Revisiting the Lasting Impacts of Incarceration[*]

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Abstract

Using newly-linked administrative and commercial data from Virginia spanning 25 years, we study the consequences of incarceration. While previous research has examined labor market outcomes and recidivism, we focus on two of the primary channels through which low-income households build wealth: asset ownership (homes and cars) and human capital formation. To identify causal effects, we use a matched difference-in-differences design. In line with much of the literature on the impact of incarceration in the U.S., we find no evidence of scarring effects on labor market outcomes or changes in recidivism beyond the incapacitation period. However, we find that incarceration leads to a persistent reduction in asset accumulation: seven years after sentencing, homeownership has declined by 1.1 percentage points (12.1%) and car ownership by 2.7 percentage points (18.1%). Incarceration also lowers human capital formation, reducing college enrollment by 1.4 percentage points (15.1%).

Keywords: Incarceration, asset accumulation, human capital, criminal justice, labor market

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1 Introduction

In the United States, nearly two million people are currently incarcerated, and 3% of adults—including 15% of Black men—have served time in prison (Shannon et al., 2017; Walmsley, 2023). These high rates have prompted research examining how incarceration impacts life trajectories, with particular attention paid to recidivism, and, more recently, to labor market outcomes. One dimension of well-being in justice-involved populations that has been comparatively difficult to study is asset ownership and the ability to accumulate wealth. Establishing evidence of a causal relationship is challenging because data on wealth or asset ownership among justice-involved populations is difficult to come by. Most prior studies have used survey data to measure asset ownership (e.g., Maroto, 2015; Turney and Schneider, 2016). However, survey datasets typically include only small samples of justice-involved families and suffer from high and non-random attrition rates.

We address these challenges by linking data from both administrative and commercial sources to create a 25-year panel on asset ownership and college enrollment among people convicted of a felony in Virginia. We use these data to examine how incarceration affects two primary channels through which low-income households build wealth: ownership of durable assets (homes and cars) and human capital formation. We additionally study impacts on outcomes that have been explored in prior work: employment and labor earnings—measured using quarterly W-2 earnings records—and recidivism. By expanding the set of outcomes considered, we allow for the possibility that incarceration affects economic trajectories through channels other than formal labor market participation.

In our main analysis, we implement a matched difference-in-differences design and focus on defendants who are convicted of a felony for the first time and recommended for prison based on Virginia's sentence guidelines scoring system. We use exact matching on offense type and sentencing score (an index of offense severity and prior criminal record), and then compare individuals who do and do not receive a carceral sentence in a standard difference-in-differences framework. The treatment group consists of individuals sentenced to incarceration (with an average sentence length of 15.6 months), while the comparison group includes those who are convicted and receive non-carceral sanctions.

Our analysis yields three main findings. First, incarceration has persistent effects on asset ownership. Seven years after sentencing, incarceration reduces homeownership by 1.1 percentage points (12.1%, relative to the treatment group mean in the year before sentencing) and car ownership by 2.7 percentage points (18.1%). These effects develop gradually, with no evidence of recovery. The reduction in homeownership operates through two channels: incarceration increases the likelihood of home sale by approximately 7 percentage points for existing homeowners and reduces the likelihood of home purchase by 0.6 percentage points for non-owners. When we consider home and car ownership at the household level, our estimates are very similar to those focusing only on the individual, suggesting that households do not absorb the long-term effects of incarceration, whether through strategic adjustments or other channels. The reduction in homeownership may be particularly consequential because housing equity is the primary way most Americans build wealth (Kuhn et al., 2020).

Second, incarceration reduces human capital accumulation, lowering the likelihood of having any college enrollment by 1.4 percentage points (15.1%) seven years after sentencing. Prior research suggests that education is an important determinant of long-term earnings potential, even when it consists only of community college and/or college credits without a degree (Kane and Rouse, 1995; Jepsen et al., 2014; Giani et al., 2020). Thus, the reduced college enrollment could have lasting consequences for economic mobility.

Third, these impacts of incarceration on asset ownership and human capital formation persist even though incarceration causes only temporary declines in recidivism, employment, and earnings. We conduct a supplementary analysis examining the impact of longer sentences (4-7 years) and still find no evidence of scarring (i.e., persistent post-release labor market declines). Overall, our employment estimates are consistent with most prior quasi-experimental work in the U.S. context (Kling, 2006; Loeffler, 2013; Harding et al., 2018; Garin et al., 2025), which finds incapacitation effects but little evidence of scarring effects on labor market outcomes.

The key identifying assumption for our main analysis is parallel trends in potential outcomes in the absence of incarceration, conditional on offense type and sentencing score. The sentencing score can be thought of as a summary of the information that legal decision-makers tend to prioritize. Hence, matching on these variables reduces concerns that sentences are determined by the same unobservables that also determine future potential outcome trajectories. Outcome trends appear parallel prior to the offense, assuaging concerns that there are unobserved shocks that influenced both offending and our outcomes of interest. A remaining threat to our research design is an adverse shock that occurs concurrently with sentencing and that correlates both with incarceration and with trends in outcomes. However, such a shock would have to have particular characteristics, generating short-term declines in flow variables like recidivism and employment, but long-term declines in stock variables like homeownership. This pattern is consistent with short-term disruptions like incarceration, but less consistent with adverse life events such as job loss or interpersonal violence, which prior literature shows have more persistent effects on employment and crime (Jacobson et al., 1993; Rose, 2018; Bennett and Ouazad, 2020; Bindler and Ketel, 2022).

Our estimates are robust to various specification choices. In particular, in an alternative difference-in-differences specification, we use discontinuities in sentencing recommendations to identify alternative treatment and comparison groups. We compare individuals just above the incarceration threshold who were incarcerated to similar individuals just below the threshold who avoided incarceration. This approach yields very similar estimates.

Our findings suggest that incarceration has lasting consequences for the primary mechanisms through which low-income households build wealth, even if it only leads to temporary declines in employment and earnings. Homeownership, in particular, has important financial implications for such households, as it has higher returns than traditional savings and preferential tax treatment. It can also serve as a commitment device for saving (Bernstein and Koudijs, 2024). While less liquid than other assets, home refinancing can be used to weather financial shocks (Lovenheim, 2011). Moreover, there are substantial transaction costs of losing a home as well as potential impacts on credit scores (Diamond et al., 2020).

Beyond direct financial implications for the incarcerated and their family members, housing also plays an important role in intergenerational wealth transmission (Daysal et al., 2023; Derenoncourt et al., 2023; Benetton et al., 2024; Binder et al., 2024), and it mediates access to stable shelter and to better neighborhoods, which in turn could have implications for intergenerational mobility (Chetty and Hendren, 2018a,b; Andrews et al., 2017). In this way, disruptions to homeownership caused by incarceration may have consequences that extend beyond the directly affected individual. Similarly, car ownership captures both asset accumulation and durable consumption, as well as facilitating access to employment opportunities not easily reachable by public transit. These features can make home and car ownership valuable for long-term financial stability and upward mobility.

We contribute to the literature on how incarceration affects economic trajectories by providing quasi-experimental evidence on incarceration's impact on asset ownership and college enrollment. Prior work has focused on recidivism and, to a lesser extent, labor market outcomes (Kling, 2006; Hjalmarsson, 2009; **?**; Kuziemko, 2013; Loeffler, 2013; Aizer and Doyle Jr., 2015; Mueller-Smith, 2015; Gupta et al., 2016; Leslie and Pope, 2017; Estelle and Phillips, 2018; Harding et al., 2018; Dobbie et al., 2018b; Stevenson, 2018; Franco et al., 2020; Bhuller et al., 2020; and, 2021; Jordan et al., 2024; Garin et al., 2025).[1] In the context of the United States, most quasi-experimental studies find that post-conviction imprisonment leads to short-term decreases in recidi-

[1] Recent papers have also considered how incarceration affects the health of those incarcerated (Hjalmarsson and Lindquist, 2022; Norris et al., 2024; Bhuller et al., 2025) and spillover effects on children (Bhuller et al., 2018; Dobbie et al., 2018a; Arteaga, 2021; Norris et al., 2021).

vism, consistent with incapacitation, but no long-term reductions in re-offending.[2] The evidence from studies incorporating labor market outcomes similarly indicates shorter-term disruptions: incarceration reduces employment and earnings during confinement, but these effects quickly dissipate after release.

By expanding the set of outcomes considered, we complement the existing work in two ways. First, we are able to show how incarceration affects the accumulation of durable investments like homeownership, car ownership, and college education—an important complement to existing literature on flow variables like employment and recidivism. Second, the expanded set of outcomes allows us to capture incarceration's impact on a broader subset of the justice-involved population: for instance, those who own cars but do not participate in the formal labor sector. This is particularly relevant in our setting, as this population often relies on informal or precarious work arrangements that fall outside official labor force measures (Western, 2018).[3]

The empirical literature on incarceration and wealth accumulation has primarily relied on survey data (Maroto, 2015; Sykes and Maroto, 2016; Zaw et al., 2016; Turney and Schneider, 2016; Maroto and Sykes, 2019). These studies have consistently documented that criminal justice contact negatively correlates with wealth and that wealth declines after incarceration. However, due to the cost of data collection, the surveys used in these studies necessarily have small samples, and in particular, limited representation of justice-involved individuals.[4] In addition, issues such as attrition, recall problems, or misreporting can introduce important measurement errors (Meyer et al., 2015; Dutz et al., 2021).[5] We extend this literature by using a large panel data set spanning 25 years, combined with both administrative and commercially available sources, and a difference-in-differences design. In addition, by restricting the sample to individuals with felony convictions, we hold conviction status fixed, allowing us to isolate the impact of incarceration.

[2]See Loeffler and Nagin (2022) for a recent review of the literature on incarceration in the United States. The authors conclude that *"Most studies [...] find that the experience of postconviction imprisonment has little impact on the probability of recidivism."* Mueller-Smith (2015) is a notable exception, finding more persistent effects on recidivism and labor market outcomes. The evidence is also different in Europe. For example, Bhuller et al. (2020) find that incarceration reduces recidivism in Norway.

[3]A few quasi-experimental papers examine how incarceration affects household finances. Avenancio-Leon and Aneja (2021) show that incarceration lowers credit scores, suggesting a credit-based channel through which incarceration may affect future homeownership. Slutzky and Xiu (2023) find that pretrial detention increases bankruptcy and foreclosure during periods of decreasing house prices, which could help explain our results.

[4]For instance, samples drawn from the National Longitudinal Survey of Youth 79 and 97 and the Survey of Income and Program Participation used in Maroto (2015), Sykes and Maroto (2016), Zaw et al. (2016) and Maroto and Sykes (2019) each include fewer than 400 individuals who were incarcerated.

[5]For instance, Turney and Schneider (2016) use data from the Fragile Families and Child Wellbeing Study, but discard a third of the sample due to non-response. The analysis does not adjust for differential attrition, which is likely given that "recent parental incarceration," which is the main explanatory variable, occurs between survey waves that are only two years apart.

We also speak to a literature that aims to understand heterogeneity in wealth accumulation processes across the income distribution (e.g., Piketty and Saez, 2003; Jr., 2005; Saez and Zucman, 2016; Poterba et al., 2017; Logan and Parman, 2017; Benhabib et al., 2017; De Nardi and Fella, 2017; Kuhn et al., 2020; Addo et al., 2024; Derenoncourt et al., 2023; Kondo et al., 2025). We extend that literature by examining the causal link between incarceration and asset ownership in a predominantly low-income population, using a quasi-experimental design and administrative data. Our findings imply that differential exposure to incarceration may play a role in shaping disparities in wealth accumulation.

Our paper contributes to a small but growing literature using difference-in-differences or event study designs to study incarceration effects, complementing the judge instrumental variable (IV) approaches that have dominated quasi-experimental research in this area. While most existing studies leverage random assignment of judges as an instrument for incarceration (Kling, 2006; Aizer and Doyle Jr., 2015; Mueller-Smith, 2015; Dobbie et al., 2018b), or use discontinuities in recommended sentences (Rose and Shem-Tov, 2024; Garin et al., 2025), recent work by Bhuller et al. (2025) on mental health consequences and Norris et al. (2024) on mortality has used difference-in-differences and event study approaches to study the impact of incarceration.[6] As in those papers, our empirical approach rests on identifying assumptions about parallel trends in potential outcomes rather than judge randomization and exclusion restrictions. The fact that our analysis reproduces the qualitative findings from judge IV and RD studies—temporary effects on employment and recidivism consistent with incapacitation, without evidence of labor market scarring or long-run recidivism impacts—provides reassurance about the robustness of these conclusions across different empirical strategies and geographical settings.

Our paper proceeds as follows. In Section 2 we describe our setting, data, and empirical approach. In Section 5 we discuss our main estimates, and in Section 6, we examine potential confounds and provide robustness tests. Section 7 concludes.

2 Background and data

2.1 Felony sentencing in Virginia

Virginia follows a voluntary sentence guidelines regime, in which judges are provided a recommended sentence range but are permitted to deviate from it. The recommended

[6] Among studies of crime and criminal justice policies, this kind of research design has also been used to look at the effect of arrests (Grogger, 1995), victimization (Bindler and Ketel, 2022; Bhuller et al., 2024; Adams et al., 2024a,b, 2025), criminal convictions (Rose, 2021; Agan et al., 2024), and fines (Mello, 2024).

sentence is calculated using a series of worksheets.[7] A defendant accumulates points on each worksheet according to the severity of the offense and details of the criminal record. The sentencing score is then converted into a recommended sentence range, which can be quite broad.

As in many jurisdictions, plea negotiation is common, and jury trials are rare. 85% of felony convictions result from a guilty plea and the rest come almost entirely from bench trials, or trial by judge instead of jury. The sentence guidelines are calculated by a probation officer or a prosecutor and are available during the plea negotiation process.

There is often a significant lag between the date of an arrest and the date of sentencing. Although procedures can vary, felony cases usually go through both a probable cause and a grand jury hearing before reaching Circuit Court for adjudication. Then, even if the defendant chooses to plead guilty, there may be an additional couple of months between the date of conviction and the date of sentencing. Those denied bail or unable to afford bail will remain detained pretrial throughout.

Those sentenced to incarceration may spend time in both prison and jail. Jails are local facilities that primarily accommodate individuals detained before trial, as well as those serving short-term sentences or awaiting transfer to prison. Since they are set up for shorter stays, they tend to have fewer recreational or enrichment opportunities. In contrast, prisons are larger and often further from one's home and family, but they tend to afford more freedom of movement, including greater access to outdoor space. Most of Virginia's prison population works during their confinement for minimal pay. There is some access to higher education in prisons. The Department of Corrections offers a few accredited courses and some community colleges permit distance learning.

Almost everyone who is convicted is required to pay court fees and/or fines. Those who are convicted but not incarcerated are often put on probation. This entails a suspended sentence as well as additional requirements, for example meeting with a probation officer. While on a suspended sentence, violation of the court terms can lead to being sent back to prison. Those sentenced to incarceration typically also have a partially suspended sentence, which they serve after release. Discretionary parole is banned in Virginia and prisoners must serve at least 85% of their original prison sentence before release.

Virginia's criminal justice system is similar to the nation as a whole and to several states considered in other recent studies of the impact of incarceration, as described in Humphries et al. (2024). For example, Virginia is similar in terms of incarceration and probation rates, but parole is less common than in other states.

[7] An example is shown in Appendix A.

2.2 Data and matching

In this subsection, we describe our data, matching process, and samples. More details about the data and matching can be found in Appendix B.

VCSC data (sentencing data). We obtain data from the Virginia Criminal Sentencing Commission (VCSC) on everyone convicted of a felony in Virginia from 1995 through 2019. This dataset includes basic case details, such as charges and sentences, in addition to the guidelines-recommended sentence and all of the inputs used to calculate it, which mostly pertain to the charge and the criminal record. The data also includes first and last name, middle initial, partial birth date, and the last four digits of the Social Security number, which we use to match to our other data sources. VCSC also provides data on probation revocations from 2006 onwards.

Virginia court data. We match the sentencing commission data to records maintained by Virginia's Circuit Courts using name, birth month, offense date, and judicial circuit. Circuit Court records are available for all but two judicial circuits from 2000-2021. We achieve a 92% match rate for the available circuits/years. We use this dataset to measure recidivism—defined as new felony charges filed in Circuit Court—and to obtain information on defendants' race and gender. We obtain data on misdemeanor sentences and pretrial detention from Virginia's District Courts, using the same matching variables. District court data are available from 2010-2021.

Employment data. We obtain earnings and employment data from the Virginia Employment Commission (VEC), which maintains records on all W-2 income reported by employers in Virginia. While this captures a broad range of formal employment, it excludes income from self-employment, independent contracting, and informal employment. We match using name and the last four digits of the Social Security number. These data are observed at a quarterly frequency from 1999 through 2019. Throughout the paper, we define earnings as total W-2 wage income. We use the Consumer Price Index to adjust earnings so that they are in 2009 dollars.

College data. We obtain data on college enrollment from the State Council of Higher Education in Virginia (SCHEV), which maintains records on all public schools and most private schools within the state.[8] Since higher education in prison mostly consists of correspondence courses through community college, we capture most within-prison post-secondary education. We match using name, birth date, and the last four digits

[8]SCHEV collects data on all institutions where students are eligible for Virginia Tuition Assistant Grants, which includes all public institutions and private institutions whose primary place of business is Virginia.

of the Social Security number. These data are observed at every semester from 2000 to the beginning of 2020.

Data aggregator. We obtain data on homeownership, car ownership, and household members from a commercial data aggregator.[9] This company collects public records data from over 3,000 counties, boroughs, and parishes in the United States, representing over 98% of the adult U.S. population. The data aggregator collects these data for two primary purposes: marketing and the construction of alternative credit scores. These scores are used to facilitate access to payday loans, or loans for purchasing cars or phones, particularly for individuals who lack formal credit histories. Given the aggregator's business model, it particularly targets individuals who are further removed from the formal economy, such as those without access to traditional banking services.

Data on real property ownership are sourced from public county tax and deed records. We refer to real property ownership as "homeownership" because justice-involved populations are unlikely to own investment properties. In addition to public records, the company also acquires data from numerous private companies.

Car ownership data comes from an extensive network of car dealerships, oil change companies, and repair shops. An individual is labeled as a car owner if they purchase a car from a dealership or bring the car in for an oil change or repair. This variable has two limitations. First, it may miss some car owners who do not purchase from dealerships or who do not take their car in for oil changes or repairs. Second, the variable does not capture the sale or loss of the car and is therefore an absorbing variable. As a result of these two limitations, we may understate or overstate the *level* of car ownership. To the extent that this measurement error does not affect the treatment and comparison group differentially, our estimates of the *impact* of incarceration should not be affected. Another implication of the second limitation is that our estimate of the impact of incarceration on car ownership does not capture its impact on car sales or loss. If incarceration increases sales or loss in addition to decreasing acquisition, our estimates will understate the total impacts on car ownership.

We match the sentencing and court records to these data sets using the first and last name, middle initial, birth month and year, and last four digits of the Social Security number. We purchased eight "snapshot" years of data, centered around the sentencing date: years -5, -3, -1, 0, 1, 3, 5, 7. Each snapshot is drawn from the relevant archive, which is preserved at the end of each month. These data are available from 1995 to 2021.

[9]While the data aggregator provides many variables, many of them are modeled constructs. We do not include them as outcomes due to their opacity. This paper includes all available non-modeled outcomes pertaining to wealth.

Match quality. We match the VCSC and court data to multiple other datasets. Given that we have access to the last four digits of the Social Security number in addition to a variety of other personally identifying information in all datasets, we expect a reasonably high match quality and rate.

Matching to the data aggregator. Our match rate is 96%. Match verification variables provided by the data aggregator suggest a high match quality: 93% of the matches made were an exact match on first name, last name and Social Security number, while only 2% of matches had a Social Security number that was linked to multiple names or names other than what we provided them. We can also verify match quality by confirming that everyone in our sample–who we know to have been recently convicted of a felony–is also shown to have a recent felony conviction in the records maintained by the data aggregator. Among the circuit-years for which conviction data is available, there is a strong concordance with the criminal record information across the two datasets. We discuss these tests in more detail in Appendix B.3 and use this concordance as a measure of match quality (see Section 6.2).

Matching to the employment and college data. The employment data should represent the universe of W-2 filings in Virginia. Likewise, the college education data represents the entirety of enrollment at public colleges in Virginia as well as most private nonprofit ones. While we expect the data for these two outcomes to be relatively complete, there could be typos and other inaccuracies in the identifying information. 36% of individuals in our sample have an employment record and 14% have a post-secondary education records. These figures are consistent with the well-documented pattern of low formal labor market participation and limited educational attainment among individuals who come into contact with the criminal justice system (Pettit and Western, 2004; Western, 2006; Looney and Turner, 2018).

2.3 Sample

We study the impact of incarceration among those experiencing their first felony conviction. We focus on individuals without prior felony convictions to understand the full trajectory initiated by the first felony sentencing event. While many in our sample have had prior felony arrests or misdemeanor convictions, the first felony conviction marks the entrance into more severe potential punishments. The parameter we estimate—the effect of incarceration among individuals receiving their first felony conviction—is conceptually different from the effect of incarceration for someone who has already been in the felony system, and combining these groups would yield weighted averages that would not reflect either parameter clearly. We report estimates for individuals with prior felony convictions separately in Section 5.4.

We impose several additional restrictions. First, for all outcomes, we restrict our sample to those at least 23 years old at the time of sentencing. This ensures that we are able to track outcomes for individuals of the age of legal majority for at least five years before sentencing. Second, in the main analysis, we restrict our sample to individuals whose sentencing score is high enough that they are recommended for prison, yet not so high that nearly everyone with the same score is incarcerated. Specifically, we include defendants whose sentencing score is within 10 points above the threshold for a prison recommendation.[10] Third, we restrict our sample to defendants sentenced within the years 2001-2014, ensuring that we can track outcomes for everyone at least five years prior to and seven years post-sentencing. Fourth, we restrict treated individuals to those with a carceral sentence of four years or less. This restriction ensures that we have three years of outcome data after completing the original carceral sentence.[11] We use this main sample for our analysis of the impact of incarceration on recidivism and homeownership. Car ownership, labor market, and college attendance data are all available for a more limited range of years. We therefore restrict the "car sample" to those sentenced within 2010-2014, the "employment sample" to 2003-2012, and the "college sample" to 2005-2013. We show that results are robust to different ways of defining the sample in Section 6.2. Appendix B.4 shows how our sample size changes as we impose our sample restrictions.

3 Descriptive statistics

Table 1 presents summary statistics for the treatment group—individuals sentenced to incarceration for four years or less—and the comparison group, who received non-carceral sentences.[12] The treatment group receives sentences that are 15.6 months long on average, whereas the comparison group does not receive any carceral sentence by construction. The treatment group is also 6 percentage points more likely to have to pay a fine or restitution. Given that several recent papers have found that court fines and/or fees do not impact recidivism or financial well-being, we think this is unlikely to play a meaningful role in explaining our results (Pager et al., 2022; Finlay et al., 2024; Giles, 2023). Finally, almost everyone (96%) in both groups has their sentence partially suspended. While on a suspended sentence the person must abide by court orders and can be placed back in prison should they fail to do so. Those who are not incarcerated begin the suspended sentence immediately, those incarcerated begin

[10] 10 points above the threshold is the 95th percentile in the sentencing score distribution. The distribution of the sentencing score and average sentences per sentencing score are shown in Appendix Figure C.1.

[11] Four years is the 88th percentile of sentence length for those with carceral sentences.

[12] As we describe in Section 4, our main research design is matched difference-in-differences, so this table includes matched observations.

it after release.

The two groups are similar in terms of age, race, criminal record, offense and homeownership status. The largest difference across the groups is gender: 25% of the comparison group is female, compared to 17% for treatment. The comparison group is also 3 percentage points more likely to own a car or have any post-secondary education and 4 percentage points less likely to be employed.

Figure 1 compares homeownership rates by age for individuals in our sample compared to the general population in Virginia.[13] Homeownership is substantially lower in our sample at all ages, though the gaps become smaller with age. Overall, 18% of individuals in our sample owned a house at some point during the 12-year observation window around the sentencing date, indicating that homeownership is a meaningful economic margin even among justice-involved populations. However, the lower rates at all ages also suggest that individuals with felony convictions are less connected to the formal economy, consistent with lower levels of labor market attachment (Garin et al., 2025) and reflecting previously documented negative correlations between homeownership and criminal justice involvement (Turney and Schneider, 2016; Maroto and Sykes, 2019).

4 Research design

We aim to study causal impacts of incarceration on labor market outcomes, recidivism, asset ownership, and post-secondary education trajectories among those incarcerated after their first felony conviction. To formalize this, we use a potential outcomes framework. Let D_i be a dummy variable for whether individual i is incarcerated on their first felony conviction. We can write the observed outcome $Y_{i,t}$ in terms of the potential outcomes as $Y_{i,t} = D_i Y_{i,t}(1) + (1 - D_i)Y_{i,t}(0)$, where $Y_{i,t}(1)$ and $Y_{i,t}(0)$ are the potential outcomes with and without incarceration measured t periods before or after the first felony conviction.

We focus on the average treatment effect on the treated (ATT) t periods after the first felony conviction:

$$\eta_t \equiv E[Y_{i,t}(1) - Y_{i,t}(0)|D_i = 1]. \qquad (1)$$

The challenge in identifying η_t is that we do not observe the average outcome for incarcerated individuals in period t if they had counterfactually not been incarcerated, $E[Y_{i,t}(0)|D_i = 1]$. We address this missing data problem by using a difference-in-

[13] For the Virginia general population, we use American Community Survey 5-year estimates of homeownership among heads of households.

differences (DiD) design to estimate

$$E[Y_{i,m} - Y_{i,-n}|D_i = 1] - E[Y_{i,m} - Y_{i,-n}|D_i = 0]. \tag{2}$$

Equation (2) is equal to η_t under the parallel trends assumption, which states that trends in $E[Y(0)]$ would have been parallel between those incarcerated (treatment group) and those without a carceral sentence (comparison group) in the absence of incarceration. We can relax this assumption by using a matched DiD design where we match treated individuals to individuals in the comparison group with the same offense type and sentencing score. This design relies on a weaker assumption, requiring the parallel trends assumption to hold only conditionally on offense type and sentencing score, rather than unconditionally across the full sample.

For estimation, we use a standard multi-period difference-in-differences setup, where D_i is defined as above, λ_i represents individual fixed effects, ν_t captures time fixed effects, and δ_t captures the treatment effect in period t, with time measured relative to the sentencing date and $t = -1$ as the omitted period:

$$Y_{i,t} = \lambda_i + \nu_t + \delta_t D_i + U_{i,t}. \tag{3}$$

We estimate this equation on the sample described in Section 2.3. In some analyses (in particular in Sections 5.4 and 5.5), we consider different restrictions on the sample, which, if our identifying assumptions hold, will result in estimates of the ATT for different target groups of incarcerated individuals.

Our design compares incarcerated individuals to those with the same offense and sentencing score who did not receive a carceral sentence. There are multiple reasons why sentencing outcomes can differ. First, sentences may vary for exogenous reasons, such as attitudes of judges, prosecutors, or public defenders. Second, sentences may vary due to differences in observed and unobserved characteristics. Differences in both observed characteristics (as described in our discussion of Table 1) and unobserved characteristics do not necessarily pose a problem for our design. Problems arise if those differences lead to violations of the parallel trends assumption, such as shocks that affect economic trajectories and sentencing, but not due to constant level differences. We discuss this in more depth in Section 6.1.

5 The lasting consequences of incarceration

5.1 Exposure to incarceration

Table 1 shows that, on average, sentences in the treatment group are 15.6 months, compared to 0 months for the comparison group. Figure 2 provides more insight into the dynamics of incarceration for our treatment and comparison groups. The figure on the left shows the likelihood of being incarcerated on a felony sentence over time for the treatment group (green circles) and the comparison group (purple diamonds). The data are observed at a quarterly frequency and describe the fraction of each quarter the individual spends incarcerated, including on future felony sentences. The figure on the right shows the estimates of δ_t in equation (3). Our treatment group is substantially more likely to be incarcerated than the comparison group, particularly in the first couple of years after sentencing. There is still a small gap in the likelihood of incarceration three years after sentencing, but the gap has almost entirely disappeared by year four.

To more easily compare regression estimates across outcomes, we aggregate the quarterly estimates into yearly estimates, where the outcome is an indicator for being incarcerated at any time during that previous year. Our asset ownership data includes observations for eight years relative to the date of sentencing: -5, -3, -1, 0, 1, 3, 5, 7. To match our home and car ownership data, we run regressions limited to these years for all outcomes. We show difference-in-differences estimates for years 1, 5, and 7 in Column 1 of Table 2 for all outcomes, and present estimates for all eight years in Appendix Table C.1. The year 1 estimate captures incapacitation across the first year, and years 5 and 7 are post-release effects. The counterfactual outcomes for the treatment group are shown in square brackets below the standard errors. These are constructed by taking the difference between the yearly observed mean for the treatment group and the yearly treatment effect estimate. By years 5 and 7 post-sentencing, the incarceration gap has almost entirely disappeared.

Felony sentences are not the only way in which someone can be incarcerated. Pretrial detention, misdemeanor sentences, and probation revocation also involve imprisonment. In Appendix Figures C.2 and C.3, we show the dynamics of incarceration for treatment and comparison groups when including these other types of incarceration.[14] Even when we take into account pretrial detention and revocations, the incarceration gap in years 5 and 7 is a statistically insignificant -0.6 and 3 percentage points, respectively (Appendix Table C.2). Beyond four years post-sentencing, outcomes are thus

[14]We conduct these analyses for fewer years due to data limitations, as described in Appendix B.4. We show results centered both around the date of sentencing (Appendix Figure C.2) and the date of offense (Appendix Figure C.3). The former positions pretrial detention prior to time 0, and the latter positions it afterwards.

unlikely to be influenced by incapacitation.

5.2 Employment, earnings, and recidivism

Figure 3 follows the same format as Figure 2 with raw averages on the left-hand side and difference-in-differences estimates on the right-hand side. The first row shows the likelihood of being employed in that quarter, and the second row shows the average quarterly earnings.

We see that incarceration leads to a sharp drop in employment and earnings during the period of incarceration, but by year three these trends return to what they would have been absent incarceration. Table 2 shows estimates aggregated to the yearly level, with employment defined as any formal sector employment in that year and earnings defined as total W-2 wage income that year. Incarceration lowers employment during the incapacitation period but after release it appears to actually increase the likelihood of being employed. The employment effects in year 7 are 1.6 percentage points ($p<.10$), a 7% increase relative to a pre-sentencing treatment group mean of 0.24. The impacts on post-release earnings are small, negative, and not statistically significant, both individually and jointly across the quarters.

The possibility that incarceration could increase post-release employment is something that has not received a lot of attention in the previous literature. However, our estimates are consistent with Garin et al. (2025), which finds positive and statistically significant estimates on employment outcomes in North Carolina and in their "precision-weighted" average across both North Carolina and Ohio. One possibility is that recently released prisoners are incentivized to maintain employment as part of their suspended sentence, similar to parole. If so, the increase in employment would fade once the individuals are no longer under supervision. To explore this, we track employment outcomes up to 12 years after sentencing, limiting our sample to those sentenced between 2003 and 2007 to ensure a 12-year follow up window. Estimates are shown in Appendix Table C.3. By years 10-12 post-sentencing, the estimate has dropped to around 1 percentage point and is no longer statistically significant, suggestive of fade-out.

Despite evidence that incarceration increases employment rates at least in the years immediately following release, it does not have a detectable effect on earnings (Table 2). The post-release estimates are small and statistically insignificant. The cumulative earnings lost due to incarceration are about $7,141 over the seven years post-sentencing, adjusted to 2009 dollars.[15]

In Appendix Figure C.4 and Appendix Table C.4, we show employment outcomes

[15] We calculate this by summing the estimates from the difference-in-differences regression with earnings as the outcome between zero and seven years post-sentencing.

for those with steady employment prior to sentencing, defined as being employed in 7 out of 8 quarters between years -3 and -1. As in the full sample, we observe sharp declines in both employment and earnings coinciding with incarceration. However, for this group, we find that average annual earnings remain lower for up to seven years after sentencing. The cumulative earnings loss for this group is substantial: $44,384 over the first seven years after sentencing. Gaps in earnings eventually appear to narrow, but this convergence occurs more slowly than in our main sample.

The bottom row of Figure 3 shows the likelihood of being charged with a felony for a crime alleged to have been committed within the previous 12 months. We omit the felony charges that led to the focal sentencing event and include only prior and future felony charges. Incarceration leads to a short-term decline in felony charges, but shows no evidence of longer-term adverse effects. The estimates for years 5 and 7 are small and statistically insignificant (Table 2).

Most of the literature on the consequences of incarceration focuses on shorter sentences.[16] The combination of our matched difference-in-differences research design and the time span of our dataset means that we can also consider the effects of longer sentences on employment and recidivism. We conduct analyses comparing individuals who served prison terms of 4-7 years to those who received no incarceration, using the same matching strategy. Within this group, the average incarceration length is 5.3 years, providing a substantial "dosage" for analyzing long-term impacts. A 7-year sentence is at the 94^{th} percentile of positive incarceration sentences; 5.3 years is the 92^{nd} percentile. In Figure 4 and Table 3, we show results: compared to those with shorter sentences, the incapacitation period is longer.[17] However, we still see no evidence of long-term scarring on labor market or recidivism outcomes up to 12 years post sentencing. The evidence suggests that incarceration even increased earnings for recently released prisoners. Overall, this analysis suggests that even longer sentences do not have enduring negative effects on labor market outcomes, relative to having a felony conviction alone.

5.3 Asset ownership and human capital accumulation

Figure 5 shows how incarceration affects homeownership, car ownership, and college enrollment. The top row shows homeownership, the second row shows car ownership, and the third row shows enrollment in higher education. The raw data shows that trends in homeownership are almost overlapping for the treatment and comparison

[16]For example, in Garin et al. (2025), the average sentence length conditional on incarceration is 17-22 months. Given the long right tail of sentencing, the medians are likely substantially shorter. The median sentence of those incarcerated in Rose and Shem-Tov (2021) is 15 months, although their dose-response model allows them to estimate the marginal effects of an additional year of sentencing up to four years.

[17]Appendix Table C.5 presents estimates and standard errors for the estimates in Figure 4.

groups before sentencing, with a noticeable divergence in trend starting at the date of sentencing. Similar divergence in trends occurs for car ownership and post-secondary education.

In year 7 after sentencing, incarceration reduced homeownership rates by 1.1 percentage points, a 12.1% decline relative to a pre-sentencing mean of 0.09, and car ownership rates by 2.7 percentage points, a 18.1% decline relative to a pre-sentencing mean of 0.15. Much of this impact accumulates in the first couple of years after sentencing. This could be due to asset sales in preparation for prison, hesitation to purchase assets in anticipation of incarceration or its immediate aftermath, or both. Appendix Figure C.5 shows incarceration's impact on homeownership for those who do versus those who do not own a home before sentencing.[18] There are relatively few defendants who owned a home at some point before sentencing, reducing our sample size. Nonetheless, it appears that incarceration reduces homeownership by about 7 percentage points for this group. Among non-homeowners, incarceration reduces the likelihood of acquiring a house by about 0.6 percentage points.

Since our data on asset ownership does not extend beyond 7 years post-sentencing, we cannot look at the impact of 4-7 year sentences beyond the incapacitation period. We therefore do not include these outcomes in that specification.

Our estimates for home and car ownership could reflect sales of assets during incarceration or a reduced likelihood of acquiring new assets. There are several reasons why a prison sentence could lead to asset sales. First, it may not make sense to retain assets that are difficult to use or maintain while incarcerated, especially if they require ongoing payments. Although most of Virginia's prison population works while incarcerated, they typically earn less than a dollar an hour and cannot participate in the regular labor market. As a result, it can be difficult to keep up with mortgage or loan payments, and individuals may need to sell assets to avoid default or to cover expenses during incarceration. Second, prison facilities provide only the bare necessities, and incarcerated individuals must pay for postage, hygiene items, over-the-counter medications, snacks, and other basic goods using their commissary account. In addition, incarceration imposes financial burdens on families, who may face costs for collect calls and long-distance visitation. Selling assets can help cushion these financial pressures.

Prison may also prevent the acquisition of assets. Logistically, it is difficult, if not impossible, to purchase a home or car while incarcerated. It may also be more challenging to make large purchases after release. Savings may be depleted by commissary expenses, collect calls, and the cost of visits from relatives and friends. Incarceration also prevents labor force participation, beyond the nominal hourly wage for prison la-

[18] As discussed in Section 2.2, car ownership is an absorbing variable, and we cannot look at car sales. If incarceration also increases the likelihood that cars will be sold or otherwise relinquished, our estimates will understate the results.

bor. We saw that those who had steady employment prior to sentencing lost an average of \$44,384 over seven years in cumulative earnings due to incarceration. Some of this money could have been saved for a down payment on a house or a car. Finally, it may take some time to recover after release from incarceration. Securing employment, housing, and basic necessities plausibly takes precedence over long-term financial investments. This is consistent with the timing of the effects: incarceration appears to lower the rate of asset accumulation even after the incapacitation period is over. Although most of the individuals in our sample are released from prison within about a year and a half, the divergence in home and car ownership rates continues to grow until five years after sentencing before stabilizing.[19] Importantly, many of the mechanisms outlined above do not require permanent behavioral changes or labor market scarring to generate lasting financial effects.

To assess whether the observed effects on asset ownership reflect true economic loss—potentially extending to family members—or are instead masked by strategic reallocation of ownership within households, we examine asset ownership at the household level. This analysis serves two purposes. First, it allows us to test whether individuals respond to incarceration by transferring ownership of homes or vehicles to other members of their household—either to avoid forfeiture or to maintain access to credit—without representing an actual reduction in household wealth. If such reallocation occurs, we would expect to see muted or no effects at the household level. Second, the household-level analysis enables us to examine whether incarceration generates broader economic spillovers that affect the financial well-being of family members. If household resources are reduced overall—due to the loss of income, increased costs, or disruption to household functioning—then incarceration could depress asset ownership at the household level more than it does at the individual level. Appendix Figure C.6 shows that the household-level estimates closely mirror those at the individual level, suggesting that families do not substantially absorb the economic consequences of incarceration, whether through strategic adjustments or other means.

Panels (e) and (f) of Figure 5 show that incarceration also has a lasting effect on post-secondary education. The gap in the post-secondary education enrollment rate between treatment and comparison groups noticeably widens in the years immediately after sentencing. By seven years after sentencing, incarceration has led to a 1.4 percentage point decrease in the likelihood of having any college enrollment, a 15% decrease relative to a 0.093 pre-sentencing mean. Most of the divergence occurs in the first couple of years after sentencing, consistent with an incapacitation effect. This pattern holds despite the fact that our data captures in-prison education programs

[19]It's also possible that incarceration lowered their credit score, in line with Avenancio-Leon and Aneja (2021).

administered through community and state colleges.

This 1.4 percentage point (15.1%) reduction in college enrollment could have long-run consequences. Past work has shown that the returns to college are high, including community college and college credits that do not end in a degree. For example, Kane and Rouse (1995) find that an additional year of college credits was associated with around a 5% increase in earnings, and that this rate was similar for 2-year and 4-year institutions. Jepsen et al. (2014) similarly find large returns to credits, and Giani et al. (2020) document that those with some college earn substantially more than those with no college after accounting for a rich set of unobservables.

5.4 Impacts among individuals with prior convictions and heterogeneity by race

Impacts for those with prior felony convictions. So far, we have examined the impact of incarceration among defendants receiving their first felony conviction. In Appendix Figure C.7 and Table C.6, we investigate the impacts of incarceration among those with prior felony convictions. Estimates for employment, earnings, and recidivism look broadly similar to those with no prior felony convictions. The impacts on car ownership and post-secondary education are slightly smaller, and the impacts on homeownership are small and statistically insignificant. The smaller magnitudes are consistent with lower base rates among this population.

Heterogeneity by race. Next, we examine heterogeneity in effects by race. Because nearly all individuals in our sample are classified as either Black or White in court records, we restrict the analysis to these two groups. Appendix Figure C.8 presents estimates disaggregated by race. We find broadly similar effects for Black and White individuals across most outcomes, including employment, earnings, recidivism, car ownership, and post-secondary education. However, the effects on homeownership are small (a 0.5 percentage point decline) and statistically insignificant for Black individuals. This may reflect the fact that baseline homeownership rates are substantially lower among Black individuals in our sample (12% are shown to ever own a home in our data, compared to 23% of White individuals). These baseline disparities may reflect a range of structural barriers to homeownership or, perhaps, the impacts of incarceration on prior generations (Myers, 2004; Bayer et al., 2017; Andrews et al., 2017).

5.5 Alternative research design

In this section, we consider an alternative research design. We use the fact that, for each offense, there is a sentencing score cutoff above which a defendant is recommended for prison and below which they are recommended either for probation or for a short jail stay, resulting in large expected differences in sentencing. Defendants just above the cutoff are 9 percentage points more likely to receive a carceral sentence, and sentences are on average 8 months longer.

We implement a difference-in-differences design comparing individuals who scored just below the cutoff (1–2 points) and received non-carceral sentences to those who scored just above (1–2 points) and received carceral sentences of four years or less. Close to the cutoff, small differences in sentencing scores can result in large differences in sentencing, while observed and unobserved characteristics of defendants on either side of the cutoff are likely fairly similar. For instance, being convicted of one rather than two counts of drug possession adds two points to a person's sentencing score, potentially pushing them above the incarceration threshold, even when the underlying offenses are nearly identical (see Appendix A). This example illustrates that whether an individual falls just above or below the threshold can be partly arbitrary, introducing an additional source of plausibly exogenous variation in incarceration likelihood.

Appendix Table C.7 summarizes characteristics of the treatment and comparison groups under this alternative design. The average prison sentence in the treatment group is 12.8 months (versus 15.6 months in our main analysis). The treatment group is 8 percentage points less likely to be female than the comparison group and 15 percentage points more likely to face drug charges.

The results using this alternative approach are strikingly similar to our main specification, as shown in Table 4. Graphical results for employment, homeownership, car ownership, and post-secondary education are shown in Figure 6; Appendix Figure C.9 shows results for the other outcomes.[20] Incarceration leads to a sharp drop in employment, earnings, and recidivism during the time of incarceration, but no evidence of post-release effects. By contrast, we find lasting effects of incarceration on homeownership, car ownership, and college enrollment. Seven years after sentencing, incarceration has reduced property ownership rates by 1.4 percentage points, representing a 13% decline relative to the pre-sentencing mean of 0.11. Car ownership rates declined by 3.4 percentage points (21.0%, relative to a pre-sentencing mean of 0.16) and post-secondary education by 1 percentage point (9.8%, relative to a pre-sentencing mean of 0.10).[21]

[20] Appendix Table C.8 presents estimates and standard errors.

[21] We show employment and recidivism outcomes up to 12 years post-sentencing in Table C.9. Again, we find no evidence of lasting effects on these outcomes.

6 Robustness

In this section, we discuss potential limitations in our difference-in-differences framework and conduct a series of robustness checks.

6.1 Considering potential confounds: simultaneous adverse shocks

The identifying assumption underpinning our main and alternative research designs is that, absent incarceration, post-sentencing outcomes for the treatment group would have followed a trend parallel to the observed outcomes in the comparison group (conditional on offense and sentence score). This assumption would be violated if factors correlated with incarceration status are also correlated with counterfactual trends in asset accumulation or labor market outcomes in the period after sentencing. Some of the factors that could contribute to a defendant being incarcerated are plausibly random, such as variation in the preferences or the effectiveness of the assigned judge, district attorney, or public defender. These factors are thus unlikely to correlate with trends in asset accumulation or education except through their impact on sentencing. Other factors, however, are more likely to be correlated with trends in counterfactual outcomes. For example, judges may systematically be more punitive towards defendants who are on a downward financial trajectory, since they see this as increasing the risk for future crime. Although it does not appear that the treatment and comparison groups are on different trajectories prior to sentencing, post-sentencing trajectories could still diverge.

A natural concern is that unobserved shocks, such as adverse life events, correlate with both sentencing and trends in post-sentencing wealth and human capital accumulation. We believe that this is unlikely to be the case in our sample for three reasons. First, we compare two groups that are similar in terms of crime severity and criminal record, suggesting that their behavior up until the time of sentencing was fairly similar.

Second, our estimates indicate that incarceration did not cause lasting adverse effects on employment, earnings, or recidivism in the treatment group. The short-term declines for these outcomes are closely timed with the carceral sentence and, once the period of incapacitation ends, these outcomes trend back to match the comparison group. If the long-term declines in home/car ownership and post-secondary education were caused by an adverse life shock coincident with sentencing, we might have expected this shock to lead to long-term declines in employment and earnings and increases in recidivism, effects documented in response to other life shocks such as crime victimization or job loss (e.g., Bindler and Ketel, 2022; Adams et al., 2024a; Rose and Shem-Tov, 2024). The patterns we observe—short-term dips in flow variables like employment but long-term declines in the accumulation of assets—are consistent with a large but temporary disruption to economic activity. This limits the scope of plausible confounding events.

Finally, the timing of the changes we observe is difficult to reconcile with the idea that an adverse shock caused both the initial crime and subsequent divergent outcomes. If the treatment group experienced an adverse event leading to both more severe crime and worse economic outcomes, we might expect this to show up as differential pre-trends before the date of the offense. We explore this by re-centering the difference-in-differences specification around the offense date instead of the sentencing date. Appendix Figures C.3 and C.10 show that trends in incarceration, employment, and earnings only start to diverge after the date of the offense, suggesting that such a hypothetical adverse shock would have had to have occurred *immediately* before the offense. This type of scenario—a sudden life event triggering criminal behavior, followed by immediate arrest and conviction—is likely rare. Indeed, while adverse life events can influence criminal behavior, the criminology literature suggests that such rapid sequences are atypical (Sampson and Laub, 1993; Farrington, 2003). In practice, arrests leading to felony convictions typically follow a pattern of repeated or sustained criminal activity. Although some individuals may transition quickly from a life shock to criminal behavior to an arrest, this is not a very frequent pattern among those with felony convictions.

6.2 Robustness to choice of sample

In our main specification, we focus on those within 10 points of the cutoff for a prison recommendation. In Appendix Figures C.11, we show estimates for those within 10 points *below* the cutoff for prison recommendation and who are therefore recommended for either probation or short jail sentences. On average, sentences for those incarcerated in this group are only 4.7 months long, compared to 15.6 months in our main specification.[22] Correspondingly, we see much smaller treatment effects, some of which are not statistically significant. However, if limit the treatment group to those sentenced to between one and four years (the average sentence for that group is 17.5 months), we see very similar estimates as our main sample.

Appendix Figure C.12 examines robustness to two sample restrictions that we made in our main analysis: (1) excluding sentences longer than four years, and (2) excluding individuals under age 23 at sentencing. Including these groups allows us to test sensitivity to younger individuals (who may be earlier in their education or asset accumulation) and to longer incarceration spells (which could produce more persistent

[22] As in our main analysis, we include only defendants with sentences of four years or less to ensure at least three years of follow-up data after the original sentence has been served.

incapacitation effects). In the "all ages" sample, the median age at sentencing drops from 34 to 28; in the "all sentences" sample, average incarceration length increases to 23 months (from 15.6), with 10% and 5% of defendants receiving sentences over 4 and 6 years, respectively. These changes substantially expand the sample, yet the overall patterns remain consistent with our main specification. This suggests that our findings are not driven by these sample restrictions.

We next explore whether our estimates for impacts on asset ownership are sensitive to issues of matching to the data aggregator. To do so, we exploit the fact that one of the variables that the data aggregator collects is the date of the person's most recent felony conviction. Since we also have this date in our sentencing commission data, we are able to verify the match by comparing dates (see Appendix B.3 for more details). Again, we find very similar results among the subsamples with this additional match verification (Appendix Figure C.13).

Lastly, we show that our results are similar if we do not match on offense type and sentencing score. Appendix Figure C.14 compares our main estimates to those obtained without matching. The results are very similar across all outcomes, with slightly larger point estimates for homeownership and car ownership, and smaller standard errors, when we do not match on offense type and sentencing score. Although foregoing matching does not lead to qualitatively different conclusions, our matched analysis may still be preferred, since it allows us to relax the parallel trends assumption. Rather than assuming that incarcerated and non-incarcerated individuals would have followed similar trends in potential outcomes unconditionally, we only require this assumption to hold conditional on offense and sentencing score—a weaker and more credible identifying assumption.

7 Conclusion

This paper provides new quasi-experimental evidence on how incarceration affects long-run economic trajectories. We build a new panel dataset spanning 25 years that captures incarceration, asset ownership, college enrollment, employment, and earnings for felony defendants, based on a combination of administrative and commercial data. Using a matched difference-in-differences design, we show that incarceration leads to persistent declines in asset ownership and human capital accumulation—two key channels through which low-income individuals build wealth. While we find that incarceration leads to only temporary declines in employment and recidivism, consistent with prior evidence, it has lasting effects on homeownership, car ownership, and post-secondary education. The persistence of gaps in asset accumulation and education, even after labor market outcomes recover, suggests that incarceration affects broader aspects of economic stability than those currently captured in the literature.

Taken together, our findings indicate that the absence of lasting labor market scarring should not be interpreted as evidence that incarceration has limited long-run effects. Even relatively short prison terms can disrupt wealth building and human capital accumulation. These effects are particularly relevant given the concentration of incarceration in economically disadvantaged communities, where they may reinforce existing disparities in wealth and mobility across generations (Andrews et al., 2017; Finlay et al., 2023).

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Table 1: Summary statistics

	Comparison group	**Treatment group**	**Difference**
	Mean	Mean	Mean difference
Criminal proceeding			
Months sentence	0.0	15.6	15.6***
Pretrial detention	0.08	0.12	0.03***
Fine or restitution	0.30	0.36	0.06***
Sentence at least partially suspended	0.96	0.96	-0.00
Demographics			
Age	36.1	35.9	-0.2
Black	0.46	0.46	-0.01
Hispanic	0.01	0.02	0.01***
Female	0.25	0.17	-0.07***
Type of offense			
Robbery	0.02	0.02	0.00
Drug	0.34	0.34	0.00
Assault	0.14	0.14	-0.00
Larceny	0.10	0.10	-0.00
Criminal history			
On probation/parole	0.35	0.37	0.02**
Past misdemeanor convictions	0.9	0.9	-0.0***
Number of cases (2001-2014)	4,936	32,898	
Outcomes year -1			
Home owner (2001-2014)	0.09	0.09	-0.00
Car owner (2010-2014)	0.18	0.15	-0.03**
Employment (2003-2012)	0.28	0.24	-0.04***
Earnings (2003-2012)	7170	6562	-608**
Any post-secondary education (2005-2013)	0.11	0.09	-0.02**

Note: This table presents summary statistics for matched observations in the treatment and comparison groups for the year prior to sentencing. The first two columns report means for the comparison group and treatment group, respectively. The third column reports the difference in means between the treatment and comparison groups. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. Homeowner, car owner, employment, earnings, and any post-secondary education are measured in the year prior to sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. The number of cases for the comparison and treated groups are, respectively: 1,635 vs. 11,454 for car ownership; 3,460 vs. 23,520 for yearly earnings and employment; and 3,042 vs. 21,150 for post-secondary education. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 2: Main specification: effects of incarceration on labor market outcomes, recidivism, assets, and human capital accumulation

	Diff-in-Diff estimates						
	Incarcerated	Employed	Earnings	Recent felony charge	Homeowner	Car owner	Any post-secondary education
Year 1	0.916***	-0.081***	-2,790***	-0.031***	-0.002	-0.015**	-0.012***
	(0.004)	(0.008)	(394)	(0.007)	(0.003)	(0.006)	(0.003)
	[0.042]	[0.239]	[5,548]	[0.062]	[0.092]	[0.188]	[0.115]
Year 5	0.007	0.019**	-319	0.004	-0.011**	-0.023**	-0.016***
	(0.005)	(0.009)	(508)	(0.007)	(0.005)	(0.010)	(0.005)
	[0.076]	[0.171]	[5,650]	[0.046]	[0.113]	[0.251]	[0.142]
Year 7	0.017***	0.016*	-294	0.004	-0.011**	-0.027**	-0.014**
	(0.005)	(0.009)	(494)	(0.006)	(0.005)	(0.011)	(0.005)
	[0.062]	[0.162]	[5,529]	[0.033]	[0.123]	[0.263]	[0.147]
$E[Y_{-1}\|D=1]$	0.001	0.241	6,163	0.077	0.091	0.149	0.093
p-value joint F-test years -5 to -1	0.789	0.600	0.187	0.699	0.796	0.436	0.826
Total obs.	299,890	215,840	215,840	264,794	302,000	104,040	193,536
N cases	37,570	26,980	26,980	33,175	37,834	13,089	24,192

Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. We present the data for three post-sentencing years: year 1 after sentencing, which captures some incapacitation from treatment, and year 5 and year 7, which capture post-release effects. Estimates of counterfactual means for the treated group $E[Y_t(0)|D=1]$ in each year are shown in square brackets. $E[Y_{-1}|D=1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 3: Effects of longer prison sentences (4-7 years) on labor market outcomes and recidivism

	Diff-in-Diff estimates			
	Incarcerated	Employed	Earnings	Recent felony charge
Year 1	0.956***	-0.225***	-3,846***	-0.077***
	(0.010)	(0.026)	(1022)	(0.017)
	[0.043]	[0.250]	[4,264]	[0.086]
Year 7	0.117***	0.009	1,137	-0.012
	(0.017)	(0.023)	(979)	(0.015)
	[0.079]	[0.174]	[2,934]	[0.056]
Year 12	0.000	0.016	1,898*	-0.019
	(0.017)	(0.025)	(1069)	(0.015)
	[0.078]	[0.147]	[3,508]	[0.049]
$E[Y_{-1}\|D = 1]$	0.002	0.230	4,655	0.094
p-value joint F-test	0.357	0.582	0.220	0.039
years -5 to -1				
Total obs.	518,436	322,938	322,938	445,086
N cases	28,802	17,941	17,941	24,727

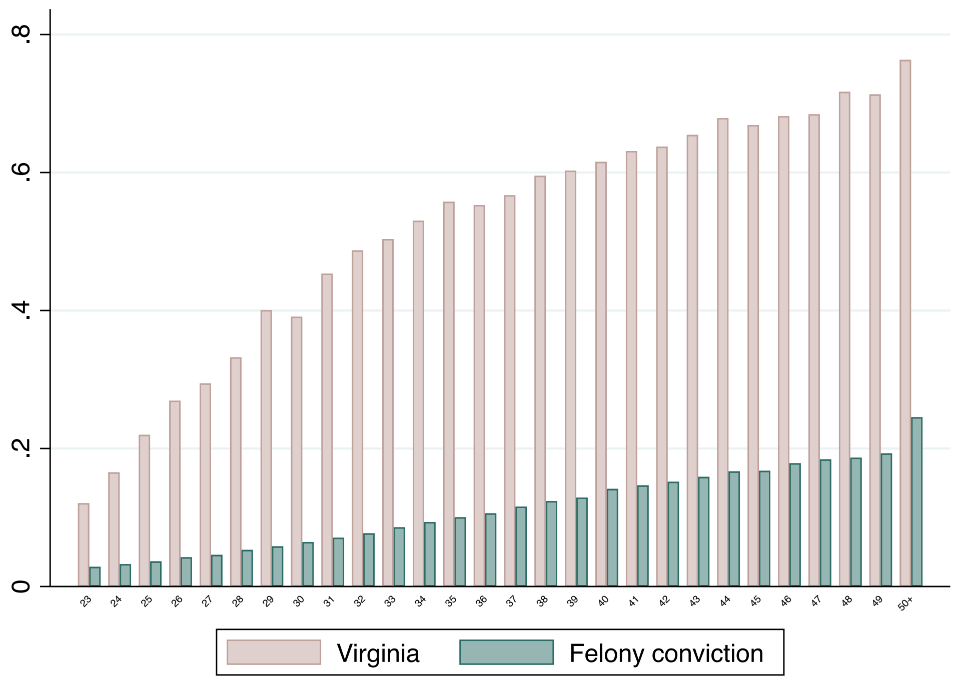
Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who received a 4-7 year prison sentence, while the comparison group comprises those who were not incarcerated, as described in Section 5.2. We present the data for three post-sentencing years: year 1 after sentencing, which captures some incapacitation from treatment, and year 7 and year 12, which capture post-release effects. Estimates of counterfactual means for the treated group $E[Y_t(0)|D = 1]$ in each year are shown in square brackets. $E[Y_{-1}|D = 1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2007 for labor market outcomes and 2001-2009 for recidivism. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 4: Alternative research design: effects of incarceration on labor market outcomes, recidivism, assets, and human capital accumulation

	Diff-in-Diff estimates						
	Incarcerated	Employed	Earnings	Recent felony charge	Homeowner	Car owner	Any post-secondary education
Year 1	0.879***	-0.041***	-1,723***	-0.038***	-0.008**	-0.019***	-0.005*
	(0.005)	(0.008)	(462)	(0.008)	(0.003)	(0.006)	(0.003)
	[0.061]	[0.220]	[4,768]	[0.072]	[0.114]	[0.208]	[0.116]
Year 5	0.010*	0.012	632	0.007	-0.018***	-0.034***	-0.009**
	(0.005)	(0.008)	(496)	(0.007)	(0.005)	(0.010)	(0.004)
	[0.063]	[0.179]	[4,673]	[0.038]	[0.134]	[0.280]	[0.137]
Year 7	0.016***	0.004	434	0.006	-0.014**	-0.034***	-0.010**
	(0.005)	(0.009)	(522)	(0.007)	(0.006)	(0.011)	(0.005)
	[0.053]	[0.174]	[4,722]	[0.025]	[0.141]	[0.288]	[0.145]
$E[Y_{-1}\|D=1]$	0.000	0.248	6,647	0.056	0.109	0.162	0.102
p-value joint F-test years -5 to -1	0.545	0.609	0.436	0.212	0.252	0.888	0.572
Total obs.	105,980	77,024	77,024	92,534	106,724	38,740	70,144
N cases	13,280	9,628	9,628	11,596	13,373	4,875	8,768

Note: This table presents the estimates from the difference-in-difference specification in which we define treatment as those who score right *above* the cutoff for a prison sentence and were incarcerated for four years or less, and comparison as those who score right *below* the cutoff and were not incarcerated, as described in Section 5.5. We present the data for three post-sentencing years: year 1 after sentencing, which captures some incapacitation from treatment, and year 5 and year 7, which capture post-release effects. Estimates of counterfactual means for the treated group $E[Y_t(0)|D=1]$ in each year are shown in square brackets. $E[Y_{-1}|D=1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * p < 0.10, ** p < 0.05, and *** p < 0.01.

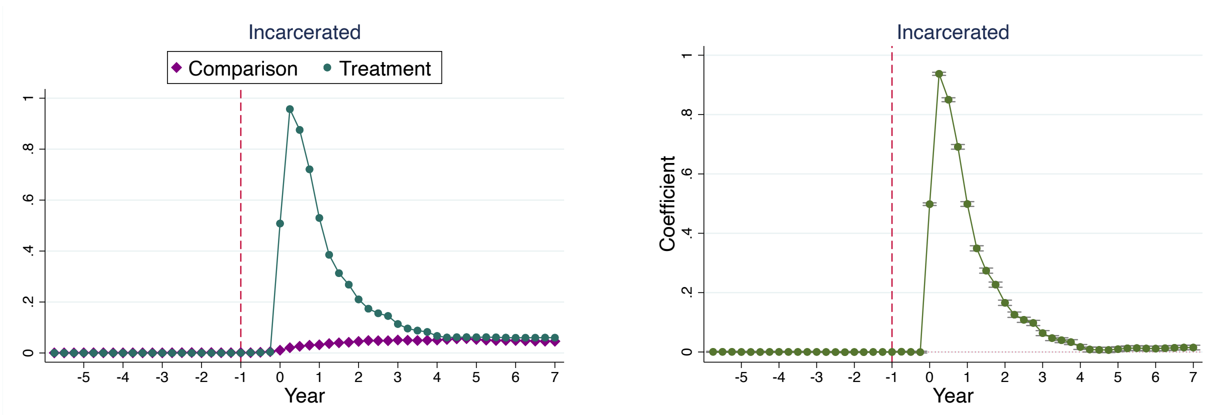
Figure 1: Homeownership rates among our sample compared to Virginia households, by age



Note: This figure compares homeownership for our sample of defendants with a first-time felony conviction to homeownership for Virginia household heads, sorted by age. Data for other Virginia residents come from the ACS 5-year estimates for heads of households. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions.

Figure 2: Exposure to incarceration

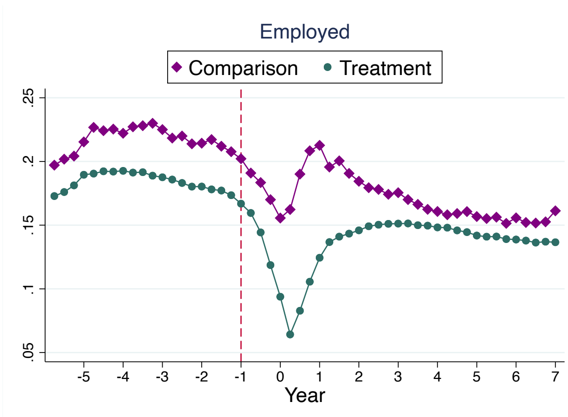
(a) Incarceration on felony sentences **(b)** Incarceration on felony sentences



Note: The figure on the left shows average outcomes for those who were incarcerated for four years or less versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figure on the right shows matched difference-in-differences estimates. The outcome is incarceration on a felony sentence, including future felony convictions. The sample is restricted to individuals sentenced in 2001-2014. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar. Estimates and standard errors are shown in Appendix Table C.1.

Figure 3: Main specification: effects of incarceration on employment, earnings, and recidivism

(a)



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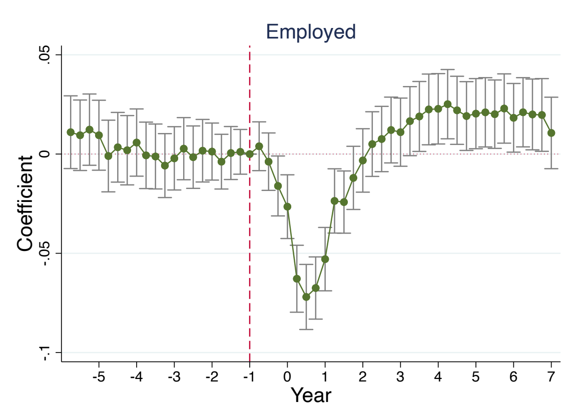

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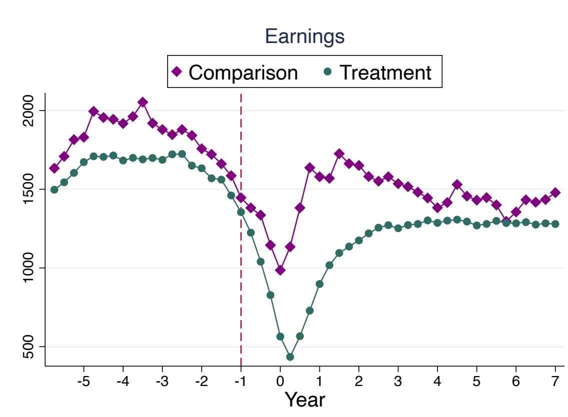

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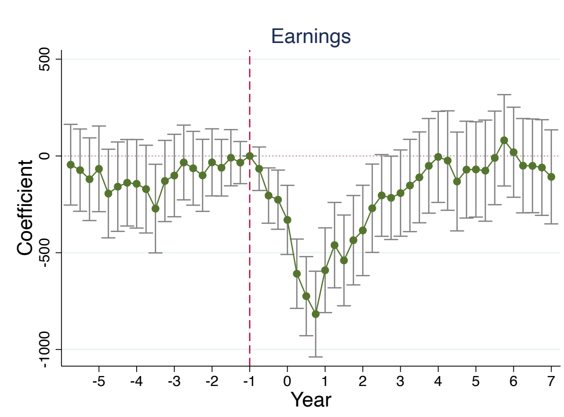

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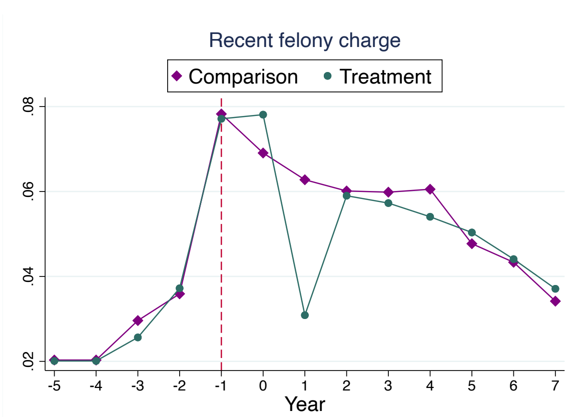

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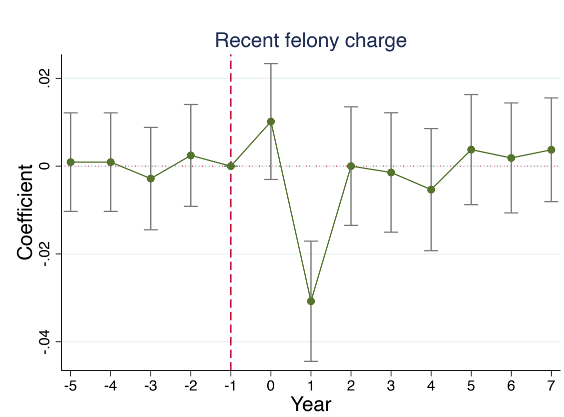
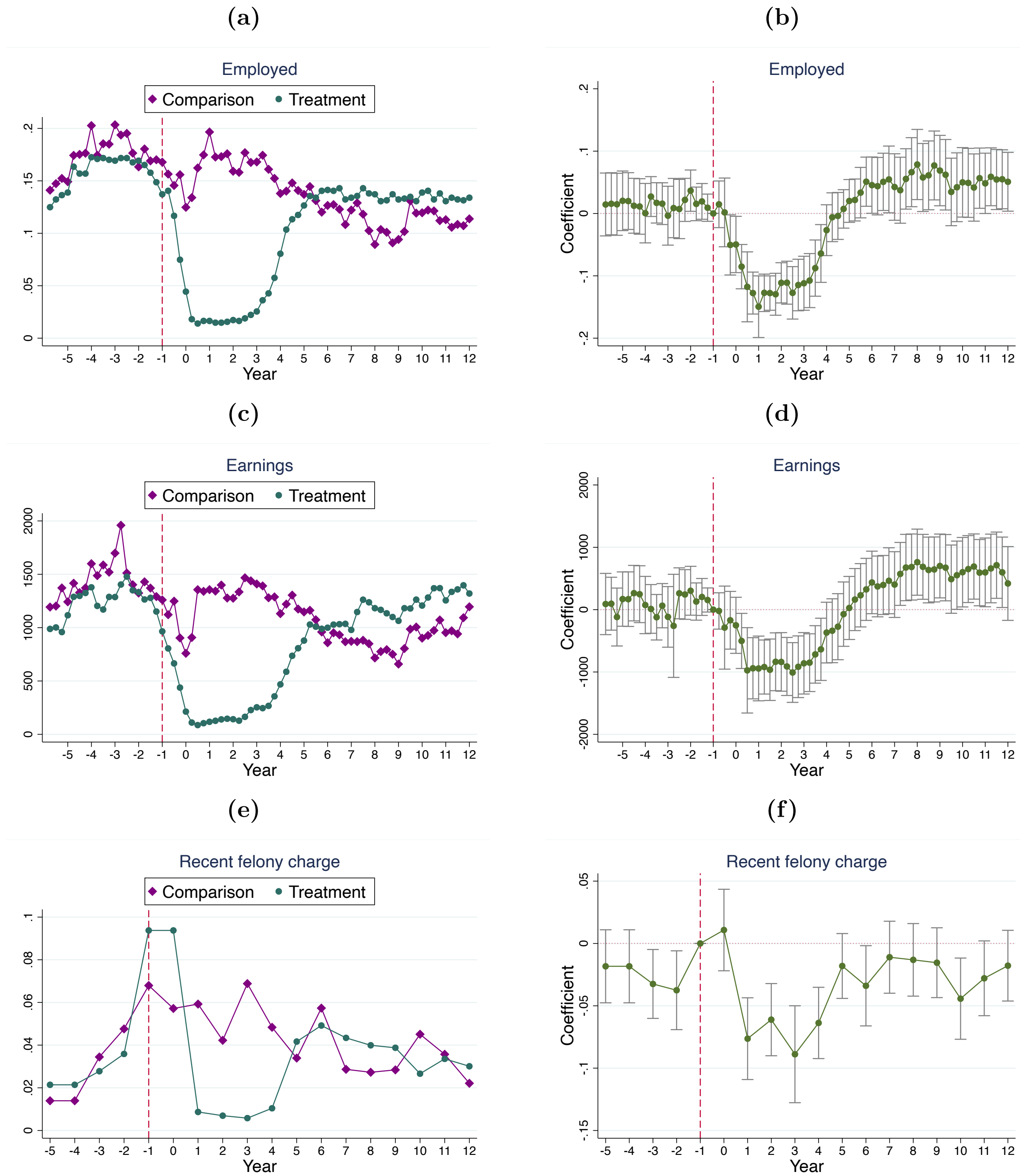


Note: The figures on the left show average outcomes for those who were incarcerated for four years or less versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figures on the right show matched difference-in-differences estimates. We focus on labor market outcomes and on prior and future felony convictions, excluding the focal felony conviction that defines treatment. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar. Estimates and standard errors are shown in Appendix Table C.1.

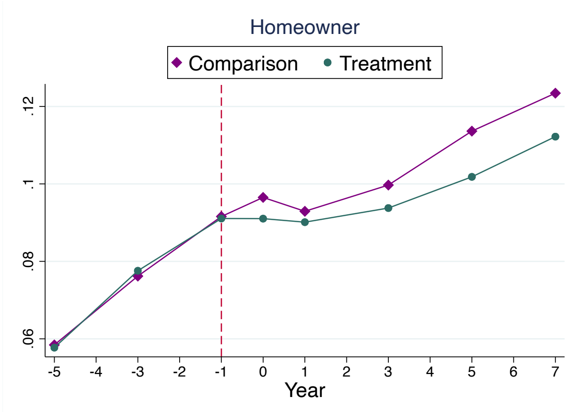
Figure 4: Effects of longer prison sentences (4-7 years) on employment, earnings, and recidivism



Note: The figures on the left show average outcomes for those who received a 4-7 year prison sentence versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 5.2. The figures on the right show difference-in-differences estimates. We focus on labor market outcomes and on prior and future felony convictions, excluding the focal felony conviction that defines treatment. The sample is restricted to individuals sentenced in 2003-2007 for labor market outcomes and 2001-2009 for all other outcomes. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar. Estimates and standard errors are shown in Appendix Table C.5.

Figure 5: Main specification: effects of incarceration on assets and human capital accumulation

(a)



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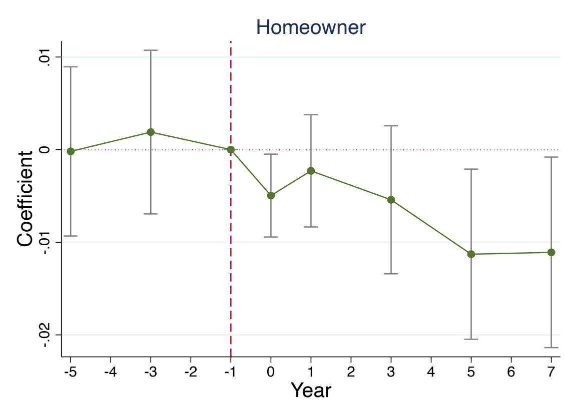

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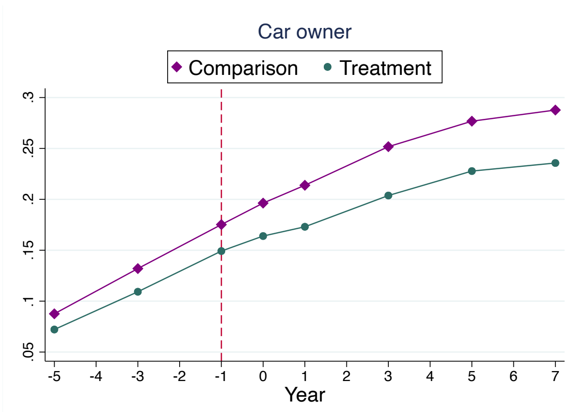

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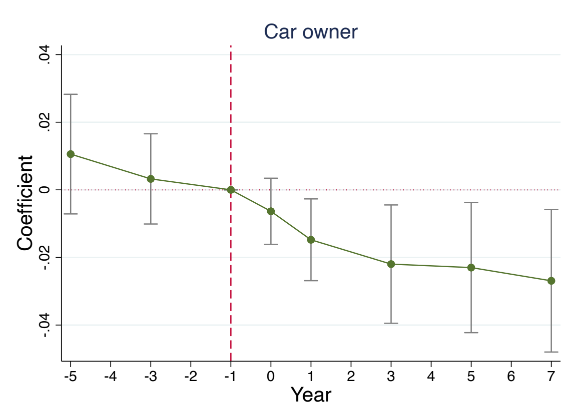

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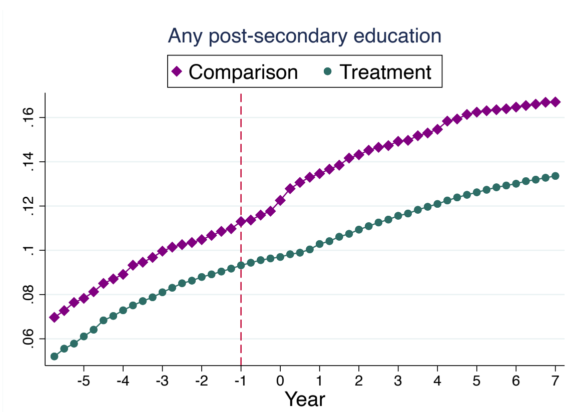

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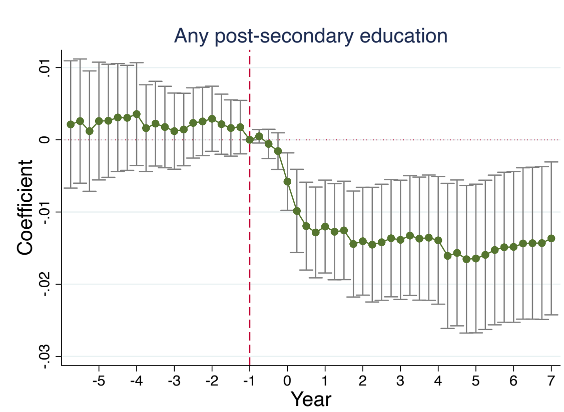
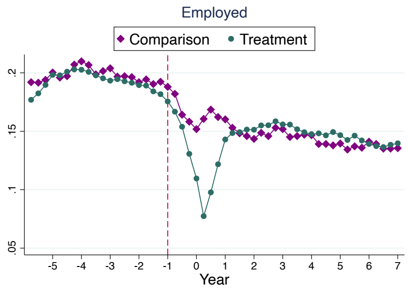


Note: The figures on the left show average outcomes for those who were incarcerated for four years or less versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figures on the right show matched difference-in-differences estimates. The sample is restricted to individuals sentenced in 2001-2014 for homeownership, 2010-2014 for car ownership, and 2005-2013 for post-secondary education. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar. Estimates and standard errors are shown in Appendix Table C.1.

Figure 6: Alternative specification: effects of incarceration on employment, assets, and human capital accumulation

(a)



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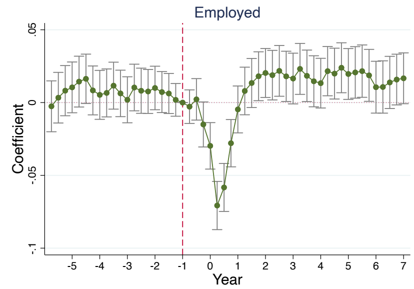

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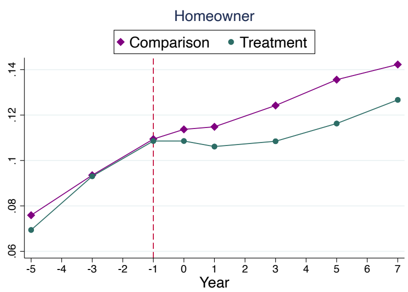

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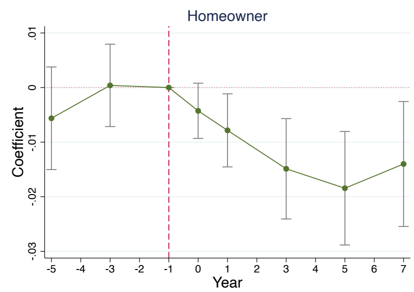

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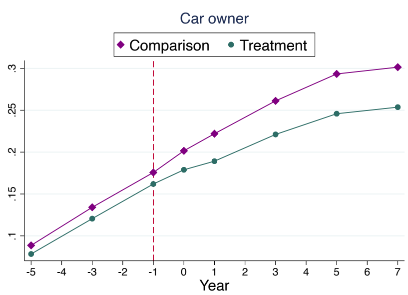

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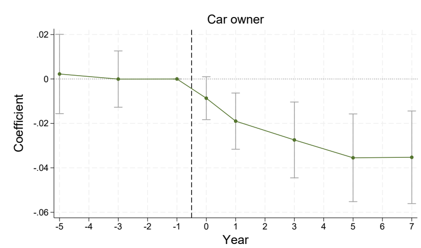

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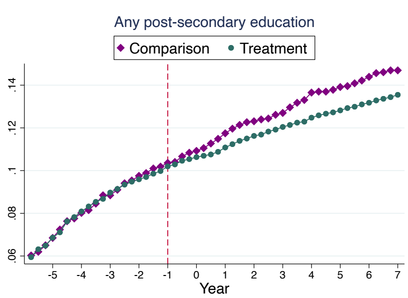
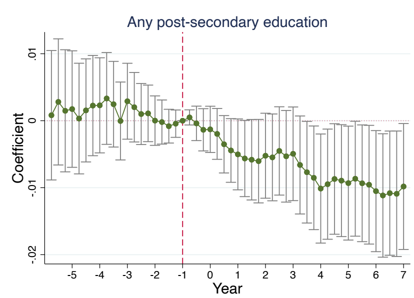


(h)



Note: Figures on the left show average outcomes for individuals who score right *above* the cutoff for a prison sentence and were incarcerated for four years or less versus those who score right *below* the cutoff and were not incarcerated, as described in Section 5.5. The figures on the right show matched difference-in-differences estimates. The sample is restricted to individuals sentenced in 2003-2012 for employment, 2001-2014 for homeownership, 2010-2014 for car ownership, and 2005-2013 for post-secondary education. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar. Estimates and standard errors are shown in Appendix Table C.8.

ONLINE APPENDIX
Revisiting the Lasting Impacts of Incarceration

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A Example of sentencing worksheet

Drug/Schedule I/II ❖ Section A

Offender Name: ____________

Primary Offense

		Score
A. Possess Schedule I or II drug		
1 count	1	
2 counts	3	
3 counts	8	
B. Sell, Distribute, Possession with Intent Schedule I or II drug		
1 count	12	
2 counts	13	
3 counts	14	
4 counts	15	
C. Sell, etc. Schedule I, II drug to minor (1 count)	11	
D. Accommodation - Sell, Distribute, Possession with Intent Schedule I or II drug		
1 count	5	
2 counts	7	
E. Sell, etc. imitation Schedule I or II drug (1 count)	4	

Primary Offense Additional Counts Total the maximum penalties for counts of the primary not scored above — 0

Years: 5 - 10 ... 1; 11 - 21 ... 2; 22 - 30 ... 3; 31 - 42 ... 4; 43 or more ... 5

Additional Offenses Total the maximum penalties for additional offenses, including counts — 0

Years: Less than 4 ... 0; 4 - 10 ... 1; 11 - 21 ... 2; 22 - 30 ... 3; 31 - 42 ... 4; 43 or more ... 5

Knife or Firearm in Possession at Time of Offense — **If YES, add 2** → 0

Conviction in Current Event Requiring Mandatory Minimum Term (6 mos or more) **If YES, add 9** → 0

Mandatory Firearm Conviction for Current Event — **If YES, add 7** → 0

Prior Convictions/Adjudications Total the maximum penalties for the 5 most recent and serious prior record events — 0

Years: Less than 7 ... 0; 7 - 26 ... 1; 27 - 48 ... 2; 49 or more ... 3

Prior Incarcerations/Commitments — **If YES, add 2** → 0

Prior Felony Drug Convictions/Adjudications — 0

Number: 1 - 2 ... 1; 3 - 4 ... 2; 5 ... 3; 6 or more ... 4

Prior Juvenile Record — **If YES, add 1** → 0

Legally Restrained at Time of Offense — 0

None ... 0
Other than parole/post-release, supervised probation or CCCA ... 1
Parole/post-release, supervised probation or CCCA ... 4

SCORE THE FOLLOWING FACTOR **ONLY** IF PRIMARY OFFENSE IS **POSSESSION OF SCHEDULE I/II DRUG** (§ 18.2-250(A,a))

Two or More Prior Felony Convictions/Adjudications — **If YES, add 2** → 0
For Possession, Possession with Intent, Distribution, Manufacture or Sale of Schedule I or II Drug

Total Score →

If total is 10 or less, go to **Section B**. If total is 11 or more, go to **Section C**.

Drug Schedule I or II/ Section A Eff. 7-1-09

B Additional details on data construction

B.1 Data sources

Virginia Criminal Sentencing Commission (VCSC) data. The VCSC provided a dataset that contains information on individuals in Virginia sentenced for a felony. The data provided to us by the VCSC includes records on almost all people convicted of a felony in Virginia from 1996 to 2020.[†] This dataset includes information on the charge(s) of conviction, date of sentencing, sentence imposed for this conviction, guidelines-recommended sentence, points accrued on each item in a worksheet, and total worksheet scores.

Data aggregator. The data aggregator collects data for two purposes: for marketing purposes (it allows companies to target individuals for advertising or special offers); and to develop alternative credit scores for use in automotive, telecommunications, payday lending, and banking industries. The data is collected both at the individual level and at the household level. Household members include anyone living at the same address at one time, including the individual.

Virginia court data. Circuit Court records are available for all but two judicial circuits from 2000-2021. This data source provides race and gender. It also provides our recidivism measure, new felony charges in Circuit Court. We obtain data on misdemeanor sentences and pretrial detention from Virginia's District Courts, using the same matching variables. District court data are available from 2010-2021.

Employment data. We obtain formal sector earnings and employment data from the Virginia Employment Commission (VEC), which maintains records on all W-2 income reported by employers in the state of Virginia. These data are observed at a quarterly frequency from 1999 through 2019. We define earnings as total W-2 wage and salary income reported by employers in the state. We use the Consumer Price Index to adjust earnings so that they are in 2009 dollars.

College data. We obtain data on college enrollment from the State Council of Higher Education in Virginia (SCHEV), which maintains records on all public schools and most private, nonprofit schools within the state. SCHEV collects data on all institutions where students are eligible for Virginia Tuition Assistance Grants, which includes all public institutions and all private institutions whose primary place of business is Virginia. The data includes information on in-prison education administered by community and state colleges. These data are observed at a quarterly frequency from the second quarter of 2000 to the first quarter of 2020.

[†]VCSC does not collect data on people convicted of the type of felonies that don't have guidelines-recommended sentences, but these felonies are rare.

B.2 Matching procedures

VCSC to Virginia Circuit Court data. Below is a description of how we match the sentencing commission data with Virginia Circuit Court data:

- We match the VCSC data to the Virginia court data using the fuzzy matching method developed by Enamorado et al. (2019) and match on first name, last name, middle initial, FIPS code, birth month, and sentence date. We drop Alexandria and Fairfax as they are not in the court data. For the years and counties in which a match is feasible, our match rate is 92%.

VCSC to VEC (employment) data. Below is a description of the matching process used to match the sentencing commission data with employment data:

- Employment data: first match attempt based on
  - first five letters of last name
  - first four letters of first name
  - last four digits of social security number
  - month and year of birth

  For defendants who did not have a match yet, a second matching procedure was completed using the following variables:
- Employment data: second match attempt based on
  - first character first name
  - full last name
  - last four digits of social security number

VCSC to SCHEV (college education) data. Below is a description of the matching process used to match the sentencing commission data with education data:

- Education data: first match attempt based on
  - full last name
  - first three letters of first name
  - first letter of middle name
  - month and year of birth
  - last four digits of social security number

  For defendants who did not have a match yet, a second matching procedure was completed using the following variables:
- Education data: second match attempt based on
  - full last name
  - first four letters of first name
  - first letter of middle name
  - month and year of birth

  For defendants who did not have a match yet, a third and final matching procedure was completed using the following variables:
- Education data: third match attempt based on
  - first five letters of last name
  - first four letters of first name
  - last four digits of social security number

B.3 Assessing match quality and potential measurement error

To assess the potential for measurement error due to imperfect matches with outcomes from the data aggregator, we compare conviction dates from the data aggregator to those recorded in the sentencing commission data.

The data aggregator provides the date of the most recent felony conviction, which we compare to that appearing in the sentencing commission data, since all individuals in our main sample have at least one felony conviction. This comparison enables us to construct simple match-verification metrics.

We have both a conservative and inclusive measure of match verification. The conservative criterion restricts the sample to defendants whose sentencing commission conviction date falls within two months of a felony conviction as recorded by the data aggregator (most are exact). The inclusive criterion also includes those whose sentencing commission date of conviction is within two months of a misdemeanor conviction, or one year of a felony conviction, as recorded by the data aggregator. Court records often involve multiple charges, both misdemeanors and felonies, that may be resolved on different dates. Given this inherent complexity, we consider the inclusive verification standard to still provide strong evidence of a valid match.

One caveat with this approach is that our data aggregator is missing criminal justice data from certain judicial circuits and years. For these circuit-years, we cannot verify the match with this method.

The conservative match-verification rate is 72.16% (78.06% among the non-missing judicial circuits) and 88.17% for the inclusive match verification (91% among non-missing judicial circuits). These high match rates—especially under the inclusive standard—are reassuring with respect to data quality.

Importantly, excluding cases without a verified felony match in the data aggregator has no material effect on our estimates. Appendix Figure C.13 shows that estimated effects are nearly identical across the full sample and the two subsamples that satisfy the conservative and inclusive match-verification test against the main specification. This reinforces confidence that measurement error due to mismatches is unlikely to drive our main findings.

B.4 Sample construction

This section details the data construction and cleaning process as well as the matching procedure implemented between the various raw datasets described above. We focus on describing how we construct the sample for our main analyses.

We begin with the sample of 580,323 conviction records from the VCSC between 1995 and 2019.

- *Data merging.* First, we match the VCSC data to our data aggregator and to Virginia court records. The aggregator provides eight "snapshot" years of data centered around the sentencing date: years -5, -3, -1, 0, 1, 3, 5, and 7. After merging, we obtain a panel of 4,642,584 observations corresponding to the original 580,323 cases.

- *Yearly variable construction.* We construct yearly earnings and education variables from unbalanced quarterly panel data. For earnings, we start with administrative quarterly earnings records. Quarters are collapsed to a single earnings value per person-date. We then merge in the dates of sentencing from the VCSC data and create a quarterly time variable that is relative to the date of sentencing, such that quarter 0 contains the sentencing date. This creates a panel of earnings information by defendant-sentence-quarter. We construct an indicator for employment, which is equal to one if the individual has any positive earnings in that quarter. We then construct yearly variables: employment is equal to one if the individual had any employment in the previous year, and earnings are the total yearly earnings. These are used for the regression tables. A parallel procedure is applied to our post-secondary education data. The data is at the semester level and covers spring, summer, and fall semesters. We convert the data to the quarterly level where spring, summer, and fall semesters correspond to quarters 1, 3, and 4 respectively. We then construct a quarterly variable that is equal to one if the individual is awarded a post-secondary education credit in that quarter or any prior quarter. We construct a yearly post-secondary education variable which is equal to one if that individual has enrolled in any post-secondary education that year or prior years. We merge this yearly data into the main data set for use in regression tables.
- *Imaginary archive data.* We then drop all cases with imaginary archive dates, that is, cases where the archive date falls after the end of data availability. After this cut, we are left with 580,009 cases.
- *First felony restriction.* Next, we restrict the sample to individuals for whom this is a first felony conviction, as recorded in both the sentencing commission and the data aggregator. This step reduces the sample to 332,101 cases (57% of total cases).
- *Age restriction.* We further restrict to individuals who were at least 23 years old at sentencing, ensuring that we observe five years of adult pre-sentencing data. This step removes an additional 86,105 cases (26% of previous cases).
- *Score and sentence restriction.* Finally, we retain only individuals who were recommended for prison, had a sentencing score within 10 points above the threshold for a prison recommendation, and, if incarcerated, received a sentence of no more than four years. These restrictions remove an additional 180,932 cases (74% of previous cases).
- *Final sample.* Our final sample consists of 65,064 unique cases.

Lastly, we note that our outcome data are not available for all years. For our main analyses, we want to make sure that we observe everyone for at least 5 years before the focal sentencing date, and for at least 7 years after. We limit observation years to match available data:

- For exposure to incarceration, we include individuals sentenced between 2001 and 2014, which represents 37,570 cases. When including pretrial detention, probation revocations, and misdemeanor sentences, we include individuals sentenced between 2012 and 2014, representing 7,667 cases.

- For homeownership, we include individuals sentenced between 2001 and 2014, which represents 37,834 cases.
- For recent felony charge from the VCSC data, we include individuals sentenced between 2001 and 2014, which represents 33,175 cases.
- For labor market outcomes, we include individuals sentenced between 2003 and 2012, which represents 26,980 cases.
- For car ownership, we include individuals sentenced between 2010 and 2014, which represents 13,089 cases.
- For post-secondary education, we include individuals sentenced between 2005 and 2013, which represents 24,192 cases.

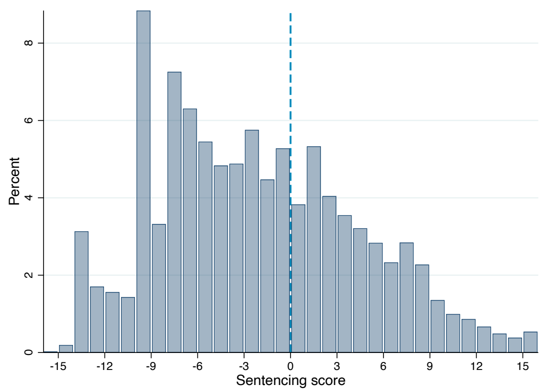
B.5 Main variables

- *Incarceration.* We define an individual as receiving a carceral sentence if their sentence includes some prison or jail time.
- *Sentencing score.* Numerical value assigned to a defendant based on the severity of their current offense and their prior criminal record. This score determines the sentencing recommendation under the Virginia sentencing guidelines. We have normalized it so that those who score 0 and above receive a recommendation for prison.
- *Recidivism.* Our yearly recidivism variable is defined as receiving a felony charge in Circuit Court for a new offense alleged to have been committed within the previous 12 months. This measure does not include revocations unless these are also accompanied by a new felony charge for a new crime.
- *Prior conviction flag.* We define someone as having a prior felony conviction if they had a prior felony conviction either in the sentencing commission data, or according to the data aggregator. This captures felony convictions not only in Virginia, but also in the rest of the United States.
- *Black.* Race of the defendant as defined in the Virginia Circuit Court data. Almost all of the individuals for which race information is available are labeled either "Black" or "White." Ethnicity is not available.
- *Female.* Gender of the defendant as defined in the Virginia Circuit Court data.
- *Employment.* We define an individual as being employed in quarter X if they appear in the Virginia Employment Commission database as having reported positive W-2 income in that quarter.
- *Earnings.* We define earnings in quarter X as the total wage income reported in a W-2 that quarter, as reflected in the Employment Commission database. We use the Consumer Price Index to adjust earnings so that they are in 2009 dollars.
- *Post-secondary education.* We define an individual as having obtained any post-secondary education by quarter X if they have enrolled in at least one higher education credit in that quarter or a previous one.
- *Homeownership.* This is defined as an individual having a deed in their name or having paid property taxes for a property in their name in the relevant year.

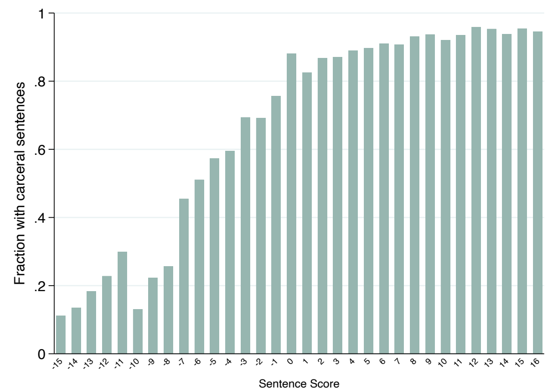
- *Car ownership.* This is defined as an individual having ever been on file at a car dealership, or having patronized an oil change company or a repair shop.
- *Household member.* This includes the individual who received a felony conviction as well as anyone who has shared an address with them up to that date.
- *Under 23.* An indicator for whether the defendant is under 23 years of age at sentencing. Since our data does not have birth month, we define age approximately as the sentence year minus the birth year.

C Additional figures and tables

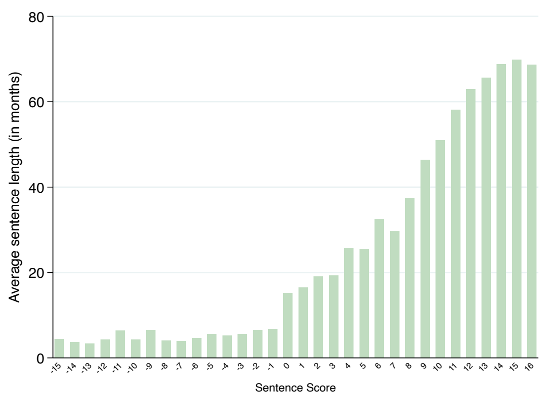
Figure C.1: Sentencing score distribution and sentencing outcomes



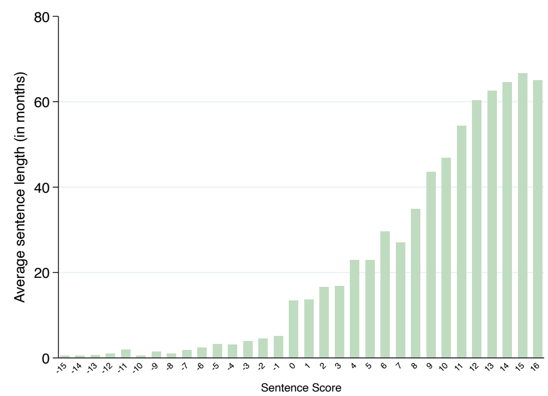
(a) Distribution of sentencing scores



(b) Fraction with carceral sentences by score



(c) Average sentence length by score (carceral only)

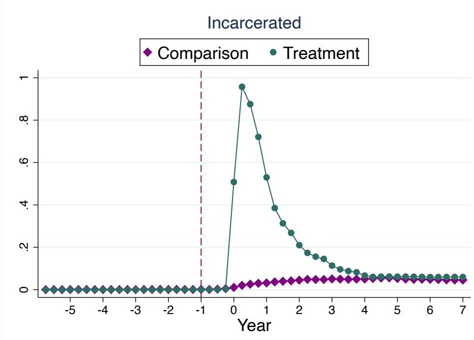


(d) Average sentence length by score (all defendants)

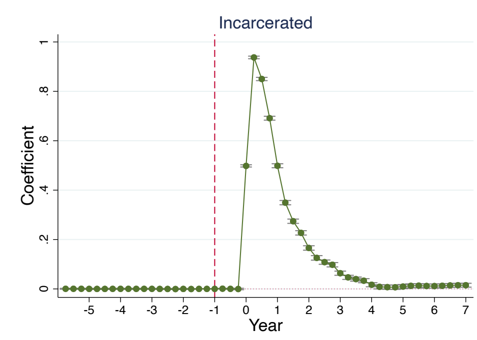
Note: This figure presents key patterns in sentencing scores and sentence outcomes, with sentencing scores truncated at the 99^{th} percentile. Panel (a) shows the distribution of sentencing scores. Panel (b) shows the fraction of individuals receiving carceral sentences by score. Panel (c) shows average sentence length by score for carcerally sentenced individuals only, while Panel (d) includes all defendants, with non-carceral sentences counting as zero. The sample is restricted to individuals sentenced between 2001 and 2014. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions.

Figure C.2: Various measures of exposure to incarceration centered around sentencing date

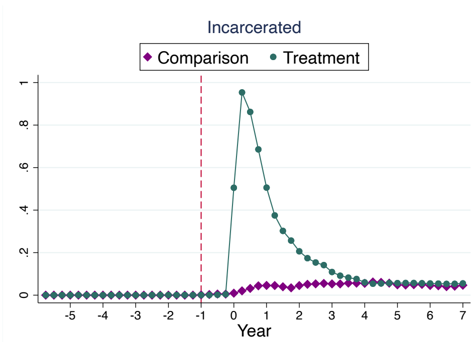
(a) Incarceration on felony sentences (2001-2014)



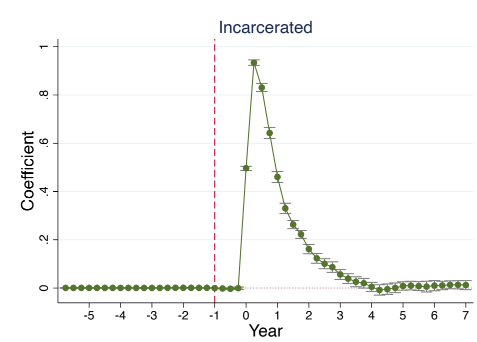
(b) Incarceration on felony sentences (2001-2014)



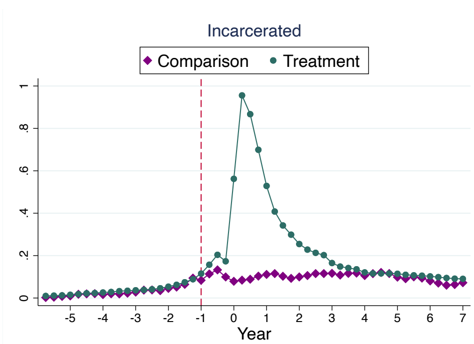
(c) Only felony sentences (2012-2014)



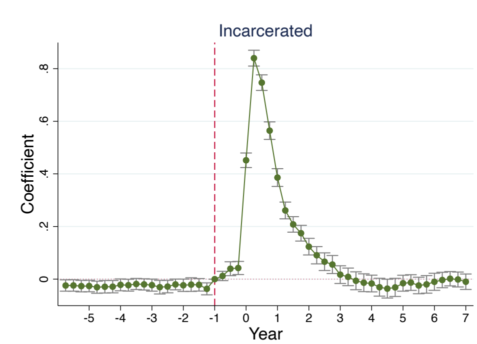
(d) Only felony sentences (2012-2014)



(e) Centered around offense date: Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



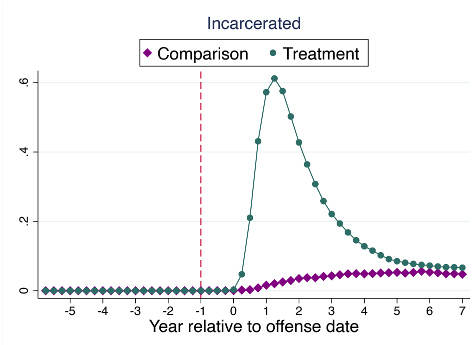
(f) Centered around offense date: Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



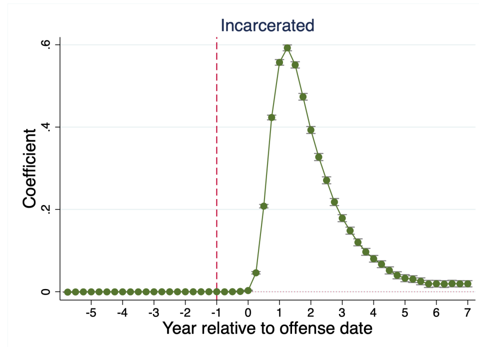
Note: The figures on the left show average outcomes for those who were incarcerated for four years or less versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figures on the right show matched difference-in-differences estimates. All figures are centered around the date of the sentencing. The first row shows incarceration on a felony sentence for the full sample, years 2001-2014. For the second two rows, the sample is restricted to individuals sentenced in 2012-2014. The middle row includes only incarceration on a felony sentence, while the bottom row additionally includes pretrial detention, probation revocation, and misdemeanor sentences. We include defendants who were recommended for prison (worksheet score between 0 and 9), who were at least 23 years old at sentencing, and had no prior felony convictions.

Figure C.3: Various measures of exposure to incarceration: centering the analysis around offense date instead of sentencing date

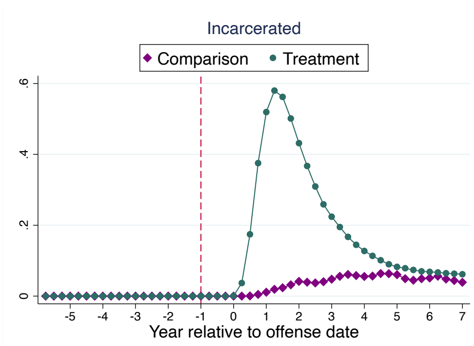
(a) Centered around offense date: Only felony sentences (2001-2014)



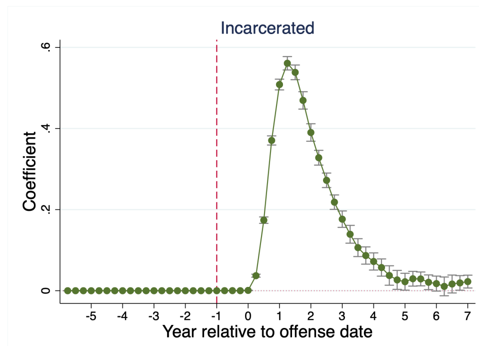
(b) Centered around offense date: Only felony sentences (2001-2014)



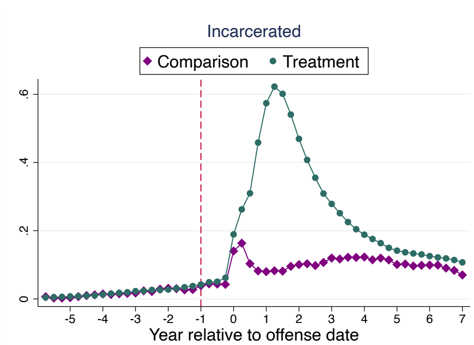
(c) Centered around offense date: Only felony sentences (2012-2014)



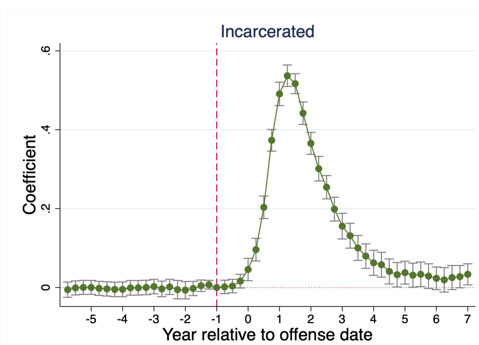
(d) Centered around offense date: Only felony sentences (2012-2014)



(e) Centered around offense date: Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



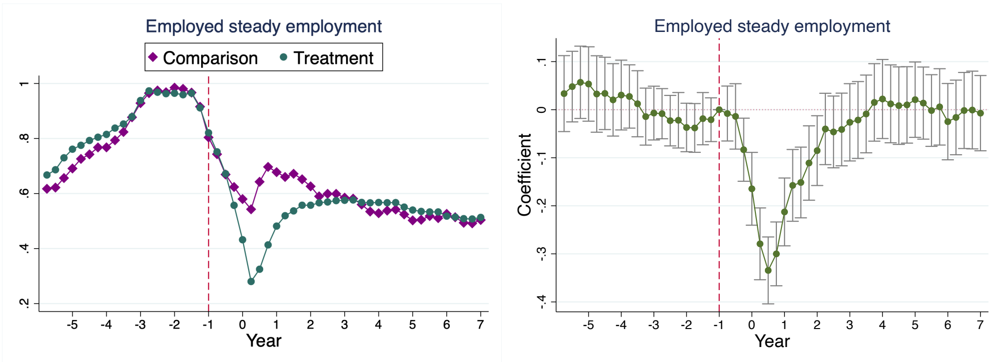
(f) Centered around offense date: Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



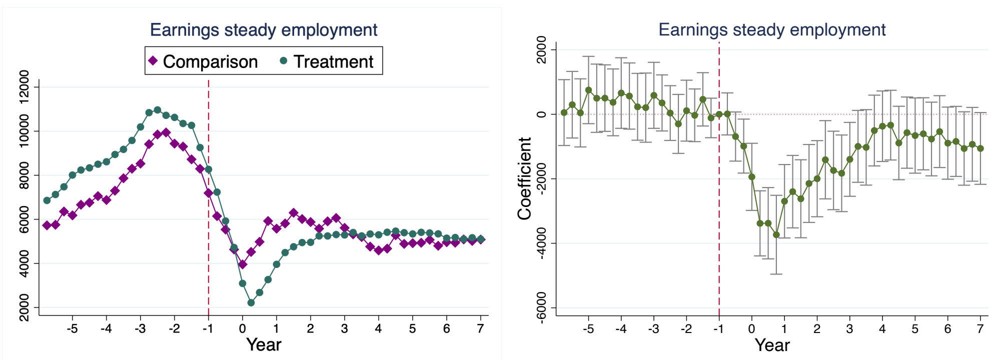
Note: The figures on the left show average outcomes for those who were incarcerated for four years or less versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figures on the right show matched difference-in-differences estimates. All figures are centered around the date of the offense rather than the date of sentencing. The first row shows incarceration on a felony sentence for the full sample, years 2001-2014. For the second two rows, the sample is restricted to individuals sentenced in 2012-2014. The middle row includes only incarceration on a felony sentence, while the bottom row additionally includes pretrial detention, probation revocation, and misdemeanor sentences. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions.

Figure C.4: Labor market outcomes for those with steady employment prior to sentencing

(a) Employment



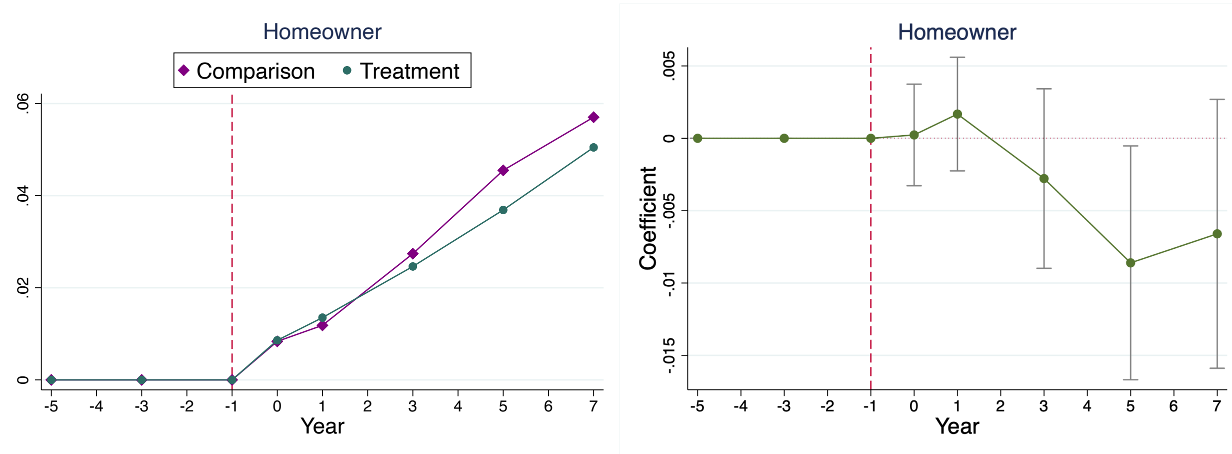
(b) Earnings



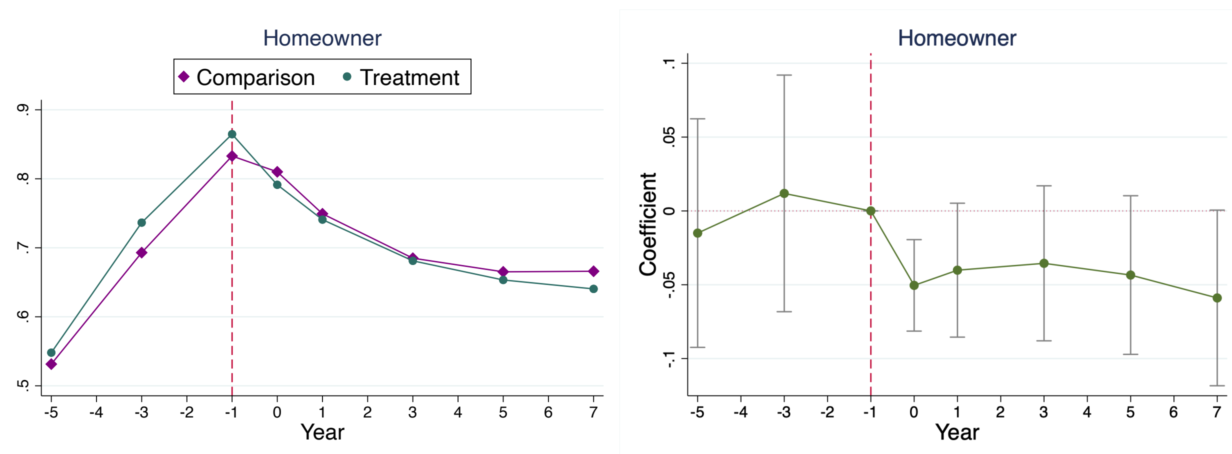
Note: The figures on the left show average outcomes for those who were incarcerated for four years or less versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figures on the right show matched difference-in-differences estimates. The sample includes only those defendants with steady employment pre-sentencing. Steady employment refers specifically to defendants who were employed for at least 7 quarters out of 8 between the 3rd year and 1.25 years pre-sentencing. The sample is restricted to individuals sentenced in 2003-2007. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions.

Figure C.5: Homeownership effects by prior homeownership status

(a) No homeownership at any point before sentencing



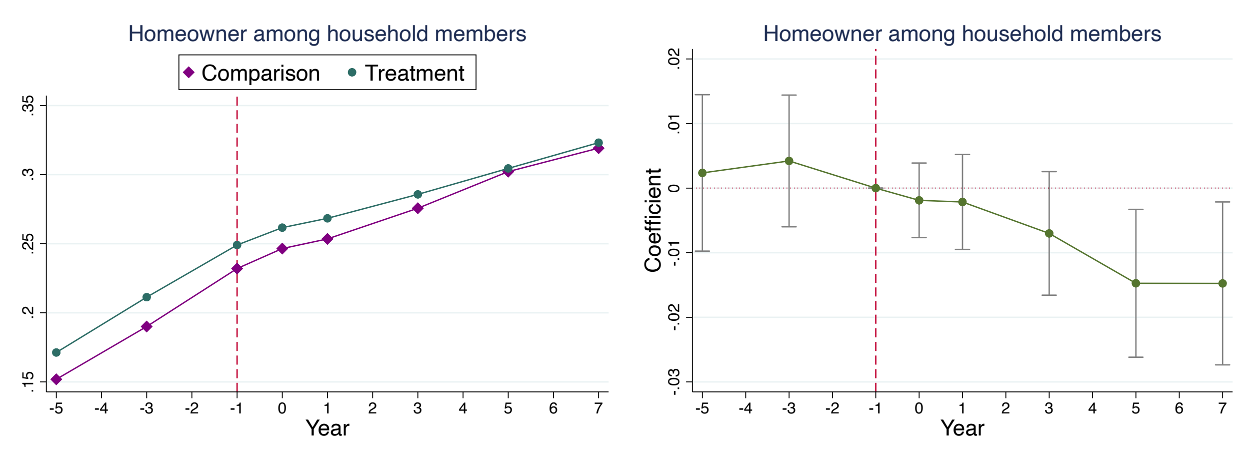
(b) Owned a home at some point before sentencing



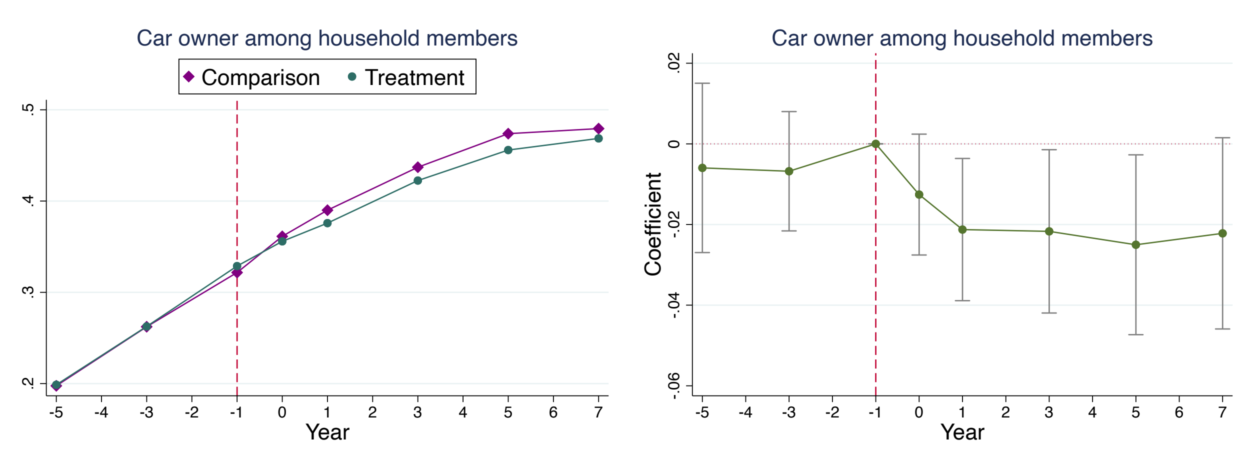
Note: The figures on the left show average outcomes for those who were incarcerated versus a matched sample of those not incarcerated, with matching on offense type and sentencing score, as described in Section 4. The figures on the right show matched difference-in-differences estimates. These figures compare the impact of incarceration on homeownership among those who did/did not own a home prior to sentencing. "No prior ownership" only applies to defendants who *did not* own a house at any point prior to the sentencing date. Conversely, "Prior ownership" only applies to defendants who *owned* at some point a house prior to sentencing. The sample is restricted to individuals sentenced in 2001-2014. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions.

Figure C.6: Impacts of incarceration on household-level home and car ownership

(a) Homeownership

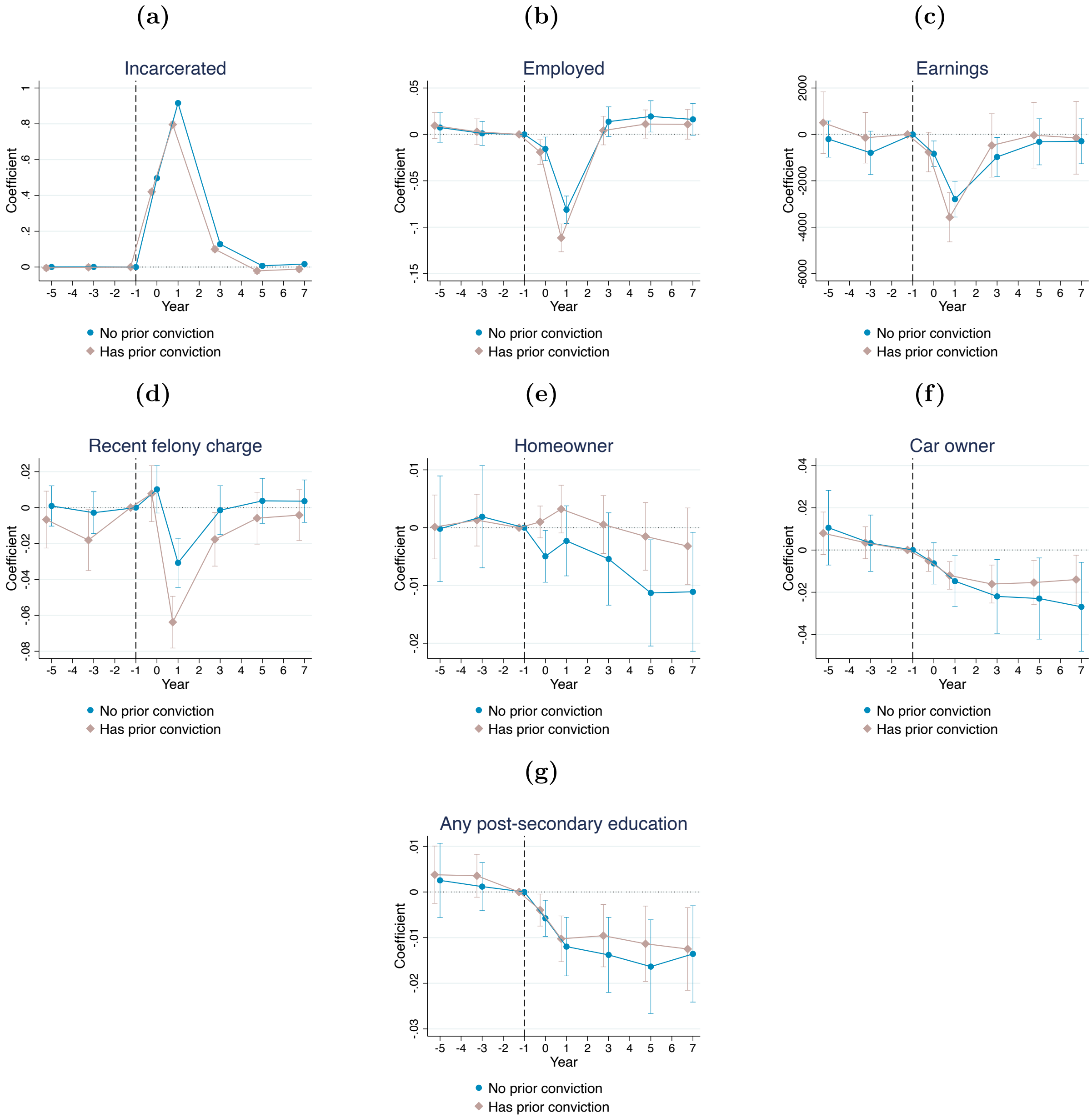


(b) Car ownership



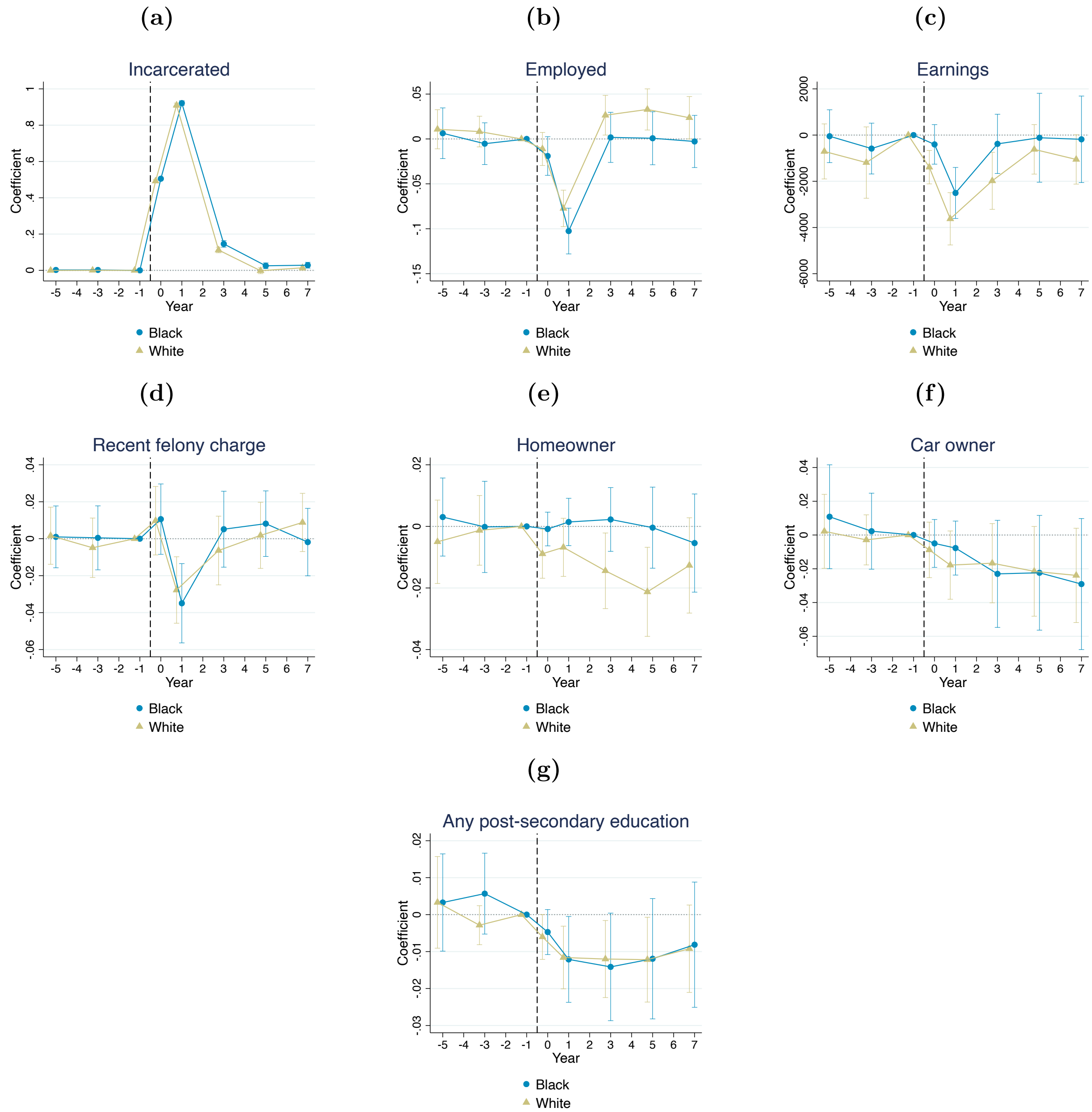
Note: The figures on the left show average outcomes for those who were incarcerated for four years or less, versus a matched sample of those not incarcerated, as described in Section 4. Outcomes are shown at the household level; they include the person who was sentenced for a felony as well as their household members, defined as individuals who shared their address at some point in time. The figures on the right show matched difference-in-differences estimates. The sample is restricted to individuals sentenced in 2010-2014 for car ownership and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions.

Figure C.7: Main estimates (those with no prior felony convictions) vs impacts for those with prior felony convictions



Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration depending on prior conviction status. The "No prior conviction" specification (blue circles) shows our main estimates, only including defendants who had no prior felony convictions. The "Has prior conviction" specification (pink diamonds), includes only defendants who had at least one prior felony conviction. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9) and were at least 23 years old at sentencing. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar.

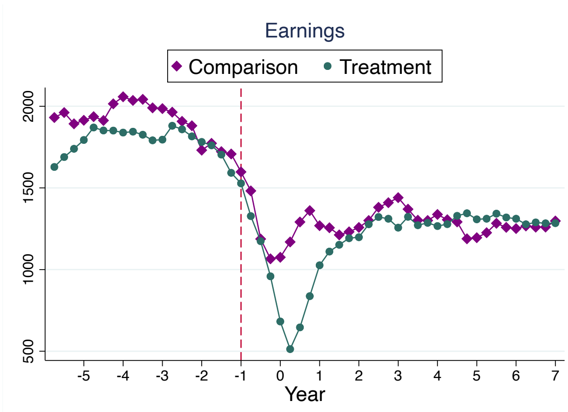
Figure C.8: Heterogeneity by race



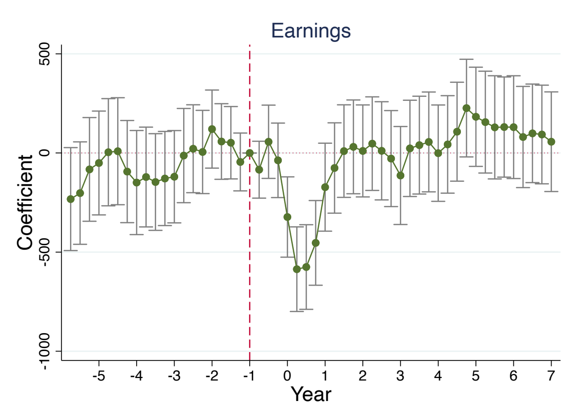
Note: These figures depict the estimates from difference-in-difference analyses examining the impact of incarceration depending on race. The "Black" specification (blue circles) includes only Black defendants, while the "White" specification (grey triangles) includes only defendants who are White. Almost everyone in our data is labeled Black or White. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education include those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes include those sentenced in 2001-2014. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar.

Figure C.9: Alternative research design: the impact of incarceration on earnings and recidivism

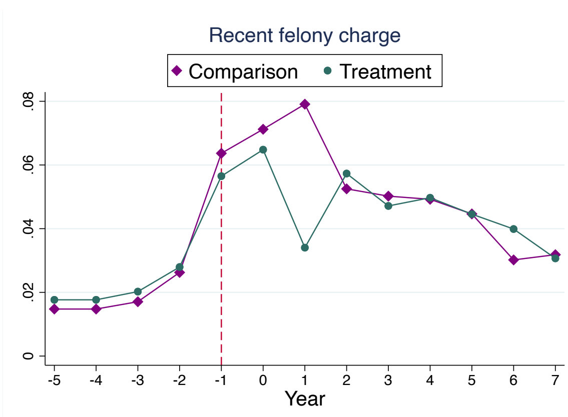
(a)



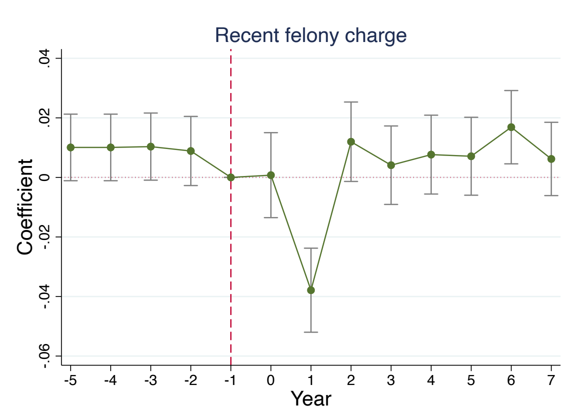
(b)



(c)

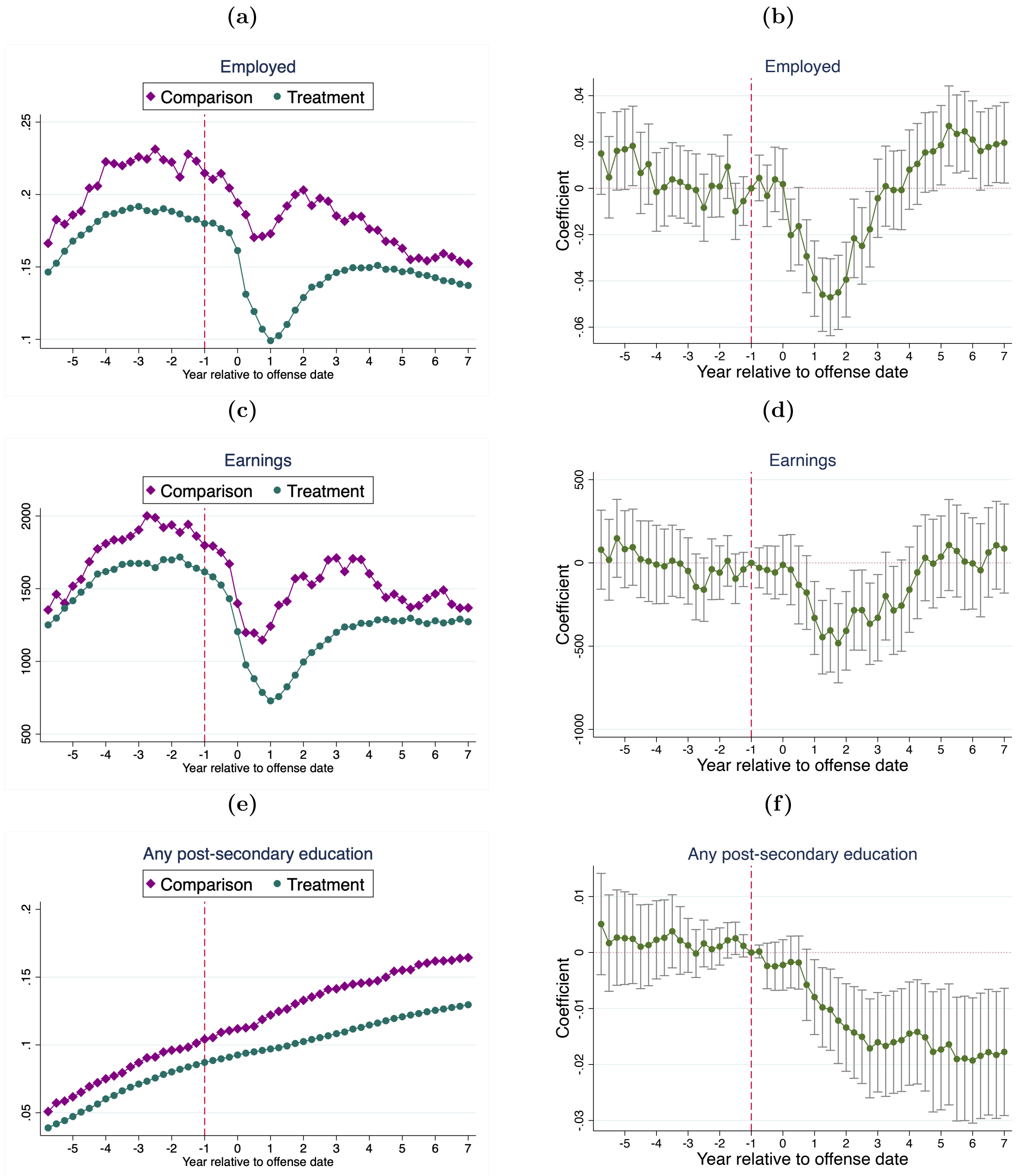


(d)



Note: Figures on the left show average outcomes for individuals who score right *above* the cutoff for a prison sentence and were incarcerated for four years or less versus those who score right *below* the cutoff and were not incarcerated, as described in Section 5.5. The figures on the right show difference-in-differences estimates. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes and 2001-2014 for recidivism. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Estimates and standard errors are shown in Appendix Table C.8

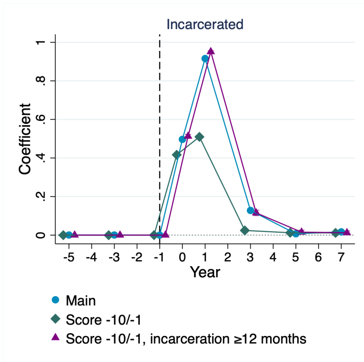
Figure C.10: Effects of incarceration on employment and earnings: centering around offense date instead of sentencing date



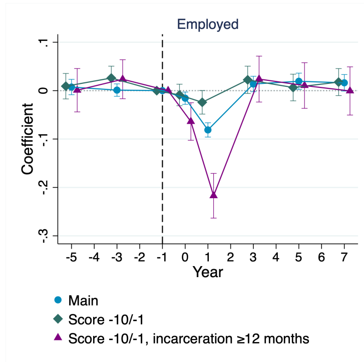
Note: The figures on the left show average outcomes for those who were incarcerated for four years or less, versus a matched sample of those not incarcerated, as described in Section 4. The figures on the right show matched difference-in-differences estimates. Quarter 0 is the quarter where the alleged offense was committed, instead of the quarter where the defendant was sentenced, as in our main specification. The sample includes defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. The sample for employment and earnings outcomes also restricts to individuals sentenced in 2003-2012.

Figure C.11: Robustness - matched difference-in-differences among those who received a recommendation for jail or probation

(a)

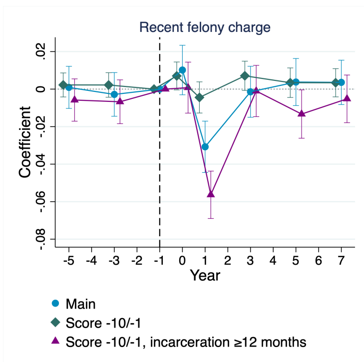


(b)

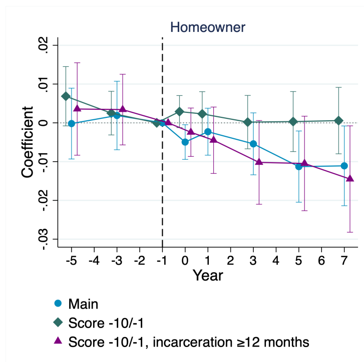


(c)

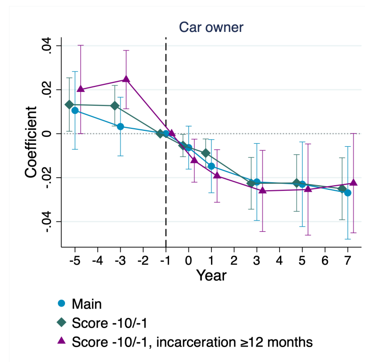
(d)



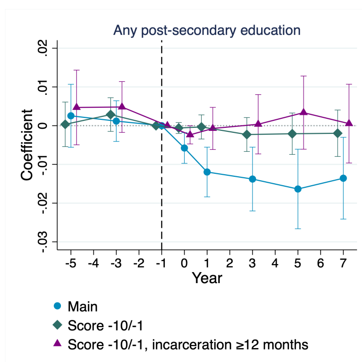
(e)



(f)



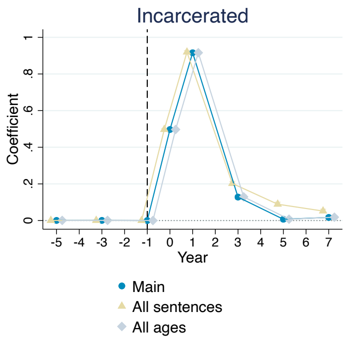
(g)



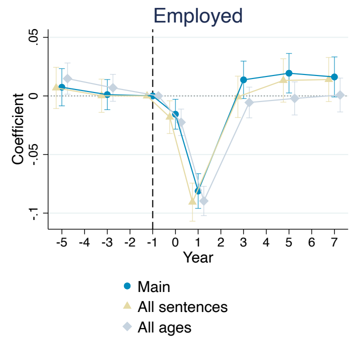
Note: These figures show matched difference-in-differences estimates for those who received different sentencing recommendations. These figures compare two additional specifications, both focused on defendants who were recommended for jail or probation (worksheet score between -10 and -1), against our main specification, which includes defendants recommended for prison (worksheet score between 0 and 9). All specifications compare those who were incarcerated versus a matched sample of those not incarcerated, with matching on offense type and sentencing score. In the "Main" specification (blue circles), treatment includes those who were incarcerated for four years or less; the average incarceration length is 15.6 months. In the specification "Score -10/-1" (green diamonds), treatment includes those who were incarcerated for four years or less; the average incarceration length is 4.7 months. In the specification "Score -10/-1, incarceration $\geq$ 12 months" (purple triangles), treatment includes those who were incarcerated for one to four years inclusive; the average incarceration length is 17.5 months. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar.

Figure C.12: Robustness - dropping restrictions on age and on length of incarceration

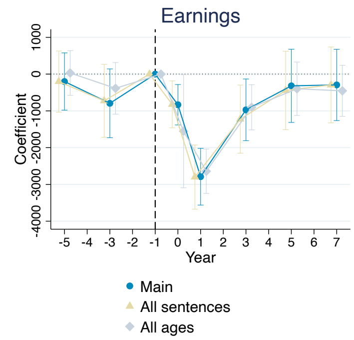
(a)



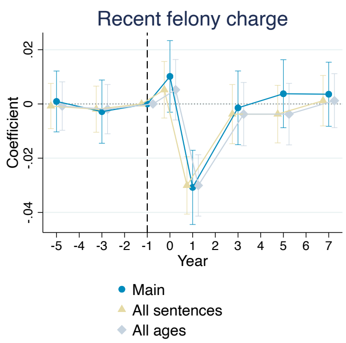
(b)



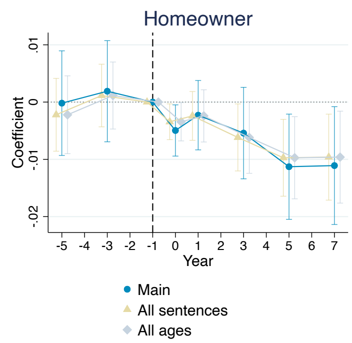
(c)



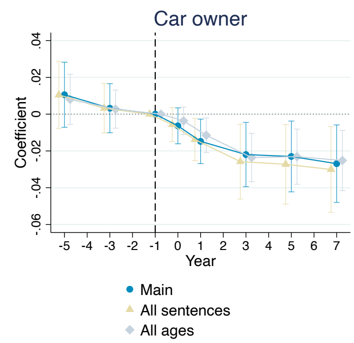
(d)



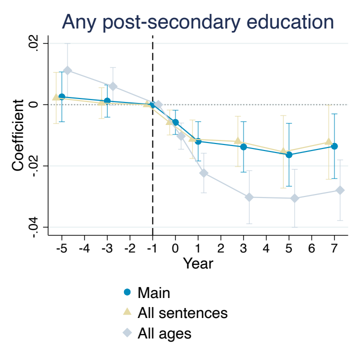
(e)



(f)

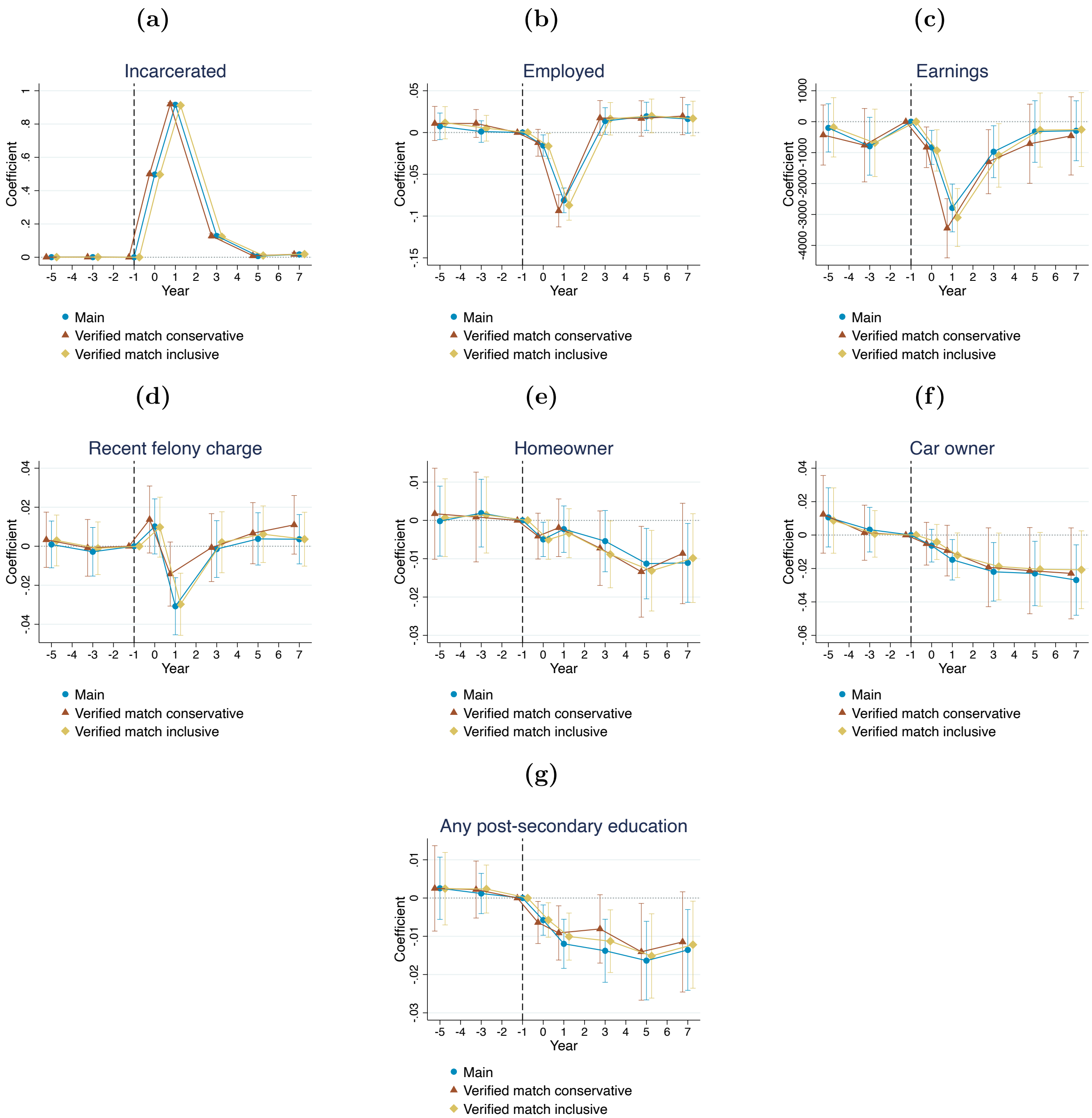


(g)



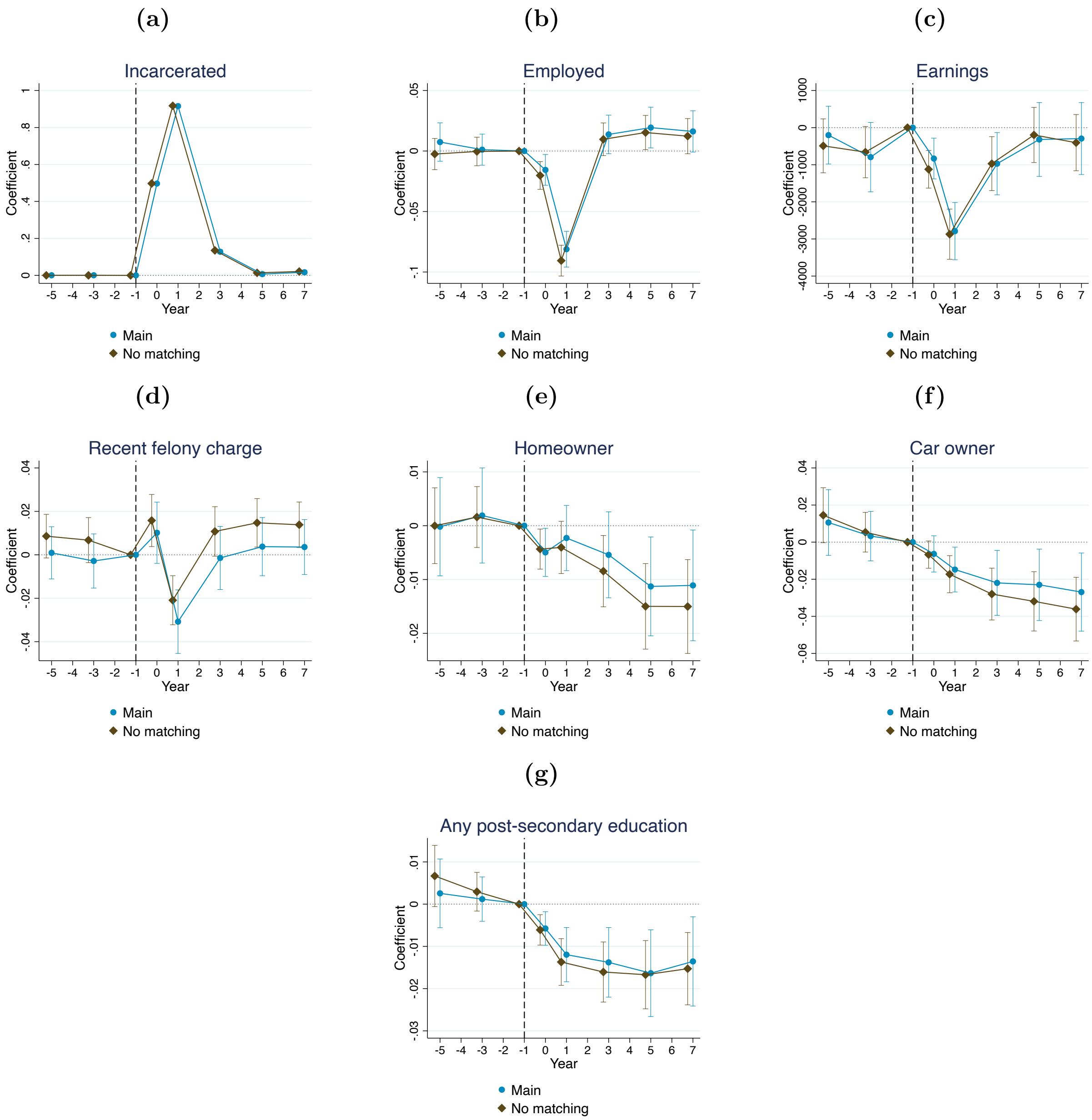
Note: These figures show matched difference-in-differences estimates for samples with relaxed restrictions on sentence length and age. The "Main" specification (blue circles) shows our main results, which limits to those over the age of 23 and defines treatment as a carceral sentence of four years or less. In the "All sentences" specification (beige diamonds), treatment includes all those who were incarcerated, regardless of the sentence length. The sample is still limited to those over the age of 23. In the "All ages" specification (grey triangles), the sample includes defendants of all ages, but treatment is still limited to those sentenced to four years or less of incarceration. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9) and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar.

Figure C.13: Robustness - subsamples with match verification



Note: These figures show matched difference-in-differences estimates depending on match verification. The "Main" specification (blue circles) shows our main results. The "Verified match conservative" specification (brown triangles) includes only defendants whose date of felony convictions matches within two months across the sentencing commission data and aggregator data (most are exact). The "Verified match inclusive" (beige diamonds) also includes those whose date of conviction is within two months but are labeled in the aggregator data as a misdemeanor instead of a felony, as well as those whose date of felony conviction is within one year across the two data sets. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar.

Figure C.14: Robustness - without matching



Note: These figures show matched difference-in-differences estimates depending on match verification. The "Main" specification (blue circles) shows our main results. The "No matching" (brown diamonds) replicates the same DiD design without implementing the matching procedure. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level; the 95% confidence interval is shown in the whisker bar.

Table C.1: Main specification: effects of incarceration on labor market outcomes, recidivism, assets and human capital accumulation

	Diff-in-Diff estimates						
	Incarcerated	Employed	Earnings	Recent felony charge	Homeowner	Car owner	Any post-secondary education
Year -5	0.001	0.007	-202	0.001	-0.000	0.011	0.003
	(0.001)	(0.008)	(397)	(0.006)	(0.005)	(0.009)	(0.004)
	[-0.001]	[0.233]	[6,763]	[0.019]	[0.058]	[0.061]	[0.058]
Year -3	0.001	0.001	-794*	-0.003	0.002	0.003	0.001
	(0.001)	(0.007)	(477)	(0.006)	(0.005)	(0.007)	(0.003)
	[-0.001]	[0.248]	[7,757]	[0.029]	[0.076]	[0.106]	[0.080]
Year 0	0.497***	-0.016**	-833***	0.010	-0.005**	-0.006	-0.006***
	(0.003)	(0.007)	(280)	(0.007)	(0.002)	(0.005)	(0.002)
	[0.011]	[0.222]	[4,604]	[0.068]	[0.096]	[0.170]	[0.103]
Year 1	0.916***	-0.081***	-2,790***	-0.031***	-0.002	-0.015**	-0.012***
	(0.004)	(0.008)	(394)	(0.007)	(0.003)	(0.006)	(0.003)
	[0.042]	[0.239]	[5,548]	[0.062]	[0.092]	[0.188]	[0.115]
Year 3	0.128***	0.014*	-972**	-0.001	-0.005	-0.022**	-0.014***
	(0.006)	(0.008)	(428)	(0.007)	(0.004)	(0.009)	(0.004)
	[0.069]	[0.189]	[6,062]	[0.058]	[0.099]	[0.226]	[0.129]
Year 5	0.007	0.019**	-319	0.004	-0.011**	-0.023**	-0.016***
	(0.005)	(0.009)	(508)	(0.007)	(0.005)	(0.010)	(0.005)
	[0.076]	[0.171]	[5,650]	[0.046]	[0.113]	[0.251]	[0.142]
Year 7	0.017***	0.016*	-294	0.004	-0.011**	-0.027**	-0.014**
	(0.005)	(0.009)	(494)	(0.006)	(0.005)	(0.011)	(0.005)
	[0.062]	[0.162]	[5,529]	[0.033]	[0.123]	[0.263]	[0.147]
$E[Y_{-1} \mid D=1]$	0.001	0.241	6,163	0.077	0.091	0.149	0.093
p-value joint F-test years -5 to -1	0.789	0.600	0.187	0.699	0.796	0.436	0.826
Total obs.	299,890	215,840	215,840	264,794	302,000	104,040	193,536
N cases	37,570	26,980	26,980	33,175	37,834	13,089	24,192

Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. "Year" represents the year relative to the date of sentencing interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D=1]$ in each year are shown in square brackets. $E[Y_{-1}|D=1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.2: Main specification: various measures of incarceration

	Diff-in-Diff estimates		
	Incarcerated Felony sent. (2001-2014)	Incarcerated Felony sent. (2012-2014)	Incarcerated Broad def. (2012-2014)
Year -5	0.001	0.001	-0.003
	(0.001)	(0.001)	(0.017)
	[-0.001]	[-0.001]	[0.023]
Year -3	0.001	0.001	0.005
	(0.001)	(0.001)	(0.018)
	[-0.001]	[-0.001]	[0.057]
Year 0	0.497***	0.493***	0.404***
	(0.003)	(0.005)	(0.020)
	[0.011]	[0.012]	[0.240]
Year 1	0.916***	0.901***	0.784***
	(0.004)	(0.012)	(0.022)
	[0.042]	[0.053]	[0.174]
Year 3	0.128***	0.126***	0.109***
	(0.006)	(0.013)	(0.024)
	[0.069]	[0.072]	[0.184]
Year 5	0.007	-0.004	-0.006
	(0.005)	(0.013)	(0.024)
	[0.076]	[0.079]	[0.176]
Year 7	0.017***	0.014	0.030
	(0.005)	(0.011)	(0.021)
	[0.062]	[0.055]	[0.099]
$E[Y_{-1}\|D = 1]$	0.001	0.001	0.164
p-value joint F-test years -5 to -1	0.789	0.773	0.630
Total obs.	299,890	60,666	60,666
N cases	37,570	7,667	7,667

Note: This table presents estimates for exposure to incarceration outcomes and from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. "Year" represents the year relative to the date of sentencing interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D = 1]$ in each year are shown in square brackets. $E[Y_{-1}|D = 1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample for the first column and the last two columns includes individuals sentenced between 2001-2014 and 2012-2014, respectively. "Felony sent." refers to felony incarceration, whereas "Broad def." includes pretrial detention, probation revocation, and misdemeanor sentences. We include defendants that were recommended for prison (worksheet score between 0 and 9), who were at least 23 years old at sentencing and had no prior felony convictions Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.3: Main specification: the impacts of incarceration on labor market outcomes and recidivism, up to twelve years post-sentencing

	Diff-in-Diff Estimates							
	Incarcerated		Employed		Earnings		Recent felony charge	
	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$
Year -5	0.001	[-0.001]	0.004	[0.217]	-869*	[6,369]	-0.002	[0.019]
	(0.002)		(0.010)		(500)		(0.007)	
Year -4	0.001	[-0.001]	-0.007	[0.244]	-1,257**	[7,660]	-0.002	[0.019]
	(0.002)		(0.009)		(636)		(0.007)	
Year -3	0.001	[-0.001]	-0.002	[0.240]	-962	[7,542]	-0.005	[0.029]
	(0.002)		(0.008)		(596)		(0.007)	
Year -2	-0.000	[0.000]	-0.009	[0.245]	-348	[6,979]	0.001	[0.036]
	(0.001)		(0.007)		(407)		(0.007)	
Year 0	0.500***	[0.012]	-0.015*	[0.224]	-824**	[4,574]	0.005	[0.073]
	(0.004)		(0.008)		(380)		(0.008)	
Year 1	0.917***	[0.043]	-0.073***	[0.244]	-2,984***	[6,000]	-0.036***	[0.069]
	(0.005)		(0.009)		(514)		(0.009)	
Year 2	0.339***	[0.065]	-0.020**	[0.228]	-2,240***	[7,275]	-0.002	[0.065]
	(0.007)		(0.009)		(632)		(0.008)	
Year 3	0.125***	[0.073]	0.014	[0.196]	-1,432**	[6,913]	-0.006	[0.066]
	(0.007)		(0.010)		(595)		(0.009)	
Year 4	0.055***	[0.069]	0.018*	[0.182]	-616	[6,055]	-0.009	[0.067]
	(0.007)		(0.011)		(664)		(0.009)	
Year 5	0.009	[0.078]	0.017	[0.173]	-695	[5,926]	0.004	[0.050]
	(0.007)		(0.011)		(772)		(0.008)	
Year 6	0.015**	[0.071]	0.024**	[0.156]	-211	[5,166]	-0.008	[0.058]
	(0.006)		(0.011)		(704)		(0.008)	
Year 7	0.016**	[0.067]	0.013	[0.159]	-309	[5,019]	-0.003	[0.047]
	(0.006)		(0.011)		(692)		(0.008)	
Year 8	0.018***	[0.062]	0.023*	[0.147]	-190	[5,001]	-0.000	[0.044]
	(0.006)		(0.012)		(595)		(0.008)	
Year 9	0.015**	[0.060]	0.026**	[0.140]	502	[4,547]	-0.009	[0.049]
	(0.006)		(0.011)		(520)		(0.008)	
Year 10	0.019***	[0.053]	0.013	[0.151]	149	[5,213]	-0.010	[0.047]
	(0.006)		(0.011)		(556)		(0.008)	
Year 11	0.006	[0.063]	0.011	[0.149]	-743	[6,214]	-0.012	[0.045]
	(0.006)		(0.012)		(660)		(0.008)	
Year 12	0.003	[0.061]	0.013	[0.144]	-806	[6,500]	-0.006	[0.033]
	(0.007)		(0.012)		(747)		(0.008)	
$E[Y_{-1}\|D=1]$	0.001		0.241		6,025		0.074	
p-value joint F-test years -5 to -1	0.928		0.366		0.380		0.763	
Total obs.	392,148		242,604		242,604		341,640	
N cases	21,786		13,478		13,478		18,980	

Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. “Year” represents the year relative to the date of sentencing interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D = 1]$ in each year are shown in square brackets. $E[Y_{-1}|D = 1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2007 for labor market outcomes and 2001-2009 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.4: Main specification: labor market outcomes for those with steady employment prior to sentencing

	Diff-in-Diff Estimates			
	Employed		Earnings	
	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$
Year -5	0.003	[0.829]	-2,608	[29,239]
	(0.038)		(2571)	
Year -4	0.003	[0.915]	-643	[33,493]
	(0.025)		(2948)	
Year -3	0.015	[0.973]	522	[37,271]
	(0.013)		(2589)	
Year -2	0.000	[1.000]	-201	[43,324]
	(.)		(1620)	
Year 0	-0.014	[0.889]	-2,735	[24,072]
	(0.026)		(1985)	
Year 1	-0.270***	[0.905]	-14,133***	[28,135]
	(0.026)		(2879)	
Year 2	-0.122***	[0.872]	-10,119***	[32,203]
	(0.031)		(3197)	
Year 3	-0.003	[0.759]	-7,662**	[31,246]
	(0.040)		(3318)	
Year 4	-0.010	[0.741]	-4,523	[26,787]
	(0.043)		(2797)	
Year 5	0.007	[0.702]	-1,773	[23,944]
	(0.043)		(2741)	
Year 6	0.028	[0.640]	-3,019	[24,002]
	(0.044)		(2598)	
Year 7	-0.020	[0.658]	-4,420*	[23,749]
	(0.041)		(2618)	
Year 8	0.023	[0.597]	-2,900	[23,024]
	(0.046)		(2820)	
Year 9	-0.002	[0.606]	-1,692	[22,246]
	(0.046)		(2812)	
Year 10	-0.072*	[0.661]	-1,130	[23,051]
	(0.042)		(2675)	
Year 11	-0.023	[0.607]	-2,056	[24,085]
	(0.045)		(2805)	
Year 12	-0.022	[0.589]	-2,032	[25,116]
	(0.045)		(2920)	
$E[Y_{-1}\|D=1]$	1.000		37,829	
p-value joint F-test	0.664		0.556	
years -5 to -1				
Total obs.	28,296		28,296	
N cases	1,572		1,572	

Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. The sample includes only those defendants with steady employment pre-sentencing. "Steady employment" is defined as being employed for at least 7 quarters out of 8 between the 3rd year and 1.25 years pre-sentencing. "Year" represents the year relative to the date of sentencing, interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D=1]$ in each year are shown in square brackets. $E[Y_{-1}|D=1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9), were at least 23 years old at sentencing, and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.5: Effects of longer prison sentences (4-7 years) on labor market outcomes and recidivism

	Diff-in-Diff Estimates							
	Incarcerated		Employed		Earnings		Recent felony charge	
	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$
Year -5	-0.003	[0.004]	-0.007	[0.202]	-253	[4,314]	-0.019	[0.040]
	(0.002)		(0.024)		(813)		(0.015)	
Year -4	-0.002	[0.003]	-0.035	[0.258]	267	[5,018]	-0.019	[0.040]
	(0.002)		(0.023)		(886)		(0.015)	
Year -3	-0.001	[0.003]	-0.026	[0.254]	-647	[5,592]	-0.033**	[0.061]
	(0.002)		(0.022)		(653)		(0.015)	
Year -2	-0.002	[0.004]	-0.020	[0.251]	63	[5,497]	-0.038**	[0.074]
	(0.002)		(0.021)		(689)		(0.017)	
Year 0	0.501***	[0.012]	-0.058***	[0.241]	-1,223	[3,342]	0.010	[0.084]
	(0.007)		(0.022)		(900)		(0.017)	
Year 1	0.956***	[0.043]	-0.225***	[0.250]	-3,846***	[4,264]	-0.077***	[0.086]
	(0.010)		(0.026)		(1022)		(0.017)	
Year 2	0.932***	[0.067]	-0.207***	[0.231]	-4,044***	[4,598]	-0.062***	[0.069]
	(0.013)		(0.017)		(874)		(0.015)	
Year 3	0.922***	[0.077]	-0.180***	[0.211]	-4,180***	[4,952]	-0.090***	[0.096]
	(0.015)		(0.022)		(939)		(0.020)	
Year 4	0.921***	[0.078]	-0.106***	[0.209]	-3,060***	[4,395]	-0.065***	[0.075]
	(0.016)		(0.022)		(881)		(0.015)	
Year 5	0.887***	[0.080]	-0.021	[0.197]	-1,146	[4,152]	-0.019	[0.061]
	(0.016)		(0.016)		(969)		(0.014)	
Year 6	0.325***	[0.076]	-0.002	[0.195]	660	[3,363]	-0.035**	[0.084]
	(0.019)		(0.020)		(918)		(0.017)	
Year 7	0.117***	[0.079]	0.009	[0.174]	1,137	[2,934]	-0.012	[0.056]
	(0.017)		(0.023)		(979)		(0.015)	
Year 8	0.014	[0.069]	0.021	[0.160]	2,196**	[2,628]	-0.014	[0.054]
	(0.015)		(0.027)		(1087)		(0.015)	
Year 9	0.028*	[0.059]	0.031	[0.133]	2,169**	[2,292]	-0.016	[0.055]
	(0.015)		(0.027)		(1099)		(0.015)	
Year 10	0.021	[0.063]	0.011	[0.161]	1,819	[3,008]	-0.045***	[0.072]
	(0.015)		(0.027)		(1107)		(0.017)	
Year 11	-0.001	[0.081]	0.003	[0.160]	2,038*	[3,235]	-0.028*	[0.062]
	(0.017)		(0.027)		(1069)		(0.016)	
Year 12	0.000	[0.078]	0.016	[0.147]	1,898*	[3,508]	-0.019	[0.049]
	(0.017)		(0.025)		(1069)		(0.015)	
$E[Y_{-1}\|D=1]$	0.002		0.223		4,655		0.094	
p-value joint F-test years -5 to -1	0.357		0.582		0.220		0.039	
Total obs.	518,436		322,938		322,938		445,086	
N cases	28,802		17,941		17,941		24,727	

Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score. The treatment group comprises those who received a 4-7 year prison sentence, while the comparison group comprises those who were not incarcerated, as described in Section 5.2. "Year" represents the year relative to the date of sentencing, interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D = 1]$ in each year are shown in square brackets. $E[Y_{-1}|D = 1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2007 for labor market outcomes and 2001-2009 for all other outcomes. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.6: Main specification for defendants with prior felony convictions: effects of incarceration on labor market outcomes, recidivism, assets, and human capital accumulation

	Diff-in-Diff estimates						
	Incarcerated	Employed	Earnings	Recent felony charge	Homeowner	Car owner	Any post-secondary education
Year -5	-0.005	0.010	582	-0.007	0.000	0.009*	0.004
	(0.008)	(0.007)	(684)	(0.009)	(0.003)	(0.005)	(0.003)
	[0.166]	[0.230]	[9,379]	[0.146]	[0.039]	[0.031]	[0.050]
Year -3	-0.001	0.002	-204	-0.018*	0.001	0.004	0.004
	(0.007)	(0.007)	(566)	(0.009)	(0.002)	(0.004)	(0.002)
	[0.179]	[0.239]	[10,471]	[0.160]	[0.048]	[0.058]	[0.065]
Year 0	0.420***	-0.018***	-776*	0.008	0.001	-0.005*	-0.004**
	(0.007)	(0.007)	(442)	(0.009)	(0.001)	(0.003)	(0.002)
	[0.171]	[0.200]	[5,647]	[0.124]	[0.056]	[0.100]	[0.091]
Year 1	0.796***	-0.113***	-3,636***	-0.064***	0.003	-0.011***	-0.010***
	(0.008)	(0.008)	(546)	(0.008)	(0.002)	(0.003)	(0.003)
	[0.180]	[0.221]	[7,089]	[0.105]	[0.054]	[0.110]	[0.103]
Year 3	0.099***	0.003	-608	-0.018**	0.001	-0.016***	-0.010***
	(0.008)	(0.008)	(709)	(0.008)	(0.003)	(0.005)	(0.003)
	[0.178]	[0.182]	[8,706]	[0.120]	[0.059]	[0.135]	[0.123]
Year 5	-0.021***	0.009	-241	-0.006	-0.002	-0.015***	-0.012***
	(0.008)	(0.008)	(753)	(0.008)	(0.003)	(0.005)	(0.004)
	[0.187]	[0.179]	[9,005]	[0.099]	[0.067]	[0.152]	[0.141]
Year 7	-0.012	0.009	-280	-0.004	-0.003	-0.014**	-0.012***
	(0.008)	(0.008)	(813)	(0.008)	(0.003)	(0.006)	(0.005)
	[0.171]	[0.170]	[9,518]	[0.073]	[0.073]	[0.158]	[0.152]
$E[Y_{-1}\|D=1]$	0.142	0.241	9,153	0.184	0.056	0.087	0.083
p-value joint F-test	0.781	0.326	0.470	0.137	0.752	0.198	0.309
years -5 to -1							
Total obs.	469,117	339,784	339,784	432,171	471,371	186,851	319,888
N cases	58,814	42,473	42,473	54,186	59,097	23,532	39,986

Note: This table presents estimates from a matched difference-in-differences specification, where individuals are matched on offense type and sentencing score, only including defendants who had prior felony convictions. The treatment group comprises those who were incarcerated for four years or less, while the comparison group comprises those who were not incarcerated, as described in Section 4. “Year” represents the year relative to the date of sentencing interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D=1]$ in each year are shown in square brackets. $E[Y_{-1}|D=1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were recommended for prison (worksheet score between 0 and 9) and were at least 23 years old at sentencing. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * p < 0.10, ** p < 0.05, and *** p < 0.01.

Table C.7: Summary statistics: alternative research design

	Comparison group	**Treatment group**	**Difference**
	Mean	Mean	Mean difference
Criminal proceeding			
Months sentence	0.0	12.8	12.8***
Pretrial detention	0.09	0.10	0.01
Fine or restitution	0.35	0.35	0.00
Sentence at least partially suspended	0.98	0.97	-0.01***
Demographics			
Age	36.8	36.2	-0.7***
Black	0.39	0.42	0.03***
Hispanic	0.01	0.02	0.01***
Female	0.28	0.19	-0.08***
Type of offense			
Robbery	0.02	0.02	0.00
Drug	0.16	0.31	0.15***
Assault	0.15	0.15	0.00
Larceny	0.12	0.14	0.02***
Criminal history			
On probation/parole	0.28	0.28	-0.01
Past misdemeanor convictions	0.9	0.9	-0.0*
Number of cases (2001-2014)	3,518	9,855	
Outcomes year -1			
Home owner (2001-2014)	0.11	0.11	-0.00
Car owner (2010-2014)	0.18	0.16	-0.01
Employment (2003-2012)	0.26	0.25	-0.01
Earnings (2003-2012)	7821	6971	-850***
Any post-secondary education (2005-2013)	0.10	0.10	-0.00***

Note: This table presents summary statistics for matched observations in the treatment and comparison groups for the year prior to sentencing for the alternative research design. We define treatment as those who score right *above* the cutoff for a prison sentence and were incarcerated for four years or less, and comparison as those who score right *below* the cutoff and were not incarcerated, as described in Section 5.5. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. The number of cases for the comparison and treated groups are, respectively: 1,275 vs 3,600 for car ownership; 2,534 vs. 7,094 for yearly earnings and employment; and 2,312 vs. 6,456 for post-secondary education. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions.

Table C.8: Alternative specification: effects of incarceration on labor market outcomes, recidivism, assets and human capital accumulation

	Diff-in-Diff estimates						
	Incarcerated	Employed	Earnings	Recent felony charge	Homeowner	Car owner	Any post-secondary education
Year -5	0.000	-0.007	-584	0.010*	-0.006	0.003	0.002
	(0.000)	(0.008)	(534)	(0.006)	(0.005)	(0.009)	(0.004)
	[0.000]	[0.250]	[7,530]	[0.008]	[0.075]	[0.075]	[0.066]
Year -3	-0.000	-0.001	-586	0.010*	0.000	0.000	0.003
	(0.000)	(0.007)	(465)	(0.006)	(0.004)	(0.007)	(0.003)
	[0.000]	[0.254]	[7,908]	[0.010]	[0.093]	[0.121]	[0.087]
Year 0	0.491***	-0.020***	-353	0.001	-0.004*	-0.009*	-0.001
	(0.003)	(0.007)	(348)	(0.008)	(0.003)	(0.005)	(0.002)
	[0.011]	[0.238]	[4,538]	[0.064]	[0.113]	[0.188]	[0.107]
Year 1	0.879***	-0.041***	-1,723***	-0.038***	-0.008**	-0.019***	-0.005*
	(0.005)	(0.008)	(462)	(0.008)	(0.003)	(0.006)	(0.003)
	[0.061]	[0.220]	[4,768]	[0.072]	[0.114]	[0.208]	[0.116]
Year 3	0.073***	0.009	-53	0.004	-0.015***	-0.026***	-0.005
	(0.006)	(0.008)	(510)	(0.007)	(0.005)	(0.009)	(0.004)
	[0.071]	[0.198]	[5,290]	[0.043]	[0.123]	[0.247]	[0.125]
Year 5	0.010*	0.012	632	0.007	-0.018***	-0.034***	-0.009**
	(0.005)	(0.008)	(496)	(0.007)	(0.005)	(0.010)	(0.004)
	[0.063]	[0.179]	[4,673]	[0.038]	[0.134]	[0.280]	[0.137]
Year 7	0.016***	0.004	434	0.006	-0.014**	-0.034***	-0.010**
	(0.005)	(0.009)	(522)	(0.007)	(0.006)	(0.011)	(0.005)
	[0.053]	[0.174]	[4,722]	[0.025]	[0.141]	[0.288]	[0.145]
$E[Y_{-1}\|D=1]$	0.000	0.248	6,647	0.056	0.109	0.162	0.102
p-value joint F-test	0.545	0.609	0.436	0.212	0.252	0.888	0.572
years -5 to -1							
Total obs.	105,980	77,024	77,024	92,534	106,724	38,740	70,144
N cases	13,280	9,628	9,628	11,596	13,373	4,875	8,768

Note: This table presents the estimates from the difference-in-difference for the alternative research design. We define treatment as those who score right *above* the cutoff for a prison sentence and were incarcerated for four years or less, and comparison as those who score right *below* the cutoff and were not incarcerated, as described in Section 5.5. "Year" represents the year relative to the date of sentencing interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D=1]$ in each year are shown in square brackets. $E[Y_{-1}|D=1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2012 for labor market outcomes, 2010-2014 and 2005-2013 for car ownership and post-secondary education respectively, and 2001-2014 for all other outcomes. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Significance levels are indicated by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.9: Alternative research design: the impacts of incarceration on labor market outcomes and recidivism, up to twelve years post-sentencing

	Diff-in-Diff Estimates							
	Incarcerated		Employed		Earnings		Recent felony charge	
	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$	DiD	$E[Y_t(0)\|D=1]$
Year -5	0.000	[0.000]	0.022	[0.190]	374	[5,017]	0.006	[0.009]
	(0.000)		(0.019)		(767)		(0.011)	
Year -4	0.000	[0.000]	0.029*	[0.199]	804	[5,472]	0.006	[0.009]
	(0.000)		(0.017)		(794)		(0.011)	
Year -3	0.000	[0.000]	0.036**	[0.194]	849	[5,683]	0.010	[0.007]
	(0.000)		(0.016)		(628)		(0.011)	
Year -2	0.000	[0.000]	0.017	[0.214]	534	[6,171]	0.008	[0.019]
	(0.000)		(0.015)		(481)		(0.012)	
Year 0	0.502***	[0.007]	-0.026***	[0.236]	-192	[4,198]	0.021*	[0.046]
	(0.004)		(0.008)		(637)		(0.013)	
Year 1	0.888***	[0.057]	-0.036***	[0.221]	-2,117***	[5,260]	-0.036**	[0.075]
	(0.008)		(0.012)		(767)		(0.014)	
Year 2	0.236***	[0.064]	0.001	[0.205]	-385	[5,238]	0.015	[0.046]
	(0.010)		(0.015)		(808)		(0.013)	
Year 3	0.073***	[0.069]	0.007	[0.195]	139	[5,236]	0.002	[0.047]
	(0.010)		(0.020)		(902)		(0.014)	
Year 4	0.012	[0.080]	0.019	[0.173]	-193	[5,298]	0.001	[0.052]
	(0.011)		(0.018)		(923)		(0.016)	
Year 5	0.014	[0.062]	0.003	[0.177]	-191	[4,991]	0.005	[0.043]
	(0.009)		(0.015)		(938)		(0.014)	
Year 6	0.011	[0.062]	0.004	[0.171]	186	[4,591]	0.010	[0.034]
	(0.010)		(0.019)		(961)		(0.013)	
Year 7	0.011	[0.062]	-0.013	[0.179]	-162	[4,811]	0.002	[0.036]
	(0.009)		(0.019)		(1039)		(0.014)	
Year 8	0.016*	[0.052]	-0.007	[0.174]	29	[4,783]	0.006	[0.032]
	(0.008)		(0.018)		(968)		(0.013)	
Year 9	0.006	[0.055]	0.006	[0.157]	500	[4,525]	0.007	[0.028]
	(0.009)		(0.017)		(1007)		(0.013)	
Year 10	0.014*	[0.046]	0.017	[0.141]	230	[4,978]	0.012	[0.021]
	(0.008)		(0.020)		(1122)		(0.012)	
Year 11	0.010	[0.044]	0.030	[0.124]	1,415	[3,976]	0.013	[0.019]
	(0.007)		(0.021)		(986)		(0.012)	
Year 12	0.007	[0.044]	0.038*	[0.119]	739	[4,860]	0.007	[0.013]
	(0.007)		(0.021)		(1102)		(0.012)	
$E[Y_{-1}\|D=1]$	0.000		0.249		6,204		0.056	
p-value joint F-test	0.433		0.212		0.570		0.811	
years -5 to -1								
Total obs.	132,714		83,052		83,052		113,994	
N cases	7,373		4,614		4,614		6,333	

Note: This table presents the estimates from the difference-in-difference for the alternative research design. We define treatment as those who score right *above* the cutoff for a prison sentence and were incarcerated for four years or less, and comparison as those who score right *below* the cutoff and were not incarcerated, as described in Section 5.5. "Year" represents the year relative to the date of sentencing interacted with treatment. Estimates of counterfactual means for the treated group $E[Y_t(0)|D = 1]$ in each year are shown in square brackets. $E[Y_{-1}|D = 1]$ denotes the mean outcome in the treatment group in the year before sentencing. The sample is restricted to individuals sentenced in 2003-2007 for labor market outcomes and 2001-2009 for all other outcomes. We include defendants who were at least 23 years old at sentencing and had no prior felony convictions. Standard errors are clustered at the individual level and are reported in parentheses. Significance levels are indicated by * p < 0.10, ** p < 0.05, and *** p < 0.01.