Revisiting the Lasting Effects of Incarceration*

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Abstract

Using administrative data from Virginia spanning 25 years, we provide new evidence on how incarceration affects the two primary channels through which low-income households build wealth: ownership of assets like homes and cars, and human capital formation. To identify causal effects, we employ matched difference-in-differences designs, leveraging Virginia's sentence guidelines scoring system. Consistent with the literature, we document that incarceration leads to temporary declines in employment, wages, and recidivism that dissipate within 3 years. However, this temporary labor market disruption is accompanied by persistent reductions in asset accumulation. Seven years after sentencing, incarceration reduces home ownership by 1.4pp and car ownership by 3.0pp (15% and 20% declines relative to baseline). We also document that incarceration lowers human capital formation, reducing college enrollment by 1.2pp (13%). Our findings suggest that while incarceration may not permanently alter recidivism or labor market outcomes, its disruption has lasting consequences.

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1 Introduction

In the United States, almost two million people are currently incarcerated, with 3% of adults and 15% of Black men having spent time in prison (Walmsley, 2023; Shannon et al., 2017). The high levels of incarceration have prompted research on its consequences, with particular attention to recidivism, and more recently, earnings, employment, and other socioeconomic outcomes. Most quasi-experimental studies on recidivism find short-term decreases, consistent with incapacitation, but no long-term reductions in offending (Loeffler and Nagin, 2022). The few quasi-experimental papers studying economic outcomes find similar results: incarceration reduces employment and earnings during confinement, but these effects quickly dissipate after release, with limited evidence of lasting effects (Kling, 2006; Loeffler, 2013; Harding et al., 2018; Garin et al., 2024).[1]

These findings could be interpreted to suggest that incarceration's impacts are predominantly short-term and concentrated during the period of confinement itself. However, the literature has focused almost exclusively on flow variables—such as criminal behavior, employment, and earnings—rather than stock variables like asset ownership or human capital accumulation. Even if incarceration doesn't permanently change criminal behavior or labor market outcomes, the disruption it causes could affect processes of asset accumulation building and educational advancement in ways that have lasting consequences for the wealth of the incarcerated and their families.

In this paper, we revisit the question of incarceration's lasting effects by examining its impact on homeownership, vehicle ownership, and postsecondary education. Using administrative panel data from Virginia, we match 25 years of felony conviction records to data on employment, wages, homeownership, car ownership, and college enrollment. In our primary specification, we conduct a matched difference-in-differences design among those who are recommended for prison based on Virginia's sentence guidelines scoring system. We first use exact matching on sentence score (an index of offense severity and criminal record) and offense type to compare similar individuals who do and do not receive a carceral sentence. We then use this matched sample in a standard difference-in-differences framework. This approach allows us to compare incarcerated individuals to defendants with similar characteristics who were not incarcerated, while still allowing for persistent differences in potential outcomes between the groups when not convicted.

Our analysis yields three main findings. First, we confirm patterns documented in prior literature: incarceration causes temporary declines in employment, wages, and recidivism that largely disappear within 3 years of sentencing. These results are

[1] Mueller-Smith (2015) and Aizer and Doyle (2015) are notable exceptions, finding more persistent negative effects on labor market outcomes for adults and children, respectively.

consistent with prior quasi-experimental work (Loeffler, 2013; Garin et al., 2024), finding incapacitation effect of imprisonment but little evidence of lasting "scarring" on labor market outcomes. However, the temporary disruption to income has cumulative effects—those who were steadily employed before sentencing lost approximately $20,000 in earnings during their period of incarceration.

The prior literature on recidivism and employment has generally only been able to look at the impacts of relatively short incarceration sentences. This begs the question of whether the long-term null effects can be attributed to the low 'dosage' of prison time. One advantage of our research approach is that it allows us to evaluate the impacts of longer carceral sentences. In an alternative specification, we define treatment as being sentenced to three to seven years in prison and use a matched difference-in-differences design to look at outcomes up to 10 years post-sentencing period. Even with these longer prison sentences, we find no evidence that incarceration has lasting scarring effects on labor market and recidivism outcomes vs conviction with no incarceration.

Second, we find that incarceration has substantial and persistent effects on asset ownership. Seven years after sentencing, incarceration reduces homeownership by 1.4 percentage points (a 15% decline relative to the pre-sentencing average of 9%) and car ownership by 3.0 percentage points (a 20% decline relative to baseline).[2] These effects develop gradually and persist 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 0.7 percentage points for non-owners. The reduction in homeownership may be particularly consequential because housing equity is the primary way most Americans build wealth.

Third, incarceration reduces human capital accumulation, with college enrollment declining by 1.2 percentage points (a 13% decline relative to baseline). While these percentage point changes may appear modest, they represent substantial relative declines given the low baseline rates in our population. Moreover, education is a key determinant of long-term earnings potential, suggesting that these effects could have lasting consequences for economic mobility, at least among the affected subgroup.

Our findings are robust to our choice of research design. In an alternative difference-in-differences (DD) specification, we utilize discontinuities in sentence recommendations to identify alternative treatment and control groups. We compare individuals just above the incarceration threshold (who received an average sentence of 16 months) to

[2]A limitation of our study is that the car ownership measure likely underestimates the true level of car ownership, which could lead to underestimation of the baseline mean and overestimation of the relative effect. On the other hand, our measure picks up car ownership at a point in time and does not track sales, making car ownership an absorbing state, and potentially leading our estimates to overstate the level and understate impact relative to baseline. These data limitations should not affect (absolute) treatment effects as long as they do not differentially impact treatment and comparison groups.

similar individuals just below the threshold who avoided incarceration. This approach yields very similar results, as do DD specifications using propensity score matching on a larger set of covariates.

Our findings—temporary effects on earnings and recidivism, but lasting effects on durable investments such as homes, cars, and human capital—are consistent with incarceration creating a temporary but significant disruption to economic life that has lasting economic and financial consequences. During incarceration, individuals cannot work in the regular labor market, making it difficult to maintain mortgage payments or save for down payments. They may need to sell assets to cover expenses while incarcerated or to avoid default. After release, the need to secure basic necessities like housing and employment may delay or prevent investments in higher education or asset purchases. Importantly, these mechanisms do not require permanent behavioral changes or labor market "scarring" to generate lasting effects on economic well-being.

Our paper makes several contributions to the existing literature. First, our paper revisits the lasting effects of incarceration, by providing the first quasi-experimental evidence on the effects of incarceration on asset ownership, thus going beyond existing studies on recidivism or labor market outcomes (Kling, 2006; Hjalmarsson, 2009; Aizer and Doyle, 2015; Kuziemko, 2013; Loeffler, 2013; Mueller-Smith, 2015; Gupta et al., 2016; Leslie and Pope, 2017; Estelle and Phillips, 2018; Harding et al., 2018; Dobbie et al., 2018; Franco et al., 2020; Bhuller et al., 2020; Rose and Shem-Tov, 2021; Jordan et al., 2022; Garin et al., 2022).[3] As for the empirical literature on incarceration and wealth, it has mostly relied on survey data (Sykes and Maroto, 2016; Maroto and Sykes, 2019; Zaw et al., 2016). Using both across- and within-person designs, these studies have consistently found that incarceration is detrimental to wealth accumulation. There is suggestive evidence that this comes both through lost earnings and interruption in family formation (Maroto and Sykes, 2019). Other studies provide indirect information about how incarceration affects wealth. Avenancio-Leon and Aneja (2021) finds that incarceration lowers *conventional* credit scores, while Humphries et al. (2024b) find that incarceration leads to a slight increase in *alternative* credit scores.[4] Since mortgage approval is based off of conventional credit scores, incarceration's impact on homeownership could be partially mediated by a credit channel. Another recent study, Slutzky and Xiu (2023), finds that pretrial detention increases foreclosure and bankruptcy rates. Foreclosures could contribute to the effects we find.

Beyond incarceration, our study contributes to the literature on wealth inequalities by examining the causal link between incarceration and asset ownership using a

[3]Recent papers have also considered the impacts of incarceration on outcomes like health (Hjalmarsson and Lindquist, 2022; Bhuller et al., 2025) or children's outcomes (Arteaga, 2021; Norris et al., 2021).

[4]Conventional credit scores are based on debt and repayment rates. Alternative credit scores, which are designed for the subprime market, are based on public records.

quasi-experimental design and administrative data. Since owning a home is the main component of wealth among low-income populations, inequalities in home ownership contribute to inequalities in wealth. One recent study found that increased differences in homeownership rates are responsible for most of the increase in the White-Black wealth gap since 1980 (LaBriola, 2024), and recent work demonstrates that housing wealth is a critical mechanism for intergenerational mobility (Daysal et al., 2023; Derenoncourt et al., 2023; Benetton et al., 2024; Binder et al., 2024). Our findings imply that differential exposure to incarceration could contribute to inequalities in wealth accumulation.

Our paper proceeds as follows. In Section 2 we describe our setting and data. In Section 3, we describe our research design. In Section 4 we show our main results, discuss potential confounds and provide robustness tests. Section 5 concludes.

2 Background

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 guidelines-recommended sentence is calculated using a series of worksheets. A person accumulates points on each worksheet according to the severity of the offense and details of the criminal record. The guidelines provide recommendations for sentence length based on an individual's score. These scores can, therefore, be considered an good summary of the covariates that legal decision-makers are likely to prioritize.

Although judges are free to deviate from the guidelines, they appear influential. 80% of sentences are within the guidelines-recommended range. If the judge chooses to deviate from this range they must provide a written explanation. In more recent years, their deviation rates have been a part of the reappointment review process.

As in many jurisdictions, plea negotiation is common and jury trials are rare. 85% of felony convictions come via 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 winding up in Circuit Court (the court where felonies are adjudicated). Then, even if the person 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 people incarcerated in Virginia work 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 often are put on probation. This entails a suspended sentence and/or certain requirements, like meeting with a probation officer. If someone fails to abide by the terms of probation or commits a new crime, probation can be revoked and the person may be required to serve out the remainder of the sentence in jail or prison. Those sentenced to incarceration are frequently also assigned a period of post-release supervision, which operates similarly to probation. Discretionary parole is banned in Virginia and prisoners must serve at least 85% of their sentence.

Appendix Section A.1 compares Virginia's criminal justice system to both national averages and to states considered in other recent studies of the impacts of incarceration. Virginia is similar in terms of incarceration and probation rates, and has similar racial and ethnic composition of its incarcerated population. However, parole is more common in other states.

2.2 Data and sample

In this subsection, we describe our data, matching process, and samples. More details about the matching can be found in Appendix A.2.

VCSC data: We obtain data from the Virginia Criminal Sentencing Commission (VCSC) on everyone convicted of a felony in Virginia from 1995 through 2019. This data 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. This data source provides race and gender. It also provides our recidivism measure, new criminal convictions in Circuit Court. This includes both felony convictions as well as misdemeanor convictions that have been reduced from felony charges. We obtain data on misdemeanor sentences and pretrial detention from Virginia's District Courts, using the same matching variables. District court data is available from 2010-2021.

Employment data: We obtain formal sector wage 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. We match using name and last four digits of the Social Security number. This data is at the quarterly level from 1999 through 2019. We were able to match 36% of our sample to VEC; 33% of our sample had at least one quarter of W-2 earnings before the sentencing date. We use the Consumer Price Index to adjust wages so that they are in 2023 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. We match using name, birthdate and last four digits of the Social Security number. This data is at the quarterly level from the second quarter of 2000 to the first quarter of 2020. We were able to match 14% of our sample to SCHEV; 10% of our sample had at least one college credit before the sentencing date.

Data aggregator: We obtain data on real property ownership, car ownership and criminal convictions from a commercial data aggregator.[5] This company collects public records data from over 3000 counties, boroughs, and parishes in the United States, representing over 98% of the adult US population. This data is collected digitally where available, or by a network of collection vendors who digitize hardcopy records. Real property ownership is public record information collected directly from county tax and deed records. We refer to real property ownership as "homeownership" both to avoid jargon and because justice-involved populations are unlikely to own investment properties. In addition to public records, the company also has contracts to acquire data from numerous private companies. Car ownership data comes from an extensive web of car dealerships, oil change companies, and repair shops. A person is labeled

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

as a car owner if they purchase a car from a dealership or bring the car in for an oil change or repair. It is a cumulative variable since it captures car acquisition but not car sales/loss.

We match 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 achieve a 96% match rate. 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. This data is available from 1995 to 2021.

Match quality and measurement error: This research entails matching across multiple data sets. Here we discuss potential measurement error concerns, including those arising from the match process. 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, we expect a reasonably high match quality. Match verification variables provided by the data aggregator also suggest strong matches. 93% of the matches made were an exact match on first name, last name and Social Security number. Only 2% of matches had a Social Security number that was linked to multiple names or names other than what we provided them. We also can 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 use this concordance as a measure of match quality, discussed more in Subsection 4.5.

As for data quality: 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 is relatively complete, there could of course be typos and other inaccuracies in the identifying information.

Given that homeownership is public record information collected directly from county tax and deed records, we expect it to be reasonably high quality. Car ownership data, however, is likely less complete than homeownership data. This could bias our results towards zero.

Classical measurement error in outcomes will increase variance but not bias results. However, we expect our measurement error to extend beyond the classical variety. False matches are effectively measurement errors in the independent variable (e.g. labeling someone as incarcerated when they are not). This type of error biases results towards zero. Incomplete information on outcomes (e.g. someone is falsely labeled as having no

postsecondary education) likewise will bias results towards zero, assuming the errors are orthogonal to sentencing.

Sample: We restrict to those at least 23 years old at the time of sentencing and to those with no prior felony conviction (24.2% of our sample). The age restriction is to ensure that we are able to track time trends for people of the age of legal majority for at least five years before sentencing. The restriction on prior felony convictions is to ensure that both treatment and control experience an identical shift in the likelihood of having a felony conviction at the sentencing. When we do not impose restrictions on prior convictions, it is more likely that individuals receiving non-carceral sentences are also facing their first felony conviction, which could, in turn, impact both crime and economic outcomes (Mueller-Smith and Schnepel, 2021; Humphries et al., 2024a). Our "Full sample" includes people 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. We use the full sample for incarceration, recidivism, and home ownership. For car ownership, which is not well populated until 2005, we use a "Car sample" which includes people sentenced within 2010-2014.[6] For our employment and wage outcomes we use an "Employment sample." Since employment data is available from 1998 through 2019, we limit the sample to those sentenced within 2003-2012. Similarly, we restrict our "College sample" to defendants who were sentenced between 2005 and 2013, in accordance with the years of available data.

3 Research design

Our main research design is a matched difference-in-differences, comparing recidivism, employment and wealth trajectories of people who received a carceral sentence (treatment group) versus a felony conviction with no carceral sentence (control group). We restrict our sample to individuals whose sentence score is high enough that they would be recommended for prison. We matched those who received a carceral sentence to similar individuals that did not, using exact matching on both the offense type and the sentencing score. By making treatment and control groups similar in terms of observed characteristics, our goal is to bolster the parallel trends assumption. The sentencing score, which reflects the severity of both current and prior offenses, can serve as a helpful summary of the characteristics that legal decision-makers consider

[6]The home/car ownership samples include the 96% of our sample that successfully matched with the data aggregator. We restrict incarceration data to that 96% for consistency. The missing rate is slightly higher for recent convictions (11%), since that comes from the data set used in alternative credit scores, and the data is suppressed unless there is a sufficient number of data points to calculate the score. We show that results are robust to different ways of defining the sample in Subsection 4.5.

relevant at sentencing. The criminal record inputs include variables related to prior misdemeanor convictions, sentences, and juvenile adjudications. It also includes prior felony convictions, but these are zero by construction for our main specification.

Since we restrict to those with no prior felony convictions, our stacked difference-in-differences approach avoids the concerns about staggered rollout.

We use a standard dynamic difference-in-differences setup, where λ_i represents individual fixed effects, and ν_t captures time fixed effects, and δ_t captures the dynamic treatment effect with time measured relative to the sentencing date (with $t = -1$ as the omitted period):

$$Y_{i,t} = \lambda_i + \nu_t + \delta_t D_i + U_{i,t}, \tag{1}$$

Different sentences among people with similar offenses and criminal records are likely partly due to exogenous factors, such as the attitudes of judges, prosecutors, or public defenders, all of which generate quasi-random variation in treatment. The different sentences are also likely due in part to differences in observed and unobserved characteristics. Table 1 presents a comparison of various characteristics between the matched treatment and control groups using the full sample at the time of sentencing. The two groups are similar in terms of age, race, criminal record, offense and home-ownership status. The largest difference across the groups is gender: 24% of the control group is female, compared to 17% for control. The control group is also 2 percentage points more likely to own a car or have any post-secondary education, and 4 percentage points less likely to be employed. However, such differences do not necessarily pose a problem in a difference-in-differences design. Time-invariant differences will be absorbed by the inclusion of individual fixed effects.

The key identifying assumption of the difference-in-differences design is that trends in outcomes would have evolved in parallel absent treatment. If there are time-varying shock or differences in trajectories that affect sentencing, the parallel trends assumption would be violated. We discuss this in more depth in Section 4.4.

4 Results

4.1 Exposure to prison

This section shows incarceration over time for the treatment and control groups in order to better understand the dynamics of treatment. First, Table 1 shows that, on average, sentences in the treatment group are 23 months, compared to 0 months for the control group. Figure 1 provides more insights into the dynamics of exposure to incarceration. The top row of Figure 1 shows incarceration on a felony sentence for

the full sample, including sentences received after the focal sentencing date. Data is at the quarterly level and describes the fraction of each quarter the person spends incarcerated. Raw yearly averages are shown on the left-hand column. The purple circles show the likelihood of being incarcerated between 5 years pre- and 7 years post-sentencing for the control group, and the green diamonds show the same for the treatment group. The right-hand column shows the coefficients on the interaction between treatment and time from the difference-in-differences specification in equation (1). Column 1 of Table 2 shows point estimates from the difference-in-differences regressions. As can be seen, our treatment group is substantially more likely to be incarcerated than the control group, particularly in the first couple of years. There is still a small gap in the likelihood of incarceration three years after sentencing, but the gap has entirely disappeared by year five.

Of course, felony sentences are not the only way in which someone can be incarcerated. There is also pretrial detention, misdemeanor sentences, and probation revocation. We have relatively complete information on misdemeanor sentences from 2010 onward and probation revocation from 2006 onward. We also have an indicator for whether someone was in custody at the most recent hearing in District Court. For felonies, this would typically be the probable cause hearing. We thus generate a proxy for pretrial detention that assumes they are detained between the date of arrest and the date of sentencing if they are in custody at the most recent hearing, and assumes they are not detained otherwise. We subtract this length from the overall sentence length if the data indicates credit for time served. This metric of pretrial detention is imperfect, but it's the best we can do with the available data.

With this data we are able to show incarceration over time including not only felony sentences, but also misdemeanor sentences, probation revocations and our pretrial detention proxy. Our sample for this analysis consists of those sentenced between 2012-2014, to give at least two years prior to sentencing and five years afterwards in which all four sources of incarceration are observed. The middle row of Figure 1 is identical to the top row except limited to 2012-2014, and produces very similar results to when we use the full time period. The bottom row shows results using all four sources of incarceration. The differences remain very similar, but are slightly more muted due to somewhat higher rates of other forms of incarceration among the control group.

Lastly, we examine the differences in financial sanctions faced by both groups. Appendix Table A.1 provides data on court-imposed debt and community supervision for the treatment and control groups. Individuals who received carceral sentences are more likely to be subjected to fines, but less likely to be required to pay restitution. Both effects net out, and 35% of both groups were obligated to pay either fines or

restitution. Additionally, recent studies have found no significant long-term effects of fines and fees on economic stability (Lieberman et al., 2023; Finlay et al., 2022). This body of literature, along with the absence of substantial differences in exposure to financial sanctions between the groups, suggests that financial sanctions are unlikely to be a primary mechanism driving our results.

4.2 The impacts of incarceration

Employment and recidivism. Figure 2 follows the same format as Figure 1 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, the second row shows the average quarterly wages, and the third row shows cumulative income starting from year -5. We see that incarceration leads to a sharp drop in employment and wages during the period of incarceration, but by year three these trends returned to what they would have been absent incarceration. Table 2 shows point estimates aggregated into the yearly level. At year 7, we can reject declines in employment greater than 0.08 percentage points relative to a 24% pre-sentencing average employment rate. The cumulative earnings lost due to incarceration are about $11,000. Between employer and employee contributions, 12.4% of W-2 income goes towards Social Security. $11,000 less in earnings translates into approximately $1,300 less in Social Security. As a reference, the median household wealth for those with a current or prior incarcerated member is $7350.[7]

In Appendix Figure A.5, we show employment outcomes for those with steady employment prior to sentencing, defined as being employed in 7 out of 8 quarters between years -3 and -1. Overall we see similar patterns: sharp declines in employment and wages coincident with incarceration but no evidence of long-term effects. The cumulative earnings lost for this group, however, is higher at roughly $20,000. The loss in Social Security is roughly $2,500.

The bottom row of Figure 2 shows the likelihood of being convicted for a crime committed within the previous 12 months (both misdemeanors and felonies) as recorded by the Virginia's Circuit Courts. As can be seen, incarceration leads to a short-term decline in recent convictions, but shows no evidence of longer-term adverse effects. At 7 years post-sentencing, we can reject increases greater than 0.06 percentage points at the 5% level.

Most of the literature on the causal effects of incarceration focuses on shorter sentences. However, the combination of our matched differences-in-differences research design and of the time span of our dataset means that we can also consider the effects

[7]This excludes defined benefit pensions and Social Security.

of longer sentences on employment and recidivism. We conduct analyses comparing individuals who served prison terms of 3-7 years to those who received no incarceration, using the same matching strategy. Within this group, the average incarceration length is 4.3 years, providing a substantial "dosage" for analyzing long-term impacts. A 7-year sentence is at the 94^{th} percentile of positive incarceration sentences; 4.3 years is the 89^{th} percentile. In Figure 3 and Table 3, we show results: compared to those with shorter sentences, the "incapacitation" period is longer and cumulative wage loss is greater. However, we still see no evidence of long-term scarring on labor market or recidivism outcomes up to ten years post sentencing.

Wealth and human capital accumulation. Figure 4 shows how incarceration affects home ownership, car ownership, and college enrollment. The top row shows whether the person owns any real property ("homeownership"), the second row shows car ownership, and the third row shows any enrollment in higher education. The raw data shows that trends in home ownership are almost overlapping for the treatment and control 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.

By seven years after sentencing, incarceration has reduced property ownership rates by 1.4 percentage points over a 9% pre-sentencing mean and it has reduced car ownership rates by 3.0 percentage points over a 15% pre-sentencing mean. Most of this impact accumulates in the first couple of years after sentencing. This could be due to a combination of people selling assets in preparation for prison and people being dissuaded from purchasing assets due to incarceration or its immediate aftermath. Appendix Figures A.6 show incarceration's impact on home ownership for those who do/do not own a home before sentencing.[8] There are relatively few people 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, out of the 85% who owned a home in the year prior to sentencing. Among non-homeowners, incarceration reduces the likelihood of acquiring a house by about 0.7 percentage points. Since our data on asset ownership does not extend beyond 7 years post-sentencing, we would not be able to look at the impact of 3-7 year sentences beyond the incpacitation period. We therefore don't include these outcomes in that specification.

There are several reasons why a prison sentence could lead people to sell assets. First, it may not make sense to keep assets you are not using, particularly if you have

[8]Since car ownership is an absorbing variable, we cannot look at car sales. We note that if incarceration also increases the likelihood cars will be sold or otherwise relinquished, our estimates will understate the results.

monthly payments on them. Although most Virginia prisoners work, they usually earn less than a dollar an hour. Unless someone has extensive savings or others who can take over payments and maintenance, selling may be a necessity. Second, prison is substantially more comfortable if you have some money in the bank. Prisons provide only the bare necessities. Those interested in purchasing postage stamps, envelopes, hygiene items, over-the-counter medication, snacks, cigarettes, etc. must have money in their commissary account. It's also costly for family members, who must bear the expense of collect calls and cross-state travel for visitation. Selling assets may help cushion the costs of incarceration.

Prison may also prevent the acquisition of assets. Logistically and practically, it likely does not make sense to purchase a home or car while incarcerated. It also may be more challenging to purchase assets after release. First, savings may be depleted by commissary expenses, visits, and collect calls. Second, incarceration prevents people from working. We saw that those who had steady employment prior to sentencing lost an average of $20,000 in cumulative income 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 restabilize after release from incarceration. A person needs to find work, find housing and get their feet under themselves before they can focus on purchasing an expensive asset. This is consistent with the timing of the effects: incarceration appears to lower the rate of wealth accumulation even after the incapacitation period is over. Although most people are, on average, released from prison within about a year and a half, the divergence in home and car ownership rates keeps growing until five years after sentencing and then appears to stabilize.[9]

Panels (e) and (f) of Figure 4 show that incarceration also has a lasting effect on postsecondary education. While the control group was more likely to have previously enrolled in college than the treated group, this gap is constant during the pre period, but then widens in the years after sentencing. By seven years post-sentencing, incarceration has led to a 1.2 percentage point decrease in the likelihood of having any college enrollment relative to a 9% pre-sentencing mean. We can't say definitively what causes the continuing divergence in postsecondary education even after release, but it's possible this is because people need to rebuild basic stability before investing in their education. People may prioritize getting an apartment and a job before taking the

[9]It's also possible that incarceration lowered their credit score, as shown in Avenancio-Leon and Aneja (2021). However, this seems less likely to play a role in car ownership. Many of those seeking to purchase a car will have their credit evaluated using an alternative credit score, which is calculated using derogatory public records and address characteristics. These alternative credit scores are particularly common among a justice-involved population, who are less likely to have conventional credit scores. In parallel work, we show that incarceration (relative to conviction without incarceration) leads to a small positive increase in these alternative credit scores, primarily because it prevents people from accumulating more criminal convictions while incarcerated (Humphries et al., 2024b).

time to enroll in vocational training or college.

4.3 Alternative specification: leveraging discontinuities in recommended sentences

In Section 4.2, the treatment and control groups were determined within the same offense and sentencing score. We argued that part of the variation in sentences stemmed from quasi-random variation due to differences in how the judge, district attorney or public defender assigned to the case handled it. In this section, we consider an alternative difference-in-differences design that leverages another source of quasi-random variation: guideline-induced jumps in recommended sentences. Of course, for both research designs, sentencing differences also stem from observed and unobserved characteristics. But in this specification, the sentence differences across treatment and control are transparently linked to large discontinuities in the guidelines-recommended sentence.

For each offense, there is a sentencing score cutoff above which a person is recommended for prison, and below which a person is recommended either for probation, or for a short jail stay. We exploit this discontinuity; again using a differences-in-differences framework, we compare people who happen to have scored just below the cutoff (1 or 2 points below) and received a non-carceral sentence, to people who scored just above the threshold (1 or 2 points above) and received a carceral sentence. Compared to the within-score matching approach, more of the variation in sentences in this approach come from idiosyncrasies of the research setting. Appendix Table 4 compares the treatment and control groups with this research design. Prison sentences are on average 16 months for the treatment group (compared to 22 months in the first research design), as shown in Table A.2. There are also differences this time in offenses: people in the treatment group are more likely to have been convicted of drug crimes.

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 5; Appendix Figures A.7 and A.8 show results for the other outcomes we consider. We see that incarceration leads to a sharp drop in employment during the time of incarceration, but by year three these trends returned to what they would have been absent incarceration. At year 7, we can reject declines in employment greater than 1.0 percentage points relative to a 25% pre-sentencing average employment rate. By contrast, as in our main specification, we find lasting effects of incarceration on wealth accumulation. By seven years after sentencing, incarceration has reduced property ownership rates by 1.6 percentage points relative to a 11% pre-sentencing mean, car ownership rates by 3.5 percentage points

relative to a 17% pre-sentencing mean, and post-secondary education by 1 percentage point relative to a 10% pre-sentencing mean..

4.4 Considering potential confounds for incarceration results

While we vary how we identify the treatment and control groups, Sections 4.2 and 4.3 both use differences-in-differences. Identification in a difference-in-differences design is based off of the assumption that counterfactual outcomes for the treatment group would have followed a parallel trend to observed outcomes for the control group, absent incarceration. If the treatment and control groups have different characteristics, this is not necessarily a problem, although it can help inform thinking about how plausible the parallel trends assumption is.

In both research designs, the treatment group was incarcerated while those in the control group were not. Accordingly, there are likely unobserved differences across the treatment and control group. Some of these differences are quasi-random and not likely to influence future wealth, as the judge, district attorney or public defender assigned to the case, or the exact sentence score that a person got (for results in Section 4.3). Other differences are more likely to be endogenous. The incarcerated group may be less able to afford a skilled private attorney, for instance, or less likely to have supportive family members showing up at the hearings.

Such endogenous differences between the treatment and control group do not necessarily pose a problem for the research design, since they would be absorbed by individual fixed effects. The two main threat to identification are (1) if the treatment group experienced some sort of adverse shock, occurring around the same time as sentencing, that led to lower rates of wealth and human capital accumulation or (2) that the treatment and control groups were on different trajectories and would have remained on different trajectories absent treatment. We cannot rule either concern out, but several factors help limit concerns.

First, we are comparing two groups who are either identical (Section 4.2) or fairly similar in terms of crime severity and criminal record (Section 4.3), suggesting that their behavior up until the time of sentencing was fairly similar. To the extent that there were some differences in offense or criminal record, we can adjust for this by combining difference-in-differences with richer propensity-score matching. We do so in a robustness test described below and find similar results.

Second, incarceration did not cause lasting adverse effects on employment, wages or recidivism relative to conviction without incarceration. The short-term declines for these outcomes are timed exactly with the carceral sentence and, once the period of

incapacitation ended, trends went back to match the control group. If the long-term declines in home/car ownership and postsecondary education were caused by some adverse life shock coincident with sentencing, we would have expected this shock to lead to long-term declines in employment/wages or increases in recidivism as well. The patterns we observe—short-term declines in flow variables like employment but long-term declines in the accumulation of stock goods—are consistent with a large but temporary disruption to economic activity. This limits the scope of plausible confounding events.

Finally, concerns about an adverse shock causing both crime and diverging outcomes are hard to reconcile with the timing. The data shows no evidence of diverging trends up to one year before sentencing. Therefore, this hypothetical adverse shock would have had to have occurred relatively close to the sentencing date. Given that there is often a significant lag between the date of the offense and the date of sentencing, the adverse life event would have to have led fairly rapidly to both crime and conviction.

This scenario—a sudden life shock leading someone to commit a crime for which they are immediately arrested and convicted—is not a particularly common one according to the criminology literature. Adverse life events can certainly influence criminal activity, but people are rarely caught and convicted immediately. By the time a person receives the first felony conviction, they have usually been committing crime for an extended period of time. It is certainly possible that an adverse life shock leads immediately to crime and then conviction, but it is not the modal story.

In our sample, 32% had an offense date more than a year prior to the date of sentencing and 50% had an offense date more than nine months prior to sentencing. If an adverse life event caused both crime and a decrease in home or car ownership, we might expect to see a decline beginning in year -1 or earlier for these subgroups. Figure A.9 shows results conditional on the gap between the offense and sentencing being greater than 270 and 365 days, respectively. Among these individuals, such a hypothetical adverse event would have occurred early enough that it may be captured by one of the pre-period coefficients, yet we still don't see evidence of diverging pre-trends.

4.5 Additional robustness tests and heterogeneity

Measurement error due to cross-data match problems. The data provided by the data aggregator includes the date of the most recent felony conviction. By comparing felony conviction dates in the aggregator data to felony conviction dates in the sentencing commission data, we are able to build a metric for match verification. We define two levels of verification: strict and lenient. The strict verification limits to people whose date of conviction in the sentencing commission data is within two

months of a felony conviction as recorded by the data aggregator (most are exact). The lenient verification 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. Working with court records can be tricky: a case can entail multiple charges, both misdemeanors and felonies, that resolve at different dates. Given this inherent messiness, we consider the lenient verification standard to still provide fairly compelling evidence of a correct match. The strict match-verification rate is 72.16% (78.06% among the non-missing judicial circuits) and 88.17% for the lenient match verification (91% among non-missing judicial circuits). We compare results for those who satisfy the strict and lenient match-verification test against the main specification in Figure A.10. Again, results are almost identical across the three groups.

Changes in household-level assets. Next, we expand our analysis to consider not only the individual convicted of a felony but also their household members. This broader approach serves two purposes. First, we explore whether households may adjust their asset holdings—either to avoid asset forfeiture or to secure more favorable loans—in response to a carceral sentence. If such reallocation occurs, we would expect no significant effects of incarceration on home and car ownership at the household level. Second, this analysis offers a more complete picture of how incarceration affects not just the individual, but also their family members. The results, shown in Appendix Figure A.11, align closely with our main findings, suggesting that households do not absorb the long-term effects of incarceration, whether through strategic adjustments or other means.

Heterogeneity across race and criminal history. In Appendix Figures A.12 and A.13, we explore heterogeneity across race and across prior felony convictions. Since almost everyone in our sample is labeled Black or White, we restrict to these two groups in the race analysis. Generally, we see similar patterns across the various subgroups although the magnitude of the effects varies. Standard errors are too large to make definitive statements about differences, but incarceration may have a larger effect on home ownership for White people. This is possibly due to the fact that Black people have lower homeownership rates overall due to a variety of barriers—including, perhaps, the impacts of incarceration on prior generations (Myers, 2004; Bayer et al., 2017). Those with a criminal record experience smaller incapacitation declines in employment or wages, perhaps because they were less likely to be employed at the start. They also experience lower declines in homeownership. Otherwise, results are fairly similar.

5 Conclusion

This paper provides new evidence on how incarceration affects economic well-being on the short and long run. We first show that incarceration's impact on employment, wages, and recidivism is predominantly short-term and consistent with incapacitation. This holds true even for longer sentences. By year 7, the impacts on these outcomes is statistically indistinguishable from zero and we can reject even small adverse effects. This is consistent with most prior studies.

However, incarceration still has lasting impacts. Seven years after sentencing, those who were incarcerated are substantially less likely to own a home or car and have fewer college education.

Overall, our findings echo previous studies showing that incarceration's main impacts stem from incapacitation. However, this does not mean that it only has short-term impacts. The temporary disruption of incarceration slows down the accumulation of assets like home ownership, car ownership, and human capital.

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Table 1: Comparing treatment and control

	Control group	**Treatment group**	**Difference**
	Mean	Mean	Mean difference
Months sentence	0.0	22.9	22.9***
Demographics			
Age	36.1	36.0	-0.1
Black	0.47	0.46	-0.00
Hispanic	0.01	0.02	0.01***
Female	0.24	0.17	-0.08***
Type of offense			
Robbery	0.04	0.04	-0.00
Drug	0.33	0.33	-0.00
Assault	0.15	0.15	0.00
Larceny	0.09	0.09	-0.00
Criminal history			
On probation/parole	0.34	0.36	0.01
Past misdemeanor convictions	0.9	0.9	-0.0***
Outcomes year -1			
Car owner	0.17	0.15	-0.02*
Home owner	0.09	0.09	0.00
Any post-secondary education	0.12	0.09	-0.02***
Employment	0.28	0.24	-0.04***
Yearly wage	7088	6440	-648**

Note: This table presents summary statistics for matched observations in the treatment and control groups for the year prior to sentencing. The sample is restricted to individuals recommended for prison (worksheet score between 0 and 9). The treatment group comprises those who were incarcerated, while the control group comprises those who were not incarcerated. Yearly wage ,employment, car ownership, homeownership, and any post-secondary education are measured in the year prior to sentencing. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. The matched sample is drawn from individuals sentenced to prison between 2001 and 2014 who were at least 23 years old and had no prior felony convictions. It consists of 4,936 observations in the control group and 36,263 in the treatment group.

Table 2: Effects of incarceration on recidivism, labor market, assets and human capital accumulation

	(1) Incarcerated	(2) Employed	(3) Wage	(4) Cumulative earnings	(5) Recent conviction	(6) Homeowner	(7) Car owner	(8) Any post-secondary education
Year -5 x treat	-0.000	0.007	-216.722	2625.728	-0.008***	-0.000	0.010	0.002
	(0.000)	(0.008)	(395.651)	(1678.548)	(0.002)	(0.005)	(0.009)	(0.004)
Year -3 x treat	-0.000	0.000	-732.324	807.922	-0.007**	-0.002	0.003	0.001
	(0.000)	(0.007)	(474.708)	(829.906)	(0.002)	(0.008)	(0.007)	(0.003)
Year 0 x treat	1.000***	-0.018**	-832.203**	-1163.939***	-0.009***	-0.006**	-0.006	-0.006**
	(0.000)	(0.007)	(304.475)	(299.507)	(0.002)	(0.002)	(0.005)	(0.002)
Year 1 x treat	0.674***	-0.091***	-2815.675***	-4311.349***	-0.043***	-0.004	-0.014*	-0.011***
	(0.003)	(0.008)	(416.231)	(622.887)	(0.005)	(0.003)	(0.006)	(0.003)
Year 3 x treat	0.178***	-0.001	-1243.173**	-8325.867***	-0.015**	-0.008*	-0.026*	-0.012**
	(0.002)	(0.008)	(442.568)	(1393.079)	(0.005)	(0.004)	(0.010)	(0.004)
Year 5 x treat	0.083***	0.013	-466.294	-9986.971***	-0.008	-0.015**	-0.027*	-0.015*
	(0.002)	(0.009)	(511.647)	(2129.125)	(0.005)	(0.005)	(0.011)	(0.006)
Year 7 x treat	0.036***	0.014	-302.722	-11047.567***	-0.006	-0.014**	-0.030*	-0.012*
	(0.001)	(0.009)	(496.102)	(2855.749)	(0.004)	(0.005)	(0.012)	(0.006)
N	328,870	235,264	235,264	235,264	288,682	328,870	113,030	211,088
Treat. mean at year -1	0.000	0.238	6077.611	33064.281	0.017	0.094	0.153	0.093

Standard errors in parentheses

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

Note: The table presents the coefficients from the matched difference-in-difference specification with matching on offense and sentence score. "Year x treat" represents the year relative to the date of sentencing interacted with treatment. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications consider only individuals at least 23 years old at the time of sentencing, with no prior felony convictions.

Table 3: Effects of longer prison sentences (3-7 years) on crime, labor market outcomes, assets and human capital accumulation

	(1) Incapacitated	(2) Employed	(3) Wage	(4) Cum. earnings	(5) Recent conviction
Year -5 x treat	-0.362***	0.103***	1712.264*	10765.646	-0.006*
	(0.009)	(0.031)	(827.674)	(8768.422)	(0.003)
Year -4 x treat	-0.360***	0.032	1461.463	13578.549	-0.007*
	(0.007)	(0.025)	(1166.931)	(10922.612)	(0.003)
Year -3 x treat	-0.355***	0.054**	1472.271	9602.982	-0.006
	(0.009)	(0.020)	(1032.798)	(8391.115)	(0.003)
Year -2 x treat	-0.357***	0.030	375.841	16831.631*	-0.014*
	(0.009)	(0.023)	(1070.760)	(8079.005)	(0.007)
Year 0 x treat	-0.361***	0.067***	1436.421*	7173.604	-0.008**
	(0.009)	(0.020)	(621.042)	(6507.144)	(0.003)
Year 1 x treat	-0.366***	0.037	1882.375*	1076.445	-0.054***
	(0.009)	(0.021)	(760.237)	(4667.294)	(0.008)
Year 2 x treat	-0.368***	0.032	2133.191	15149.677	-0.046***
	(0.007)	(0.019)	(1487.565)	(10116.625)	(0.007)
Year 3 x treat	-0.396***	0.075***	2439.585***	-24344.503***	-0.032***
	(0.004)	(0.009)	(494.157)	(1639.420)	(0.004)
Year 4 x treat	-0.406***	0.071***	1986.148***	-25232.658***	-0.021***
	(0.004)	(0.008)	(406.214)	(1677.901)	(0.005)
Year 5 x treat	-0.405***	0.066***	2195.666***	-25796.943***	-0.006
	(0.004)	(0.008)	(429.886)	(1640.129)	(0.005)
Year 6 x treat	-0.403***	0.075***	2829.682***	-25950.506***	-0.006
	(0.004)	(0.008)	(409.976)	(1561.130)	(0.005)
Year 7 x treat	-0.405***	0.077***	2731.794***	-19841.543***	-0.007
	(0.004)	(0.008)	(435.307)	(1215.409)	(0.005)
Year 8 x treat	-0.402***	0.083***	2460.982***	-19485.500***	-0.009
	(0.004)	(0.008)	(379.371)	(1267.022)	(0.005)
Year 9 x treat	-0.402***	0.074***	2317.156***	-19007.109***	-0.015**
	(0.004)	(0.008)	(456.380)	(1143.866)	(0.005)
Year 10 x treat	-0.402***	0.085***	3085.479***	-18751.094***	-0.024***
	(0.004)	(0.009)	(475.255)	(1113.850)	(0.005)
N	600,831	504,064	504,064	504,064	610,041
Treat. mean at year -1	0.002	0.230	5322.202	27793.875	0.016

Standard errors in parentheses

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

Note: The table presents the coefficients from the difference-in-difference specification that includes fixed effects at the person and year level, with the treatment group restricted to defendants serving prison sentences between 3 and 7 year. "Year x treat" represents the year relative to the date of sentencing interacted with treatment. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications consider only individuals at least 23 years old at the time of sentencing, with no prior felony convictions.

Table 4: Alternative specification: the effects of incarceration on recidivism, labor market, assets and human capital accumulation, leveraging discontinuities in sentencing guidelines within a DD design

	(1) Incarcerated	(2) Employed	(3) Wage	(4) Cumulative earnings	(5) Recent conviction	(6) Homeowner	(7) Car owner	(8) Any post-secondary education
Year -5 x treat	0.000	-0.022	-1709.971	3685.141	0.004	-0.006	0.002	0.002
	(0.000)	(0.023)	(1549.707)	(5280.485)	(0.003)	(0.005)	(0.009)	(0.004)
Year -3 x treat	0.000	-0.003	-1474.278	820.937	0.001	0.000	-0.000	0.003
	(0.000)	(0.021)	(1347.297)	(2785.781)	(0.003)	(0.004)	(0.006)	(0.003)
Year 0 x treat	1.000***	-0.065***	-1441.565	-1983.178	0.003	-0.005*	-0.009	-0.001
	(0.000)	(0.019)	(989.388)	(1066.683)	(0.003)	(0.003)	(0.005)	(0.002)
Year 1 x treat	0.509***	-0.142***	-4945.802**	-7470.593***	-0.043***	-0.009**	-0.019**	-0.005
	(0.005)	(0.022)	(1516.278)	(2009.319)	(0.006)	(0.003)	(0.006)	(0.003)
Year 3 x treat	0.096***	-0.002	-1016.688	-10014.978**	-0.002	-0.017***	-0.027**	-0.005
	(0.003)	(0.023)	(1473.691)	(3671.255)	(0.006)	(0.005)	(0.009)	(0.004)
Year 5 x treat	0.036***	0.022	1317.364	-9903.240	0.002	-0.021***	-0.035***	-0.009*
	(0.002)	(0.023)	(1427.820)	(5395.152)	(0.005)	(0.005)	(0.010)	(0.004)
Year 7 x treat	0.014***	-0.002	899.816	-8457.970	0.001	-0.016**	-0.035***	-0.010*
	(0.001)	(0.025)	(1508.732)	(7028.470)	(0.005)	(0.006)	(0.011)	(0.005)
N	109,990	26,544	26,544	26,544	95,451	109,990	40,118	72,248
Treat. mean at year -1	0.000	0.748	20061.596	108980.340	0.017	0.111	0.165	0.102

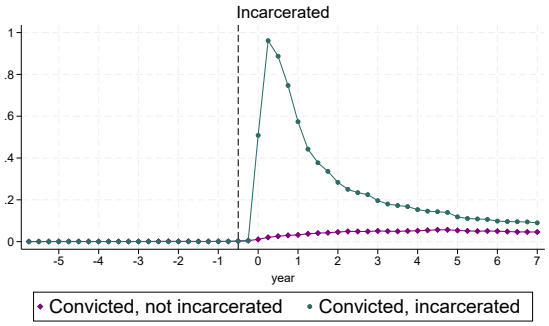
Standard errors in parentheses

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

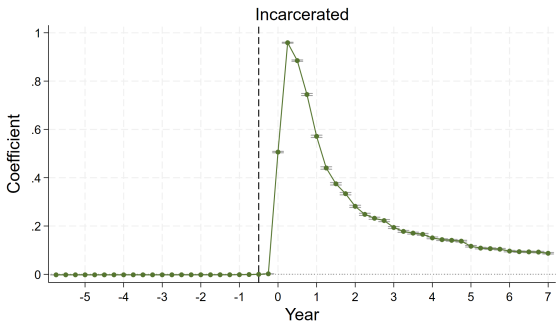
Note: The table presents the coefficients from the difference-in-difference specification that in which we define treatment as those who score right above the cut-off for a prison sentence and were incarcerated and control is those who score right below and were not. "Year x treat" represents the year relative to the date of sentencing interacted with treatment. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications consider only individuals at least 23 years old at the time of sentencing, with no prior felony convictions.

Figure 1: Exposure to incarceration: time trends and difference-in-difference estimators with leads and lags

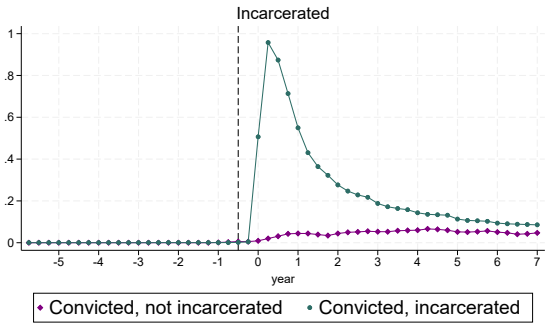
(a) Only felony sentences (2001-2014)



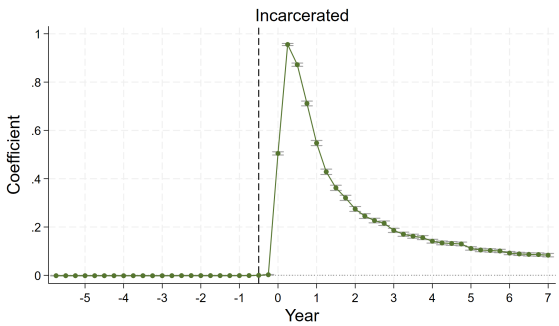
(b) Only felony sentences (2001-2014)



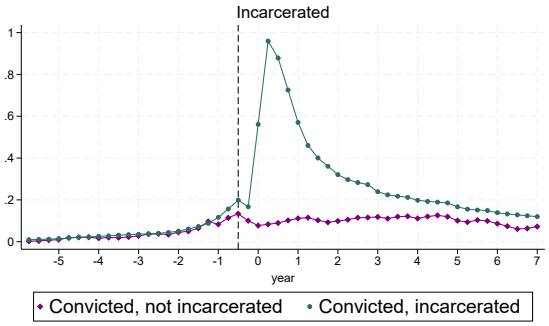
(c) Only felony sentences (2012-2014)



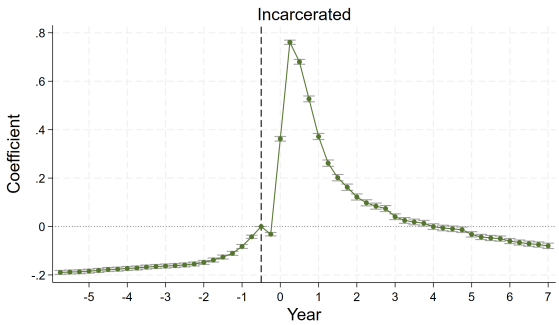
(d) Only felony sentences (2012-2014)



(e) Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)

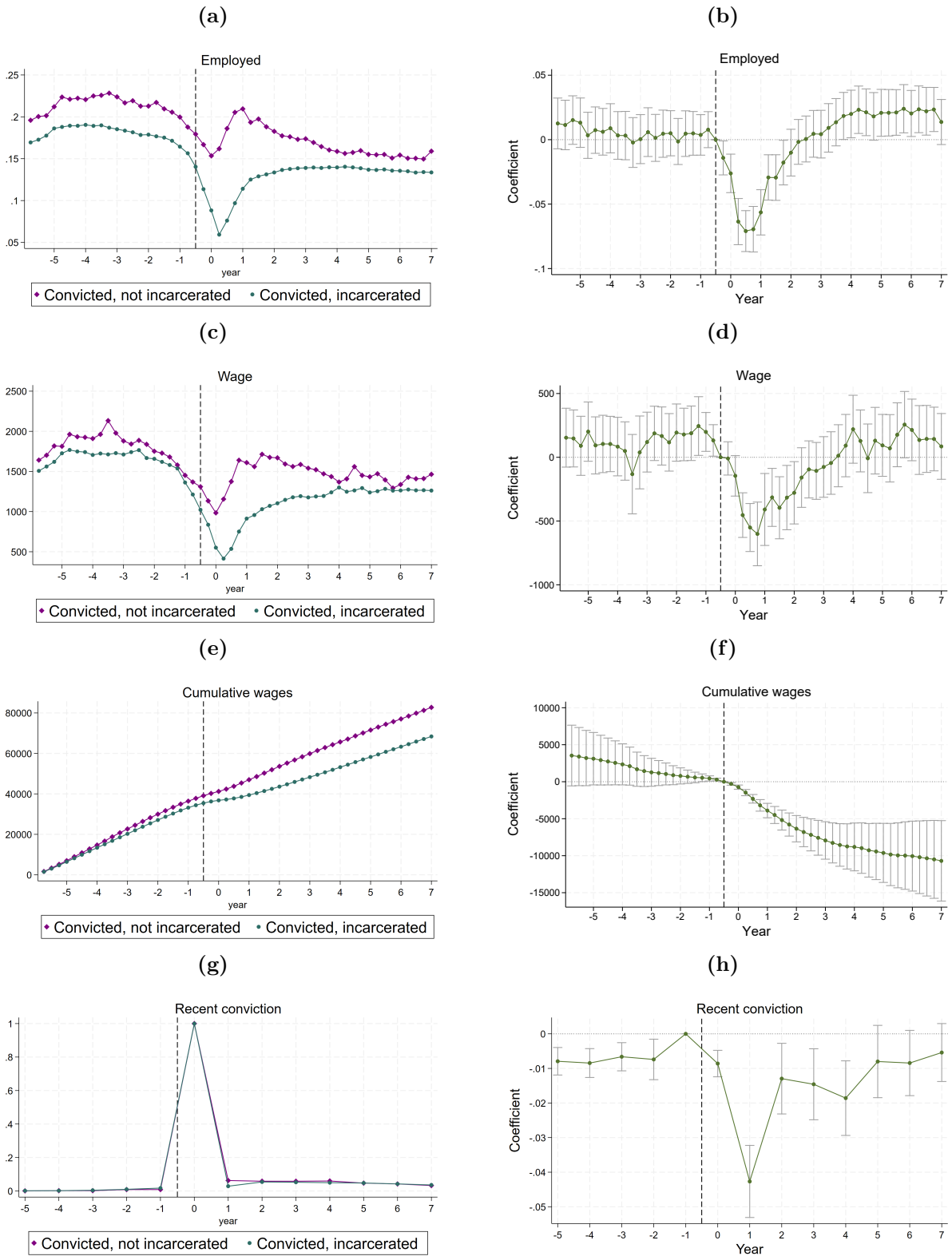


(f) Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



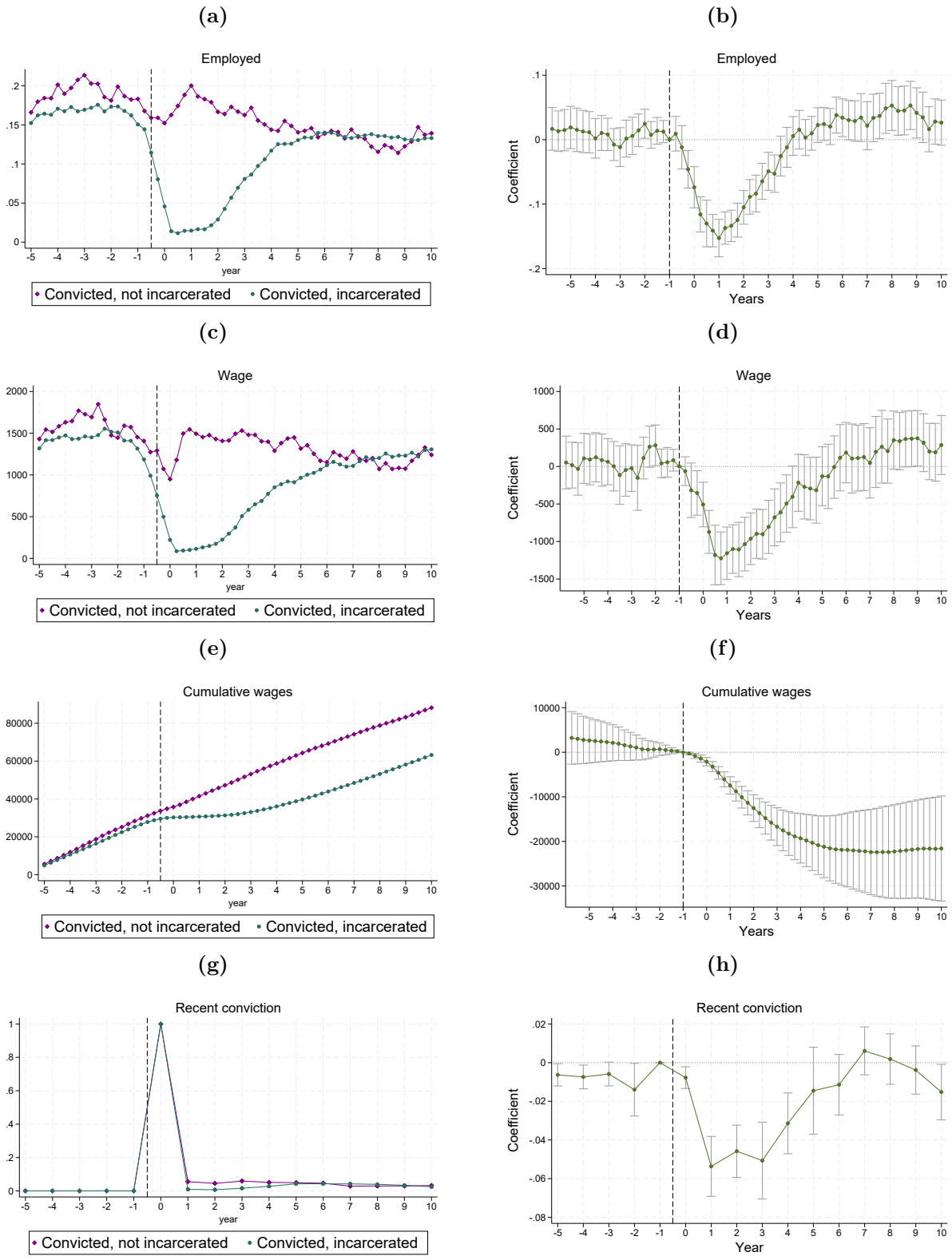
Note: The figures on the left show the yearly likelihood of being incarcerated for treatment vs control. The figures on the right show matched difference-in-differences estimates. The top two rows show incarceration on felony sentences while the bottom row includes pretrial detention, probation revocation and misdemeanor sentences. The sample includes defendants that are recommended for prison (worksheet score between 0 and 9). The top row includes everyone sentenced between 2001-2014, at least 23 years old, and with no prior felony convictions. The bottom two rows are similar except restricted to those sentenced between 2012-2014.

Figure 2: The effects of incarceration on employment, wages, earnings and recidivism: time trends and difference-in-difference estimators with leads and lags



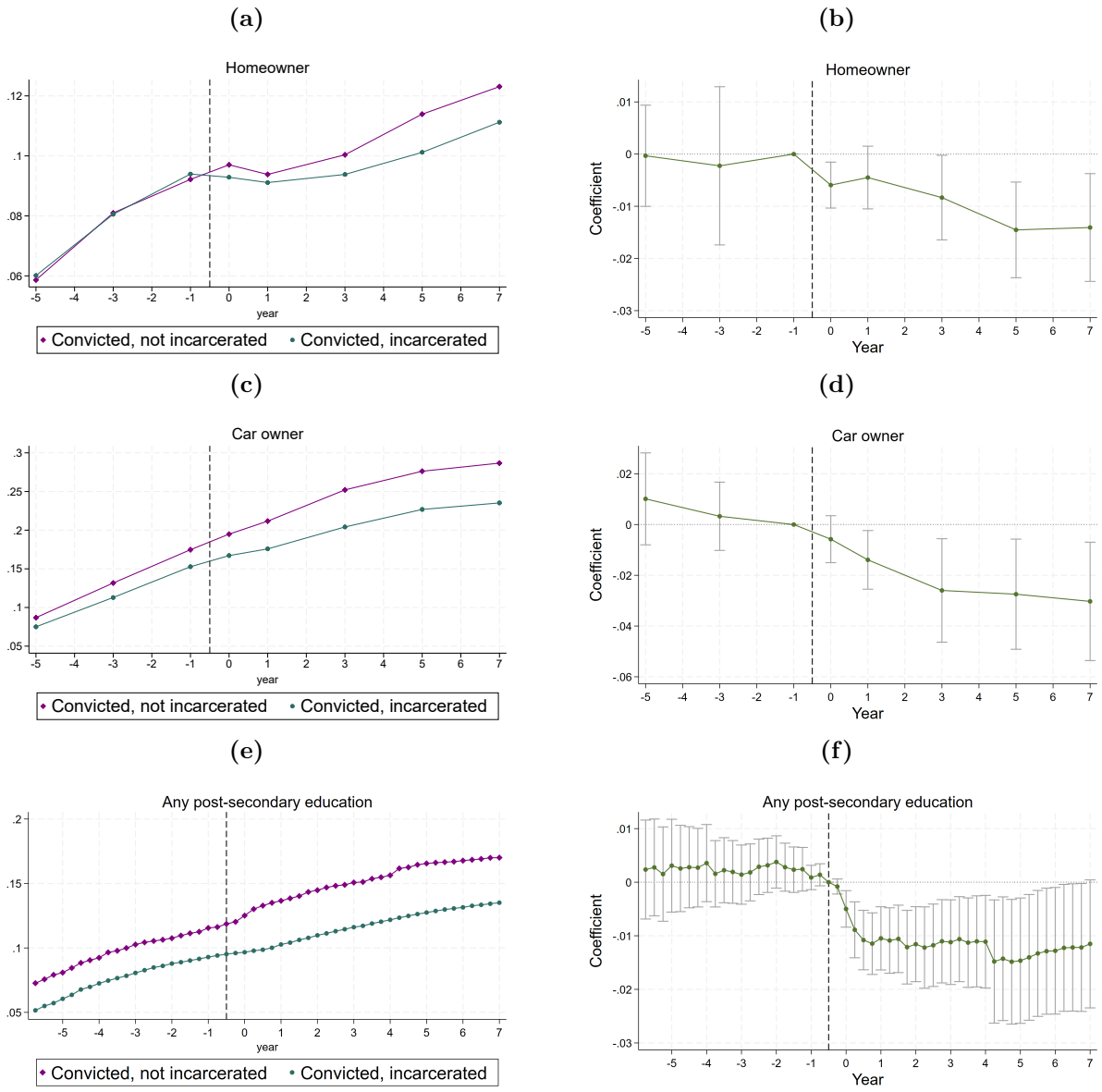
Note: The figures on the left show average outcomes for those who were incarcerated versus a matched sample of those not incarcerated. The figures on the right show matched difference-in-differences estimates. The sample includes defendants that are recommended for prison (worksheet score between 0 and 9). The samples restrict to people at least 23 years old with no prior felony convictions. The sample for employment and wage outcomes also restricts to people sentenced in 2003-2012. The sample for recent criminal convictions includes those sentenced in 2001-2014.

Figure 3: Effects of longer prison sentences (3-7 years) on employment, wages, earnings and recidivism: time trends and difference-in-difference estimators with leads and lags



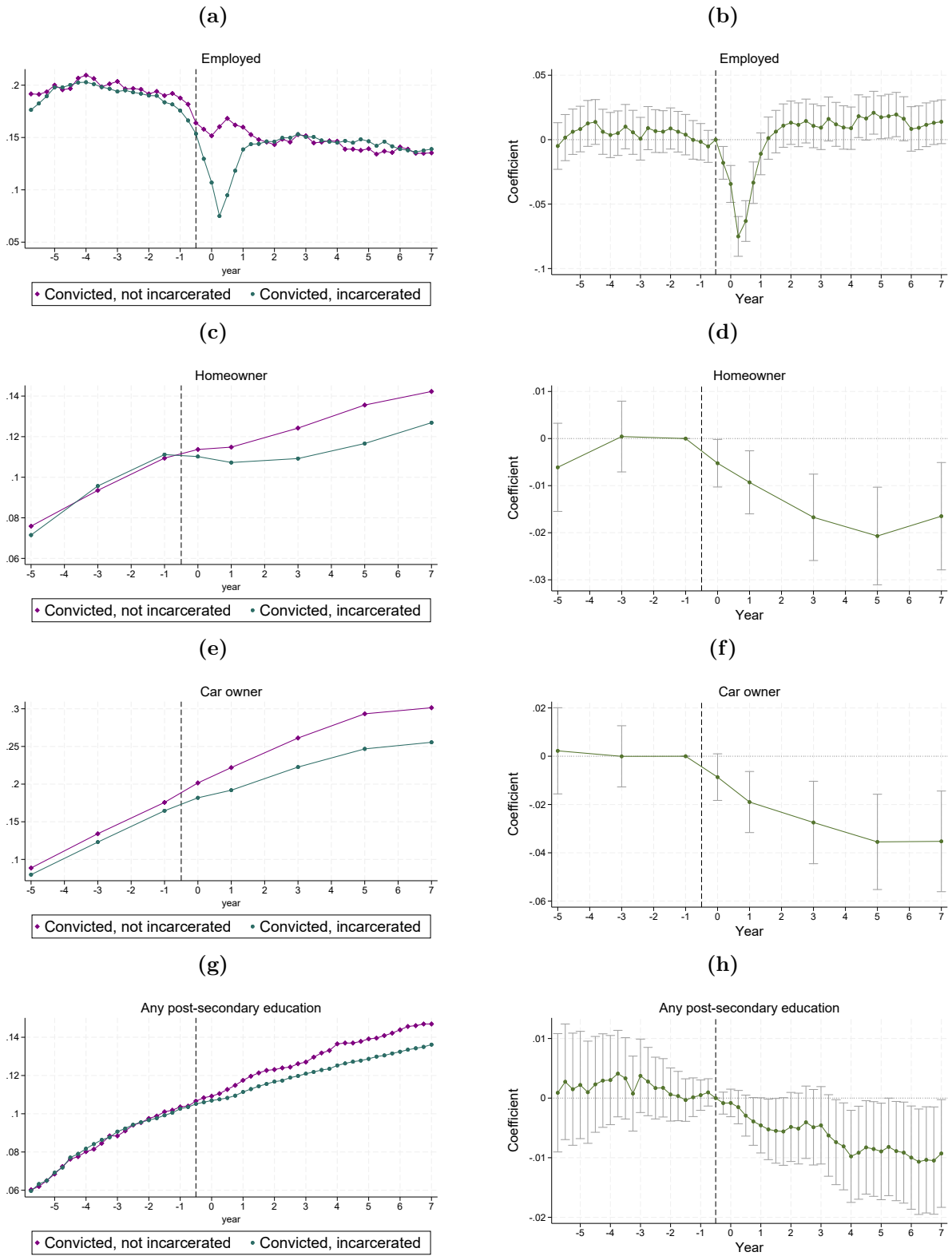
Note: The figures on the left show average outcomes for those who received a 3-7 year prison sentence versus a matched sample of those not incarcerated. The figures on the right show difference-in-differences estimates. The samples restrict to people at least 23 years old with no prior felony convictions. The sample for employment and wage outcomes includes people sentenced in 2003-2009. The sample for recent criminal convictions includes those sentenced in 2001-2010.

Figure 4: The effects of incarceration on wealth and human capital accumulation: time trends and difference-in-difference estimators with leads and lags



Note: The figures on the left show average outcomes for those who were incarcerated versus a matched sample who were not incarcerated. The figures on the right show matched difference-in-differences estimates. The sample includes defendants that are recommended for prison (worksheet score between 0 and 9). The sample for homeownership includes people sentenced from 2001-2014, the sample for car ownership includes those sentenced from 2010-2014. The sample for post-secondary education comprises individuals sentenced from 2005 to 2013. For all outcomes, we keep only those at least 23 years old at sentencing and with no prior felony conviction.

Figure 5: Alternative specification - leveraging discontinuities in the sentence guidelines schema: employment, homeownership, car ownership and college education.



Note: Figures on the left show average outcomes for individuals with incarceration-length scores just above the {0,1} cutoff (incarcerated) versus those just below the {-2,-1} cutoff (not incarcerated), while the right figures present difference-in-differences estimates. The samples vary by outcome: employment (sentenced 2003-2012), homeownership (2001-2014), car ownership (2010-2014), and post-secondary education (2005-2013). In all cases, we restrict the sample to individuals who were at least 23 years old at sentencing with no prior felony convictions.

A Appendix

A.1 Comparing Virginia's criminal justice system to other states

This appendix section shows how Virginia's criminal justice system compares to the U.S. overall, as well as to several states considered in recent related studies: Georgia, Michigan, North Carolina, Ohio, and Texas. First, we re-create figures from Norris et al. (2021) with an additional label for Virginia. Following Norris et al. (2021), we use 2004 data from the Pew Center on three-year recidivism rates, 2004 data on incarceration rates from the Bureau of Justice Statistics, and 2004 data on violent and property crime rates from the FBI Uniform Crime Reporting Program.[10]

Panel (a) of Appendix Figure A.1 shows that while Virginia has similar incarceration rates to the US average and other states, it has slightly lower recidivism (the 3-year recidivism rate is around 28%). Panel (b) shows that Virginia's property and violent crime rates are lower than the selection of states highlighted, but it is not an outlier in comparison to the rest of the states in the sample.

Appendix Figure A.2 shows prison and jail incarceration rates for the U.S., Virginia, and the five comparison states.[11] Virginia's prison incarceration rate, shown in Panel (a), is 447 per 100,000 people. This is somewhat higher, but comparable to the national rate, and roughly equal to the median among the five comparison states. The rate at which people are jailed in Virginia – 273 per 100,000 – is on the higher end compared to the national average and the five comparison states. Although it is not an obvious outlier relative to either the national average or the five comparison states, when interpreting our results, it is helpful to keep in mind that Virginia tends to rely more on jails than prisons and that conditions may vary across these two settings.

We next consider the racial and ethnic make-up of the prison population in Virginia. Figure A.3 displays the relative ratio of incarceration rates for Black vs White and Hispanic vs White residents.[12] The ratio for Black:White residents in Virginia is 4.3, just below the national average of 4.8 and roughly equal to the average of 4.4 of the other five comparison states. As in other states, Black residents are over-represented in the carceral population. The ratio for Hispanic:White residents is 0.5 for Virginia, lower than the national average of 1.3 and most comparison states.

Lastly, we compare probation and parole rates. Virginia's probation rate is close to the national average and that of most comparison states. However, the parole rate in Virginia – 22 per 100,000 residents – is much lower than the benchmarks. This is because discretionary parole was virtually abolished in Virginia for felonies committed after 1995, resulting in inmates being required to serve at least 85% of their sentences, with the possibility to earn good-time credits toward early release, leading to the small number of individuals on parole in Virginia. This also means that the initial sentence is more closely linked to time spent incarcerated than in other places.

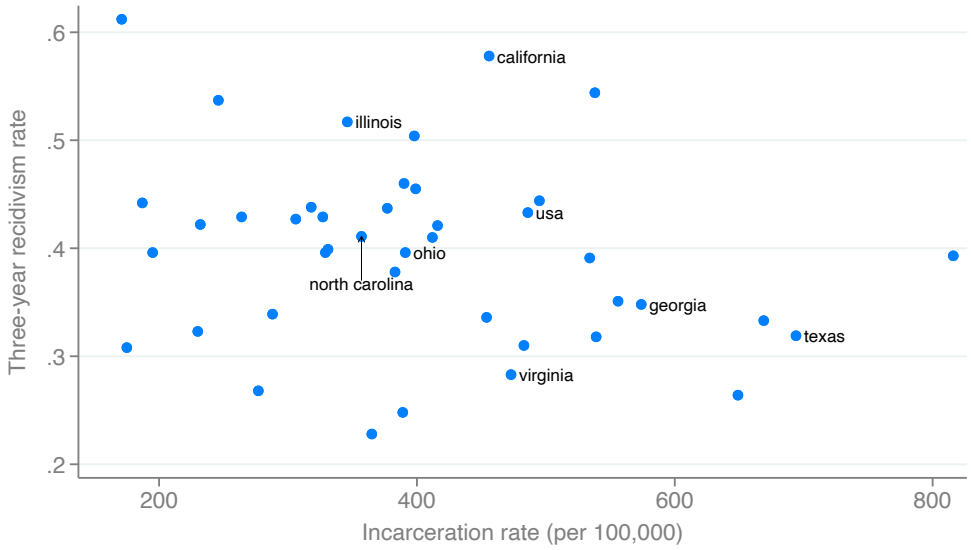
[10] This data can be found at https://www.pewtrusts.org/-/media/legacy/uploadedfiles/pcs_assets/2011/pewstateofrecidivismpdf.pdf, https://bjs.ojp.gov/content/pub/pdf/p04.pdf, and https://www2.fbi.gov/ucr/cius_04/.

[11] We use data from the Prison Policy Initiative. This data can be downloaded from https://www.prisonpolicy.org/reports/correctionalcontrol2018.html.

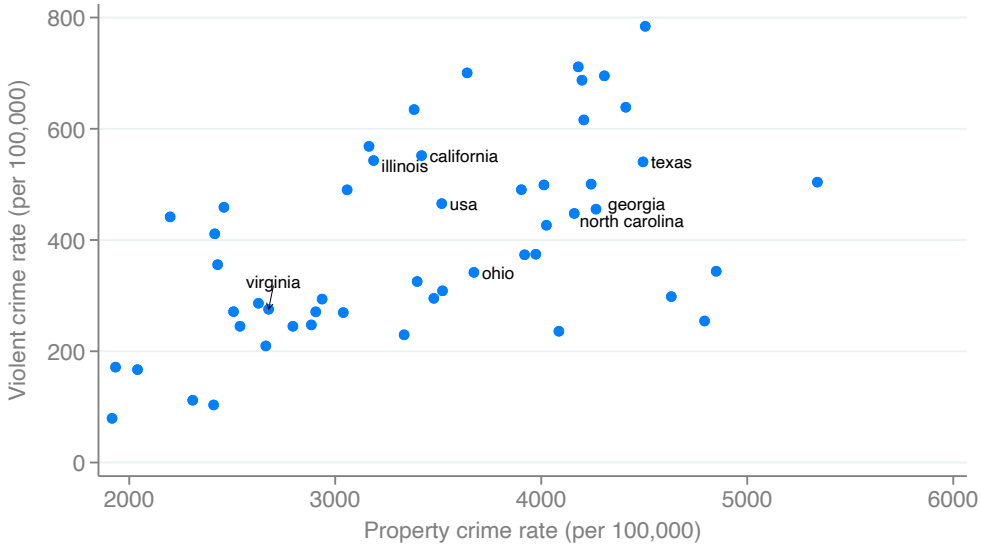
[12] These ratios read as follows: If out of every 100,000 Hispanic residents 200 are incarcerated, and out of every 100,000 White residents 400 are incarcerated, the Hispanic:White ratio is 0.5.

Figure A.1: State-level comparisons of recidivism, incarceration, and crime

(a) 3yr-Recidivism rates vs incarceration rates



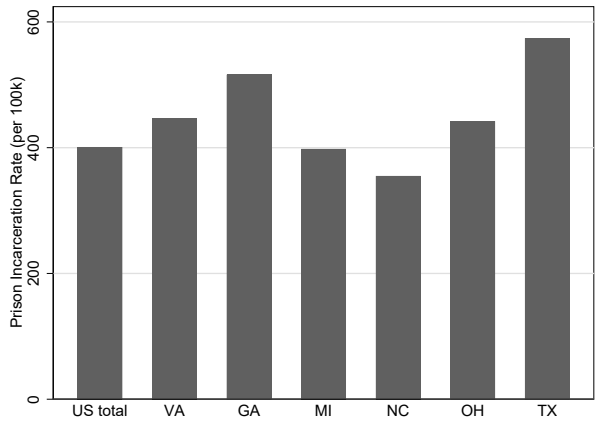
(b) Violent crime rates vs property crime rates



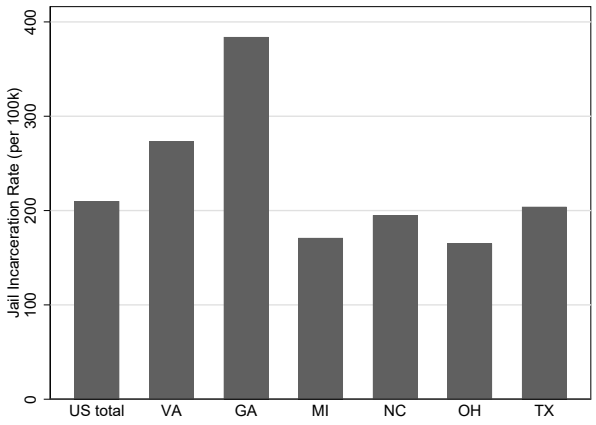
Note: Scatterplots of 2004 incarceration rates, 2004 three-year recidivism rates, and 2004 crime rates. Data gathered from the Pew Center, Bureau of Justice Statistics, and the FBI Uniform Crime Reporting Program.

Figure A.2: Incarceration rates

(a) Prison Incarceration Rates

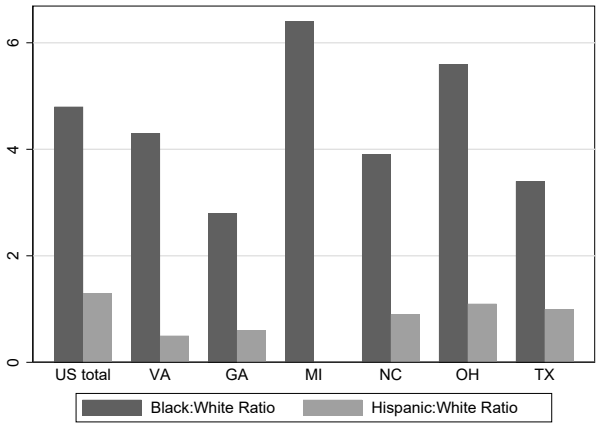


(b) Jail Incarceration Rates



Note: This figure shows the prison (Panel A) and jail (Panel B) incarceration rates, respectively, per 100,000 residents for Virginia, the U.S. overall, Georgia, Michigan, North Carolina, Ohio, and Texas. Based on 2017 and 2014 data respectively from the Prison Policy Initiative (December 2018 press release).

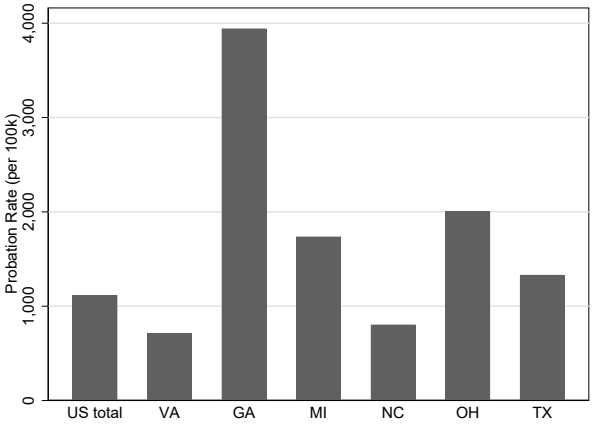
Figure A.3: Racial and ethnic composition of the imprisoned population



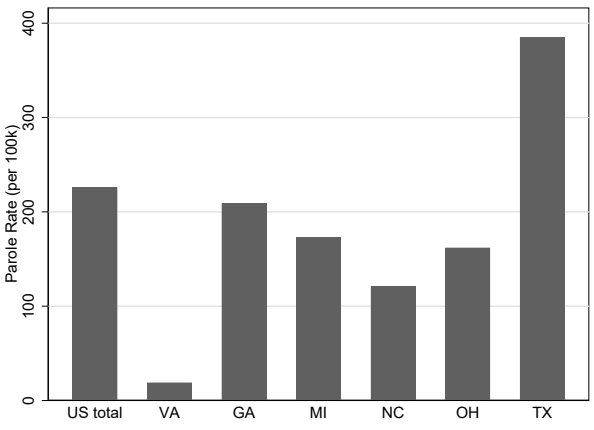
Note: This figure plots the ratio of incarceration rates for Black vs White residents (darker bars) and Hispanic vs White residents (lighter bars), for Virginia, the U.S. overall, Georgia, Michigan, North Carolina, Ohio, and Texas in 2019. Data from sentencingproject.org, used to calculate incarceration by ethnicity, is not available for Michigan.

Figure A.4: Virginia supervision rates comparison

(a) Probation rates



(b) Parole rates



Note: Panel (a) shows the probation rate in Virginia per 100,000 people and Panel (b) shows the parole rate in Virginia per 100,000 people, both compared to the rates for the U.S. total, Georgia, Michigan, North Carolina, Ohio, and Texas. Based on 2016 data from the Prison Policy Initiative (December 2018 press release).

A.2 Matching Schema

VCSC to Virginia Circuit Court data. Below is a description of the matching process used to 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 which 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:

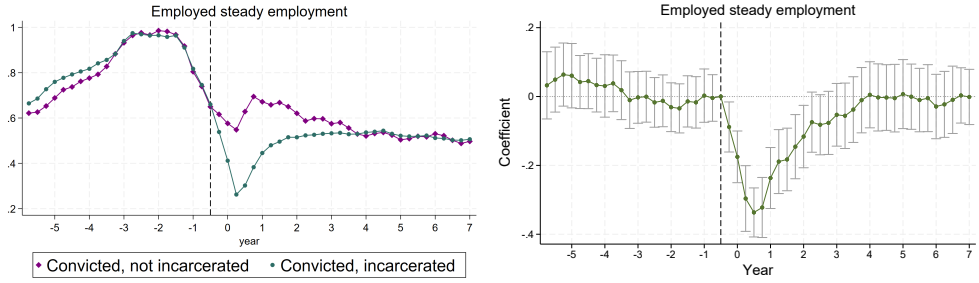
- Education data: first match attempt based on
  - full last name
  - first three letters of first name
  - first letter of middle name
  - month and year of birth
  - last four digits of social security number

  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

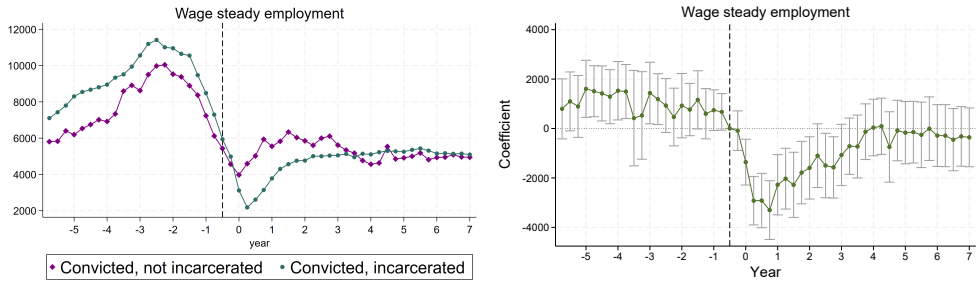
  For defendants who did not have a match yet, a third and final matching procedure was completed using the following variables:
- Education data: third match attempt based on
  - first five letters of last name
  - first four letters of first name
  - last four digits of social security number

Figure A.5: Labor market outcomes for those with steady employment prior to sentencing

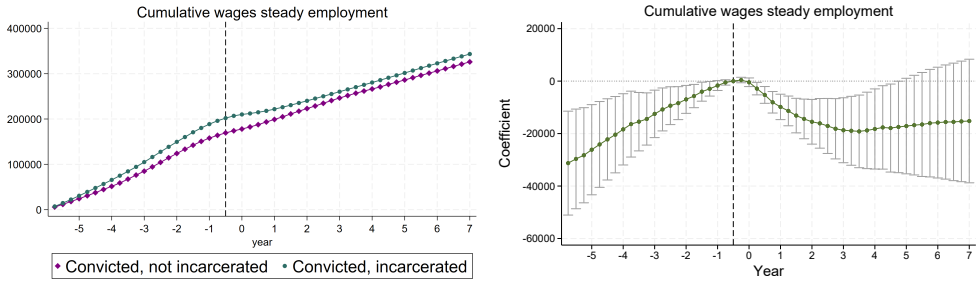
(a) Employment



(b) Wage



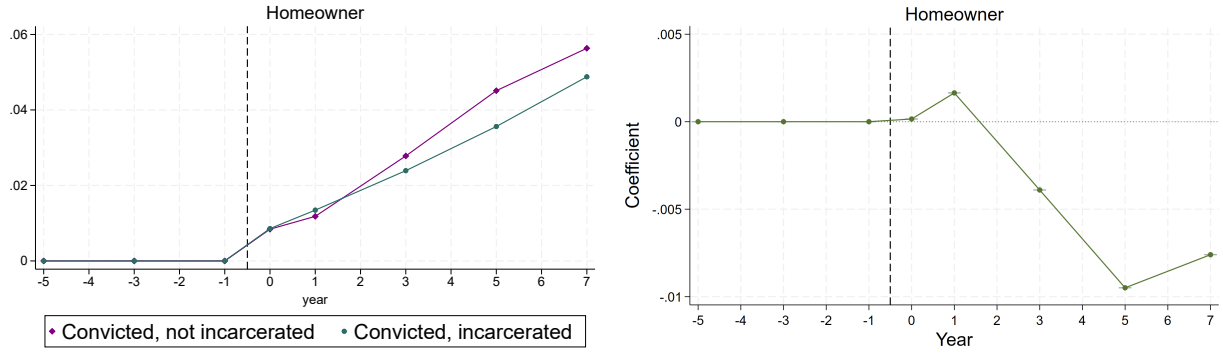
(c) Cumulative earnings



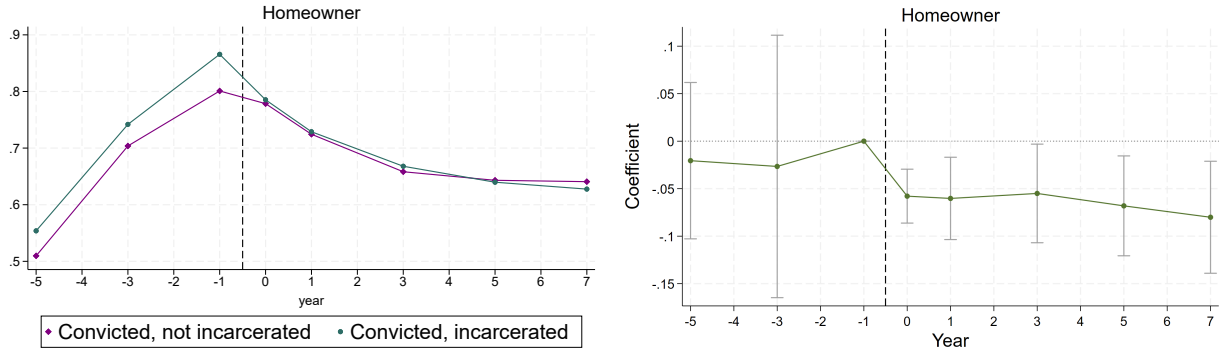
Note: The figures on the left show average outcomes for those who were incarcerated versus a matched sample who were not incarcerated. The figures on the right show matched difference-in-differences estimates. The sample includes only those defendants with steady employment pre-sentencing. "Steady employment" 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. Our samples include only individuals aged 23 or older with no prior felony convictions who were sentenced between 2003 and 2012.

Figure A.6: Heterogeneity: by prior home ownership

(a) No prior home ownership



(b) Prior home ownership



Note: These figures compare the impact of incarceration on home ownership 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 prior to the sentencing date. Conversely, "Prior ownership" only applies to defendants who *owned* a house prior to sentencing. The figures on the left show average outcomes for those who were incarcerated versus a matched sample who were not incarcerated. The figures on the right show matched difference-in-differences estimates. The samples are restricted to individuals at least 23 years old with no prior felony convictions, who were sentenced between 2010 and 2014.

Table A.1: Court debt and community supervision

	(1) Treatment group	(2) Control group
Panel A: Sentencing Commission dataset		
Court debt		
Required to pay		
Fine	14 %	6 %
Restitution	23 %	30 %
Fine or restitution	35 %	35 %
Community supervision		
Type of supervision		
Sentence overhang	96 %	98 %
Post release		
Term	3 %	2 %
Supervision period	4 %	2 %
Probation period (supervised)		
Definite	60 %	61 %
Indefinite	23 %	19 %
Length among those supervised (in months)		
Sentence overhang	82	66
Post release		
Term	79	76
Supervision period	60	71
Probation period (supervised)		
Definite	41	34
Number of observations	10,264	3,518

Note: This table shows the percentage of defendants with court debts and community supervision, along with the duration of supervision for those under supervision and the amount of court debt for those required to pay. The information is presented in two panels: Panel A reflects our main sample from the Sentencing Commission dataset. The court debt categories isas follows: "Required to Pay" indicates the percentage of individuals obligated to pay court debt. For community supervision, we report both the type and duration. The supervision types include: "Sentence Overhang", defined as the difference between the total time imposed before suspension and the total effective time to serve, "Term" and "Supervision Period" and "Probation Period" which may be either definite (a specific probation duration at sentencing) or indefinite (no fixed end date at sentencing).

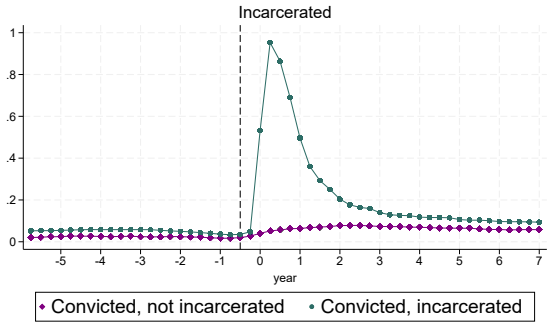
Table A.2: Alternative specification – leveraging discontinuities in the sentence guidelines schema: Comparing treatment and control

	Control group	**Treatment group**	**Difference**
	Mean	Mean	Mean difference
Months sentence	0.0	15.9	-15.9***
Demographics			
Age	36.8	36.2	0.6***
Black	0.4	0.4	-0.03***
Hispanic	0.0	0.0	-0.01***
Female	0.3	0.2	0.09***
Type of offense			
Violent crime	0.2	0.2	-0.009
Robbery	0.0	0.0	-0.010***
Drug	0.2	0.3	-0.146***
Assault	0.1	0.1	0.001
Larceny	0.1	0.1	-0.017***
Criminal history			
On probation/parole	0.3	0.3	0.013
Total misdemeanor convictions	11.0	11.1	-0.131
Outcomes			
Car owner	0.2	0.2	0.011
Any post-secondary education	0.1	0.1	0.0
Home owner	0.1	0.1	-0.002
Observations	3518	10264	13782
Employed	0.7	0.7	-0.00
Wage	22676	21073	1603**
Observations	2622	7332	9954

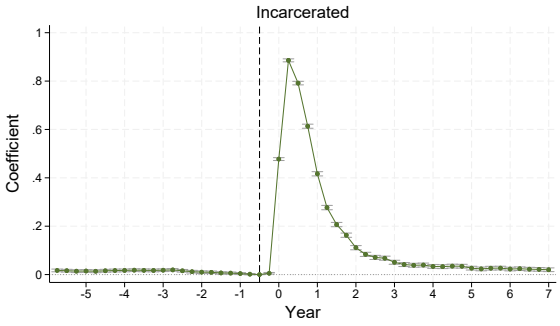
Note: This table presents summary statistics for treatment and control groups in the year prior to sentencing for the alternative specification. The treatment group includes everyone whose sentence guidelines score is in {0,1} and were incarcerated, and the control group includes everyone whose score is in {-2, -1} and were not incarcerated. Wage employment, and wealth outcomes data represent the average values for the year prior to sentencing. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. The matched sample was drawn from individuals sentenced to prison between 2001 and 2014 who were at least 23 years old and had no prior felony convictions.

Figure A.7: Alternative specification - leveraging discontinuities in the sentence guidelines schema: incarceration

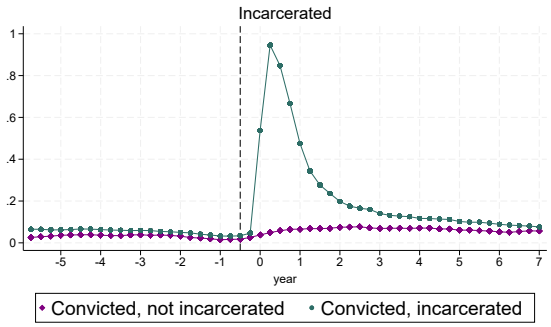
(a) Only felony sentences (2001-2014)



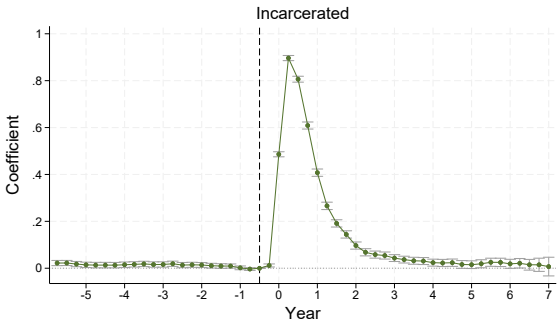
(b) Only felony sentences (2001-2014)



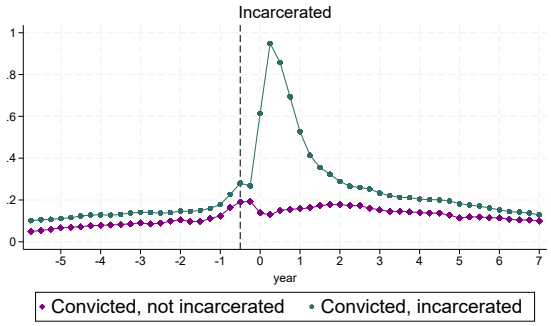
(c) Only felony sentences (2012-2014)



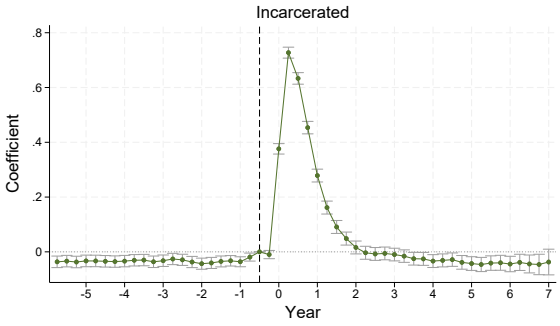
(d) Only felony sentences (2012-2014)



(e) Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)

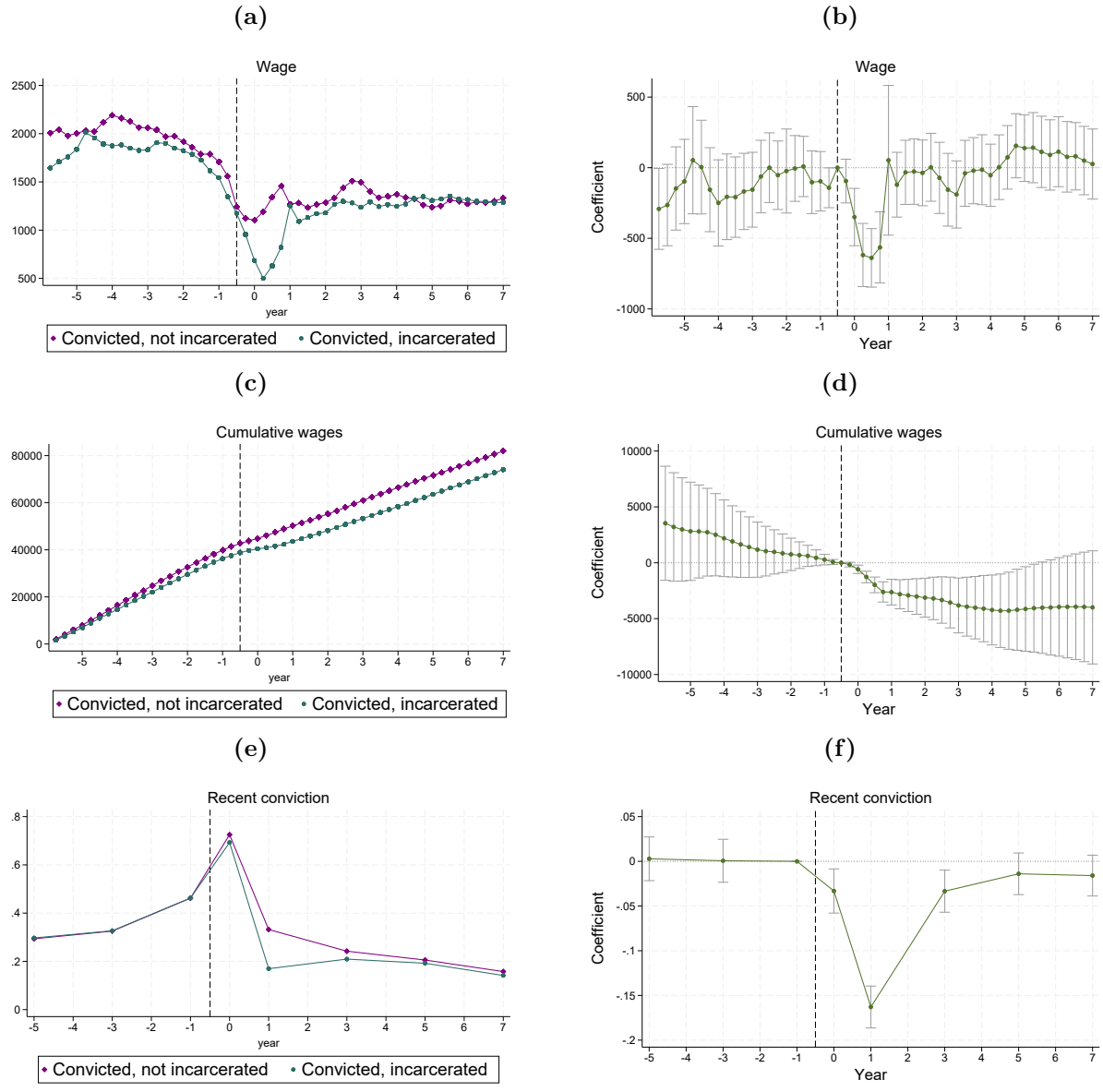


(f) Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



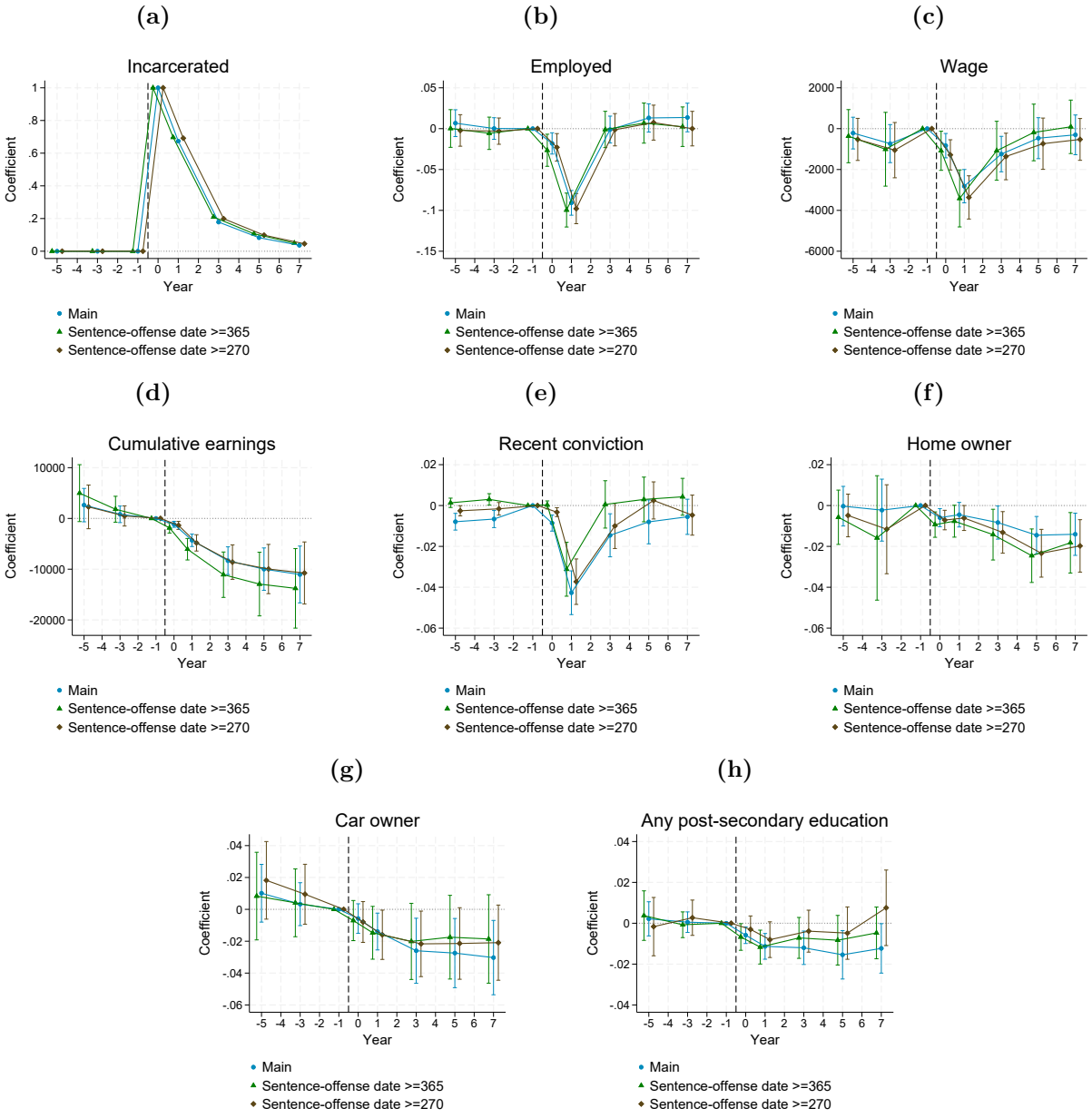
Note: These figures depict the incarceration results of the alternative specification. The figures on the left shows the yearly likelihood of being incarcerated for those whose incarceration-length score is right above the cutoff {0,1} and actually were incarcerated compared to those who scored right below the cutoff {-2,-1} and were not incarcerated. The figures on the right show difference-in-differences estimates. The top two rows show incarceration on felony sentences while the bottom row includes pretrial detention, probation revocation and misdemeanor sentences. The top row includes everyone sentenced between 2001-2014, at least 23 years old, and with no prior felony convictions. The bottom two rows are similar except restricted to those sentenced between 2012-2014.

Figure A.8: Alternative specification - leveraging discontinuities in the sentence guidelines schema: wages, earnings and recidivism



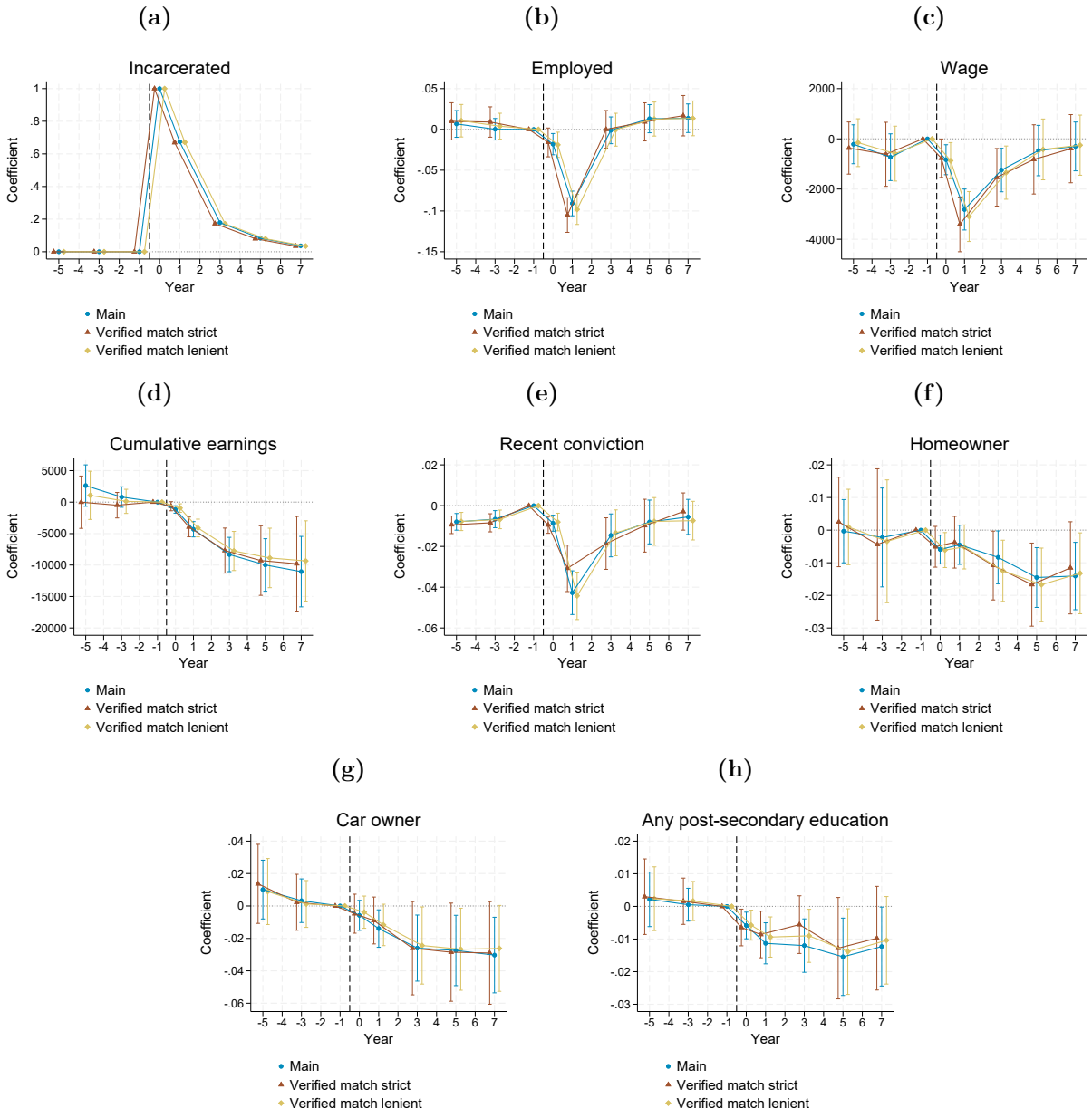
Note: These figures depict results of the alternative specification. The figures on the left show average outcomes for those whose incarceration-length score is right above the cutoff {0,1} and who were incarcerated versus those who scored right below the cutoff {-2,-1} and were not incarcerated. The figures on the right show difference-in-differences estimates. The samples restrict to people at least 23 years old with no prior felony convictions. The wage outcomes sample covers individuals sentenced between 2003 and 2012, while the recent criminal convictions sample includes those sentenced between 2001 and 2014.

Figure A.9: Robustness - long time lag between offense and sentencing



Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes. We present three types of specifications. The "Main" specification (blue circles) is identical to that shown in our main results section. The "Sentence-offense date => 365" specification (green triangles) includes only defendants with a time lag between the offense and the sentence greater than 365 days. The "Sentence-offense date => 270" specification (brown diamonds) includes only defendants with a time lag between the offense and the sentence greater than 270 days (approximately 9 months). The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications include only those at least 23 years old at sentencing and with no prior felony convictions.

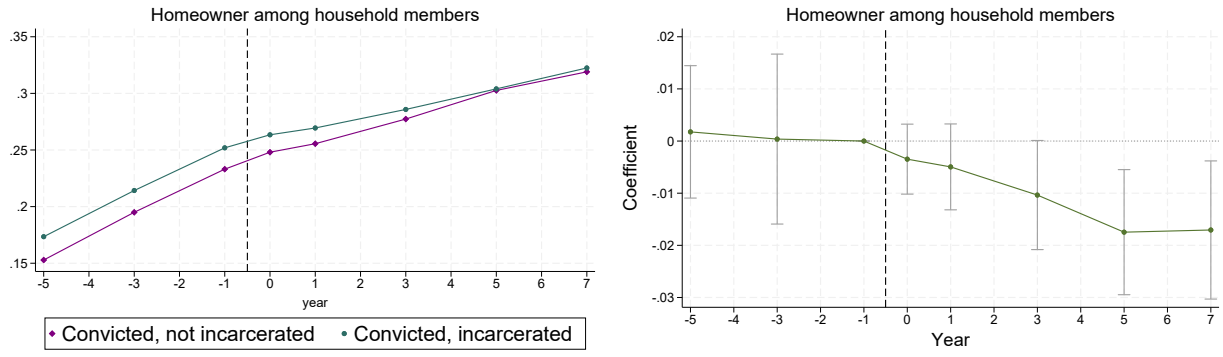
Figure A.10: Robustness - subsamples with match verification



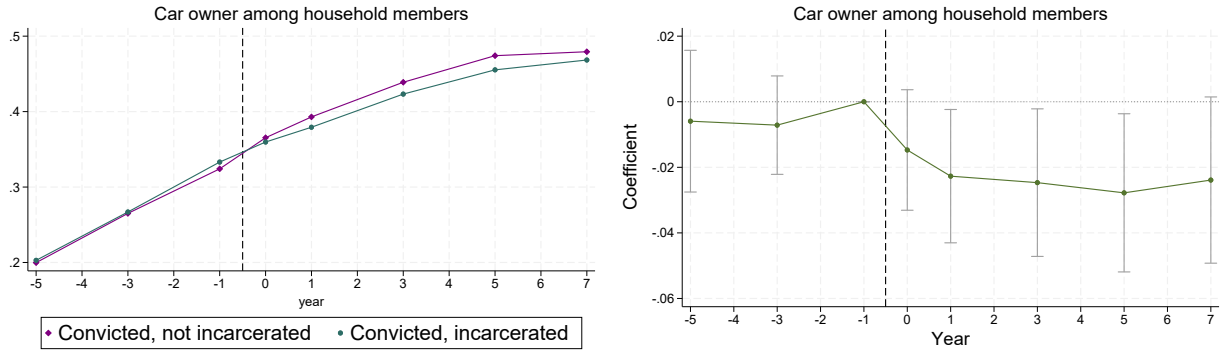
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes across different samples. The "Main" specification (blue circles) shows our main results. The "Verified match strict" specification (brown triangles) includes only defendants whose date of felony convictions matches within two months across the sentencing commission data and aggregator data (most are exact). The "Verified match lenient" (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 samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications consider only individuals at least 23 years old at sentencing and with no prior felony convictions.

Figure A.11: Home and car ownership of household members

(a) Home ownership

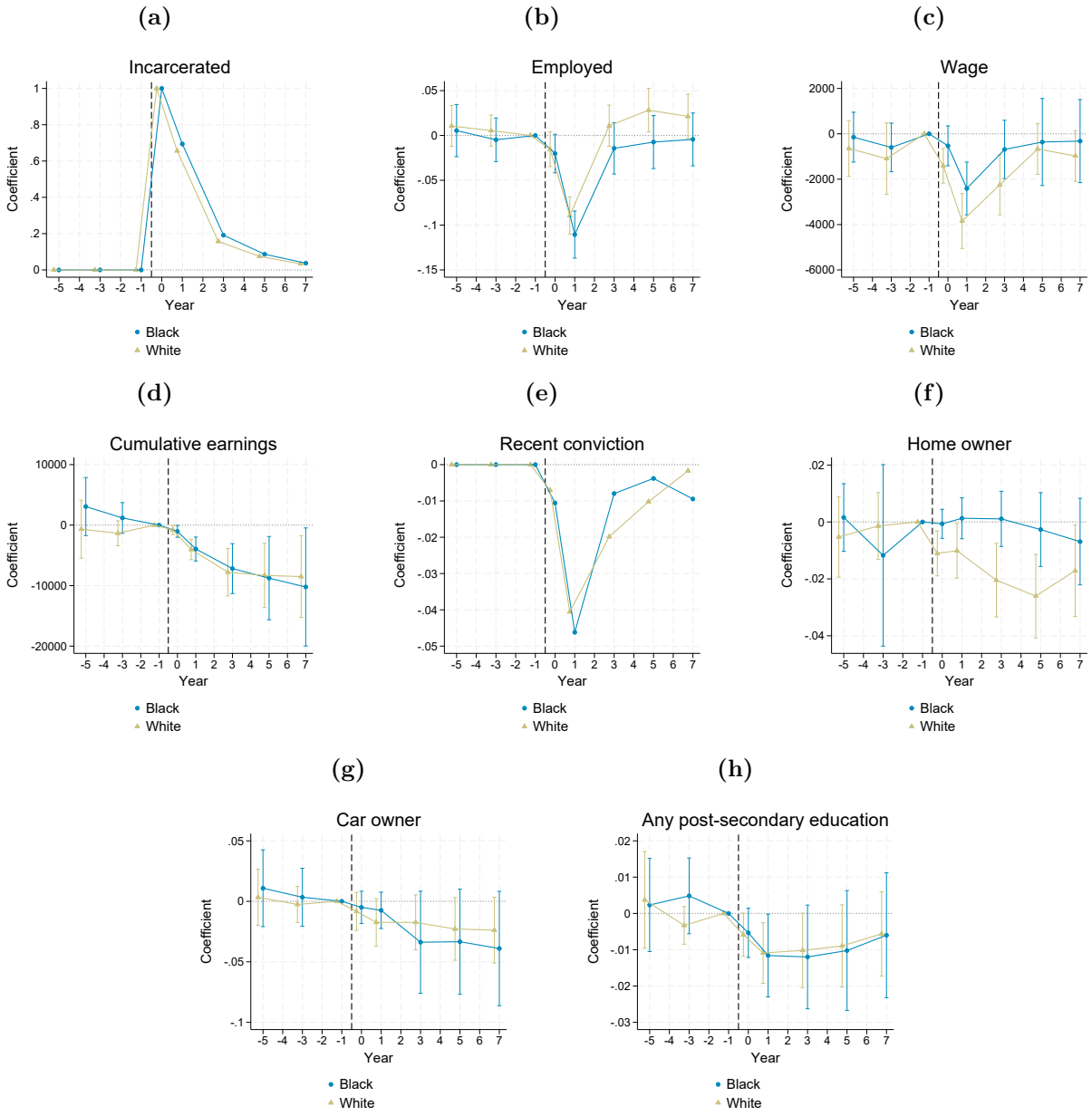


(b) Car ownership



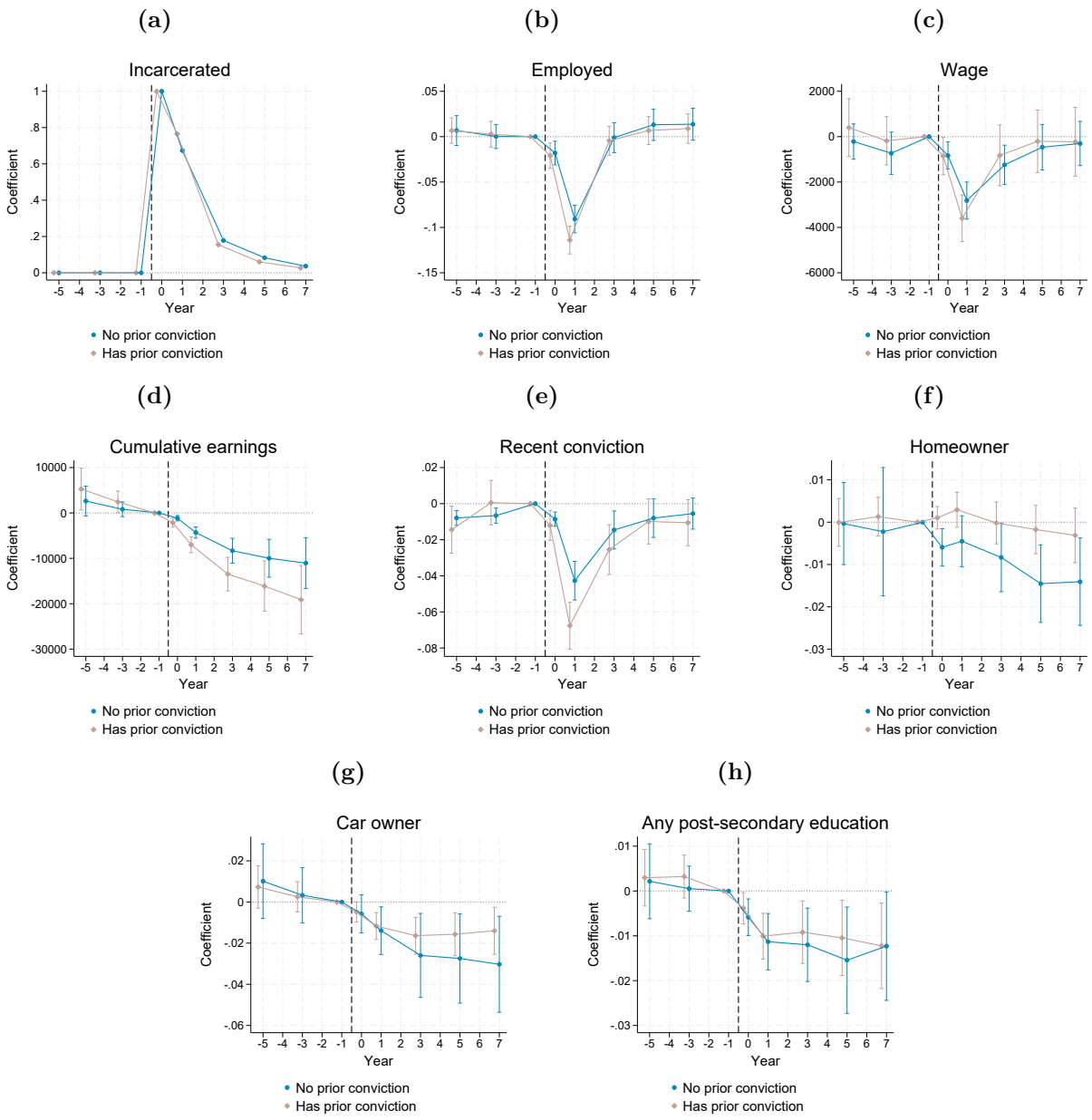
Note: These figures illustrate the results of analyses examining the impact of incarceration on various outcomes defined at the household instead of the individual level and using the main specification. The sample for car ownership includes individuals sentenced from 2010 to 2014, while all other outcomes include individuals sentenced from 2001 to 2014. All specifications are limited to individuals aged 23 or older with no prior conviction.

Figure A.12: Heterogeneity by race



Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes across different samples. 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 includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications include only those at least 23 years old at sentencing and with no prior felony convictions.

Figure A.13: Heterogeneity by criminal history



Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes. We present two types of specifications: the "No prior conviction" specification (blue circles), which is identical to that shown in our main results section and includes individuals with no prior felony conviction, and the "Has prior conviction" specification (pink diamonds), which includes only defendants with a prior felony conviction according to our data aggregator. The samples for the labor market outcomes are limited to those sentenced in 2003-2012. The samples for car ownership and post-secondary education includes those sentenced in 2010-2014 and 2005-2013, respectively. All other outcomes includes those sentenced in 2001-2014. All specifications include only those at least 23 years old with no prior conviction.