674)	(0.00898)
Rural	0.0855***	0.0845***	0.0845***	0.0422***
	(0.00631)	(0.00630)	(0.00630)	(0.00405)
Unknown (urban or rural)	0.0596***	0.0592***	0.0592***	0.0244***
	(0.0102)	(0.0102)	(0.0102)	(0.00706)
Constant	-0.167***	-0.164***	-0.165***	-0.208***
	(0.0139)	(0.0138)	(0.0143)	(0.00912)
Observations (person-years)	72,923	72,923	72,923	33,845
Respondents	8,792	8,792	8,792	3,778

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

Note: Standard errors clustered at the individual level.

Table A3. Cox Proportional Hazard Models of Entry into and Exit from Homeownership

	Entry into Homeownership	Exit from Homeownership
Previously arrested	-0.1000	0.0520
	(0.0787)	(0.120)
Previously charged	-0.0997	0.149
	(0.0796)	(0.111)
Previously convicted	-0.114†	0.284**
	(0.0633)	(0.0911)
Previously incarcerated	-0.294**	0.446***
	(0.0976)	(0.130)
Currently incarcerated	-1.106*	0.169
	(0.460)	(0.327)
Female	-0.0805*	0.0304
	(0.0396)	(0.0671)
Race/ethnicity		
Black (non-Hispanic)	-0.475***	0.0964
	(0.0569)	(0.0875)
Hispanic	-0.190***	-0.126
	(0.0552)	(0.0882)
Other (non-Hispanic)	-0.315**	0.0258
	(0.105)	(0.163)
Cohabitating	1.217***	-0.983***
	(0.0420)	(0.0670)
Own labor income last year (in 10,000s)	0.0831***	-0.0799***
	(0.00888)	(0.0134)
Spouse/partner labor income last year (in 10,000s)	0.0366***	-0.0325***
	(0.00488)	(0.00963)
Individual net worth at 20 (in 10,000s)	0.0312***	-0.0110
	(0.00499)	(0.00751)
Highest degree completed		
High school	0.333***	-0.305**
	(0.0791)	(0.0968)
Associate's, some college	0.536***	-0.631***
	(0.103)	(0.147)
Bachelor's	0.788***	-0.648***
	(0.0924)	(0.129)
Graduate or professional degree	0.891***	-0.699***
	(0.115)	(0.179)
Current student	-0.0887†	0.207*
	(0.0535)	(0.0814)
Ever used marijuana since last interview	-0.311***	-0.0318
	(0.0568)	(0.0859)
Ever used hard drugs since last interview	-0.138	0.209
	(0.115)	(0.171)
Ever carried a gun since last interview	0.268***	-0.00840
	(0.0755)	(0.115)
Household structure in 1997		
One biological parent, one stepparent	-0.0816	0.131
	(0.0580)	(0.0871)
One biological parent only	-0.158***	0.0405

	(0.0476)	(0.0725)
Other (no biological parents present)	-0.337***	0.0286
	(0.0995)	(0.143)
Parents' education		
High school	0.0521	-0.184*
	(0.0592)	(0.0859)
Some college	0.0255	-0.145
	(0.0624)	(0.0943)
Bachelor's	-0.0249	-0.0691
	(0.0730)	(0.111)
Graduate or professional degree	-0.132†	-0.182
	(0.0790)	(0.129)
Parents' net worth in 1997 (in 10,000s)	0.00195	-0.00119
	(0.00128)	(0.00234)
Region		
Northeast	-0.453***	-0.0127
	(0.0592)	(0.0950)
Midwest	0.142**	-0.167*
	(0.0462)	(0.0784)
West	-0.248***	0.0939
	(0.0512)	(0.0849)
Rural	0.453***	-0.142†
	(0.0474)	(0.0760)
Unknown (urban or rural)	0.843***	0.432**
	(0.0745)	(0.159)
Observations (respondents)	619,969	159,992

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