Incarceration and Wealth*

John Eric Humphries, Cécile Macaire, Aurélie Ouss,
Megan T. Stevenson, Winnie van Dijk†

PRELIMINARY DRAFT

January 2025

Abstract

Recent research indicates that the economic harms of incarceration, relative to being convicted but not incarcerated, are mostly short term. Yet these papers have looked at *flow* variables like employment, wages and recidivism, rather than *stock* variables like asset ownership. In this paper, we provide new evidence about how incarceration affects wealth accumulation as measured by home and car ownership: two of the main wealth components among the low-income population. To identify the impacts of incarceration, we use several different matched difference-in-differences specifications that leverage Virginia's sentence guidelines scoring system. We first replicate recent findings that incarceration (relative to noncarceral conviction) leads to only short-term, incapacitative declines in employment, wages and recidivism. However, a short-term lack of employment can lead to non-trivial lost earnings: among those steadily employed prior to sentencing, 16 months of incarceration results in a ∼$20,000 loss in cumulative earnings. It also leads to persistently lower rates of home and car ownership, due both to increased sales of assets prior to incarceration and lower purchases while incapacitated. Seven years after sentencing, incarceration results in a 16% decline in homeownership and a 35% decline in car ownership relative to the presentencing average. We also show that incarceration lowers the rate of human capital accumulation – a soft type of wealth – measured by the likelihood of college enrollment.

JEL codes: K42, J24

*Thanks to Meredith Farrar-Owens and others at the Virginia Criminal Sentencing Commission and to Tod Massa at the State Counsel for Higher Education in Virginia for providing data. Many thanks to Kamelia Stavreva for research assistance, as well as to the UVA law librarians.

†Humphries: Yale University and NBER, Macaire: Yale University, Ouss: University of Pennsylvania and NBER, Stevenson: University of Virginia Law School, van Dijk: Yale University and NBER.

The United States has high rates of wealth inequality (Hamilton and Darity, 2009). The top 10% of the U.S. population owns roughly 75% of the country's wealth, while the bottom 50% owns only 1% (Saez and Zucman, 2016; Kuhn et al., 2020). Wealth inequality is also an important component of racial inequality: the median Black household has only 10% of the wealth owned by the median White household (Derenoncourt et al., 2023).

Socio-legal scholars have long speculated that the United States' astronomical per capita incarceration rate – roughly 7 times higher than other Western democracies (Walmsley, 2023) – contributes to wealth inequalities, at least at the bottom end of the wealth distribution (e.g., Alexander, 2010). Incarceration entails being removed from the economy during the age at which young people are often building a career or buying their first home. This type of disruption could have lasting effects on wealth accumulation. Long-term time trends are suggestive of a relationship. After falling for many decades, wealth inequality turned around in the 1980s and has been rising since (Saez and Zucman, 2016; Kuhn et al., 2020; Derenoncourt et al., 2023). While attention has largely been focused on the top of the wealth distribution, there has also been a divergence at the bottom end, with a widening gap between the 25th and 50th percentile (Kuhn et al., 2020). The timing of this divergence is coincident with the incarceration boom, which started in the early 1980s and then plateaued around 2007. Wealth and incarceration also exhibits strong cross-sectional correlational: the median wealth of a household with a family member currently or previously incarcerated is only 7% the median wealth of a household without (Zaw et al., 2016; Weller et al., 2022).

Quasi-experimental research on the economic impacts of incarceration has thus far not considered the effect incarceration may have on wealth accumulation. Focusing almost exclusively on formal labor market outcomes, most studies have found a temporary decline associated with incapacitation but no long-term effects (Kling, 2006; Loeffler, 2013; Harding et al., 2018; Garin et al., 2022).[1] Studies looking at how incarceration affects recidivism similarly mostly find short-term declines but no changes in long run behavior (see, e.g., Loeffler and Nagin, 2022). These studies would therefore appear to suggest that incarceration's impact is only temporary.

However, even if incarceration doesn't have a lasting impact on behavior (as measured by employment and recidivism), it may have a cumulative impact on economic well-being. Incarceration creates basic logistical challenges for wealth accumulation. Those incarcerated may have trouble covering monthly bill payments (including mortgage payments), storing possessions, and arranging maintenance while gone. Accordingly, they may need to sell their home, car or other assets – or, have such items

[1] Mueller-Smith (2015) and Aizer and Doyle (2015) are notable exceptions.

repossessed. In most cases, people will be unable to purchase a home or other investment assets while incarcerated, nor will they be able to earn wages towards a down payment. After release, they may need time to find an apartment and job before they are ready to start saving or investing. On the other hand, there are some mechanisms through which incarceration could have a positive effect on an individual's balance sheet. For instance, they will be freed from certain household expenses, like groceries and rent. And, should they have a medical emergency, such expenses will be covered by the state. This makes the net effect of incarceration on wealth more ambiguous.

In this paper, we provide new evidence about how incarceration affects wealth. Using rich identifiers, we match 25 years of data on people convicted of a felony in Virginia to records on employment, wages, homeownership, car ownership and college education. We employ several different matched difference-in-differences designs that leverage Virginia's sentence guidelines schema. The guidelines-recommended sentence is calculated using a series of scoring worksheets. Those who score above the cutoff on the first worksheet are recommended for prison, while those who score below are recommended for jail or probation. The difference-in-differences design compares outcomes before and after sentencing across treatment and control groups. In our primary specification, we leverage discontinuities in the sentence guidelines score. Our control group consists of those right below the cutoff and who *did not* receive a carceral sentence (i.e., were not sentenced to jail or prison). Our treatment group consists of those right above the cutoff who *did* receive a carceral sentence: 16 months on average.[2] By restricting to those within two points of the sentence score cutoff, we are defining a treatment and control group that is de facto matched on observable characteristics relevant to sentencing. The other research designs use alternative matched difference-in-differences designs, varying both the sample used and the matching technique. Results are highly consistent across the different specifications. Since everyone in our sample is convicted, the research designs identify the impact of incarceration relative to being convicted but not incarcerated.

We first look at incarceration's impact on employment and wages, using data from the Virginia Employment Commission matched to records maintained by the Virginia Criminal Sentencing Commission. We restrict the sample for these analyses to people who are observed as having at least one quarter of W-2 income prior to sentencing; this reduces concerns about matching problems and allows us to focus on a group most

[2]This research design allows us to study the extensive margin of incarceration (the contrast between incarceration versus no incarceration), rather than the effect of a longer prison sentence, which would be the implied estimand in an RD design. Moreover, this research design has power to detect smaller effects compared to the RD. This comes at the cost of making the treatment and control groups less comparable before sentencing. However, difference-in-differences does not require treatment and control groups to be identical, it instead requires parallel trends in counterfactual outcomes: an assumption that we provide evidence to support.

likely to be affected. Like most of the prior literature, we see temporary declines in employment and wages during the first several years after incarceration but no detectable long-term impact. In all, those who were steadily employed before sentencing lost $20,000 of cumulative earnings due to incarceration. Since Social Security contributions are a total of 12.4%, this translates into $2480 less in Social Security. We find similar patterns for recidivism, measured as new criminal convictions. Incarceration leads to a short term decline but no lasting impacts.

Next, we look at incarceration's impact on the accumulation of home and car ownership. Homeownership comes from county tax and deed records and car ownership comes from records maintained by dealerships, repair shops and oil change places. Both were collected by a commercial data aggregator. We find that incarceration leads to declines in home and car ownership: an effect that appears immediately after sentencing and continues to accumulate until about year five. By seven years after sentencing, incarceration has reduced homeownership by 1.6 percentage points, relative to a 11% homeownership rate before sentencing. Incarceration reduced car ownership by 3.5 percentage points over the same time frame, relative to a 17% pre-sentencing average. The homeownership effect comes from both people selling their homes due to incarceration and people being prevented from purchasing them. Due to limits in the car ownership measure, we are only able to capture incarceration's impact on car purchasing; if it also increases sales, our estimates would understate the impact.

Homes and cars are, of course, not the entirety of a person's wealth. However, we argue that changes in home and car ownership brought about by incarceration are a decent proxy for changes in wealth. First, our measures are expected to capture the bulk of the asset side of wealth. Survey data shows that homes constitute the majority of assets for a household with an incarcerated member (Weller et al., 2022) and that houses and cars make up a large majority of assets owned by those in the bottom 50% of the wealth distribution (Kuhn et al., 2020). One potential problem is that our measures do not capture changes in debt, particularly mortgage debt, which is the largest component of household debt for low income and/or justice involved populations (Weller et al., 2022; Kuhn et al., 2020). A key assumption of our article is that, at least in the long run, home equity outpaces home debt making homeownership net beneficial for wealth. While we cannot directly verify this here, it is generally supported in the literature (Herbert et al., 2013).

We also find that incarceration affects the accumulation of human capital – a soft form of wealth. We match our sentencing data to records on college enrollment maintained by the State Council on Higher Education in Virginia. By seven years post-sentencing, incarceration has reduced the likelihood of any post-secondary education by 1 percentage point relative to a 10% pre-sentencing average.

The estimates described above come from our primary specification, which restricts to observations near the sentence score cutoff, but our results are extremely robust to variations of sample and specification. We conduct two matched difference-in-differences design among those who are recommended for prison (sentence score above cutoff) and find patterns that are almost identical to our primary specification. One uses exact matching on sentence score and offense and the other uses propensity score matching across a wide variety of covariates; treatment = incarceration. We conduct two similar matched difference-in-differences designs using those recommended for jail or probation (sentence scores below cutoff) and find qualitatively similar results but smaller effect sizes. This is consistent with the fact that most of those incarcerated receive only short jail sentences – with reduced treatment contrast we expect reduced impact. In an alternative approach, we define treatment as being incarcerated for at least 12 months and control as being not incarcerated. Using this definition of treatment, we find similar results in the below-cutoff sample as we did in the primary specification and the above-cutoff sample.

Our analysis isolates the impact of incarceration relative to conviction without incarceration. If having a conviction on the record also reduces wealth, our estimates will understate the full impact of criminal justice involvement. Descriptive evidence provides an indication that conviction may be impactful. Employment rates and wages shift down in level for those convicted but not incarcerated, going from 55% to 40% quarterly employment and from $6,000 to $4,000 quarterly wages. We also see a sharp downshift in the rate at which those convicted but not incarcerated become homeowners and a multiple-year pause in accumulation of postsecondary education. Such declines are consistent with the claim that felony conviction has lasting adverse economic impact. However, such declines could also be consistent with, for example, an adverse life event that caused both crime and economic deterioration.

Wealth inequalities are an area of active research and scholars are still working to understand its determinants. Wealth inequalities derive partially from income inequalities, which can compound over time as wealth is transmitted through generations (Saez and Zucman, 2016; Kuhn et al., 2020; Derenoncourt et al., 2023). They also derive from different savings rates, driven in part by homeownership. Owning a home is the main component of wealth among low-income populations. 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). Historical policies such as slavery and redlining also contribute to racial wealth inequality (Aaronson et al., 2021; Derenoncourt et al., 2023). Our study contributes to this literature by examining the causal link between incarceration and asset ownership using a quasi-experimental design and administrative data.

The empirical literature on incarceration and wealth 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. (2024) find that incarceration leads to a slight increase in *alternative* credit scores, primarily because it prevents new convictions via incapacitation.[3] 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.

The baseline rates of homeownership, car ownership and postsecondary education are relatively low in our sample. However, the implications of our findings are not necessarily limited to the minority subpopulation at the margins of owning a home or car or enrolling in postsecondary education. Everyone has some form of wealth, even the indigent. This could include durable goods like furniture, cellphones, clothing, or kitchenware. It could also include less durable items; Baker et al. (2024) find that households in the lowest income quintile hold about $725 in consumer goods inventories, which they measure using data on grocery, retail, and pharmacy purchases. A person who is sent to prison must arrange for the possessions they value to be sold or stored. Their assets may get lost or damaged while they are incapacitated. And, while incarcerated, they are prevented from making more investments. The disruption of incarceration is likely to have an impact on the accumulation of assets, both large and small.

Moreover, the low baseline rates of home/car ownership and postsecondary education could be the *outcome* of many decades of staggeringly high incarceration rates in certain communities. The dynamic processes responsible for wealth disparities in this country are complex. However, our research suggests that incarceration plays at least some role in the low rates of wealth among justice-involved populations.

In Section 1 we describe our setting, data, and research design. In Section 2 we show our main results, discuss potential confounds and provide robustness tests. Section 3 concludes.

[3]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 and address characteristics.

1 Background

1.1 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. Our paper leverages discontinuities in the first worksheet, which determines whether someone will be recommended for prison (sentence $\geq$ one year). If the person scores above the cutoff on the first worksheet, they are recommended for prison. If they score below the cutoff, they are recommended for jail (sentence $<$ one year) or probation. The exact recommendation will be calculated using the subsequent worksheets.

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. Given that they influence the expected outcomes of the case, they may also influence pretrial detention status.

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 the bail that has been set 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.

1.2 Data and sample

In this subsection, we describe our data, matching process, and samples. More details about the data and matching can be found in Appendix A.2-A.3- .

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. 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.[4] 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. Our recidivism measure, new criminal convictions, is also public record data gathered from county courts. 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 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 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

[4]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.

those deriving 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.

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 be typos and other inaccuracies in the identifying information. To minimize measurement error due to failed matches, we restrict our employment analyses to those with at least one quarter of W-2 earnings before sentencing.

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.

We appear to have relatively complete coverage on recidivism for most of Virginia's judicial circuits and most years. There is one judicial circuit that does not provide data to the aggregator at all and two others that did not for selected years. Among the circuit-years for which conviction data is available, there is a strong concordance with the criminal record information we received from the sentencing commission. We use this concordance as a measure of match quality, discussed more in Subsection 2.5.

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. 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. For car ownership, which is not well populated until 2005, we use a "Car sample" which includes people sentenced within 2010-2014.[5] 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. We also limit to those who had some W2 income prior to sentencing. Similarly, we restrict our "College sample" to defendants who were sentenced between 2005 and 2013, in accordance with the years of available data.

1.3 Research design

Our research leverages discontinuities in the sentence guidelines schema. However, we do so within a difference-in-differences design instead of a regression discontinuity design.[6] This allows us to focus on the extensive margin of conviction, which we think is more relevant to wealth. It also increases the treatment contrast relative to control from 8 additional months of incarceration to 16. Our main analysis sample only includes those who scored right above or right below the cutoff for a prison recommendation on the first worksheet. We normalize the scores so that everyone who scores 0 and above is recommended for prison and everyone who scores below 0 is recommended for jail or probation. Our control group consists of those who scored a -1 or a -2 and who *did not* receive a carceral sentence. Our treatment group is those who scored a 0 or 1 and who *did* receive a carceral sentence. The specification is shown below, in which we regress outcomes ($Y_{i,t}$) on dummies for the date, a treatment indicator, interactions between the two, and individual fixed effects (λ_i). Depending on the data source, the date is either at the quarterly level or years -5, -3, -1, 0, 1, 3, 5 and 7 relative to sentencing. δ are the coefficients of interest. Since we restrict to those with no prior felony convictions, this stacked difference-in-differences approach avoids the concerns about staggered rollout.

$$Y_{i,t} = \alpha + treat_i * \beta_0 + date_t * \beta_1 + date_t * treat_i * \delta + \lambda_i + \epsilon_{i,t} \quad (1)$$

[5]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 2.5.

[6]Regression discontinuity results are shown in Appendix A.5. The point estimates are qualitatively similar to the difference-in-differences specification, but somewhat smaller. This is consistent with our expectations that the extensive margin is likely to have more impact than the intensive margin. It also may be due to the fact that this design evaluates a smaller "dosage" of incarceration: nine additional months rather than 16. However, results are are generally not statistically significant.

Figure 1 shows the sentence guidelines score and its relationship with sentencing. This helps motivate our difference-in-differences design. Figure 1a shows a distribution of the sentence guidelines score, truncated at -10 and 10. There is no visible evidence of bunching around the cutoff, as there might have been if the scores were manipulated to push people above or below. Figures 1b-c show discontinuities in the sentence for those right above and below the cutoff. Those right above the cutoff are 35 percentage points more likely to receive a prison sentence (incarcerated least a year). However, most people in the control group received at least some amount of incarceration: an average of 5 months for those right below the cutoff versus an average of 13 months for those right above. A comparison of outcomes for those right above or below the cutoff, either in a regression discontinuity design or difference-in-differences, would teach us the difference between shorter and longer sentences. But if most of the impacts of incarceration accumulate within the first five months, such a comparison would understate the full impacts of incarceration. This motivates us to impose the additional sample restriction: including only those who were incarcerated in the treatment group and only those who were not incarcerated in the control group. With a 16 month average sentence in the treatment group, there is a substantial difference between treatment and control.

Table 1 compares various characteristics across the treatment and control group, using the full sample at the time of sentencing. Many of the differences are statistically significant, but small. For instance, the average age in the control group is 36.81 and the average age in treatment is 36.2: highly statistically significant but not very meaningful. The largest differences are in gender (a difference of 9 percentage points) and the likelihood of a drug crime (a difference of 15 percentage points). In Section 2.5 we employ several matching techniques to reduce these differences in observed characteristics.

There are a number of observed differences across the treatment and control group, and there are likely to be unobserved differences as well. 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 was some time-varying shock that affected treatment and control groups differently, this assumption would be undermined. We discuss this in more depth in Section 2.4.

2 Results

2.1 Exposure to prison

This section shows incarceration over time for the treatment and control groups in order to better understand the dynamics of treatment. The top row of Figure 2 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 treatment x time from the difference-in-differences specification (β_2). 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 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.

2.2 The impacts of incarceration

Employment and recidivism. Figure 3 follows the same format as Figure 2 with raw averages on the left-hand side and difference-in-differences estimates on the right hand side. The first row shows the likelihood of being employed in that quarter, the second row shows the average quarterly wages, and the third row shows cumulative income starting from year -5. We use the employment sample here, which is restricted to the 33% who were present in the Virginia Employment Commission data before sentencing. We see that incarceration leads to a sharp drop in employment and wages during the time 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 4.3 percentage points relative to a 75% presentencing average employment rate. The cumulative earnings lost due to incarceration are about $8,500. Between employer and employee contributions, 12.4% of W-2 income goes towards Social Security. $8,500 less in earnings translates into $1,054 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 $2,480.

In Appendix Figure A.6, we show employment outcomes for all defendants, not limiting to those with at least one quarter of formal sector employment prior to sentencing. As expected, incarceration has a smaller impact among a group of people with less labor-market attachment. Nonetheless, we see similar patterns: short-term declines coincident with incapacitation and no evidence of lasting effects.

The bottom row of Figure 3 shows the likelihood of being convicted for a crime committed within the previous 12 months (both misdemeanors and felonies) as recorded by our data aggregator. As can be seen, incarceration leads to a short-term decline in recent convictions, but shows no evidence of longer-term adverse effects. We can reject increases in yearly recidivism rates greater than 0.3% at the 5% level.

Wealth and human capital accumulation. Figure 4 shows how incarceration affects the accumulation of wealth. 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

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

in home/car ownership and post-secondary education are almost overlapping for the treatment and control groups before sentencing, with a noticeable divergence in trend starting at the date of sentencing.

By seven years after sentencing, incarceration has reduced property ownership rates by 1.6 percentage points over a 11% pre-sentencing mean and it has reduced car ownership rates by 3.5 percentage points over a 17% 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.7 and A.8 show incarceration's impact on home and car ownership for those who do/do not own a home or car before sentencing. 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 4 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 1.4 percentage points. Our car ownership variable, unfortunately, generally fails to capture when cars are sold. The measured impact of incarceration on car ownership should thus be interpreted as the impact on purchasing. If incarceration also increases the likelihood cars will be sold or otherwise relinquished, our estimates will understate the results.

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 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. For one, savings may be depleted by commissary expenses, visits, and collect calls. For two, 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 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.[8]

Figures 4e shows that incarceration also has a lasting effect on postsecondary education. While the treatment and control groups had a similar likelihood of having any college enrollment prior to sentencing, a gap appears at the time of sentencing and widens over the subsequent years. By seven years post-sentencing, incarceration has led to a 1 percentage point decrease in the likelihood of having any college enrollment relative to a 10% pre-sentencing mean. Incarceration also impacts the intensive margin of education. If we condition on those who had at least one semester of college enrollment prior to sentencing, we find that incarceration reduces the total number of college credits by 5 relative to a 41 pre-sentencing mean (Appendix Figure A.9). Again, the gap in the total number of college credits a person has enrolled in widens over time, even after the incapacitation period has ended. 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 time to enroll in vocational training or college.

2.3 Descriptive evidence about felony conviction

This paper is predominantly about the impacts of incarceration. However, the data provides some descriptive evidence about how outcomes evolve before and after a felony conviction. Figures 3 and 4 include time trends in outcomes for those convicted of a felony but not incarcerated (purple circles). We see small dips in employment and wages around the time of sentencing, but by year two, trends have re-stabilized. However, they re-stabilized at a substantially lower level than before conviction: shifting from about a 55% employment rate down to 40% and from an average quarterly wage of about $6,000 down to about $4,000. For homeownership, we see a noticeable break in

[8]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, which tends not to have conventional credit scores.[9] 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., 2024).

the trendline in the year after sentencing. The rate at which people accumulate new homes flattens during the year after the first conviction, then picks up again at a lower rate than previously. Conviction is also associated with a slowdown in the pursuit of college education.

The descriptive data is consistent with felony conviction having lasting adverse impacts on employment, wages, homeownership and postsecondary education. However it is not dispositive. With the exception of homeownership, the trend-change begins before sentencing. For employment and wages, the decline is coincident with the date of the arrest, suggesting a role for other aspects of criminal justice involvement beyond conviction. It's also possible that all or part of the decline is due to some other adverse life event that caused both crime and lower economic outcomes.

2.4 Considering potential confounds for incarceration results

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.

The treatment group differs from the control group in two main ways. First, the treatment group has slightly higher sentence-guidelines scores. This means they are facing slightly more serious charges and have slightly lengthier criminal records. Second, 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, such as the judge, district attorney or public defender assigned to the case. Other diferences 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 main threat to identification would be 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. We cannot rule this out, but several factors help limit concerns.

First, we are comparing two groups who are fairly similar in terms of crime severity and criminal record, 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 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 life. 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.10 shows results; there is no evidence of diverging pre-trends in year -1 or earlier.

Finally, we provide some suggestive evidence If there was some sort of adverse life shock coincident with sentencing, it might also affect household member, household members or other associates. We provide some tentative evidence about this in Figure A.19, which shows difference-in-differences estimates for the following outcomes:

homeownership and car ownership by a household member.[10] We see little evidence e that incarceration affects homeownership and car ownership for household members. Of course, there could be a "canceling out" effect whereby the negative effects caused by the adverse shock are balanced by positive spillover effects of incarceration. Or it could be that the ways in which our data aggregator defines these relationships is too noisy or broad to detect anything.

2.5 Robustness and heterogeneity

Alternative specifications: Our main specification leverages a discontinuity in Virginia's sentence guidelines schema. This approach allows us to compare outcome trends across two fairly similar groups who received very different sentences in part due to idiosyncrasies of the sentence guidelines. However, it's not clear that such a discontinuity is necessary for the research design. Our first set of robustness tests explores alternative research designs that don't leverage the discontinuity. We first look at those who were recommended for a prison sentence because they scored above the cutoff. We employ a difference-in-differences design with exact matching on sentence score and offense, with treatment defined as receiving an incarceration sentence and control defined as being convicted but not incarcerated. We match control to treatment to evaluate the impact of treatment on the treated. We limit to those who score within 10 points above the cutoff since, above that, virtually everyone is incarcerated. Results are shown in Figure 5. We find very similar results to our main specification. Results are also very similar if we use propensity score matching rather than exact matching, as shown in Appendix Figure A.11. Propensity score matching allows us to match on a wider range of characteristics, including gender, race, age, calendar year, judicial circuit, and a variety of criminal history characteristics.

We then use a similar matched difference-in-differences technique to look at the impacts of incarceration among those who scored within 10 points below the cutoff and were therefore recommended for jail or prison. Recommendations are not binding and some of these individuals received lengthy sentences of a year or more. But the median sentence for those incarcerated in this sample is only three months. Results are shown in Figure 6. We find suggestive evidence of similar patterns, but results are much more attenuated. A couple months of incarceration does not appear to be sufficient to generate detectable impacts on outcomes. We then look at the subset of individuals in this sentence score range who received longer sentences. Defining treatment as incarceration of at least 12 months and control as no incarceration, we redo the matched difference-in-differences design. Once again we find results similar to

[10]The data aggregator considers someone to be part of your household if you share an address at that date.

our main specification. Results are likewise similar if we use propensity score matching rather than exact matching (Appendix Figure A.12).

Robustness across time periods: Figure A.14 shows results across several different time restrictions: the "Full sample" (those sentenced between 2001-2014), the "Employment sample" (those sentenced between 2003-2012 and who have at least one quarter of formal sector employment before sentencing), the "Car sample" (those sentenced between 2010 and 2014). Note that varying the samples in this way means that some outcomes will be missing for some people either in the early or late years. As long as they are missing equivalently for both treatment and control groups this is not necessarily a problem. As can be seen, results are similar across the specifications, although the confidence intervals on the employment are larger than the other samples, consistent with the smaller sample size.

Main specification with matching: Figure A.15 compares our main results to two other specifications which incorporate matching to reduce differences in observed characteristics across treatment and control. Both specifications match the treatment sample (sentence guidelines score in {0,1} and incarcerated) to observations from the control group and then use difference-in-differences on the matched groups.[11] One technique uses exact matching based on offense and gender (the two characteristics that differed most across treatment and control) and the other uses nearest neighbor matching, where distance is measured by a propensity score using offense type, gender, race, number of criminal convictions, and age as predictors. We then run the difference-in-differences regressions with these match-weighted samples. The matching techniques reduce our effective sample size somewhat and therefore increase the confidence intervals. But overall, the results look quite similar.

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.

[11] More details about the matching procedure can be found in Appendix A.4.

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.16. Again, results are almost identical across the three groups.

Heterogeneity across race and criminal history: We evaluate heterogeneity across race and across those who do/don't have 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 homeownership for White people and a larger effect on postsecondary education for Black people. 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.

3 Conclusion

This paper provides new evidence on how incarceration affects wealth. We first show that incarceration's impact on employment, wages, and recidivism is predominantly short-term and consistent with incapacitation. 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 homeownership, car ownership, and education.

References

Aaronson, Daniel, Daniel Hartley, and Bhashkar Mazumder, "The Effects of the 1930s HOLC "Redlining" Maps," *American Economic Journal: Economic Policy*, November 2021, *13* (4), 355–92.

Aizer, Anna and Joseph J. Jr. Doyle, "Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges," *Quarterly Journal of Economics*, May 2015, *130* (2), 759–803. MAG ID: 2118051501.

Alexander, M., *The New Jim Crow: Mass Incarceration in the Age of Colorblindness*, New Press, 2010.

Avenancio-Leon, Carlos and Abhay Aneja, "No Credit For Time Served? Incarceration and Credit-Driven Crime Cycles," Technical Report, Working Paper October 2021.

Baker, Scott R, Stephanie Johnson, and Lorenz Kueng, "Financial Returns to Household Inventory Management," *Journal of Financial Economics*, 01 2024, *151.*

Derenoncourt, Ellora, Chi Hyun Kim, Moritz Kuhn, and Moritz Schularick, "Wealth of Two Nations: The U.S. Racial Wealth Gap, 1860-2020*," *The Quarterly Journal of Economics*, 09 2023, *139* (2), 693–750.

Enamorado, Ted, Benjamin Fifield, and Kosuke Imai, "Using a probabilistic model to assist merging of large-scale administrative records," *American Political Science Review*, 2019, *113* (2), 353–371.

Farrar-Owens, Meredith, "The evolution of sentencing guidelines in Virginia: An example of the importance of standardized and automated felony sentencing data," *Federal Sentencing Reporter*, 2013, *25* (3), 168–170.

Garin, Andrew, Dmitri Koustas, Carl McPherson, Samuel Norris, Matthew Pecenco, Evan Rose, and Yotam Shem-Tov, "The Impact of Incarceration on Employment and Earnings," Working Paper 2022.

Goldsmith-Pinkham, Paul, Maxim Pinkovskiy, and Jacob Wallace, "The great equalizer: Medicare and the geography of consumer financial strain," Technical Report, National Bureau of Economic Research 2023.

Hamilton, Darrick and William Darity, "Race, Wealth and Intergenerational Poverty," *The American Prospect*, August 2009.

Harding, David J, Jeffrey D Morenoff, Anh P Nguyen, and Shawn D Bushway, "Imprisonment and labor market outcomes: Evidence from a natural experiment," *American Journal of Sociology*, 2018, *124* (1), 49–110.

Herbert, Christopher E., Daniel T. McCue, and Rocio Sanchez-Moyano, "Is Homeownership Still on Effective Means of Building Wealth for Low-Income and Minority Households? (Was it Ever?)," Technical Report, Joint Center for Housing Studies at Harvard University September 2013.

Humphries, John Eric, Aurelie Ouss, Megan Stevenson, and Winnie van Dijk, "Criminal Justice and Alternative Credit," Technical Report, Working Paper March 2024.

Imbens, Guido and Stefan Wager, "Optimized regression discontinuity designs," *Review of Economics and Statistics*, 2019, *101* (2), 264–278.

Kling, Jeffrey R., "Incarceration Length, Employment, and Earnings," *American Economic Review*, June 2006, *96* (3), 863–876.

Kolesár, Michal and Christoph Rothe, "Inference in regression discontinuity designs with a discrete running variable," *American Economic Review*, 2018, *108* (8), 2277–2304.

Kuhn, Moritz, Moritz Schularick, and Ulrike I. Steins, "Income and Wealth Inequality in America, 1949-2016," *Journal of Political Economy*, 2020, *128* (9), 3469–3519.

LaBriola, Joe, "Housing Market Appreciation and the White-Black Wealth Gap," *Social Problems*, 07 2024.

Loeffler, Charles E., "Does Imprisonment Alter the Life Course? Evidence on Crime and Employment From a Natural Experiment," *Criminology*, 2013, *51* (1), 137–166.

__ **and Daniel S. Nagin**, "The impact of incarceration on recidivism," *Annual Review of Criminology*, 2022, *5*, 133–152.

Maroto, Michelle and Bryan L Sykes, "The Varying Effects of Incarceration, Conviction, and Arrest on Wealth Outcomes among Young Adults," *Social Problems*, 07 2019, *67* (4), 698–718.

Mueller-Smith, Michael, "The Criminal and Labor Market Impacts of Incarceration: Identifying Mechanisms and Estimating Household Spillovers," Working Paper August 2015.

Norris, Samuel, Matthew Pecenco, and Jeffrey Weaver, "The Effects of Parental and Sibling Incarceration: Evidence from Ohio," *American Economic Review*, September 2021, *111* (9), 2926–63.

Rose, Evan and Samuel Noris, "Laffer's Day in Court: The Revenue Effects of Criminal Justice Fees and Fines," Technical Report, Working Paper 2024.

Saez, Emmanuel and Gabriel Zucman, " Wealth Inequality in the United States since 1913: Evidence from Capitalized Income Tax Data *," *The Quarterly Journal of Economics*, 02 2016, *131* (2), 519–578.

Slutzky, Pablo and Sheng-Jun Xiu, "The Financial Consequences of Pretrial Detention," Working Paper 2023.

Sykes, Bryan L. and Michelle Maroto, "A Wealth of Inequalities: Mass Incarceration, Employment, and Racial Disparities in U.S. Household Wealth, 1996 to 2011," *RSF: The Russell Sage Foundation Journal of the Social Sciences*, 2016, *2* (6), 129–152.

Walmsley, Roy, "World Prison Population List, 11th Edition," Technical Report, Institute for Criminal Policy Research July 2023.

Weller, Christian E., Akua Amaning, and Rebecca Vallas, "America's Broken Criminal Legal System Contributes to Wealth Inequality," Technical Report, Center for Economic Progress December 2022.

Zaw, Khaing, Darrick Hamilton, and William Darity, "Race, Wealth and Incarceration: Results from the National Longitudinal Survey of Youth," *Race and Social Problems*, 02 2016, *8*.

Table 1: Comparing treatment and control

	Control group		**Treatment group**		**Difference**
	Mean	SD	Mean	SD	Mean difference
Months sentence	0.0	0.0	15.9	23.7	-15.9***
Demographics					
Age	36.8	10.9	36.2	10.4	0.6***
Black	0.39	0.49	0.42	0.49	-0.03***
Hispanic	0.01	0.11	0.02	0.14	-0.01***
Female	0.28	0.45	0.19	0.39	0.09***
Type of offense					
Violent crime	0.166	0.372	0.175	0.380	-0.009
Robbery	0.018	0.133	0.028	0.164	-0.010***
Drug	0.156	0.363	0.302	0.459	-0.146***
Assault	0.148	0.356	0.148	0.355	0.001
Larceny	0.118	0.323	0.136	0.343	-0.017***
Criminal history					
On probation/parole	0.283	0.451	0.270	0.444	0.013
Total misdemeanor convictions	10.974	13.505	11.105	13.308	-0.131
Outcomes					
Car owner	0.176	0.381	0.165	0.371	0.011
Any post-secondary education	0.1	0.3	0.1	0.3	0.0
Home owner	0.109	0.312	0.111	0.314	-0.002
Observations	3518		10264		13782
Employed	0.75	0.44	0.75	0.43	-0.00
Wage	22676	39943	21073	31853	1603**
Observations	2622		7332		9954

Note: This table presents summary statistics for treatment and control groups in the year prior to sentencing. 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 and employment data are restricted to individuals who had wages prior to sentencing and represent the average in the year prior to sentencing.

Table 2: Main specification coefficients

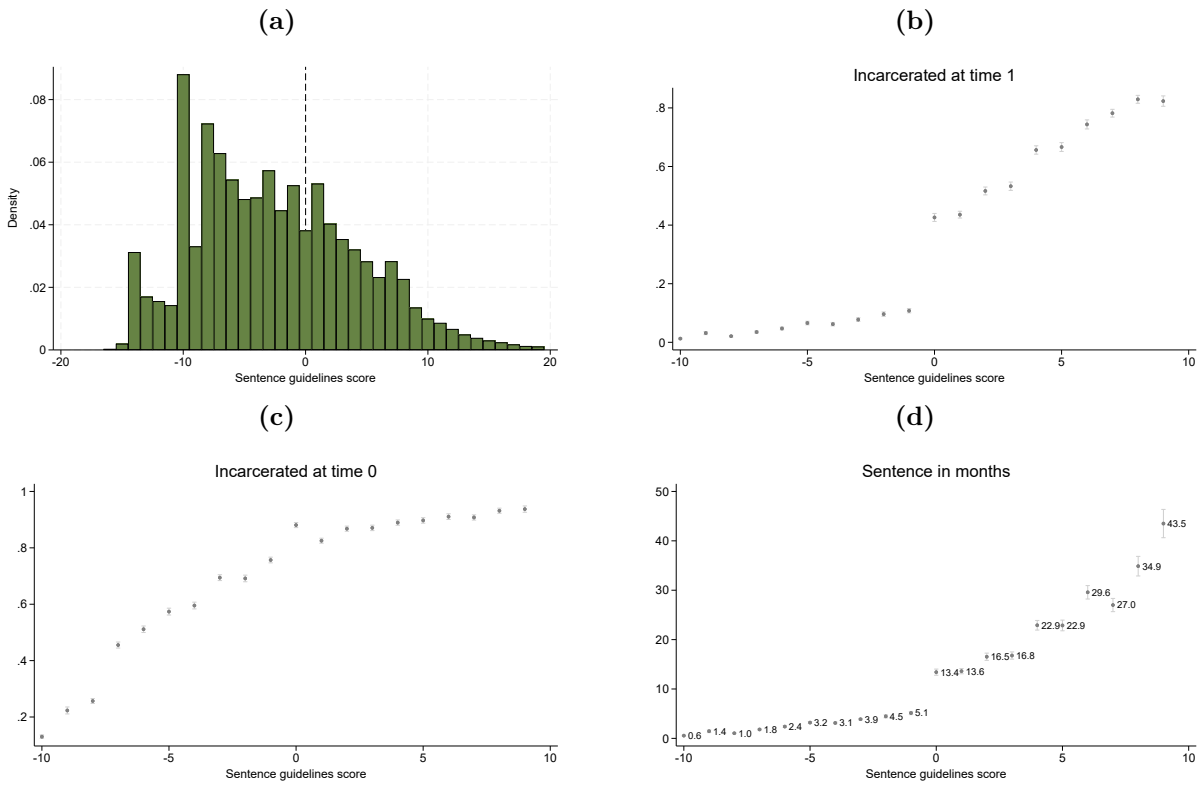
	(1) Incarcerated	(2) Employed	(3) Wage	(4) Cum. earnings	(5) Recent conviction	(6) Homeowner	(7) Car owner	(8) Any post-secondary education
Year -5 x treat	0.000	-0.022	-1709.971	3685.141	0.003	-0.006	0.002	0.002
	(.)	(0.023)	(1549.707)	(5280.485)	(0.012)	(0.005)	(0.009)	(0.004)
Year -3 x treat	0.000	-0.003	-1474.278	820.937	0.001	0.000	-0.000	0.003
	(.)	(0.021)	(1347.297)	(2785.781)	(0.012)	(0.004)	(0.006)	(0.003)
Year 0 x treat	1.000	-0.065***	-1441.565	-1983.178	-0.033**	-0.005*	-0.009	-0.001
	(.)	(0.019)	(989.388)	(1066.683)	(0.013)	(0.003)	(0.005)	(0.002)
Year 1 x treat	0.509***	-0.142***	-4945.802**	-7470.593***	-0.163***	-0.009**	-0.019**	-0.005
	(0.005)	(0.022)	(1516.278)	(2009.319)	(0.012)	(0.003)	(0.006)	(0.003)
Year 3 x treat	0.096***	-0.002	-1016.688	-10014.978**	-0.033**	-0.017***	-0.027**	-0.005
	(0.003)	(0.023)	(1473.691)	(3671.255)	(0.012)	(0.005)	(0.009)	(0.004)
Year 5 x treat	0.036***	0.022	1317.364	-9903.240	-0.014	-0.021***	-0.035***	-0.009*
	(0.002)	(0.023)	(1427.820)	(5395.152)	(0.012)	(0.005)	(0.010)	(0.004)
Year 7 x treat	0.014***	-0.002	899.816	-8457.970	-0.016	-0.016**	-0.035***	-0.010*
	(0.001)	(0.025)	(1508.732)	(7028.470)	(0.012)	(0.006)	(0.011)	(0.005)
N	109,990	26,544	26,544	26,544	109,990	109,990	40,118	72,248
Treat. mean at year -1	0.000	0.748	20061.596	108980.340	0.462	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 includes fixed effects at the person and year level. "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 and with at least one quarter of formal sector employment prior to sentencing. 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.

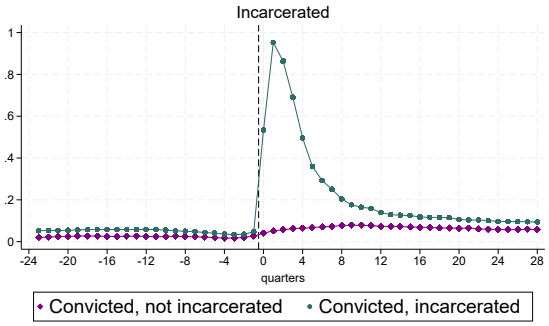
Figure 1: Sentence guidelines score: histogram and relationship with sentencing



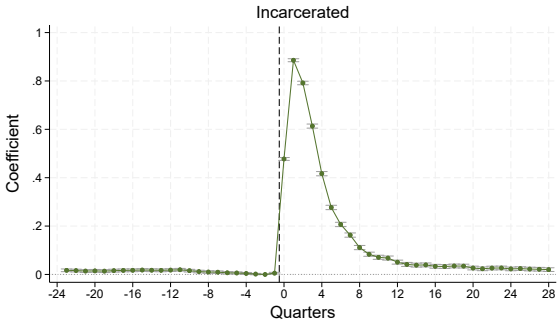
Note: The figure on the top left shows a histogram of the sentence guidelines score, truncated at -20 and 20. Everyone who scores above a zero is recommended for prison. The remaining figures plot the probability of having a sentence that is at least one year, the probability of having any carceral sentence, and the sentence length in months against the sentence guidelines score.

Figure 2: Incarceration

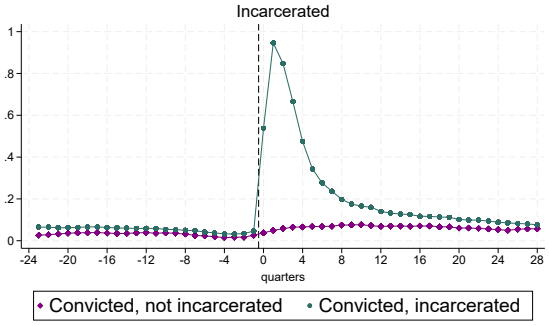
(a) Only felony sentences (2001-2014)



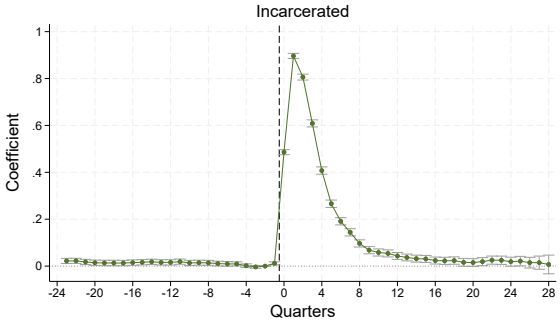
(b) Only felony sentences (2001-2014)



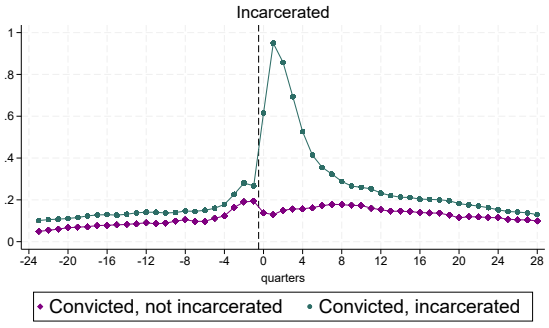
(c) Only felony sentences (2012-2014)



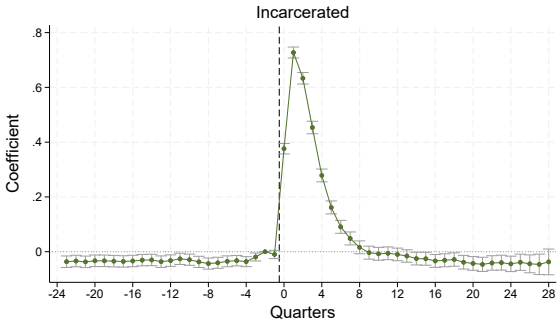
(d) Only felony sentences (2012-2014)



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

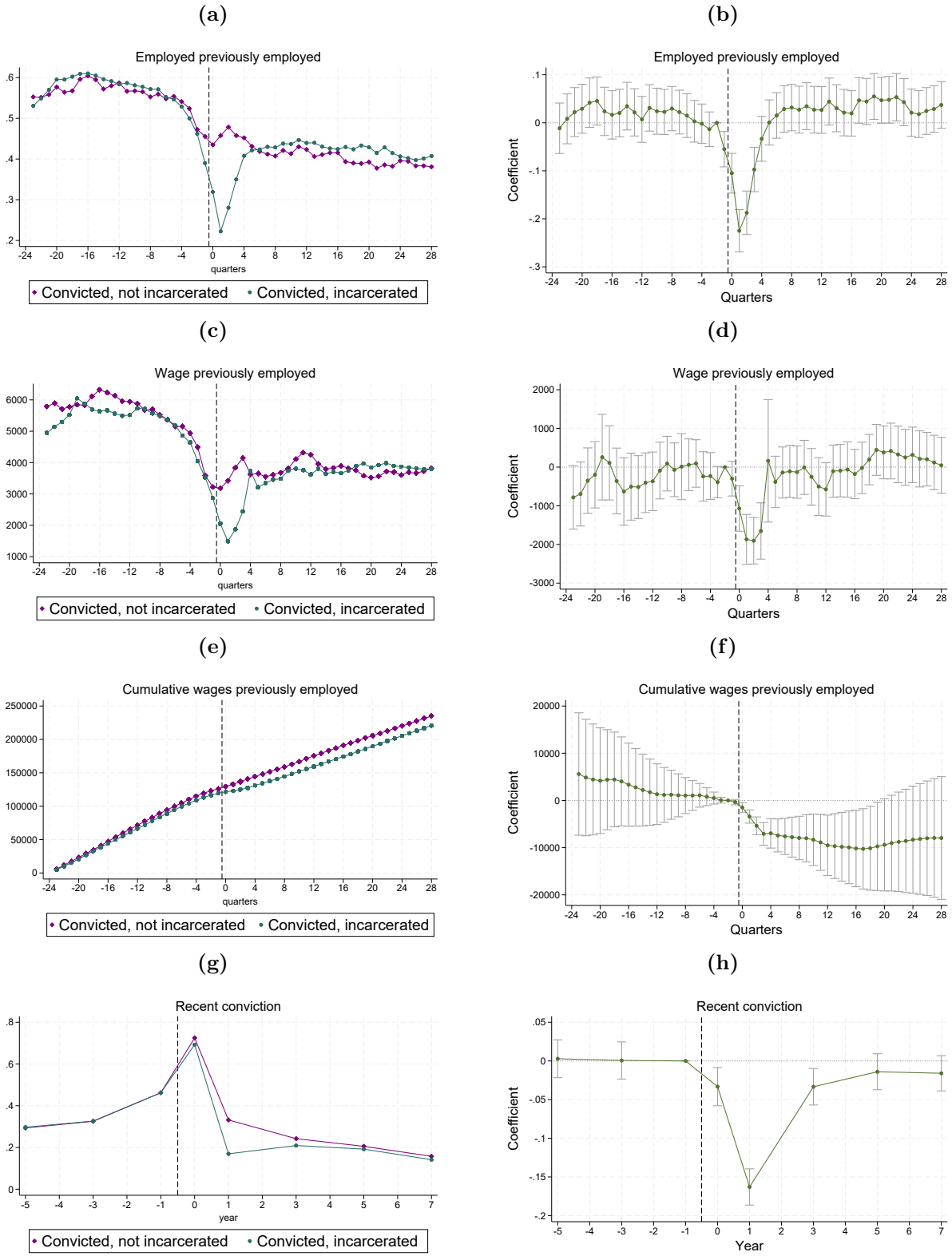


(f) Including pretrial detention, probation revocation and misdemeanor sentences (2012-2014)



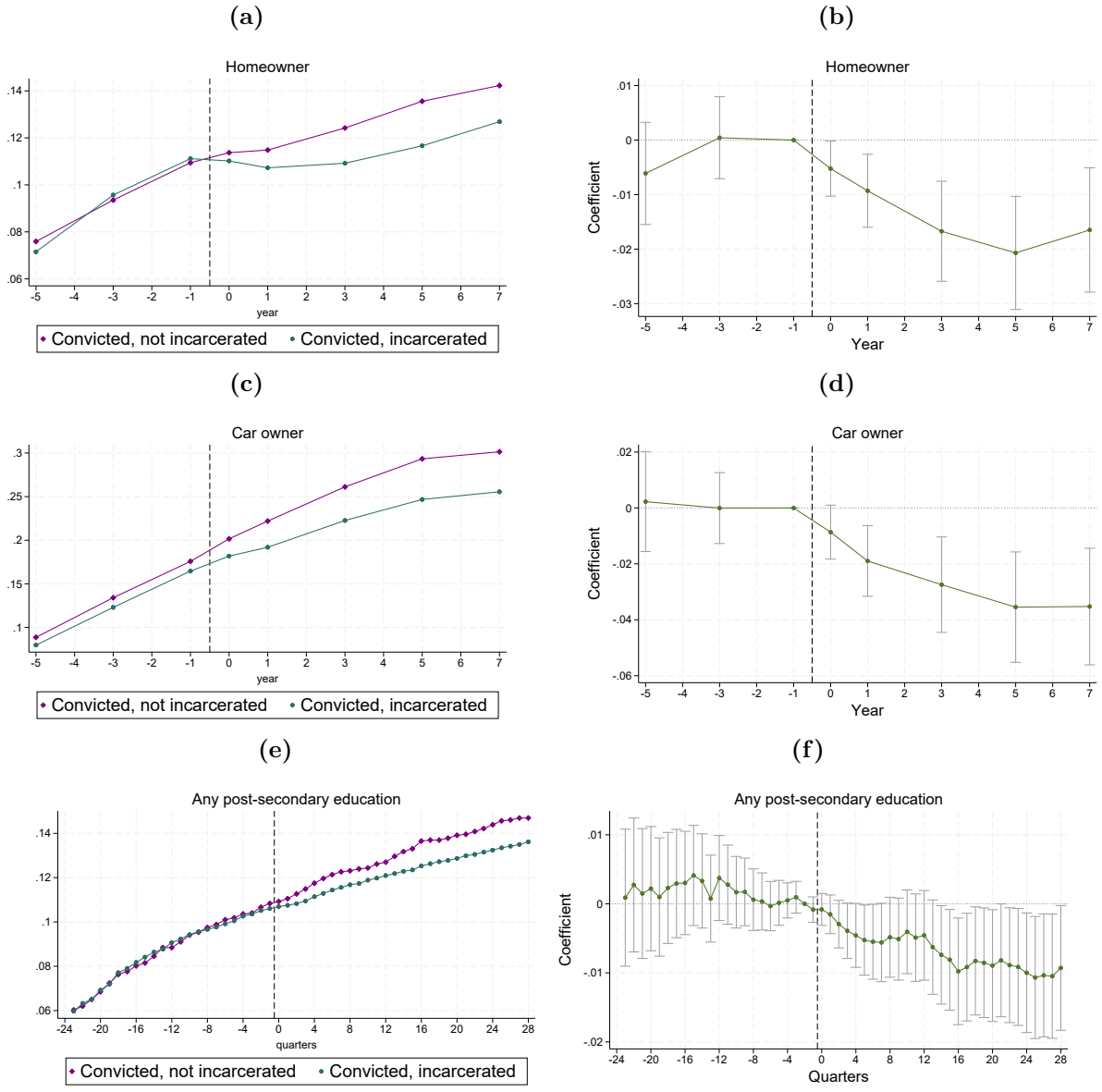
Note: The figures on the left shows the yearly likelihood of being incarcerated for those whose incarceration-length score is right above the cutoff {0,1} and actually were incarcerated compared to those who scored right below the cutoff {-2,-1} and were not incarcerated. The figures on the right show 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 3: Employment, wages, earnings and recidivism



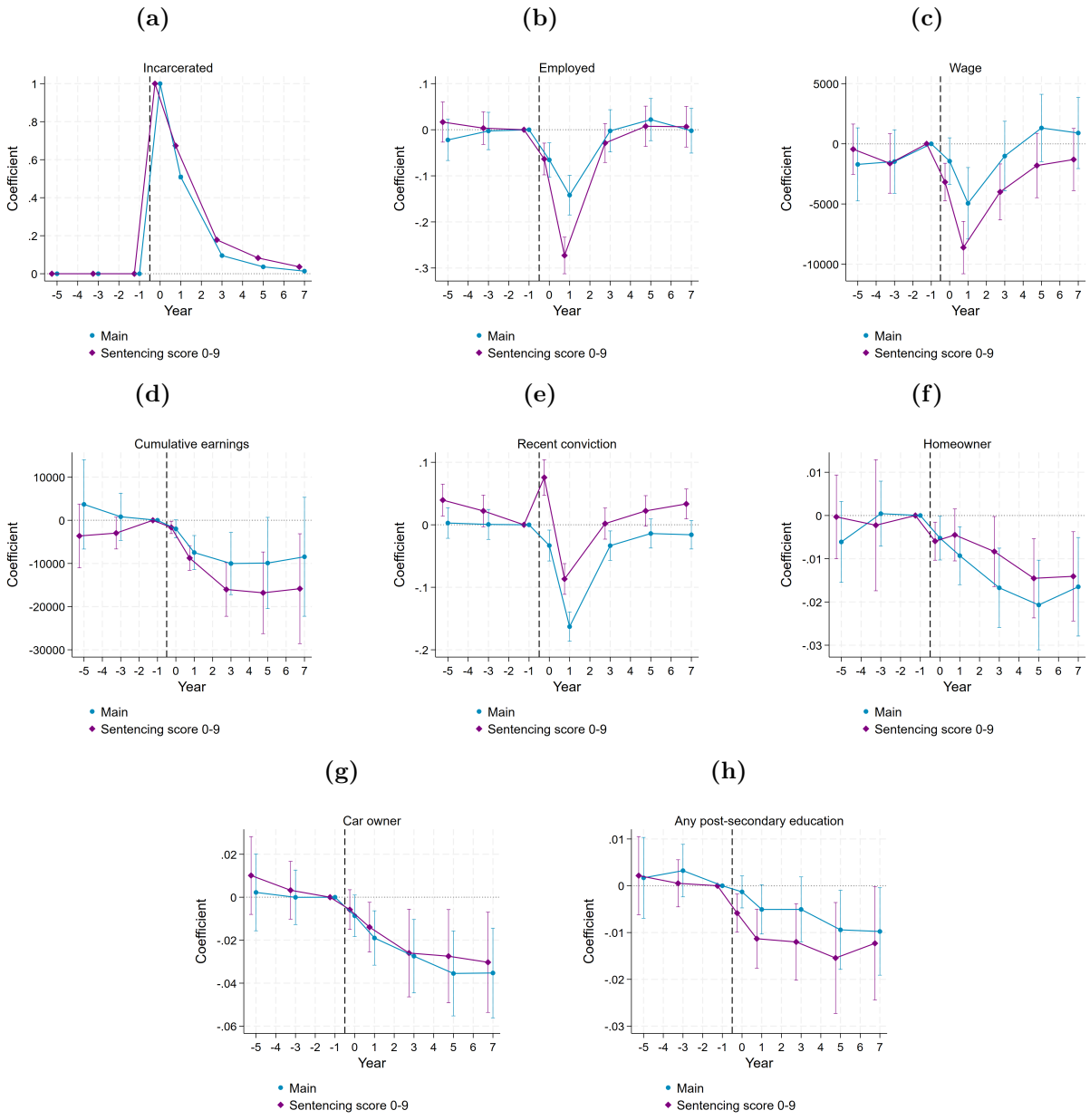
Note: The figures on the left show average outcomes for those whose incarceration-length score is right above the cutoff {0,1} and who were incarcerated versus those who scored right below the cutoff {-2,-1} and were not incarcerated. The figures on the right show difference-in-differences estimates. The samples restrict to people at least 23 years old with no prior felony convictions. The sample for employment and wage outcomes also restricts to those with at least one quarter of formal sector employment prior to sentencing and includes people sentenced in 2003-2012. The sample for recent criminal convictions includes those sentenced in 2001-2014.

Figure 4: Wealth and human capital accumulation



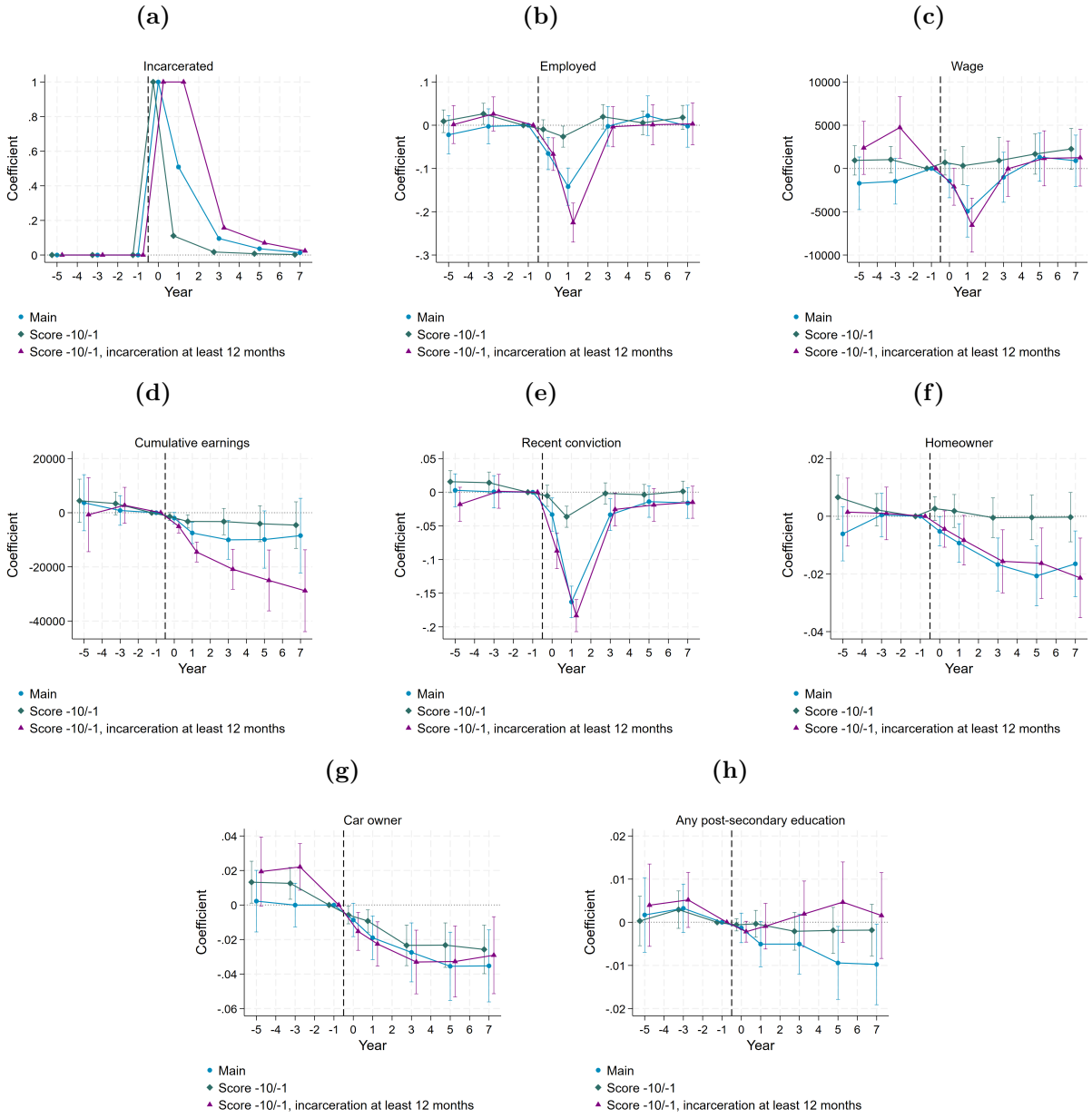
Note: The figures on the left show average outcomes for those whose incarceration-length score is right above the cutoff {0,1} and who were incarcerated versus those who scored right below the cutoff {-2,-1} and were not incarcerated. The figures on the right show difference-in-differences estimates. The 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: matched difference-in-differences among those who received a prison recommendation



Note: These figures illustrate the comparison between our main specification and a specification restricted to positive scores (from 0 to 9). In the latter specification (purple diamonds), observations are matched using exact matching. The treatment group consists of defendants with scores within this range who were incarcerated, while the control group includes those who were not incarcerated. The samples for the labor market outcomes are limited to those sentenced in 2003-2012 and with at least one quarter of formal sector employment prior to sentencing. 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 individuals who were at least 23 years old at the time of sentencing and had no prior felony convictions.

Figure 6: Alternative specification: matched difference-in-differences among those who received a recommendation for jail or probation



Note: These figures illustrate the comparison between our main specification and a specification restricted to negative scores (from -10 to -1). In the latter specification (green diamonds), observations are matched using exact matching. The treatment group consists of defendants with scores within this range who were incarcerated, while the control group includes those who were not incarcerated. We also present another specification with varied treatment (purple triangles): here, the treatment group comprises defendants incarcerated for *at least 12 months*, while the control group includes those who did not get incarcerated. The samples for the labor market outcomes are limited to those sentenced in 2003-2012 and with at least one quarter of formal sector employment prior to sentencing. 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 individuals who were at least 23 years old at the time of sentencing and had 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.[12]

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.[13] 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.[14] 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.

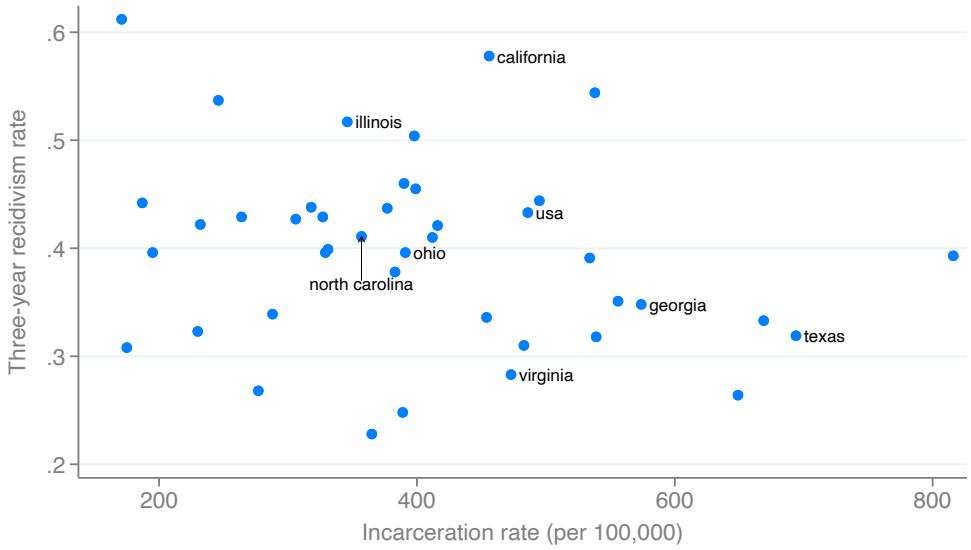
[12]This data can be found at https://www.pewtrusts.org/-/media/legacy/uploadedfiles/pcs_assets/2011/pewstateofrecidivismpdf.pdf, https://bjs.ojp.gov/content/pub/pdf/p04.pdf, and https://www2.fbi.gov/ucr/cius_04/.

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

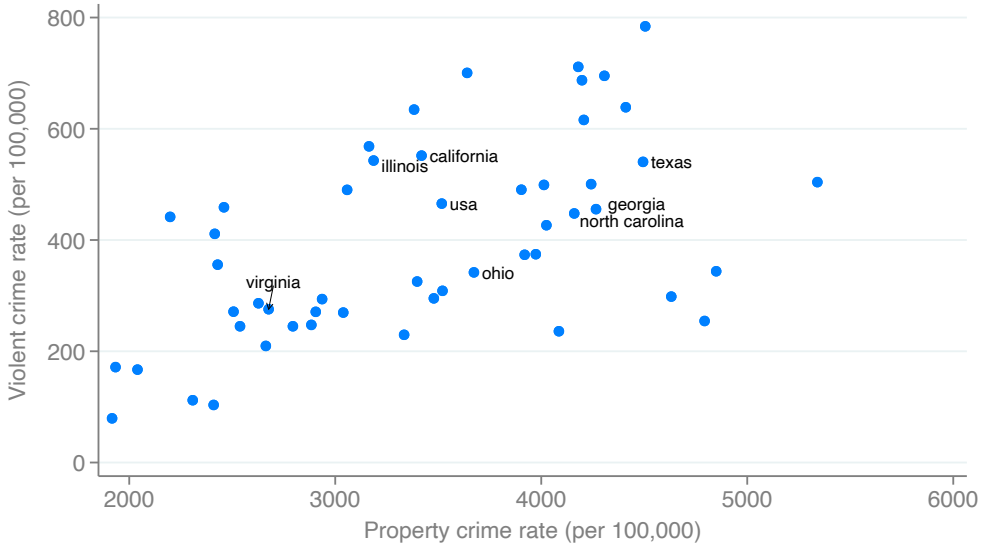
[14]These ratios read as follows: If out of every 100,000 Hispanic residents 200 are incarcerated, and out of every 100,000 White residents 400 are incarcerated, the Hispanic:White ratio is 0.5.

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

(a) 3yr-Recidivism rates vs incarceration rates



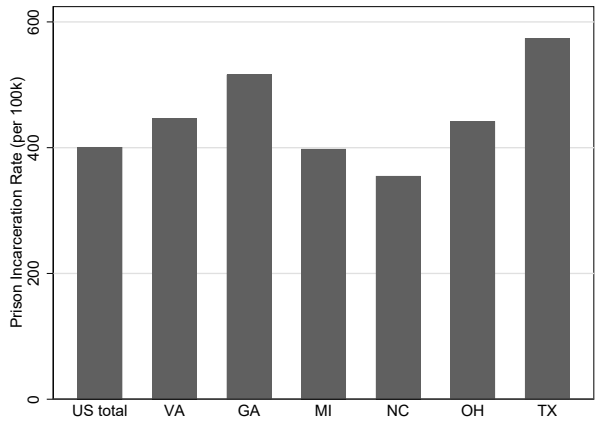
(b) Violent crime rates vs property crime rates



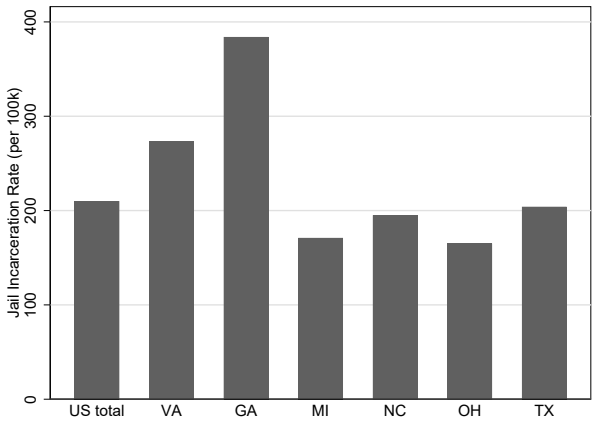
Note: Scatterplots of 2004 incarceration rates, 2004 three-year recidivism rates, and 2004 crime rates. Data gathered from the Pew Center, Bureau of Justice Statistics, and the FBI Uniform Crime Reporting Program.

Figure A.2: Incarceration rates

(a) Prison Incarceration Rates

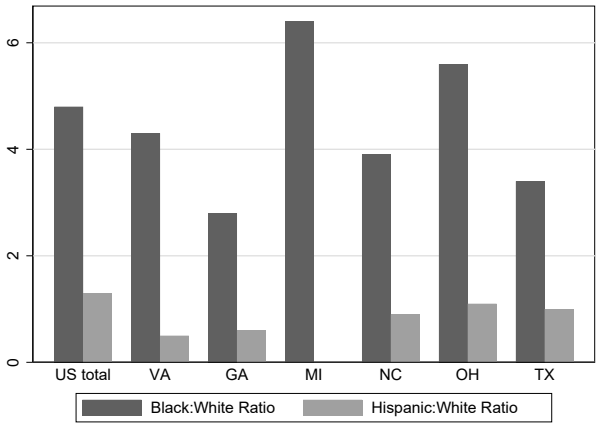


(b) Jail Incarceration Rates



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).

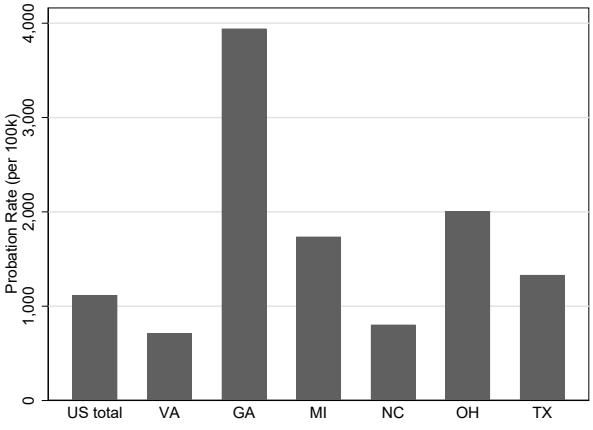
Figure A.3: Racial and ethnic composition of the imprisoned population



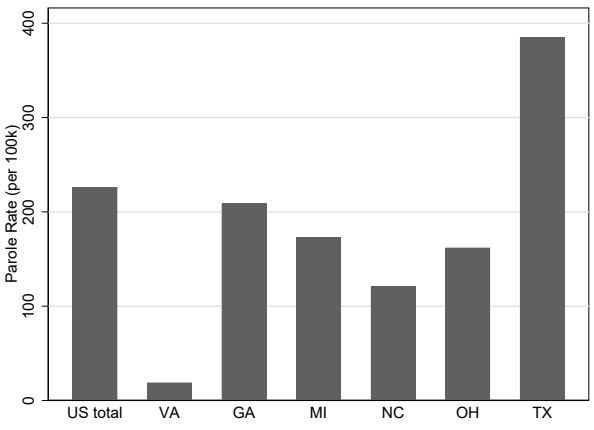
Note: This figure plots the ratio of incarceration rates for Black vs White residents (darker bars) and Hispanic vs White residents (lighter bars), for Virginia, the U.S. overall, Georgia, Michigan, North Carolina, Ohio, and Texas in 2019. Data from sentencingproject.org, used to calculate incarceration 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 Data

The data aggregator provided us with two data sets. PB is used for marketing purposes and allows companies to target individuals for advertising or special offers. RV is used for developing alternative credit scores for use in automotive, telecommunications, payday lending and banking industries. Here we describe the data in more details along with the discussion of why we selected the outcome variables that we did.

PB:

- PB data includes characteristics on three levels: the individual, the household (which includes the individual) and householdmember/associates of the individual. The household includes everyone living at the same address at one time, including the individual. householdmember are defined as people with the same last name living at the same address. Associates are people that share the same address at one point or are registered together as business owners. The definition of these relationships rely heavily on address history. However, the variables pertaining to address in our vendor's data is not very well populated. 40% of the sample has no recorded address and 60% has only one recorded address. This makes us skeptical that household members, householdmember and associates are well defined. We make little use of this data for that reason.
- The data on the individual includes variables pertaining to address history, property ownership, derogatory court records, business and professional licenses, post-secondary education, marital status and whether they are deceased. Many of these outcomes are rare in the sample, either because the data is not available or because the outcome is simply infrequent. For instance:
  - Only 1.74% are listed as married.
  - Only 1.2% are listed as deceased.
  - Only 1.94% are listed as having a professional license and only 2.91% are listed as being associated with a business via business registration.
  - As previously noted, 40% are listed as having no recorded address.

  We do not have the statistical power to be able to detect impacts on rare outcomes and, at least for some of these, we have reason to believe there are substantial missing-data problems.
- Among the variables connected with property ownership, we consider home and car ownership to be the most interesting. While the data also provides information about ownership of watercraft or airplanes, this is uncommon.
- Among the derogatory court records, we focus for this paper on new criminal convictions. The data also contains information about evictions and civil court liens/judgments. We are working on a separate research paper about the impact of incarceration and conviction on credit and include these outcomes there.
- Finally, the data includes some imputed variables, such as their estimated income range and banking experience. Given the opacity of these constructed measures, we exclude them from our analysis.

RV:

- The RV data contains five alternative credit scores: automotive, telecommunications, banking, short-term, and cross-industry. The data included in this data set are inputs to the credit scores and are therefore governed by FCRA. We evaluate the impact of incarceration and conviction on credit in other work.
- RV contains some of the same data points as PB, such as property ownership and derogatory records. However, the data is slightly less well populated. Our vendor does not provide alternative credit scores unless there is a sufficient amount of data to calculate these scores. In addition to the 4% of the sample that did not find a match in our vendor's data, an additional 7% were marked as "insufficient data" in the RV data set; their attributes were repressed. For this reason, we use PB data when available. The one exception is criminal convictions. The PB data was somewhat inconsistent in marking the date associated with a criminal conviction. Sometimes it was the offense date, sometimes the arrest date, sometimes the conviction date. The RV data, which is more highly regulated, consistently marked the date of conviction to the date of *actual* conviction. We therefore prefer this measure when looking at recidivism.
- Most of the variables included in RV that are not present in PB were composite indexes, such as the individual's willingness or ability to repay a loan. These are estimated using a proprietary model based mostly on derogatory public records and address history. Given the capacity of these composite indexes, we do not consider them as outcomes in our analysis.
- RV contains information about voter registration, however felons lose the right to vote in Virginia and less than 5% of our sample is marked as registered to vote.
- RV contains information about mortgage type (fixed versus adjustable) but this variable is only populated for 11% of homeowners.

A.3 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 of social
  - 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 of social

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 of social

  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 of social

A.4 Matching combined with diff-diff as robustness

We employ matching methods to minimize differences in observed characteristics between treatment and control groups, as elaborated in Subsection 2.4. Our approach incorporates two distinct matching techniques: exact matching and nearest neighbor matching using propensity score distance with replacement. These methods are implemented using the *matchit* command in the *MatchIT* library, specifying "exact" and "nearest" as method options.

Exact matching aims to establish pairs of treated and control individuals who are identical on selected covariates, thereby ensuring perfect balanced distributions of covariates between the groups. We utilize offense type and gender as covariates for exact matching, since these were the variables most out of balance. The technique generates weights for use in the difference-in-differences regression.

The second technique is nearest neighbor matching with the propensity score as the distance metric. The propensity score is built using offense type, gender, race, number of criminal convictions, and age. The inclusion of additional covariates enhances the flexibility of this method without introducing computational burdens. Weights are derived based on the distance between individuals in terms of their propensity scores, which are estimated probabilities of receiving the treatment variable based on observed covariates.

Given the significant sample size disparities between the treatment and control groups, we employ replacement in nearest neighbor matching. This practice ensures that each individual in the control group has an opportunity to be matched with a suitable counterpart from the treatment group, mitigating the impact of sample size imbalances on the validity of our analysis.

A.5 Regression discontinuity

In this Appendix, we provide alternative evidence on the impacts of incarceration using a regression discontinuity design. Although judges have the final say over sentencing in Virginia, each person convicted of a felony gets a guidelines-recommended sentence which is calculated using a series of worksheets. Sentence recommendations change discontinuously at some scores. Exploiting one of these different discontinuities, we estimate the effects of incarceration on the intensive margin (sentence length).

Sample. For these analyses, we particularly drop offense categories in which the seriousness of the offense mandates a recommended prison sentence, since we don't have variation at the margins for these cases. The omitted offense categories include murder and voluntary manslaughter, rape, aggravated DWI, some more serious drug offenses more serious types of assault, burglary, robbery, and other miscellaneous offenses. These constitute roughly 25% of the sample.

Calculating the sentencing score. The Virginia sentence guidelines were developed in the 1980s to harmonize practices across judges and reduce disparities across similar defendants (Farrar-Owens, 2013). Information on the sentence guidelines is available to all parties during negotiations. The first worksheet determines whether a person convicted of a felony is recommended for prison (more than one year of incarceration). This worksheet, called "Worksheet A", consists of a series of questions pertaining to the offense and criminal history. Each question has a number of points associated with it; the sum of these points is the "incarceration-length score." Those who score above a cutoff are recommended for prison. Those who score below the cutoff are recommended for probation or jail, where recommended jail sentences are under a year in length.

Offenses are sorted into 16 offense categories, and each category has a slightly different worksheet. The worksheets are filled out by a probation officer or a prosecutor and then given to a judge during sentencing. The worksheet package contains a cover sheet, which has a summary of information related to the case. The guidelines-recommended sentence and range is displayed prominently on the cover sheet.

Empirical approach. To conduct this analysis, we compare people who score just below and just above our worksheet thresholds. The main assumption for this to yield causal estimates of the effects of tougher sentences is that potential outcomes are smooth across the cutoff. This might not hold if, for example, legal actors are able to manipulate the scores. Three institutional details in our setting help mitigate this concern. First, the sentence guidelines are discretionary, not binding. Thus it is not necessary for legal actors to manipulate the score to achieve a certain sentence. Second, legal actors may pay more attention to the final recommended sentence as calculated on Worksheet B or Worksheet C, rather than the intermediary score calculated on Worksheet A. Therefore, concerns of manipulation on the incarceration-length score (derived from Worksheet A) might not be as strong, simply because it's less salient. Third, from the legislator's standpoint, the goal of these worksheets was to reduce unjustified disparities. Therefore, it seems unlikely that the sharp sentencing discontinuities observed at the cutoff in the incarceration-length score were created on purpose. We provide below evidence that there is no change in characteristics at the cutoff.

An additional challenge in our setting is that the running variable is discrete, generating difficulties in estimating accurate confidence intervals. To address this, we adopt the technique developed by Kolesár and Rothe (2018) – "K&R" henceforth – designed specifically for regression discontinuity with a discrete running variable. As in other RD settings, we want to estimate a function of the form:

$$y_{i,s} = \beta * \mathbb{1}(s \geq 0) + f(s) * \mathbb{1}(s \geq 0) + g(s) * \mathbb{1}(s < 0) + \epsilon \tag{2}$$

where $y_{i,s}$ is the outcome of the person in case i having obtained a sentencing score of s.[15] Our main coefficient of interest is β. The challenge is to estimate the form of $f(.)$ and $g(.)$, especially close to the cutoff.

Typical approaches in RD consist of fitting specifications on either side of the cutoff. However, these approaches assume that bias can be minimized by reducing the bandwidth. In the discrete setting, the bandwidth cannot asymptotically go to zero, because there are no observations in between each discrete bin. The scarcity of points close to the cutoff could lead to misspecification error: in the absence of additional assumptions, it is unclear what the behavior of the functions of interest would be close to the cutoff, resulting in misspecified confidence intervals.

K&R offer an approach to determine confidence intervals, by estimating plausible behaviors of the potential outcome function close to the cutoff based on its behavior at other points. By fitting a linear regression through points at the left and right of the cutoff, we might be missing non-linearities closer to the cutoff. We cannot use observations "very close" to the cutoff to estimate this, since the discrete nature of the score hinders the credibility of limit arguments. K&R determine credible bounds for the second derivatives of $f(.)$ and $g(.)$ close to the cutoff, based on its behavior further from the cutoff, to estimate the magnitude of plausible deviations from the linear estimation. We need to choose a parameter K which is the upper bound of the absolute value of the second derivative of the conditional expectation function. This tells us how quickly the functions $f(.)$ and $g(.)$ can change. Using K, we can construct confidence intervals that reflect how far away from the linear approximation the true conditional expectation function might be based on its expected behavior at other points.

To choose K, we follow the approach developed by Imbens and Wager (2019) and implemented by Goldsmith-Pinkham et al. (2023). We take a large window of nine points to the left of the cutoff and fit a quadratic function of the sentencing score to the data.[16] We take the coefficient on the quadratic term, take the absolute value, and multiply it by four. Intuitively, this means that we allow the rate of change (2nd derivative) of $f(.)$ at the cutoff to be two times that of the estimated rate of change between -9 and -1 from a second order polynomial. When we estimate the optimal bandwidth, we obtain an optimal choice of equal to or close to 5 for many of our main outcomes. In order to keep bandwidths constant across outcomes and time periods, we use a bandwidth of 5 in all specifications.

RD Results Table A.1 examines whether there are discontinuities around the cutoff in demographics, offense, and criminal record in the year prior to sentencing. For all

[15] As a reminder, the sentencing score is either the incarceration-length score or the probation/jail score.
[16] We focus on the left of the cutoff, since we have more observations there.

regression discontinuity results we use the technique described above (Kolesár and Rothe, 2018). The point estimates are generally small, and most are not statistically significant. Table A.2 shows how scoring about the cutoff affects sentences. Scoring just above the cutoff increases the probability of a prison sentence (at least 12 months) by 35 percentage points and extends the total sentence length by 7.6 months. Those just below the cutoff receive an average sentence of 5 months.

However, the cutoff has a more modest impact (9 percentage points) on the likelihood of any carceral sentence. Therefore, the RD specification is more oriented towards identifying the impact of the intensive margin of incarceration rather than the extensive.

Table A.3 shows how scoring above the cutoff affects outcomes seven years post-sentencing. The point estimates are qualitatively similar to the difference-in-differences specification, but somewhat smaller. This is consistent with our expectations that the extensive margin is likely to have more impact than the intensive margin. It is also likely due to the fact that this design evaluates a smaller "dosage" of incarceration: eight additional months rather than 16. However, results are generally not statistically significant. There is also an substantial overlap in the confidence intervals between the two specifications, making magnitude comparisons only suggestive.

Table A.1: RD balance table

	(1) Black	(2) Female	(3) Age	(4) Total misdemeanor convictions	(5) Index of prior convictions	(6) On probation or parole	(7) Violent crime
RD Estimate:	0.016 [-0.021,0.052]	-0.024 [-0.045,-0.002]	-0.067 [-0.415,0.282]	0.015 [-0.063,0.093]	0.030 [-0.021,0.082]	0.042 [0.005,0.079]	0.000 [-0.014,0.015]
N	64,090	64,091	73,157	72,437	73,157	49,605	73,157
Mean for defendants scoring between -2 and -1	0.22	36.84	0.40	1.57	0.47	0.10	0.01
	Robbery	Drug	Assault	Larceny	Home owner	Car owner	
RD Estimate:	0.005 [-0.003,0.013]	0.011 [-0.020,0.042]	-0.005 [-0.014,0.005]	0.039 [-0.014,0.092]	0.001 [-0.011,0.013]	-0.019 [-0.038,0.000]	
N	73,157	73,157	73,157	73,157	73,157	26,063	
Mean for defendants scoring between -2 and -1	0.24	0.09	0.23	0.12	0.17		

Note: This table evaluates whether there are discontinuities at the cutoff in various predetermined characteristics using the method described in Kolesár and Rothe (2018). We examine characteristics in year -1, restricting the sample to defendants who are at least 23 years old, have no prior felony convictions, do not have an offense that automatically requires prison, and were sentenced between 2001 and 2014. For car ownership, we restrict the sample to those sentenced between 2010 and 2014.

Table A.2: RD sentencing outcomes

	(1) Any incarceration	(2) Incarcerated at least 1 year	(3) Months sentence
RD Estimate:	0.0940 [0.019,0.169]	0.353 [0.332,0.374]	7.612 [6.849,8.374]
N	71,781	71,781	71,781
Mean for defendants scoring between -2 and -1	0.73	0.10	4.68

Note: This table presents RD estimates using the Kolesár and Rothe (2018) method. We look at the probability of incarceration, the probability of a prison sentence (at least 12 months) and the length of the sentence. We restrict the sample to defendants who are at least 23 years old, no prior felony convictions, do not have an offense that automatically requires prison, and were sentenced between 2001 and 2014.

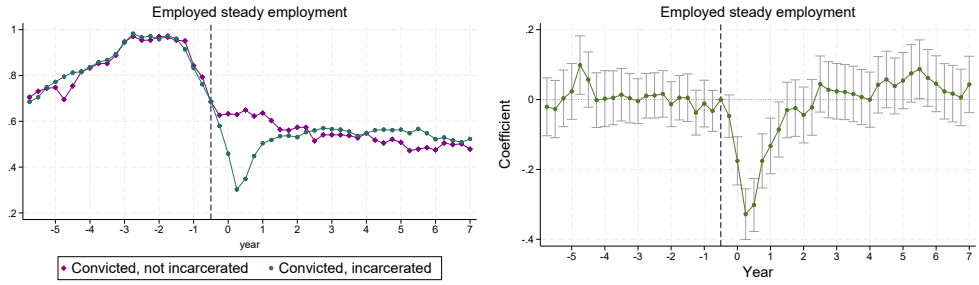
Table A.3: RD outcomes for year 7

	(1) **Incarcerated**	(2) **Employed**	(3) **Wage**	(4) **Cum. earnings**	(5) **Recent conviction**	(6) **Homeowner**	(7) **Car owner**	(8) **College credits**
RD Estimate:	0.004	-0.001	-1005.575	-15931.313	0.004	0.001	-0.019	-7.435
	[0.001,0.007]	[-0.017,0.014]	[-2227.095,215.945]	[-32143.684,281.056]	[-0.008,0.016]	[-0.011,0.013]	[-0.038,0.000]	[-17.033,2.162]
N	71,781	52,286	52,286	52,286	71,781	73,157	26,063	10,446
Mean for defendants scoring between -2 and -1	0.00	0.51	25347.08	377394.01	0.08	0.12	0.25	39.94

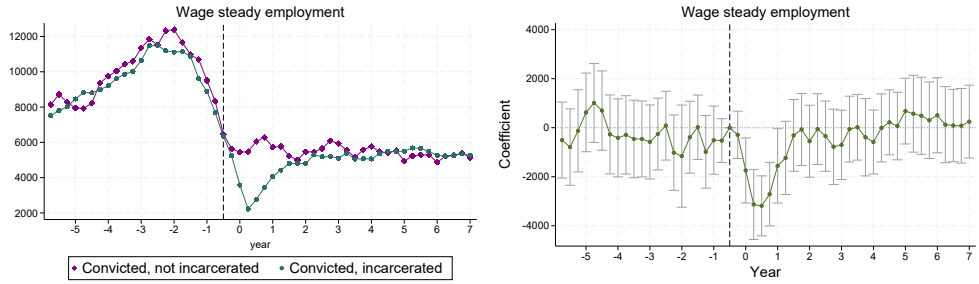
Note: This table presents the regression discontinuity estimates using the sentence score as a running variable and the Kolesár and Rothe (2018) method. We look at outcomes in year 7, restricting the sample to defendants who are at least 23 years old, with no prior felony convictions, and who do not have an offense that automatically requires prison. The sample for the employment outcome is limited to those with at least one quarter of formal sector employment prior to sentencing and includes individuals sentenced between 2003 and 2012. The sample for car ownership includes individuals sentenced between 2010 and 2014. The sample for post-secondary education comprises individuals sentenced from 2005 to 2013. For all other outcomes, the sample consists of individuals sentenced between 2001 and 2014. The bottom row displays outcome means for defendants with sentence guideline scores of -2 and -1.

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

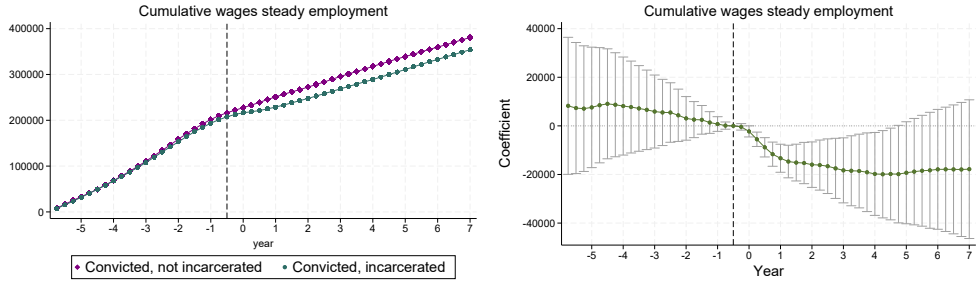
(a) Employment



(b) Wage

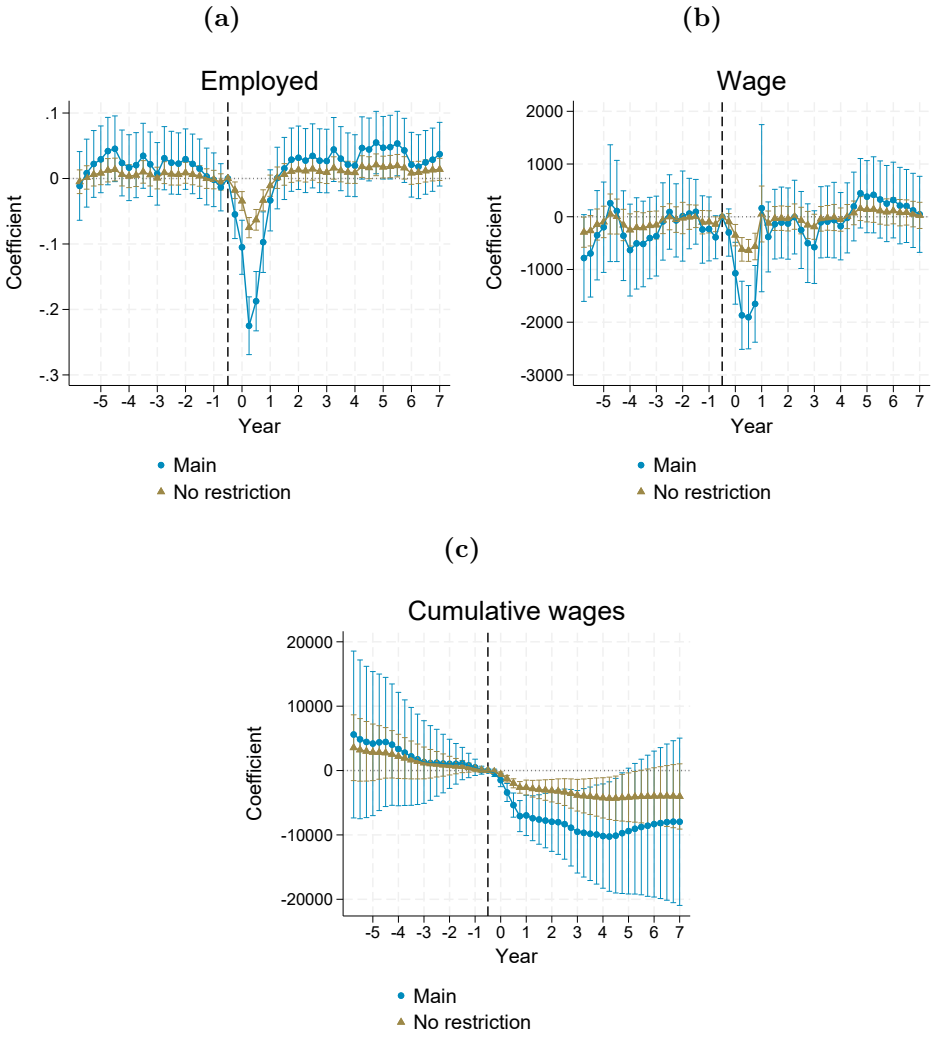


(c) Cumulative earnings



Note: These figures show how incarceration affects wages, employment and cumulative earnings, focusing exclusively on defendants with steady employment pre-sentencing. "Steady employment" refers specifically to defendants who were employed for at least 7 quarters out of 8 between the 3rd year and 1.25 years pre-sentencing. The figures on the left display average outcomes for individuals with an incarceration-length score just above the cutoff 0,1 who were incarcerated, compared to those with a score just below the cutoff -2,-1 who were not incarcerated. The figures on the right show difference-in-differences estimates. Our samples include only individuals aged 23 or older with no prior felony convictions who were sentenced between 2003 and 2012.

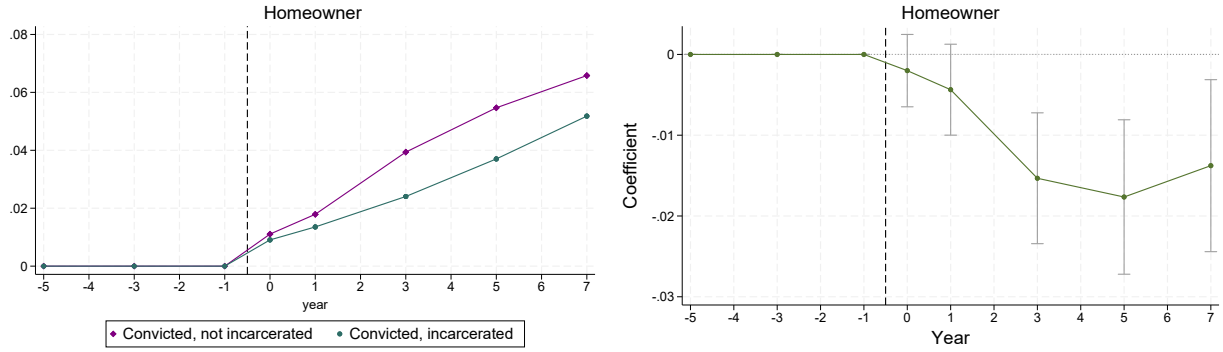
Figure A.6: Labor market outcomes including those with no prior employment



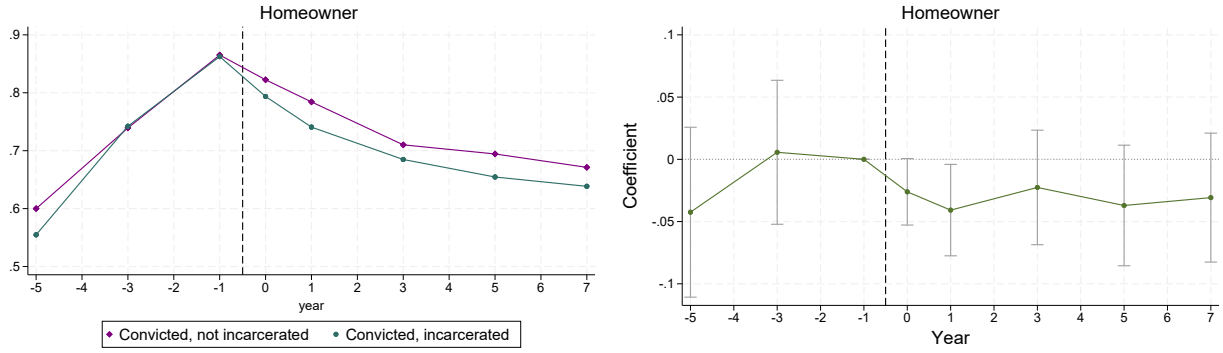
Note: These figures show how incarceration affects wages, employment, and cumulative earnings, including defendants with no prior employment. "Main" (blue circles) refers to the main specification of our paper, which focuses on defendants with at least one quarter of formal sector employment prior to sentencing. "No restriction" (brown triangles) does not impose any restrictions on their pre-sentencing employment status. Our sample includes only individuals aged 23 or older with no prior felony convictions who were sentenced between 2003 and 2012.

Figure A.7: Homeownership

(a) No prior homeownership



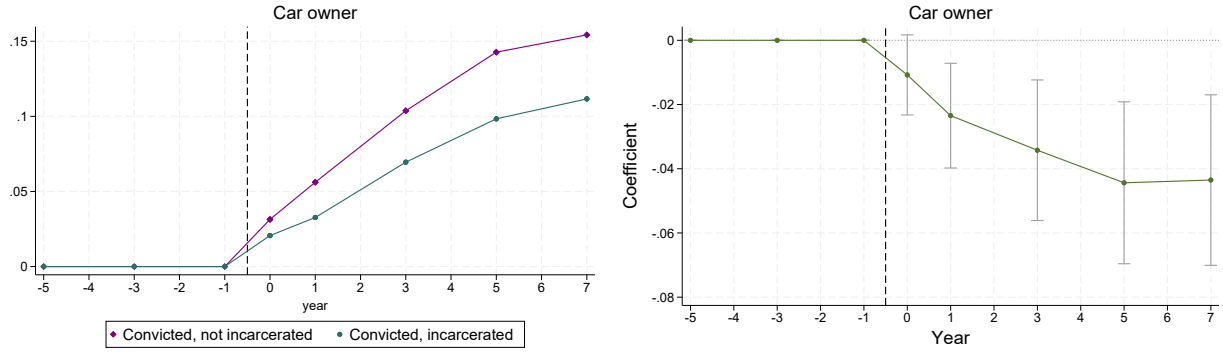
(b) Prior homeownership



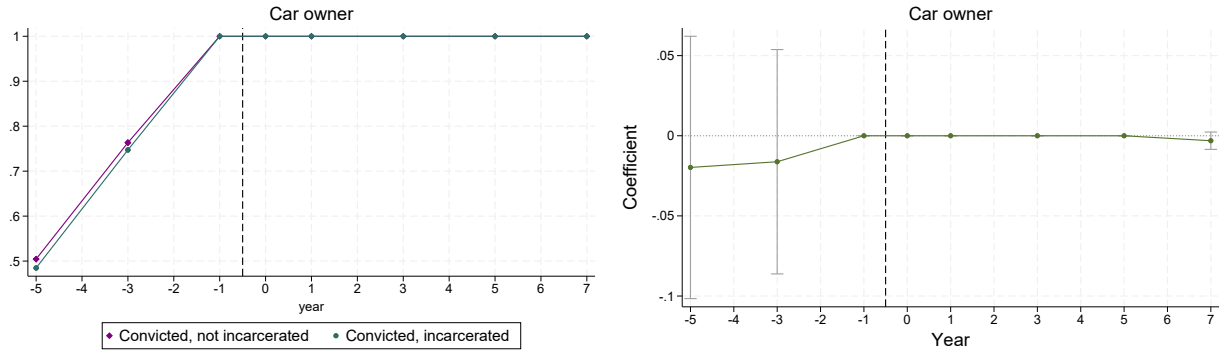
Note: These figures distinguish between property ownership before the sentencing date. "No prior ownership" only applies to defendants who *did not* own a house prior to the sentencing date. Conversely, "Prior ownership" only applies to defendants who *owned* a house prior to sentencing. The figures on the left show average outcomes for those whose incarceration-length score is just above the cutoff 0,1 and who were incarcerated, compared to those who scored just below the cutoff -2,-1 and were not incarcerated. The figures on the right show difference-in-differences estimates. The samples are restricted to individuals at least 23 years old with no prior felony convictions.

Figure A.8: Car ownership

(a) No prior car ownership

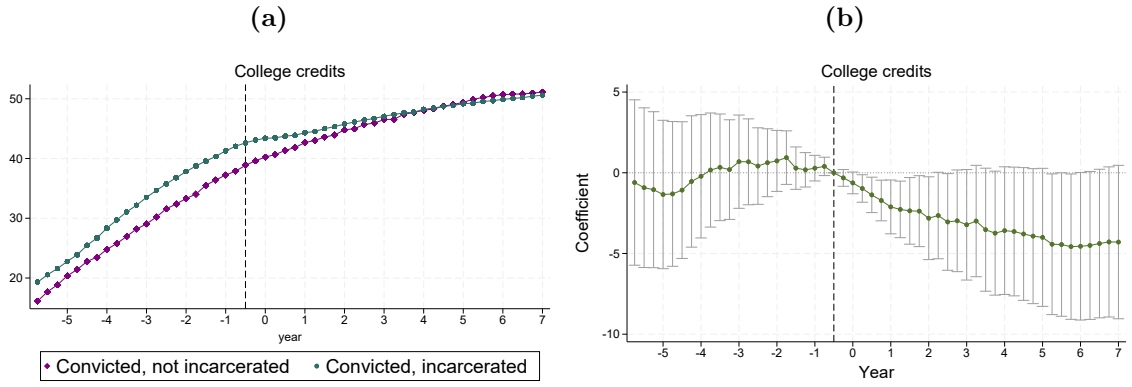


(b) Prior car ownership



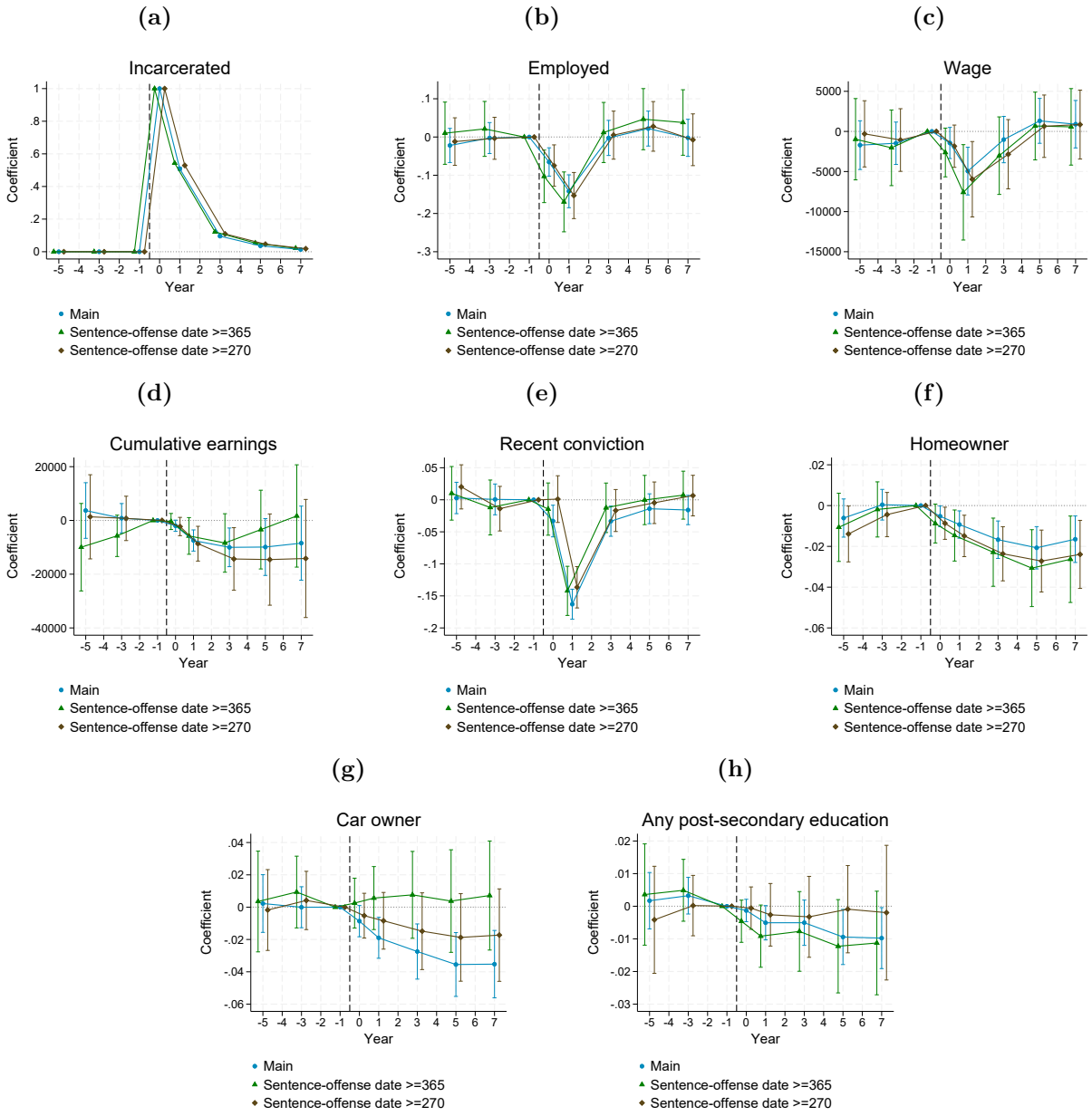
Note: These figures distinguish between property ownership before the sentencing date.
"No prior car ownership" only applies to defendants who *did not* own a car prior to the sentencing date. Conversely, "Prior car ownership" only applies to defendants who *owned* a car prior to sentencing. The figures on the left show average outcomes for those whose incarceration-length score is just above the cutoff 0,1 and who were incarcerated, compared to those who scored just below the cutoff -2,-1 and were not incarcerated. The figures on the right show difference-in-differences estimates. The samples include only individuals aged 23 or older with no prior felony convictions who were sentenced between 2010 and 2014.

Figure A.9: College credits



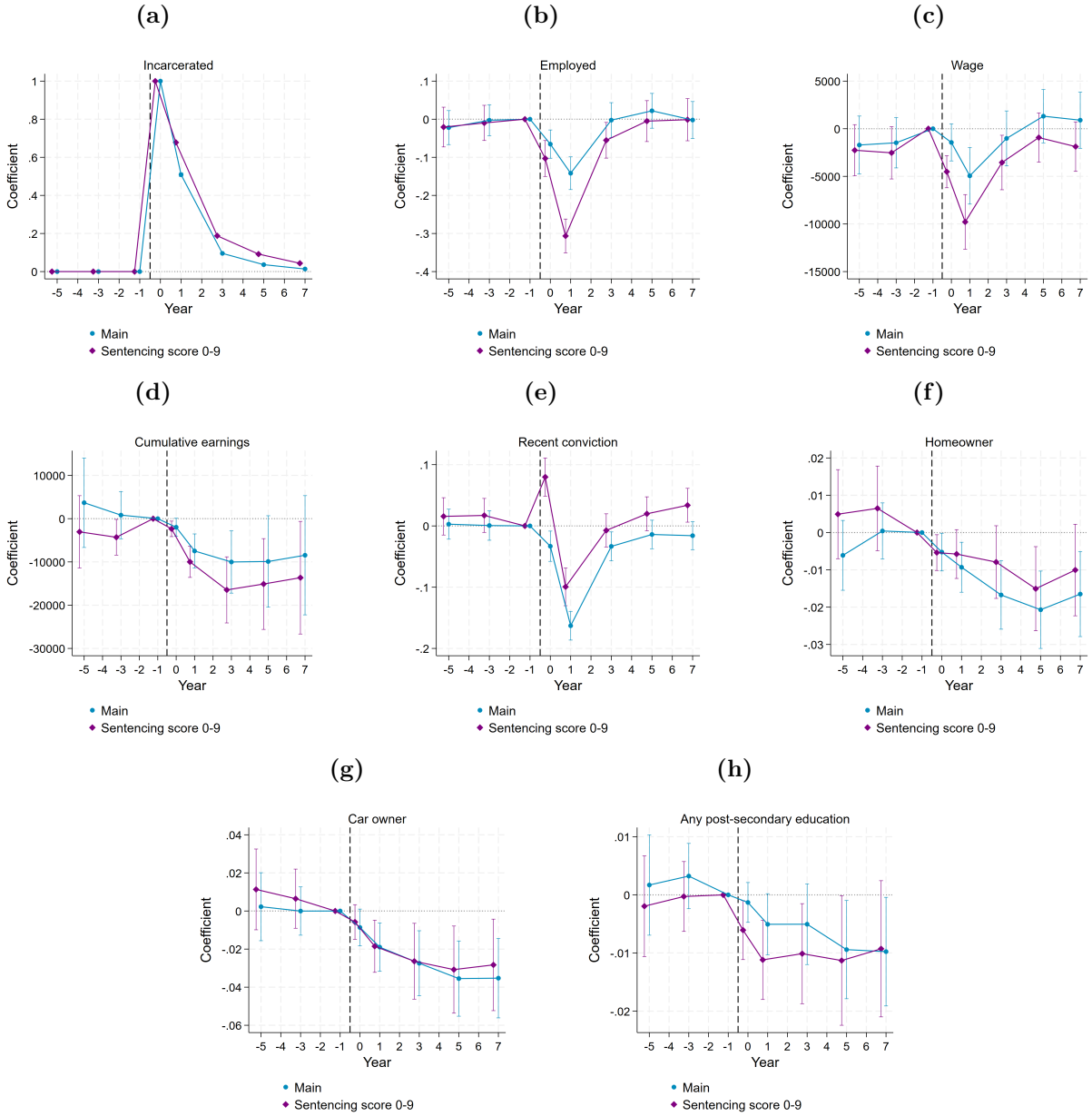
Note: The figures on the left show average outcomes for those whose incarceration-length score is right above the cutoff {0,1} and who were incarcerated versus those who scored right below the cutoff {-2,-1} and were not incarcerated. The figures on the right show difference-in-differences estimates. We includes defendants sentenced from 2005-2014 and who have at least one college enrollment prior to sentencing, We keep only those at least 23 years old at sentencing and with no prior felony conviction.

Figure A.10: Robustness - long time lag between offense and sentencing



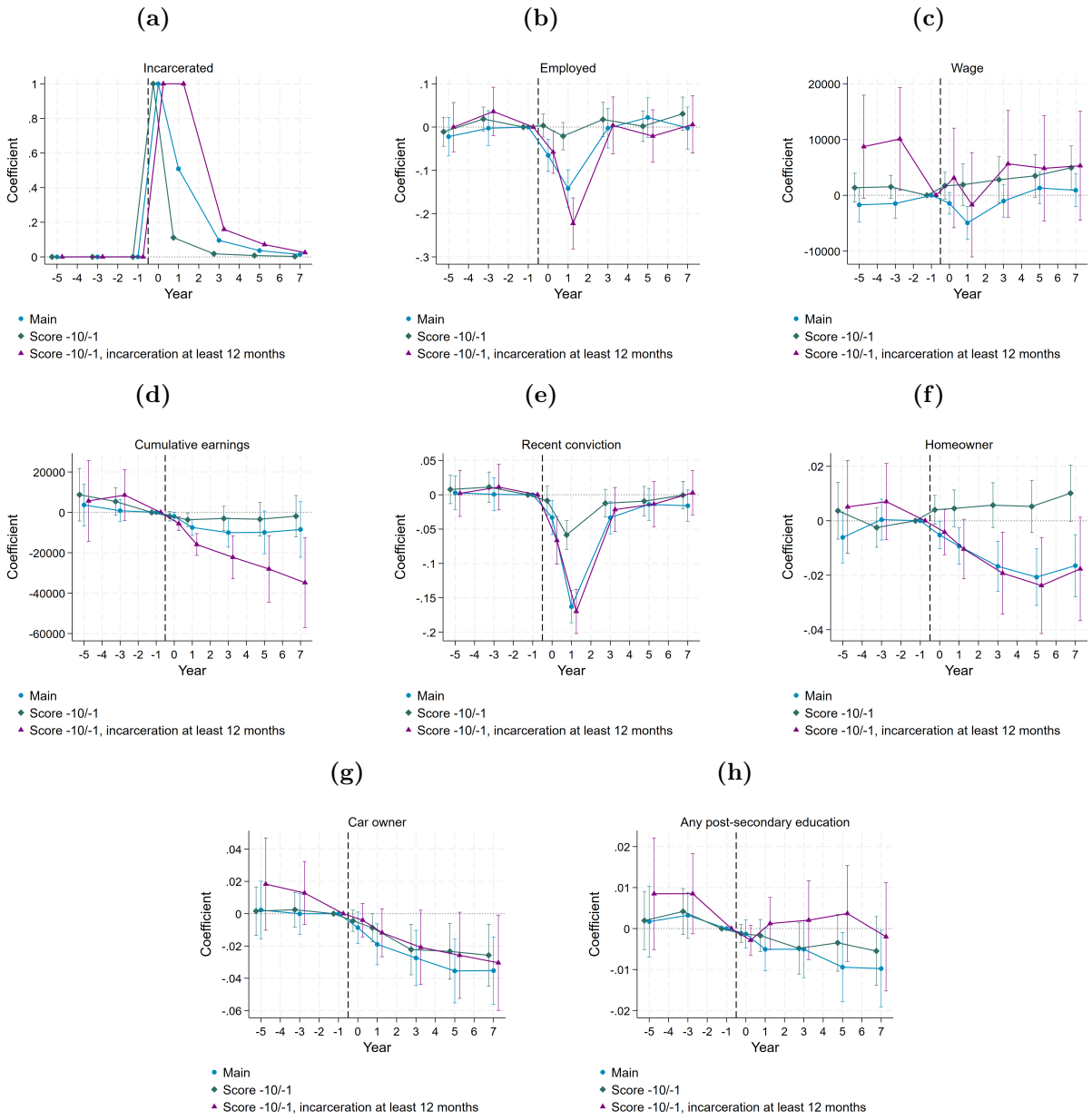
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes. We present three types of specifications. The "Main" specification (blue circles) is identical to that shown in our main results section. The "Sentence-offense date => 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 and with at least one quarter of formal sector employment prior to sentencing. 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.11: Alternative specification: propensity score matching + DD among those who received a recommendation for prison



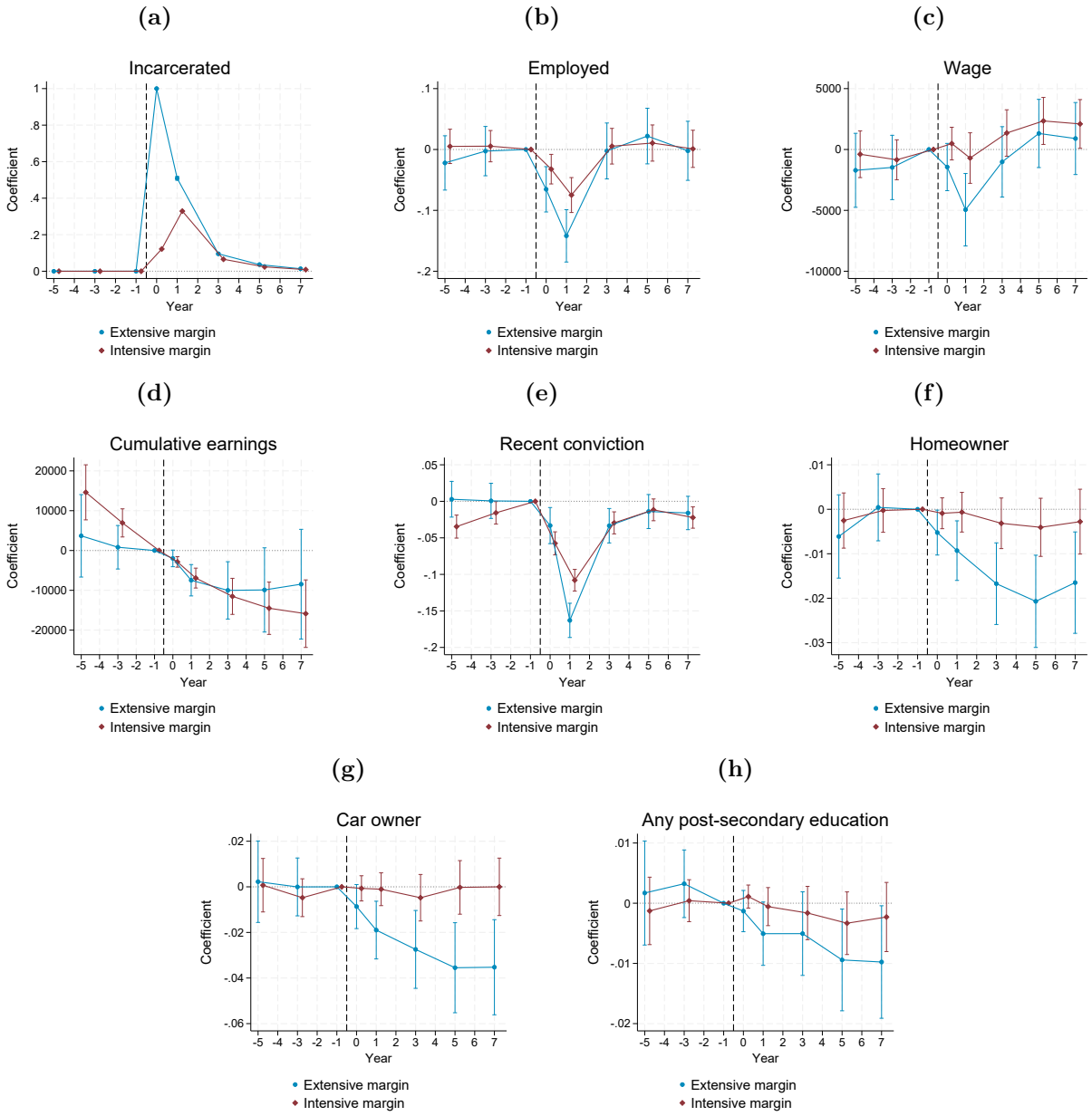
Note: These figures illustrate the comparison between our main specification and a specification restricted to positive scores (from 0 to 9). In comparison to our main figures, we use propensity score matching to the nearest neighbor with replacement. In the latter specification (purple triangles), the treatment group consists of defendants with scores in this range who were incarcerated, while the control group includes defendants within the same score range who were not incarcerated. The samples for the labor market outcomes are limited to those sentenced in 2003-2012 and with at least one quarter of formal sector employment prior to sentencing. 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 individuals who were at least 23 years old at the time of sentencing and had no prior felony convictions.

Figure A.12: Alternative specification: propensity score matching + DD among those who received a recommendation for jail or probation



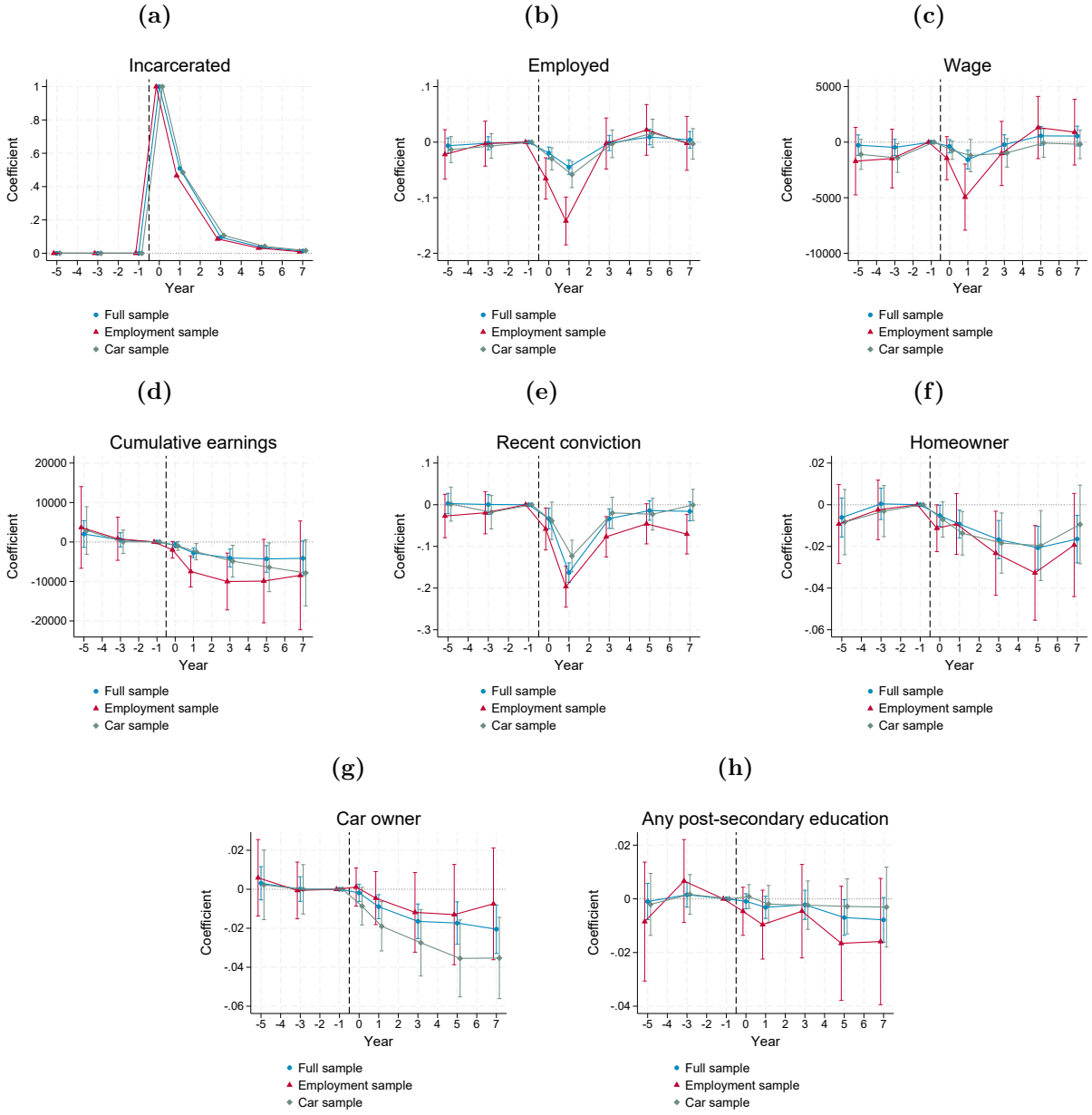
Note: These figures illustrate the comparison between our main specification and a specification restricted to negative scores (from -10 to -1). In comparison to our main figures, we use propensity score matching to the nearest neighbor with replacement. In the latter specification (green diamonds), observations are matched using exact matching. The treatment group consists of defendants with scores within this range who were incarcerated, while the control group includes those who were not incarcerated. We also present another specification with varied treatment (purple triangles): here, the treatment group comprises defendants incarcerated for at least 12 months, while the control group includes those who did not get incarcerated. The samples for the labor market outcomes are limited to those sentenced in 2003-2012 and with at least one quarter of formal sector employment prior to sentencing. 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 individuals who were at least 23 years old at the time of sentencing and had no prior felony convictions.

Figure A.13: Robustness - different ways of defining treatment



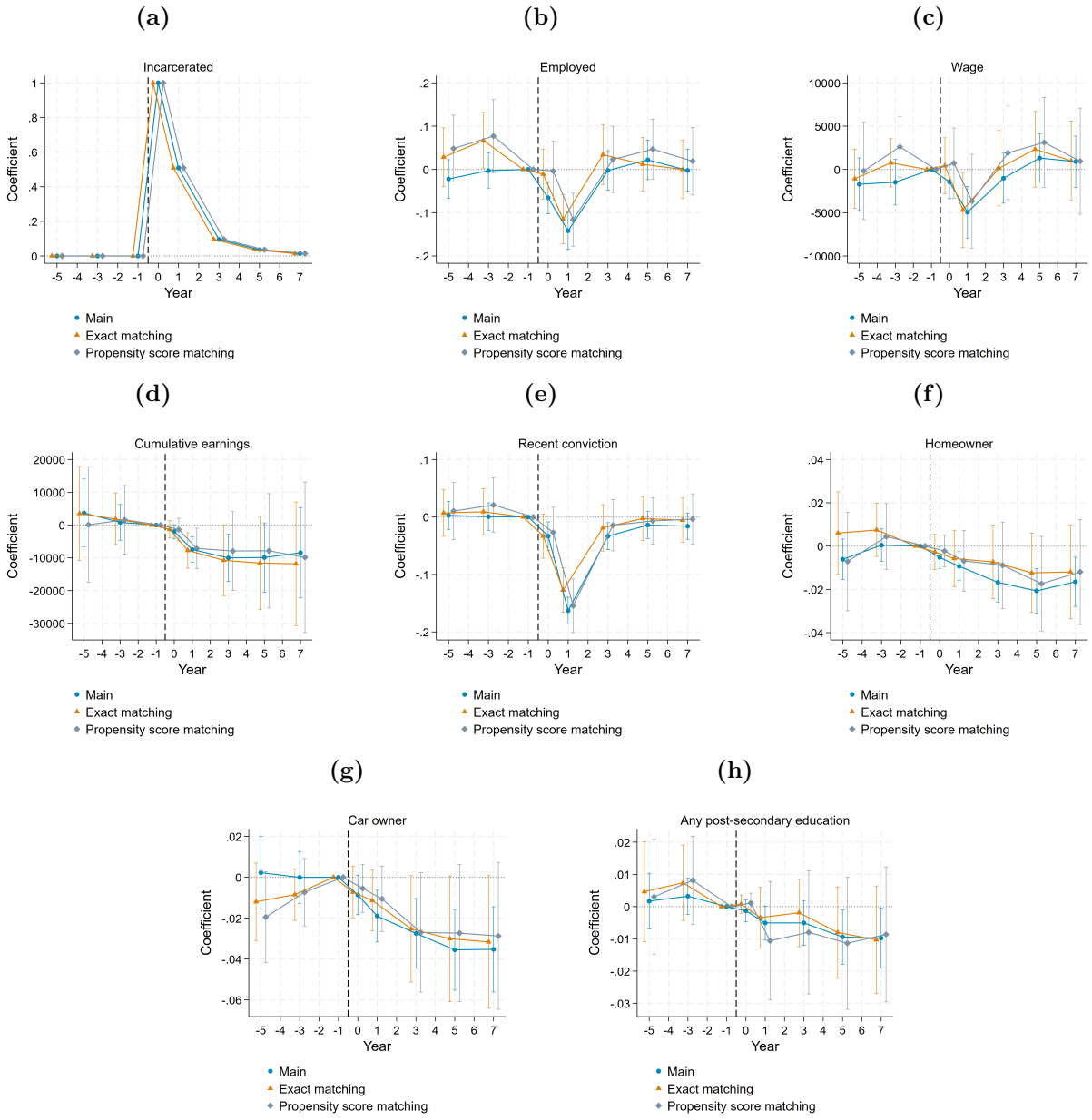
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes. We present three specification types. The "Main" specification (blue circles) is the same as that shown in our main results section: treatment is defined as scoring in {0,1} and receiving a carceral sentence and treatment is defined as scoring in {-2,-1} and receiving a non-carceral sentence. The "Incar. vs convict." specification (purple triangles) defines treatment as receiving a carceral sentence; control is defined as receiving a non-carceral sentence. The "Below/above cutoff" specification (green diamonds) defines treatment as scoring in {0,1} and control as scoring in {-2,-1}. The samples for the labor market outcomes are limited to those sentenced in 2003-2012 and with at least one quarter of formal sector employment prior to sentencing. 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.14: Robustness - different time periods



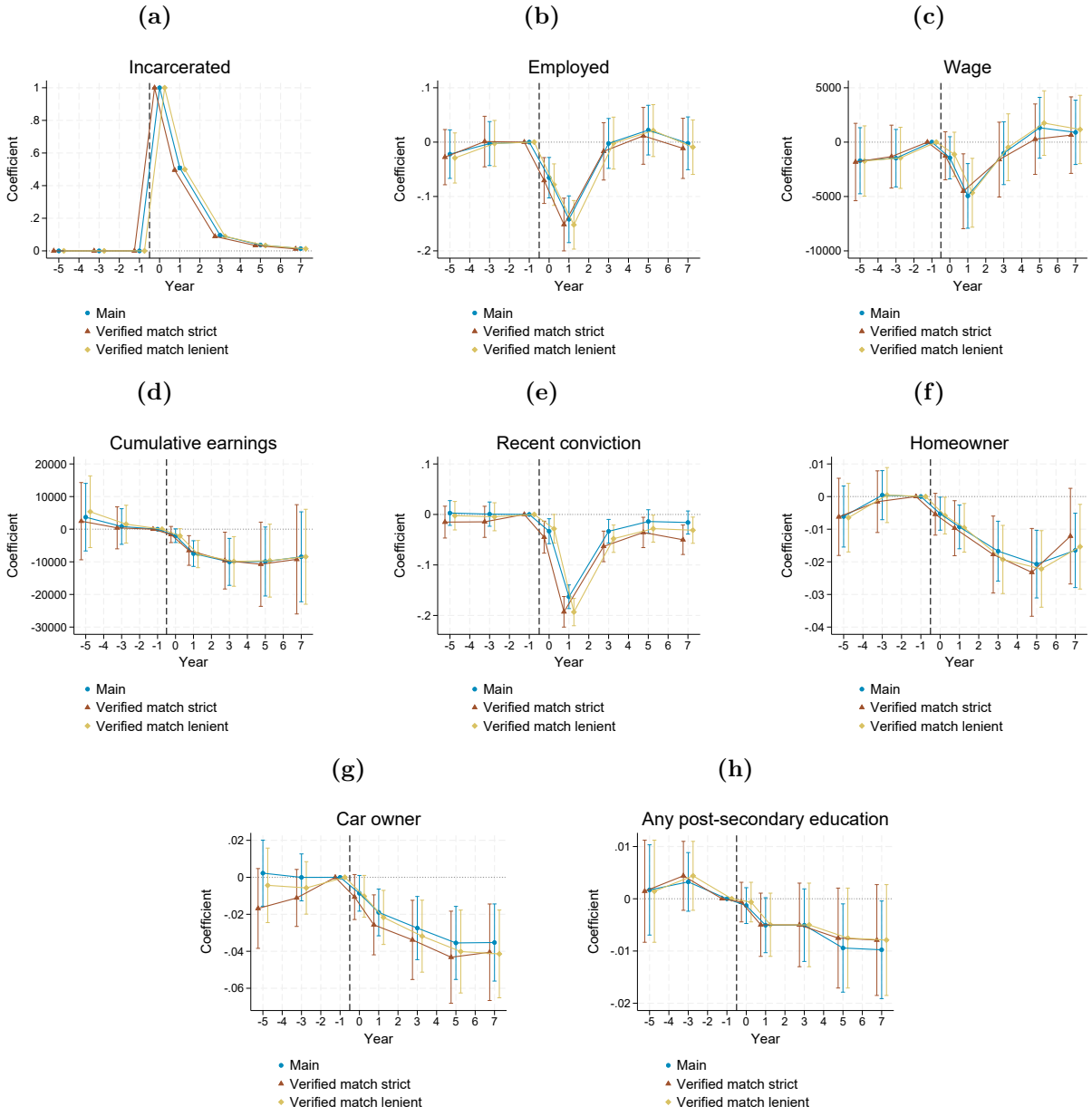
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes across different time periods. We present three specification types, all of which are identical to the main specification except for the sample used. The "Full sample" specification (blue circles) includes those sentenced in 2001-2014. The "Employment sample" (red triangles) includes those sentenced in 2003-2012 and limits to those with at least one quarter of formal sector employment prior to sentencing. The "Car sample" (green diamonds) includes those sentenced in 2010-2014.

Figure A.15: Robustness - matching with difference-in-difference



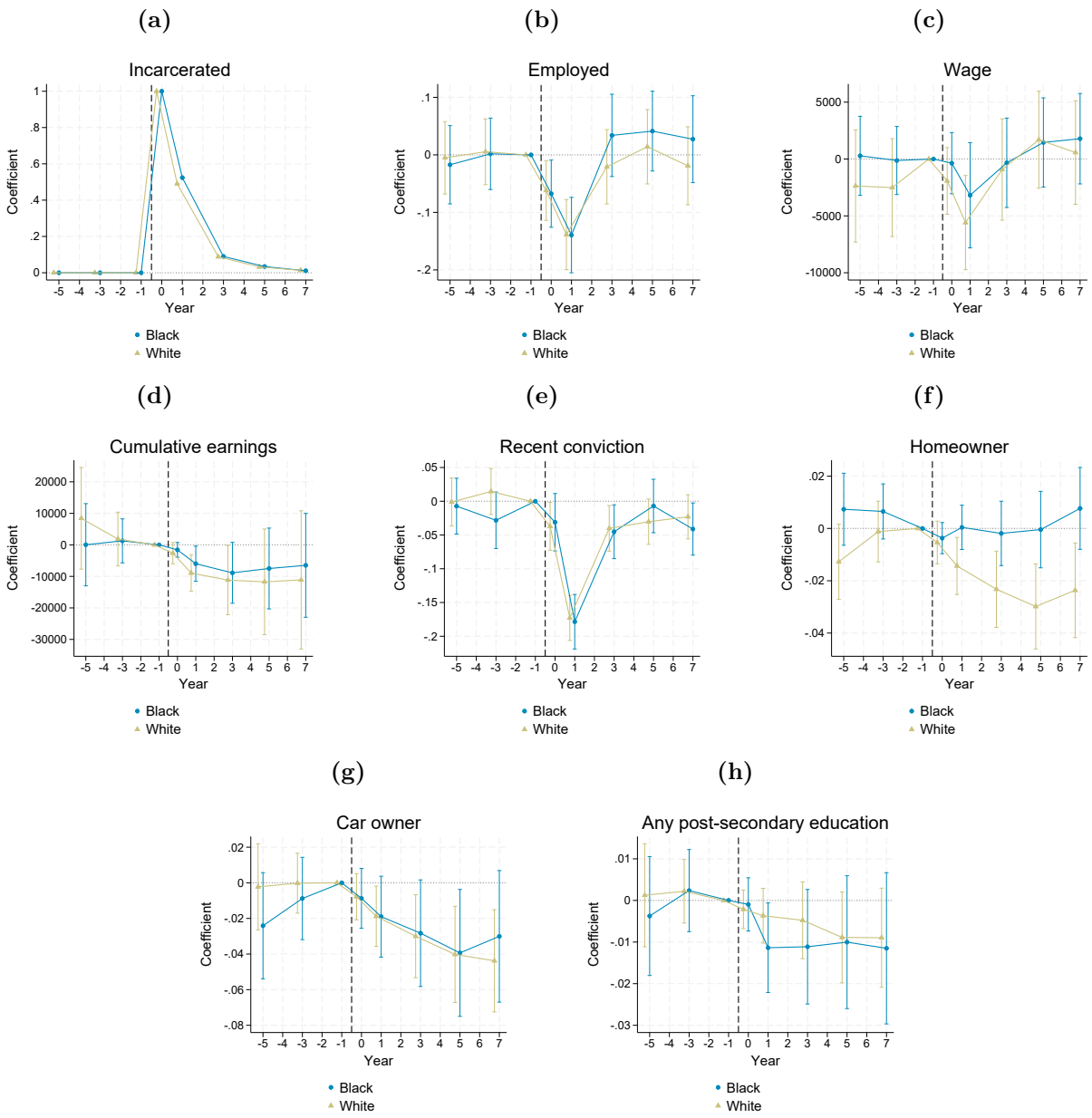
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration using weights derived from a matching procedure. We present three types of specifications: the "Main" specification (blue circles), which mirrors that shown in our main results section. The "Exact matching" specification (orange triangles) corresponds to the same regressions but utilizes weights generated using the exact matching technique. The "Propensity score matching" (grey diamonds) uses weights generated with nearest neighbor matching with the propensity score as the distance metric. The samples for the labor market outcomes are limited to those sentenced in 2003-2012 and with at least one quarter of formal sector employment prior to sentencing. 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 years old and with no prior felony convictions.

Figure A.16: Robustness - subsamples with match verification



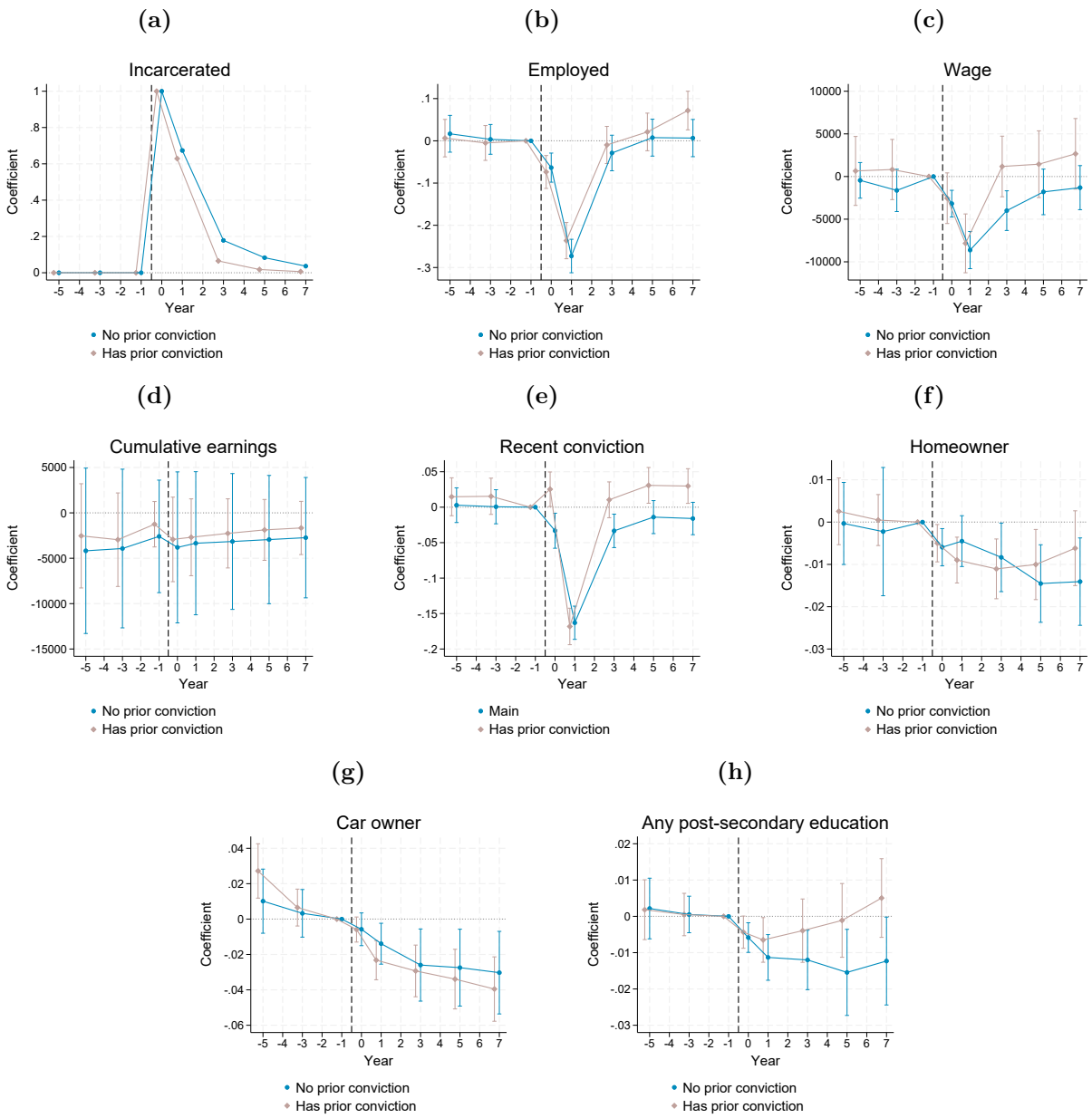
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes across different samples. The "Main" specification (blue circles) shows our main results. The "Verified match strict" specification (brown triangles) includes only defendants 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 and with at least one quarter of formal sector employment prior to sentencing. 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.17: 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. We present heterogeneity by race. The "Black" specification (blue circles) selects 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 and with at least one quarter of formal sector employment prior to sentencing. 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.

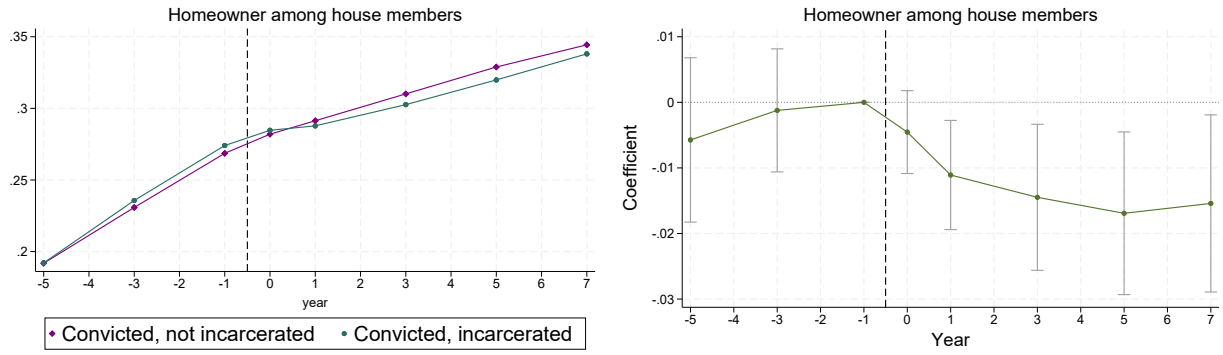
Figure A.18: Heterogeneity by criminal history



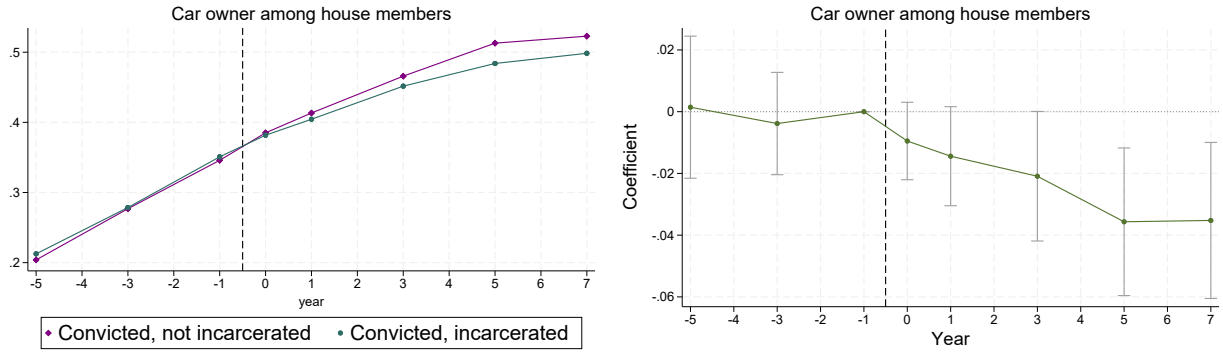
Note: These figures depict the results of difference-in-difference analyses examining the impact of incarceration on various outcomes. We present two types of specifications: the "Main" specification (blue circles), which is identical to that shown in our main results section and includes individuals 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 and with at least one quarter of formal sector employment prior to sentencing. 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.

Figure A.19: Wealth of household members

(a) Home ownership



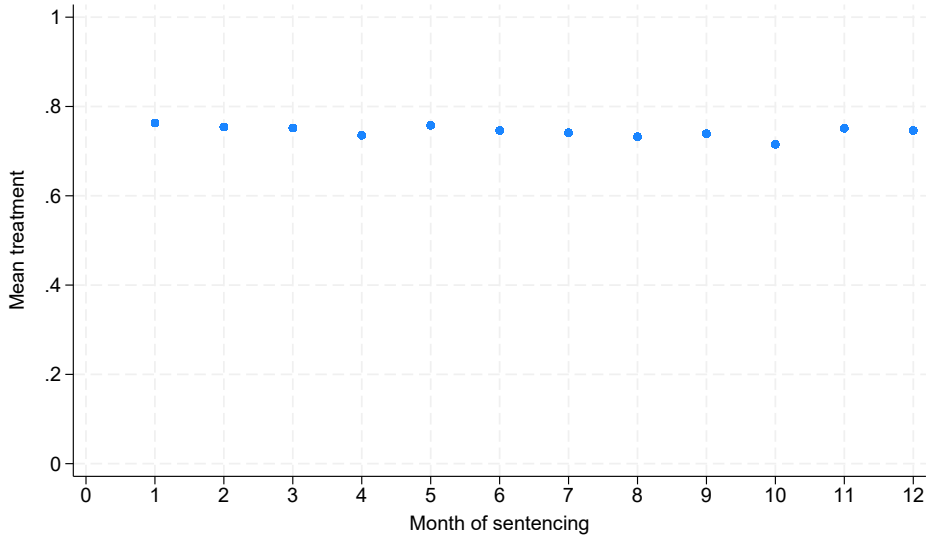
(b) Car ownership



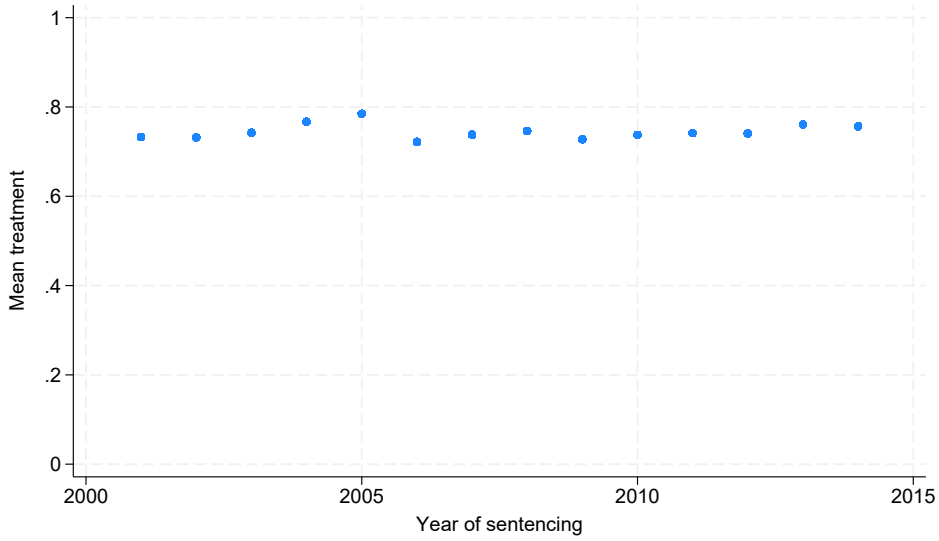
Note: These figures illustrate the results of analyses examining the impact of incarceration on various outcomes for defendants' household members using the "main" specification. We present data on homeownership and car ownership by household members. The sample for car ownership includes individuals 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.

Figure A.20: Seasonality and long-term trends

(a) Month of sentencing



(b) Year of sentencing



Note: These figures display seasonality tests, showing the proportion of cases receiving treatment by month and year of sentencing. Our samples include only individuals aged 23 or older with no prior felony convictions who were sentenced between 2001 and 2014, representing our full sample.

Table A.4: 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
Panel B: Circuit Court dataset		
Court debt		
Amount among those required to pay		
Fine	818	638
Restitution	6,386	5,212
Court costs	1,033	915
Fine and restitution	3,841	4,092
Fine, restitution and court costs	2,143	1,893
Paid		
Fines and court costs	51 %	55 %
Restitution	40 %	37 %
Number of matched observations	8,525	2,876

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, while Panel B represents defendants matched with the Circuit Court dataset. The court debt categories are as follows: "Required to Pay", "Amount Among Those Required to Pay" and "Paid".' "Required to Pay" indicates the percentage of individuals obligated to pay court debt. "Amount Among Those Required to Pay" is the average debt amount restricted to those obligated to pay, and "Paid" is the percentage who settled their 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, "Post-Release 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).