A 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.[52]

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 (around 28% 3-year recidivism rates). 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.[53] Virginia's prison incarceration rate, shown in Panel (a), is 447 per 100,000 people. This rate 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.[54] 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 others states, Black residents are over-represented in the carceral population. The ratio for Hispanic:White residents is 0.5 for Virginia, lower than national average of 1.3 and most comparison states.

Lastly, we compare probation and parole rates (Figure A.4). Virginia's probation rate is close to the national average, as are most comparison states, with the exception of Georgia. However, the parole rate in Virginia – 22 per 100,000 residents – is much lower than the benchmarks. This difference is because discretionary parole was virtually abolished in Virginia for felonies committed after 1995, with inmates being required to serve at least 85% of their sentences, with the possibility to earn good-time credits toward early release. This also means that the initial sentence is more closely linked to time spent incarcerated than in other places.

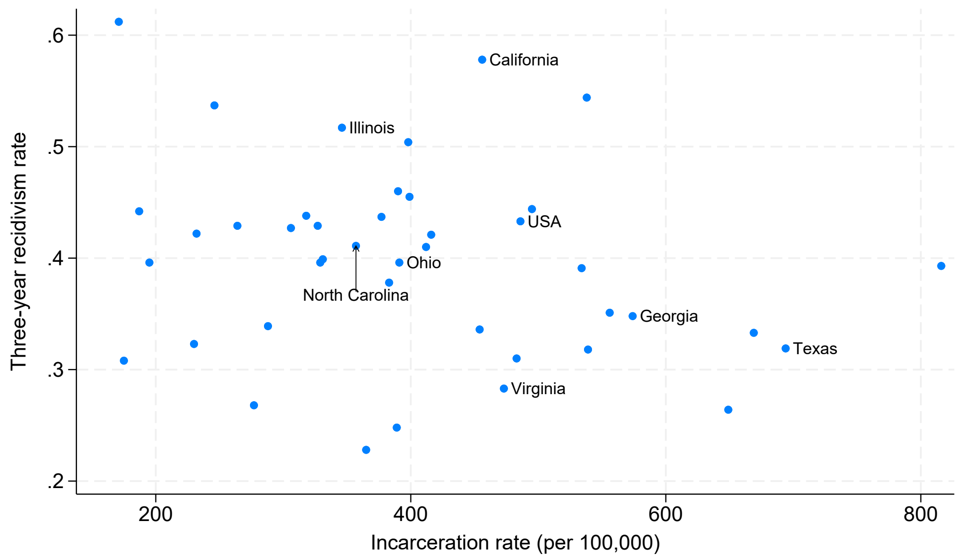
[52] This data can be found at https://www.pewtrusts.org/-/media/legacy/uploadedfiles/pcs_assets/2011/pewstateofrecidivismpdf.pdf, https://bjs.ojp.gov/content/pub/pdf/p04.pdf, and https://www2.fbi.gov/ucr/cius_04/.

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

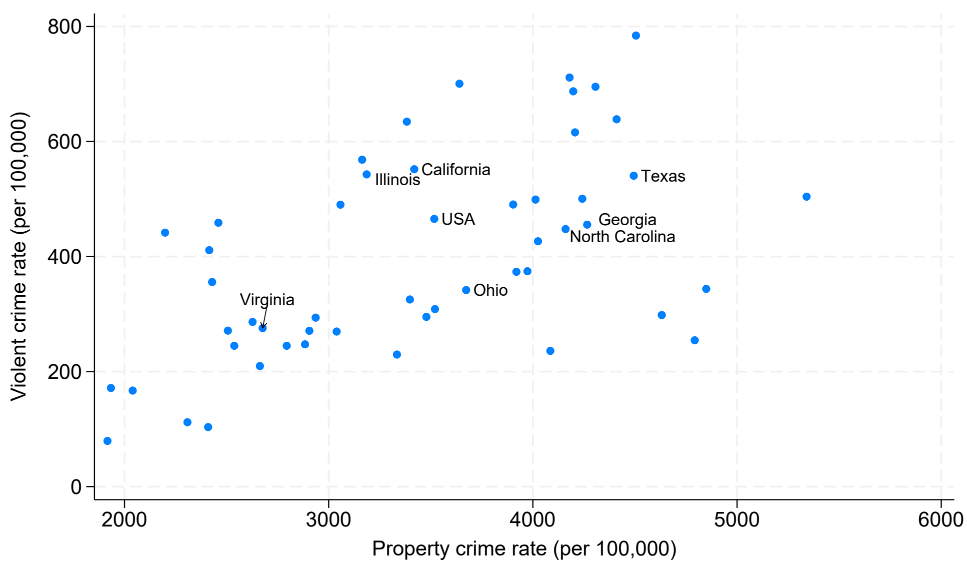
[54] These ratios read as follows: If out of every 100,000 Hispanic residents 200 are incarcerated, and out of every 100,000 White residents 400 are incarcerated, the Hispanic:White ratio is 0.5.

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

(a) 3yr-recidivism rates vs incarceration rates



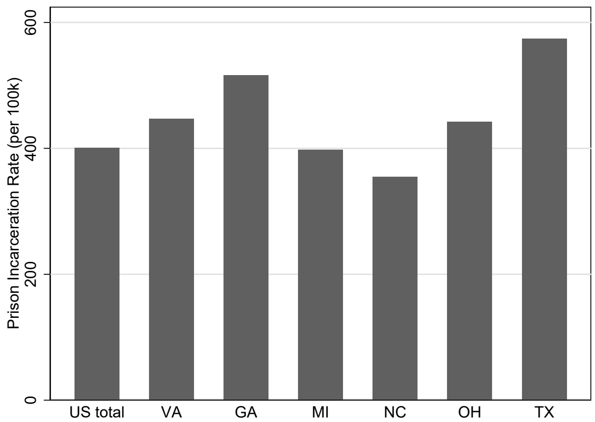
(b) Violent crime rates vs property crime rates



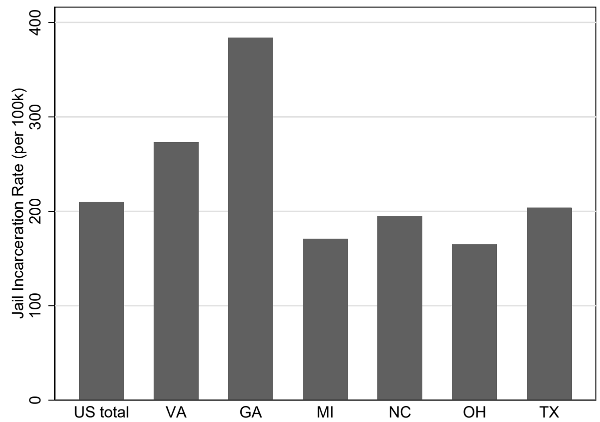
Note: Scatterplots of 2004 incarceration rates, 2004 three-year recidivism rates, and 2004 crime rates. Data gathered from the Pew Center, Bureau of Justice Statistics, and the FBI Uniform Crime Reporting Program.

Figure A.2: Incarceration rates

(a) Prison incarceration rates

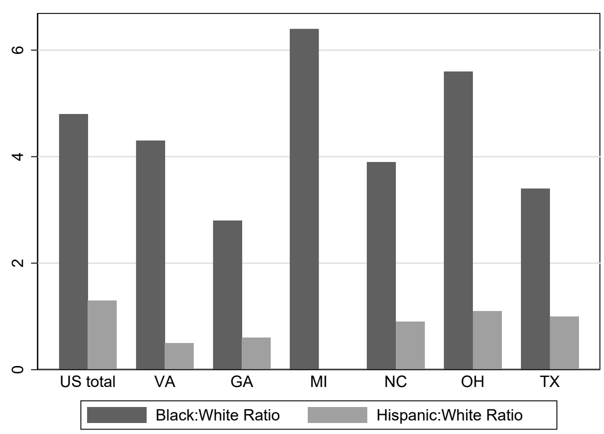


(b) Jail incarceration rates



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).

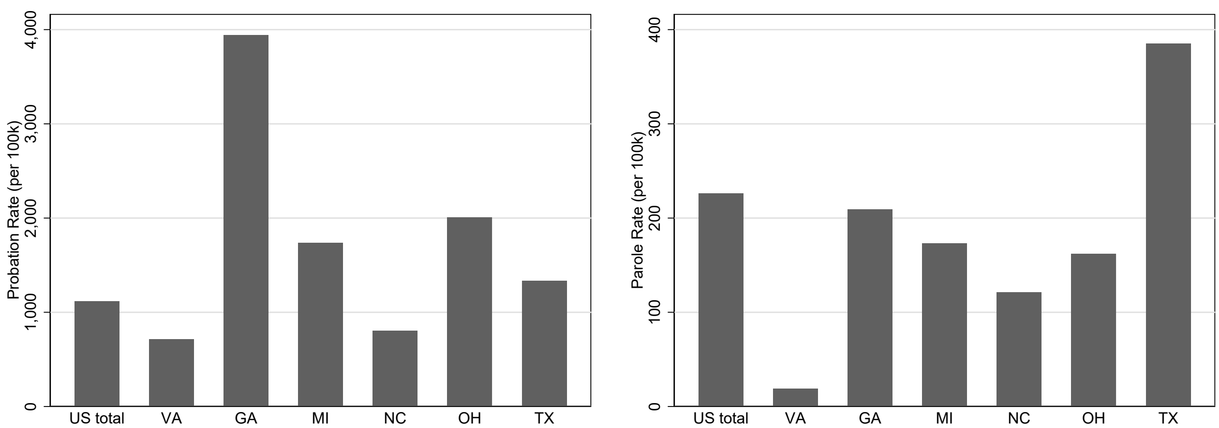
Figure A.3: Racial and ethnic composition of the imprisoned population



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).

B Additional details on data construction

B.1 Main data sources

Virginia Circuit Courts (VCC) data. The Virginia Court system keeps all Circuit Court case records publicly available for anyone to search. We obtained this data from Ben Schoenfeld who web-scraped records from the courts and made the corresponding data available on http://virginiacourtdata.org/ for public download. This data covers criminal cases in which at least one charge is a felony. It contains information on charges (type and date), on the defendant (gender, race, partial birth date, and FIPS code of residence), and on Circuit Court proceedings for these cases (type, outcome, and judges on the proceedings) and is available for the period 2000-2019. All of Virginia is covered except for Alexandria and Fairfax counties. This is the primary data source for our 2SLS analysis with judge stringencies.

Virginia Criminal Sentencing Commission (VCSC) data. The VCSC provided a dataset that contains information on individuals in Virginia sentenced for a felony. This is used as supplementary data for our 2SLS analysis (to construct our measure of prior convictions) and as the main source for the RD analysis. The data provided to us by the VCSC includes records on all people convicted of a felony in Virginia from 1996 to 2020. This data includes information on the charge(s) of conviction, date of sentencing, sentence imposed for this conviction, guidelines-recommended sentence, points accrued on each item in a worksheet, and total worksheet scores. This data does not contain information on demographics or prior and future charges, so we match it to data from Virginia's Circuit Courts as described below.

B.2 Supplementary data sources

Virginia District Courts (VDC) data. The Virginia Court system also keeps all District Court case records publicly available for anyone to search. As with the Circuit Court data, we obtained this data from Ben Schoenfeld's web-scraped records (http://virginiacourtdata.org/). This data covers all dockets filed in District Court, including felonies and misdemeanors. The District Court is a court of limited jurisdiction; felony charges that are filed there cannot be adjudicated there. We use this data to obtain information about pretrial detention, as used in the RD specification that subsets to those never previously incarcerated.

Virginia residency data. We obtain information on residency status from a private vendor, matched to the VCSC data with name, social security number and partial birth date. We use the residency data to look at differential mobility in the RD sample. The vendor provided us with information as to which state the matched individual resides in post-sentencing. We receive snapshots of information from them 1, 3, 5, 7 years post-sentencing date, and we construct a variable indicating if an individual is in the state of Virginia 5 and 7 years post-sentencing. 7.7% of observations are missing residency.

IRS zip code income data. This is publicly available data produced by the IRS of average zip code earnings. We use the 2005 vintage and match in by zip onto our samples. This is supplementary data to our IV and RD analysis.

B.3 Data construction

This section details the data construction and cleaning process as well as the matching procedure implemented between the various raw datasets described above.

IV data. We begin with the sample of 3.4 million dockets from the VCC data between 2000 and 2019.

- In addition to dockets with felony charges – the focus of our analysis – the data also includes many dockets pertaining to technical issues (failures to appear in court, revocations, bond hearings, etc.) as well as some pertaining to misdemeanors. We only keep dockets pertaining to new felony charges (roughly 50% of all dockets), leaving roughly 1.6 million felony dockets remaining. We also drop dockets that are missing disposition date or initiation date, as well as cases where the disposition is on a weekend. This represents roughly 77,000 dockets, or less than 5% of the remaining sample.
- Sometimes prosecutors file separate dockets for different charges against the same defendant. This could happen if, for instance, the defendant was arrested for multiple burglaries or drug selling occasions. These nonetheless get processed together as one effective case. For our analyses, we define a "case" – our main unit of analysis – as composing all dockets with the same defendant and either the same or consecutive case numbers. Consecutive case numbers means that they were all filed at the same time. Docket level descriptors are aggregated to the case level (i.e., a case is considered "convicted" if at least one charge was adjudicated guilty). The 1.6 million dockets correspond to 773,553 cases.
- Some courts do not regularly fill out judge information. We drop all courts where less than 80% of judge names are filled out. These courts cover 171,718 cases or 22.2% of cases resulting in 601,835 remaining cases.
- Each case can have multiple hearings. Judge information is provided at the hearing level. We have hearing-level data for 502,732 cases, or 84% of cases.
- We then drop cases entirely missing judge information (37,191 cases dropped or 7.4% of cases resulting in 465,541 cases left).
- We limit ourselves to larger courts with multiple judges overseeing felony cases. In our main sample, we drop judges who see less than 100 cases over 3 years, and all observations in a court-by-year with only one judge. In our main specification, we require that we have at least 3 years per court where multiple judges are present, to avoid including courts and years in which judges simply overlapped because of turnover. In total, these sample restrictions lead us to drop 18,777 cases (4% of the sample), leaving us with 446,764 cases.
- We called clerks in the remaining courts to understand how cases were assigned. In our main specification, we dropped courts where the clerks described a case assignment mechanism that clearly wasn't quasi-random; for instance, ones in which cases are assigned based on judge specialization. We also drop one court after 2010 due to decreased data availability. This represents 121,931 cases, (27% of remaining cases). This leaves us with 324,799 cases.
- Lastly, we use the VCC data to calculate recidivism, defined as a new felony charge in Circuit Court within X years for various values of x. The VCC data

goes through 2019. The sample we use for most of the analysis, which is cases that have 7 years of data, is 183,381 observations. In a robustness check, we expand our analysis sample to include all available years. Our main sample includes only cases disposed prior to 2012 in order to have seven years post-disposition. In this robustness check, we expand the sample up to 2015 for outcomes in years 2-4 and up to 2018 for outcomes in year 1.

RD data. We begin by using the VCSC felony data as our universe of cases for each individual convicted of a felony in Virginia. We start with 458,164 observations between 2000 and 2018 (years for which we also have CC data, used to measure recidivism). From there we create two main samples for the RD analyses, as well as a supplementary sample that we use for robustness checks.

Incarceration-length RD data. The first sample leverages the discontinuity in the incarceration-length score as calculated in Worksheet A. This is the sample that we use to measure the effect of longer prison stays vs. shorter jail stays. For this set of analyses, we impose four restrictions on the sample.

- First, we drop offense categories in which the seriousness of the offense mandates a recommended prison sentence, since we do not 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 26% of the sample, or 118,364 cases.
- Second, we drop certain offense categories because the distribution of the sentence guidelines scores is not smooth, potentially due to the scoring of worksheets for those categories. Since the RD method requires a smooth evolution of potential outcomes across the running variable, these could be problematic for our design, even if this is mechanically due to the way in which points are accrued. The offense categories dropped are fraud, traffic, and weapons; these constitute 20% of the remaining data, or 72,026 cases. Our main results are robust to including these offense categories.
- Third, we drop individuals who are recorded as having no points in the incarceration-length score: 0.2% of the sample, or 758 cases. We infer that these are likely data errors, since about 10% of these individuals are recommended for prison despite being far below the cutoff at which a prison recommendation is warranted.
- We then match the VCSC sentencing data to the VCC data. VCC data allows us to construct our primary measure of new criminal justice contact (new felony charges in circuit court) as well as race, gender, arrest date, and prior charges. We drop cases from Fairfax and Alexandria, which are not in the CC data. We use 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. For the years and counties in which a match is feasible, our match rate is 92%. Our final sample has 230,357 observations.

Probation/jail RD data. The second sample leverages the discontinuity in the probation/jail score found in Worksheet B. For this set of analyses, we impose similar sample restrictions as described previously.

- First, we drop anyone whose primary offense makes them ineligible for probation, as well as those convicted of violent offenses, since almost none of these are probation-eligible. This represents 59% of the data, or 269,437 cases.
- As previously, we drop individuals who are recorded as having no points in the probation/jail score (0.8%, or 1,576 cases) due to suspected data entry errors. We also drop offense categories for which there are only 2 points between our focal cutoff (probation/jail) and the secondary cutoff (short jail/long jail sentence), which represents 6.8% (or 12,765 cases) of the Worksheet B sample. The remaining offense categories either only have one cutoff (about half of cases) or have 3 points between the focal and secondary cutoff.
- For this data we also restrict to a sample where the Circuit Court match is feasible, using the same procedure as that described for the incarceration-length RD data. Our final sample has 130,692 cases.

Supplementary RD data. Finally, we create a supplementary sample that matches the Worksheet B sample to information on pretrial detention from the VDC data. This reduces our sample significantly since the VDC data is only available from 2010-2019. Since we use three years of follow up, the sample includes those convicted of a felony between 2010-2016: 49,246 cases.

Comparison between IV and RD data. While the data for the RD and the IV analyses come from the same general sources and have significant overlap, there are some key differences.

- The group of cases in the RD data is a subset of those in the 2SLS data, since the RD sample just covers those whose felony charges led to a conviction. For both sets of analyses, we have approximately 80% of Virginia's population since the VCC data misses Alexandria and Fairfax counties.
- In addition, as described above, we further subset the RD sample to include offense types that could, in theory, have led to defendants being on either side of the different RD thresholds.
- Tables 1 and H.1 present summary statistics for each sample.

B.4 Variable construction and definitions

Variable definitions.

- *Incarceration.* We define a person to be incarcerated if at least one of the charges resulted in a positive (greater than zero) carceral sentence.
- *Noncarceral conviction.* We define a person to be convicted if at least one charge led to a sentence, but no charge resulted in a carceral sentence.
- *Dismissal.* We define a case as dismissed if all charges were dismissed or withdrawn by prosecution (nolle prosequi); or if the defendant was acquitted of all charges.

- *Recidivism.* Our main measure of recidivism is whether a person has a new felony charge in Circuit Court for an offense that allegedly happened after the focal charge date. This measure does not include revocations unless these are also accompanied by a new felony charge for a new crime. We create these variables for recidivism in year 1, years 2-4, years 5-7, and years 1-7 cumulative. For the RD analyses, since we have more years of data, we also include measures for years 8-10 and years 1-10.
- *Recidivism-new conviction.* This is similar to our main recidivism measure, but here the indicator refers to a new conviction on a Circuit Court felony charge for a crime committed within the relevant time periods.
- *Recidivism-new incarceration.* Again, this outcome is similar to the previous variable, except the indicator means there is a new carceral sentence resulting from a Circuit Court felony charge for a crime committed within the relevant time period.
- *Prior conviction flag.* We define someone as having a prior felony conviction if they have a case in the VCSC data in the 5 years prior to the first offense date of their current case. We use VCSC data to build our prior conviction flag because our data goes back to 1996. This gives us at least 5 years of information on prior felony convictions for all cases in the 2SLS sample.
- *Judge on the case.* We define the judge on the case in the following way. Our main measure is the judge that appears when the "pleading" or the "remarks" variable in the hearings data is marked as "sentencing", "judgement", "dismissal", "conviction", or "final order". If this does not appear on a case, we fill in with the judge present on the disposition date. Finally, if the judge is still missing, for any remaining listings where there is an available judge, we use the maxmode to determine the presiding judge. In our sample, roughly 80% of hearings are in front of the judge whom we define as the judge for the case.[55]
- *Black.* Race of the defendant as defined in the VCC data. Almost all of the people for which race information is available are labeled either "Black" or "White." Ethnicity is not available.
- *Female.* Gender of the defendant as defined in the VCC data.
- *Incarceration Length.* This variable indicates how long in months an individual is imprisoned (if they have a carceral sentence). It will be 0 otherwise.
- *Income generating.* This is a variable that is used to determine whether the individual has new felony charges for an income-generating type of crime. We consider the following charges to be income-generating: burglary, drug charges (excluding drug possession), fraud, larceny, robbery, or prostitution.
- *Has misdemeanor.* An indicator if the current case has a misdemeanor charge as recorded in the Circuit Court data.
- *% of people in zip earning <25K.* Share of people earning less than 25K in a zip code, using matched IRS average zip code level earnings data.

[55] The other hearings could be seen by another judge because the primary judge is absent that day (sick or on vacation) or if the case was reassigned.

C Additional details on bias in 2SLS estimands

C.1 Proof of Proposition 1

When CPM holds but UPM does not, a shift from z_c to z_c' holding z_i fixed induces three types of flows: $d \to c$, $d \to i$, and $i \to c$. The reduced form effect is thus given by

$$E[Y(T(z_c', z_i)) - Y(T(z_c, z_i))] = \omega_{d\to c}\Delta_{d\to c}^{Y_c - Y_d} + \omega_{i\to c}\Delta_{i\to c}^{Y_c - Y_i} + \omega_{d\to i}\Delta_{d\to i}^{Y_i - Y_d}. \quad (1)$$

Since the overall probability of incarceration is fixed at z_i, the share of cases flowing into and out of incarceration must be equal in size (i.e., it must be that $\omega_{d\to i} = \omega_{i\to c}$). Hence, we can rewrite equation (1) as

$$E[Y(T(z_c', z_i)) - Y(T(z_c, z_i))] = \omega_{d\to c}\Delta_{d\to c}^{Y_c - Y_d} + \omega_{i\to c}\left[\Delta_{i\to c}^{Y_c - Y_i} + \Delta_{d\to i}^{Y_i - Y_d}\right]. \quad (2)$$

Next, observe that

$$\Delta_{d\to i}^{Y_i - Y_d} = \Delta_{d\to i}^{Y_i - Y_c} + \Delta_{d\to i}^{Y_c - Y_d}.$$

Hence, equation (2) can be rewritten as:

$$\begin{aligned} E[Y(T(z_c', z_i)) - Y(T(z_c, z_i))] &= \omega_{d\to c}\Delta_{d\to c}^{Y_c - Y_d} + \omega_{i\to c}\left[\Delta_{i\to c}^{Y_c - Y_i} + \Delta_{d\to i}^{Y_i - Y_c} + \Delta_{d\to i}^{Y_c - Y_d}\right] \\ &= \omega_{d\to c}\Delta_{d\to c}^{Y_c - Y_d} + \omega_{i\to c}\Delta_{d\to i}^{Y_c - Y_d} + \omega_{i\to c}\left[\Delta_{d\to i}^{Y_i - Y_c} - \Delta_{i\to c}^{Y_i - Y_c}\right]. \end{aligned}$$

For the denominator of the Wald estimand, we have

$$E[T_c(z_c', z_i) - T_c(z_c, z_i)] = \omega_{d\to c} + \omega_{i\to c}.$$

Constructing the Wald estimand, we obtain equation (7):

$$\frac{E[Y(T(z_c', z_i)) - Y(T(z_c, z_i))]}{E[T_c(z_c', z_i) - T_c(z_c, z_i)]} =$$

$$\underbrace{\frac{\omega_{d\to c}\Delta_{d\to c}^{Y_c - Y_d} + \omega_{i\to c}\Delta_{d\to i}^{Y_c - Y_d}}{\omega_{d\to c} + \omega_{i\to c}}}_{\text{Positively-weighted avg. of } Y_c - Y_d \text{ treatment effects}} + \underbrace{\frac{\omega_{i\to c}}{\omega_{d\to c} + \omega_{i\to c}}\left[\Delta_{d\to i}^{Y_i - Y_c} - \Delta_{i\to c}^{Y_i - Y_c}\right]}_{\text{Bias term}}.$$

Moving from CPM to the stronger UPM assumption simplifies equation (7). First, recall that UPM($Z_c|Z_i$) implies that there can only be flows into $T = c$ when increasing Z_c from z_c to z_c'. Second, recall that fixing judge stringency $Z_i = z_i$ implies that the net probability of incarceration must remain constant. This second point implies that any flows from $T = i$ to $T = c$ would need to be compensated by flows from $T = d$ to $T = i$. Since UPM($Z_c|Z_i$) rules out flow from $T = d$ to $T = i$, there can be no flows from $T = i$ to $T = c$ since Z_i is fixed. This implies that $\omega_{i\to c} = 0$, which simplifies equation (7) to

$$\frac{E[Y(T(z_c', z_i)) - Y(T(z_c, z_i))]}{E[T_c(z_c', z_i) - T_c(z_c, z_i)]} = \Delta_{d\to c}^{Y_c - Y_d}.$$

C.2 Bias with four treatments

Here, we calculate the bias from a 2SLS estimate in a simple setting with four mutually exclusive treatments. For example, these could be dismissed; convicted without incarceration; convicted with a short carceral sentence; or convicted with a long carceral sentence: $T \in \{d, c, s, l\}$. The mutually-exclusive stringencies would then be: Z_d, Z_c, Z_s, Z_l. We assume CPM and the other assumptions, except for UPM (see Section 3.1 for details).

In the example below, we characterize bias when using differential stringencies to determine the causal effect of conviction vs dismissal. Let's consider two judges who have the same z_s and z_l, but different z_c. Following the notation from Appendix C.1, ωs represent shares of switchers. For example, $\omega_{d\to c}$ represents the proportion of people switching from $T = d$ to $T = c$ when shifting conviction stringency from z_c to z'_c, holding z_s and z_l fixed.

The set of potential movers when changing z_c (holding fixed z_s and z_l) under CPM are: (1) $d \to c$, (2) $s \to c$, (3) $l \to c$, (4) $d \to s$, (5) $d \to l$, and (6) $l \to s$. Note that this is just one possible direction of switches that would be compatible with CPM. For instance, for (6), we could have reversed flows and allowed for $s \to l$ instead $l \to s$; but under CPM we can only have one, not the other. The same applies for (5).

As with 3 treatments, holding z_s fixed means that flows in and out of $T = s$ have to be equal, and holding z_l fixed means flows in and out of $T = l$ have to be equal. This means that $\omega_{s\to c} = \omega_{l\to s} + \omega_{d\to s}$ and $\omega_{d\to l} = \omega_{l\to s} + \omega_{l\to c}$.

The reduced form effect is thus given by:

$$\begin{aligned}
&E[Y(T(z'_c, z_s, z_l)) - Y(T(z_c, z_s, z_l))] = \\
&\Delta^{Y_c-Y_d}_{d\to c}\omega_{d\to c} + [\Delta^{Y_s-Y_d}_{d\to s}\omega_{d\to s} + \Delta^{Y_c-Y_s}_{s\to c}\omega_{s\to c}] + [\Delta^{Y_l-Y_d}_{d\to l}\omega_{d\to l} + \Delta^{Y_c-Y_l}_{l\to c}\omega_{l\to c}] + \Delta^{Y_s-Y_l}_{l\to s}\omega_{l\to s},
\end{aligned} \tag{3}$$

where brackets have been placed around two sets of terms to simplify the explanation of the next steps below.

For any difference in two potential outcomes, we can always rewrite it as $Y_k - Y_j = (Y_k - Y_m) - (Y_j - Y_m)$. Using this, the first term in the square brackets in equation (3) can be rewritten as follows:

$$\begin{aligned}
[\Delta^{Y_s-Y_d}_{d\to s}\omega_{d\to s} + \Delta^{Y_c-Y_s}_{s\to c}\omega_{s\to c}] &= [\Delta^{Y_s-Y_d}_{d\to s}\omega_{d\to s} + \Delta^{Y_c-Y_s}_{s\to c}(\omega_{d\to s} + \omega_{l\to s})] \\
&= [\Delta^{Y_s-Y_d}_{d\to s}\omega_{d\to s} + (\Delta^{Y_c-Y_d}_{s\to c} - \Delta^{Y_s-Y_d}_{s\to c})\omega_{d\to s} + \Delta^{Y_c-Y_s}_{s\to c}\omega_{l\to s}] \\
&= [\Delta^{Y_c-Y_d}_{s\to c}\omega_{d\to s} + (\Delta^{Y_s-Y_d}_{d\to s} - \Delta^{Y_s-Y_d}_{s\to c})\omega_{d\to s} + \Delta^{Y_c-Y_s}_{s\to c}\omega_{l\to s}].
\end{aligned} \tag{4}$$

Similarly, the second term in the square brackets from equation (3) can be rewritten:

$$\begin{aligned}
[\Delta^{Y_l-Y_d}_{d\to l}\omega_{d\to l} + \Delta^{Y_c-Y_l}_{l\to c}\omega_{l\to c}] &= [\Delta^{Y_l-Y_d}_{d\to l}(\omega_{l\to s} + \omega_{l\to c}) + \Delta^{Y_c-Y_l}_{l\to c}\omega_{l\to c}] \\
&= [\Delta^{Y_l-Y_d}_{d\to l}\omega_{l\to s} + \Delta^{Y_l-Y_d}_{d\to l}\omega_{l\to c} + (\Delta^{Y_c-Y_d}_{l\to c} - \Delta^{Y_l-Y_d}_{l\to c})\omega_{l\to c}] \\
&= [\Delta^{Y_l-Y_d}_{d\to l}\omega_{l\to s} + \Delta^{Y_c-Y_d}_{l\to c}\omega_{l\to c} + (\Delta^{Y_l-Y_d}_{d\to l} - \Delta^{Y_l-Y_d}_{l\to c})\omega_{l\to c})].
\end{aligned} \tag{5}$$

So, equation (3) can be written as:

$$
\begin{aligned}
&E[Y(T(z_c', z_s, z_l)) - Y(T(z_c, z_s, z_l))] = \\
&\qquad \Delta_{d\to c}^{Y_c - Y_d}\omega_{d\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{d\to s} + \Delta_{l\to c}^{Y_c - Y_d}\omega_{l\to c} \\
&\qquad + (\Delta_{d\to s}^{Y_s - Y_d} - \Delta_{s\to c}^{Y_s - Y_d})\omega_{d\to s} \\
&\qquad + (\Delta_{d\to l}^{Y_l - Y_d} - \Delta_{l\to c}^{Y_l - Y_d})\omega_{l\to c} \\
&\qquad + \Delta_{l\to s}^{Y_s - Y_l}\omega_{l\to s} + \Delta_{s\to c}^{Y_c - Y_s}\omega_{l\to s} + \Delta_{d\to l}^{Y_l - Y_d}\omega_{l\to s}.
\end{aligned} \tag{6}
$$

Next, the last row of equation (6) can be rewritten as:

$$
\begin{aligned}
&\Delta_{l\to s}^{Y_s - Y_l}\omega_{l\to s} + \Delta_{s\to c}^{Y_c - Y_s}\omega_{l\to s} + \Delta_{d\to l}^{Y_l - Y_d}\omega_{l\to s} \\
&= \Delta_{l\to s}^{Y_s - Y_l}\omega_{l\to s} + (\Delta_{s\to c}^{Y_c - Y_d}\omega_{l\to s} - \Delta_{s\to c}^{Y_s - Y_d}\omega_{l\to s}) + \Delta_{d\to l}^{Y_l - Y_d}\omega_{l\to s} \\
&= \Delta_{l\to s}^{Y_s - Y_l}\omega_{l\to s} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{l\to s} - (\Delta_{s\to c}^{Y_s - Y_l}\omega_{l\to s} + \Delta_{s\to c}^{Y_l - Y_d}\omega_{l\to s}) + \Delta_{d\to l}^{Y_l - Y_d}\omega_{l\to s} \\
&= \Delta_{s\to c}^{Y_c - Y_d}\omega_{l\to s} + (\Delta_{l\to s}^{Y_s - Y_l} - \Delta_{s\to c}^{Y_s - Y_l})\omega_{l\to s} + (\Delta_{d\to l}^{Y_l - Y_d} - \Delta_{s\to c}^{Y_l - Y_d})\omega_{l\to s}.
\end{aligned} \tag{7}
$$

Rewriting equation (6), we get:

$$
\begin{aligned}
&E[Y(T(z_c', z_s, z_l)) - Y(T(z_c, z_s, z_l))] = \\
&\qquad \Delta_{d\to c}^{Y_c - Y_d}\omega_{d\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{d\to s} + \Delta_{l\to c}^{Y_c - Y_d}\omega_{l\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{l\to s} \\
&\qquad + (\Delta_{d\to s}^{Y_s - Y_d} - \Delta_{s\to c}^{Y_s - Y_d})\omega_{d\to s} \\
&\qquad + (\Delta_{d\to l}^{Y_l - Y_d} - \Delta_{l\to c}^{Y_l - Y_d})\omega_{l\to c} \\
&\qquad + (\Delta_{l\to s}^{Y_s - Y_l} - \Delta_{s\to c}^{Y_s - Y_l})\omega_{l\to s} + (\Delta_{d\to l}^{Y_l - Y_d} - \Delta_{s\to c}^{Y_l - Y_d})\omega_{l\to s}.
\end{aligned} \tag{8}
$$

And the first row of equation (8) can be rewritten as:

$$
\begin{aligned}
&\Delta_{d\to c}^{Y_c - Y_d}\omega_{d\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{d\to s} + \Delta_{l\to c}^{Y_c - Y_d}\omega_{l\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{l\to s} \\
=&\Delta_{d\to c}^{Y_c - Y_d}\omega_{d\to c} + \Delta_{s\to c}^{Y_c - Y_d}(\omega_{d\to s} + \omega_{l\to s}) + \Delta_{l\to c}^{Y_c - Y_d}\omega_{l\to c} \\
=&\Delta_{d\to c}^{Y_c - Y_d}\omega_{d\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{s\to c} + \Delta_{l\to c}^{Y_c - Y_d}\omega_{l\to c}.
\end{aligned} \tag{9}
$$

Equation (3) can thus be expressed in terms of $d \to c$ treatment effects (first line of equation (10)) and differences in the same treatment effects between different subgroups (remaining lines of equation (10)):

$$
\begin{aligned}
&E[Y(T(z_c', z_s, z_l)) - Y(T(z_c, z_s, z_l))] = \\
&\underbrace{\Delta_{d\to c}^{Y_c - Y_d}\omega_{d\to c} + \Delta_{s\to c}^{Y_c - Y_d}\omega_{s\to c} + \Delta_{l\to c}^{Y_c - Y_d}\omega_{l\to c}}_{\text{Weighted } d \to c \text{ treatment effects}} \\
&\qquad \underbrace{\begin{aligned}
&+ (\Delta_{d\to s}^{Y_s - Y_d} - \Delta_{s\to c}^{Y_s - Y_d})\omega_{d\to s} \\
&+ (\Delta_{d\to l}^{Y_l - Y_d} - \Delta_{l\to c}^{Y_l - Y_d})\omega_{l\to c} \\
&+ (\Delta_{l\to s}^{Y_s - Y_l} - \Delta_{s\to c}^{Y_s - Y_l})\omega_{l\to s} \\
&+ (\Delta_{d\to l}^{Y_l - Y_d} - \Delta_{s\to c}^{Y_l - Y_d})\omega_{l\to s}
\end{aligned}}_{\text{Differences in subgroup treatment effects}}
\end{aligned} \tag{10}
$$

Next, the denominator of the Wald estimator will be given by:

$$
E[T_C(z_c', z_s, z_l) - T_C(z_c, z_s, z_l)] = \omega_{d\to c} + \omega_{s\to c} + \omega_{l\to c}. \tag{11}
$$

Finally, dividing equation (10) by equation (11), we end up with two terms. The first term is a weighted average of margin-specific treatment effects of moving from $T = d$ to $T = c$ for three groups of compliers. The weights here are all weakly positive and sum to one. The second term is a weighted average of the four bias terms, where each term is the difference in the treatment effect of a given margin for two different sets of compliers, and the weights are weakly positive.[56] This implies that the bias will depend on the heterogeneity of treatment effects. For example, under a constant effects assumption, the bias terms are all zero.

Note that this expression parallels the expression derived in Appendix C.1 where we have a proper weighted average of the margin-specific effects of interest and an additive weighted bias term, where the size of the bias depends on how heterogeneous the margin-specific treatment effects are.

C.3 Interpreting conditional 2SLS estimates

In the main paper, we consider the comparison of two judges that have the same stringency on one margin, but different stringencies on another margin. For example, for the Wald estimands, we consider two judges that have the same incarceration stringency $Z_i = z_i$, but different conviction stringencies Z_c. Here, we consider what the IV estimand identifies when exclusion, random assignment, relevance, and the conditional pairwise monotonicity (CPM) assumptions hold, and what changes when swapping out CPM for the unordered partial monotonicity assumption (UPM). Specifically, we consider the case where we first condition on a set of judges who have the same incarceration stringency $Z_i = z_i$ but potentially differ in their conviction stringency. We assume Z_c can take on values $\{z_c^0, ..., z_c^K\}$ where the set is ordered such that $z_c^k \leq z_c^{k'}$ if $k \leq k'$.

In Appendix C.1, we derive the Wald estimand when comparing two judges with the same incarceration stringency but different conviction stringencies. This gives us:

$$\text{Wald}(z_c', z_c|z_i) =$$
$$\frac{E[Y(T(z_c', z_i)) - Y(T(z_c, z_i))]}{E[T_c(z_c', z_i) - T_c(z_c, z_i)]} = \frac{E[Y|Z_c = z_c', Z_i = z_i] - E[Y|Z_c = z_c, Z_i = z_i]}{E[T_c|Z_c = z_c', Z_i = z_i] - E[T_c|Z_c = z_c, Z_i = z_i]} =$$
$$\underbrace{\frac{\omega_{d\to c}\Delta_{d\to c}^{Y_c - Y_d} + \omega_{i\to c}\Delta_{d\to i}^{Y_c - Y_d}}{\omega_{d\to c} + \omega_{i\to c}}}_{\text{Weighted avg. of } Y_c - Y_d \text{ treatment effects}} + \underbrace{\frac{\omega_{i\to c}}{\omega_{d\to c} + \omega_{i\to c}}\left[\Delta_{d\to i}^{Y_i - Y_c} - \Delta_{i\to c}^{Y_i - Y_c}\right]}_{\text{Bias term}}.$$

Now, we derive what is identified in this setting by IV when using judges with varying conviction stringency but the same incarceration stringency. For notational simplicity, we leave the conditioning on $Z_i = z_i$ implicit throughout this derivation. The IV estimand is given by: $\alpha^{IV} = \frac{E[Y(Z_c - E[Z_c])}{E[T_c(Z_c - E[Z_c]]} = \frac{cov(Y, Z_c)}{cov(T_c, Z_c)}$ Following Imbens and

[56] As discussed above, $\omega_{s\to c} = \omega_{l\to s} + \omega_{d\to s}$ and $\omega_{d\to l} = \omega_{l\to s} + \omega_{l\to c}$. With these two identities, it is straightforward algebra to show that $\omega_{d\to c} + \omega_{s\to c} + \omega_{l\to c} = \omega_{d\to s} + \omega_{l\to c} + \omega_{l\to s} + \omega_{l\to s}$, making the second term a weighted average of the four bias terms.

Angrist (1994) closely, first consider the numerator:

$$
\begin{aligned}
E[Y \cdot (Z_c - E[Z_c])] &= \sum_{l=0}^{K} \lambda_l E[Y|Z_c = z_c^l](z_c^l - E[Z_c]) \\
&= \sum_{l=0}^{K} \lambda_l E[Y|Z_c = z_c^0](z_c^l - E[Z_c]) \\
&+ \sum_{l=1}^{K} \lambda_l \sum_{k=1}^{l} \left(E[Y|Z_c = z_c^k] - E[Y|Z_c = z_c^{k-1}]\right)(z_c^l - E[Z_c]) \\
&= \sum_{k=1}^{K} \left(\left(E[Y|Z_c = z_c^k] - E[Y|Z_c = z_c^{k-1}]\right) \sum_{l=k}^{K} \lambda_l (z_c^l - E[Z_c]) \right) \\
&= \sum_{k=1}^{K} \text{Wald}(z_c^k, z_c^{k-1}|z_i) \left(\left(E[T_c|Z_c = z_c^k] - E[T_c|Z_c = z_c^{k-1}]\right) \sum_{l=k}^{K} \lambda_l (z_c^l - E[Z_c]) \right)
\end{aligned}
$$

Next, the denominator using a similar set of steps can be written as:

$$
\begin{aligned}
E[T_c(Z_c - E[Z_c])] &= \sum_{l=0}^{K} \lambda_l E[T_c|Z_c = z_c^l](z_c^l - E[Z_c]) \\
&= \sum_{k=1}^{K} \left(\left(E[T_c|Z_c = Z_c^k] - E[T_c|Z_c = Z_c^{k-1}]\right) \sum_{l=k}^{K} \lambda_l (z_c^l - E[Z_c]) \right)
\end{aligned}
$$

Putting these together, we get:

$$
\alpha^{IV} = \sum_{k=1}^{K} \theta_{k,k-1} \text{Wald}(z_c^k, z_c^{k-1}|z_i)
$$

where

$$
\theta_{k,k-1} = \frac{\left(E[T_c|Z_c = z_c^k] - E[T_c|Z_c = z_c^{k-1}]\right) \sum_{l=k}^{K} \lambda_l (z_c^l - E[Z_c]))}{\sum_{k=1}^{K} \left(E[T_c|Z_c = z_c^k] - E[T_c|Z_c = z_c^{k-1}]\right) \sum_{l=k}^{K} \lambda_l (z_c^l - E[Z_c])}.
$$

Other than the implicit conditioning on $Z_i = z_i$, this formula is the same as the formula derived in Imbens and Angrist (1994), but the Wald estimand may not always be a pairwise LATE as in Imbens and Angrist (1994). Under the CPM assumption and other standard IV assumptions, the Wald estimand recovers the term given in equation (7) in Section 3. Thus, rather than a weighted average of pairwise local-average treatment effects, we recover a weighted average of the potentially biased margin-specific local average treatment effects. Under the stronger UPM assumption, or under a constant-effects assumption, equation (7) reduces down to a standard margin-specific LATE as in Imbens and Angrist (1994) and the conditional 2SLS estimand can be interpreted as a positively-weighted average of LATEs where the weights sum to one.

Based on these results, a natural path forward would be to estimate separate 2SLS regressions, conditional on each value of Z_i. Angrist and Pischke (2009) propose doing this in a single 2SLS regression where the instrument Z_c is interacted with all possible values of Z_i. They refer to this as the "saturate and weight" approach. However, in finite samples, this approach can result in many weak instruments and the problems that arise in such setting (Angrist and Pischke, 2009; Blandhol et al., 2022).

Table C.1 shows estimates where the treatment and instrument have been interacted with the other judge stringency. Some caution should be taken in interpreting these estimates, as splitting our sample into thirds quickly leads to large standard errors and small first-stage F-statistics. We report four specifications that include increasingly rich sets of controls which are described in the table notes. Across all specifications for the impacts of conviction, the majority of estimates are positive, nearly all estimates are positive when including richer controls, and all negative estimates are statistically insignificant with very large standard errors. Across estimates we see very similar trends with large impacts of conviction in the first year that accumulate over time.

Table C.1: **The impacts of conviction and incarceration on recidivism: interacting treatment and instruments with stringency bins**

	Impacts of conviction with incarceration stringency bins				Impacts of incarceration with dismissal stringency bins			
	(1) **Year 1**	(2) **Year 2-4**	(3) **Year 5-7**	(4) **Year 1-7**	(5) **Year 1**	(6) **Year 2-4**	(7) **Year 5-7**	(8) **Year 1-7**
Specification 1								
Convict x bottom 3rd	0.121	-0.048	0.118	0.161	-0.016	-0.108	0.039	-0.114
	(0.075)	(0.140)	(0.108)	(0.165)	(0.067)	(0.088)	(0.089)	(0.117)
Convict x middle 3rd	0.263**	0.184	0.290	0.574**	-0.187**	0.023	-0.050	-0.081
	(0.125)	(0.281)	(0.180)	(0.289)	(0.073)	(0.096)	(0.091)	(0.134)
Convict x top 3rd	0.345	-1.060	0.463	-0.106	-0.025	0.018	0.053	0.091
	(0.440)	(0.966)	(0.643)	(0.949)	(0.054)	(0.093)	(0.075)	(0.115)
Specification 2								
Convict x bottom 3rd	0.112	-0.042	0.157	0.206	-0.017	-0.105	0.014	-0.137
	(0.089)	(0.148)	(0.129)	(0.195)	(0.071)	(0.093)	(0.094)	(0.124)
Convict x middle 3rd	0.275*	0.114	0.372	0.628*	-0.194**	0.006	-0.039	-0.099
	(0.154)	(0.306)	(0.229)	(0.357)	(0.078)	(0.105)	(0.096)	(0.147)
Convict x top 3rd	0.224	-0.981	0.460	-0.053	-0.026	0.057	0.065	0.126
	(0.393)	(0.752)	(0.600)	(0.865)	(0.058)	(0.096)	(0.079)	(0.120)
Specification 3								
Convict x bottom 3rd	0.073	0.111	0.131	0.275**	-0.073	-0.098	0.026	-0.146
	(0.055)	(0.086)	(0.088)	(0.121)	(0.076)	(0.081)	(0.080)	(0.110)
Convict x middle 3rd	0.109	0.122	0.218*	0.391**	-0.271	-0.007	-0.044	-0.158
	(0.075)	(0.122)	(0.117)	(0.165)	(0.218)	(0.208)	(0.204)	(0.295)
Convict x top 3rd	0.112	-0.067	0.236	0.266	-0.041	0.063	-0.034	0.055
	(0.101)	(0.148)	(0.156)	(0.227)	(0.141)	(0.177)	(0.155)	(0.234)
Specification 4								
Convict x bottom 3rd	0.032	-0.007	0.008	0.030	-0.103***	-0.069*	-0.058*	-0.141***
	(0.028)	(0.043)	(0.038)	(0.054)	(0.023)	(0.036)	(0.032)	(0.044)
Convict x middle 3rd	0.021	0.023	0.008	0.054	-0.065**	-0.042	-0.045	-0.094*
	(0.034)	(0.051)	(0.048)	(0.068)	(0.027)	(0.041)	(0.036)	(0.052)
Convict x top 3rd	0.057*	0.032	0.085**	0.097	-0.004	0.079**	0.068*	0.138***
	(0.030)	(0.049)	(0.042)	(0.061)	(0.025)	(0.040)	(0.036)	(0.052)
Observations	183381	183381	183381	183381	183381	183381	183381	183381

Note: This table shows 2SLS estimates of the impact of conviction and incarceration on future charges. For conviction, each specification interacts conviction and conviction stringency with residualized incarceration stringency terciles. For incarceration, each specification interacts incarceration and incarceration stringency with residualized dismissal stringency terciles. Specification 1 includes our standard set of fixed effects: court-by-year, court-by-month of year, and day-of-week dummies. Specification 2 replaces court-by-year and court-by-month of year dummies with court-by-year-by-month of year dummies. As the tercile interactions only condition on three bins of incarceration or dismissal stringency, Specification 3 further adds dummies for deciles of residualized judge incarceration or dismissal stringency. The final specification replaces the conviction instrument or incarceration instrument interacted with residualized incarceration or dismissal stringency terciles with judge dummies. Standard errors are clustered at the judge-year level. The sample is restricted to cases observed for 7 years. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

C.4 Average UPM

UPM$(Z_c|Z_i)$ represents a form of "strict" monotonicity, in that it is defined over every z_c shift, holding z_i constant. Yet, similar to what has been shown in the binary context, such a strict assumption is not necessary to yield a causal estimand. Frandsen et al.

(2023) propose a condition called "average monotonicity," which requires a positive correlation between each individual's *potential* treatment status and judge stringency across all judges. They show that average monotonicity is sufficient (along with other standard IV assumptions) to yield a causal estimand in the binary treatment context.

Here we propose an extension of Frandsen et al.'s (2023) average monotonicity condition into the three treatment setting and refer to this as "average UPM($Z_c|Z_i$)." We focus on the condition that is relevant to the specification where we are instrumenting for conviction and controlling for the incarceration stringency; average UPM($Z_i|Z_d$) is defined similarly.

We first introduce an additional piece of notation. Let G be a group variable where $g \in G$ maps (Z_c, Z_i) onto potential treatment $T_c(Z_c, Z_i)$. G is the collective and mutually exclusive set of groups g. In the binary treatment, binary instrument context, G consists of compliers, defiers, always takers, and never takers.

A5b: Average UPM($Z_c|Z_i$): For all (g, z_i) in the support of (G, Z_i) the following conditions must hold:

$$Cov(T_c(Z_c, Z_i), Z_c|Z_i = z_i, G = g) \geq 0 \tag{12}$$

$$Cov(T_i(Z_c, Z_i), Z_c|Z_i = z_i, G = g) = 0 \tag{13}$$

To illustrate a difference between UPM($Z_c|Z_i$) and average UPM($Z_c|Z_i$), consider a shift from z_c to $z_c' > z_c$, holding z_i constant. If there exists a group g for whom this instrument shift would induce them from conviction to dismissal, UPM($Z_c|Z_i$) would be violated but average UPM($Z_c|Z_i$) might not be. As long as the probability of conviction for each group is positively correlated with the overall conviction propensity of judges, average UPM($Z_c|Z_i$) is satisfied.

Average UPM($Z_c|Z_i$), along with A1-A4, is sufficient for equations (3) and (4) to yield margin-specific and causal estimands. We build off of Blandhol et al. (2022) for the proof. First, note that the second line of A5b, combined with A2 and A3 (random assignment and exclusion) assure that the exogeneity condition outlined in Blandhol et al. (2022) is met. In our setting, this exogeneity condition means that $G, Y(T = c) \perp Z_c|Z_i$. G is orthogonal to Z_c (conditional on Z_i) due to the random assignment assumption. $Y(T = c)$ is orthogonal to Z_c because, if you hold Z_i fixed, Z_c will not be correlated with the probability of incarceration for any group.

With exogeneity in hand, the remainder of the proof is provided by Blandhol et al. (2022). Blandhol et al. (2022) focus on a condition they call "monotonicity-correct," which they show is sufficient for the 2SLS estimator with covariates to be weakly causal (i.e., the weights on all group-specific treatment effects are weakly positive and the estimate does not depend on the levels of the dependent variable). In the appendix, they derive the monotonicity condition that is both sufficient and necessary for weakly causal estimates, which is the condition in line one of A5b, when written in our notation and in the terms relevant to our setting.[57] They do not focus on this condition in the

[57] The necessary and sufficient condition for weakly causal estimates is presented in the paragraph between equation (28) and equation (29) in the appendix proof for Proposition 9 (page 50) of the version from August 9, 2022. Our Z_c would be written $\dot{Z}$ in their notation, our Z_i would be their X, and our $T_c^g(Z_c)$ would be

main text because "such fortuitous averaging would be difficult to defend." In the judge IV context, however, this "fortuitous averaging" could naturally occur. For instance, a judge who punishes harshly overall may be relatively lenient on certain types of offenders. This would violate both the monotonicity-correct condition as well as UPM. But as long as relatively harsh judges increase punishment *on average* for all groups, an occasional judge who bucks the trend for certain groups is not a problem.

C.5 Interpreting 2SLS estimates with controls

Appendix section C.3 derived the 2SLS estimand when conditioning on a specific value of Z_i. The estimation results reported in Section 4 control for Z_i rather than condition. This section discusses how to interpret these 2SLS estimates. In particular, following Blandhol et al. (2022), 2SLS specifications that control for Z_i (and potentially other covariates) can still be interpreted as a positively-weighted sum of the Wald estimates we derived in Section 3, as long as one additional assumption is met.

Blandhol et al. (2022) considers what 2SLS recovers when covariates are included as controls, but are not fully saturated as in the "saturate and weight" approach. They show that covariates can introduce substantial bias and result in estimands that are not what they call "weakly causal." They define an estimand as weakly causal when it (i) does not depend on the levels of the potential outcomes when holding treatment effects (differences) constant and (ii) it does not apply negative weights to any subgroup. Blandhol et al. (2022) goes on to discuss what assumptions are necessary and sufficient for 2SLS with controls to recover weakly causal parameters. For our setting, with a scalar multi-valued instrument, one additional assumption needs to hold:[58]

A4b. Rich covariates: The linear projection of Z on X is equal to the conditional expectation of Z given X. That is $L[Z|X] = X'E[XX']^{-1}E[XZ] = E[Z|X]$.

Assumption A4b implies that we need to include a rich set of controls. Note that assumption A4b differs from assumption A4 as Section 3.2 abstracted away from covariates. Here we provide the more general version of the assumption, which allows for other covariates. When the only covariate is Z_i, we need rich controls for Z_i. When instruments are only randomly assigned conditional on a vector of covariates $\mathbf{X}$, then we must include a sufficiently rich set of controls for the full vector of covariates, including Z_i.

Blandhol et al.'s (2022) Proposition 11 provides an expression for what the 2SLS estimand recovers. A small rearrangement of that expression allows it to be written as a positively-weighted average of Wald estimands. Under assumptions A1-A5 or A1-A4 and A6, these Wald estimands are equivalent to those we derive in Section 3.4. Thus, under assumptions A1-A3, A4b and A6, 2SLS recovers a positively-weighted average of terms that are margin-specific causal effects plus additive bias terms. Under assumptions A1-A3, A4b, and A5, 2SLS recovers a positively-weighted average of

$\mathbb{1}(Z \in \mathbb{Z}_j(g))$.

[58]Note that assumptions A1-A3, and A5 satisfy the other needed assumptions in Blandhol et al. (2022). In particular, A5 implies their "Ordered strong monotonicity" (OSM). Assumption A6 also satisfies the OSM, but violates their definition of exclusion, which can result in biased Wald estimates, similar to those we derive under CPM.

margin-specific treatment effects.

Table C.2 shows that our estimates are not sensitive to the richness of our control variables. Each specification adds increasingly detailed sets of dummies for place, time, and the other judge stringency as described in the table notes. All specifications are similar to the estimates we report in the main paper, and trend towards larger estimates when including richer set of controls. Like our main estimates, we find large increases in recidivism from conviction that accumulate over time, while incarceration has a negative effect in the first year, which remains relatively constant when looking at one year, one to four year, or one to seven years.

Table C.2: The impacts of conviction and incarceration on recidivism: robustness to richness of controls

	Impacts of conviction				Impacts of incarceration			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Year 1	**Year 2-4**	**Year 5-7**	**Year 1-7**	**Year 1**	**Year 2-4**	**Year 5-7**	**Year 1-7**
Specification 1								
Fut. charge	0.100**	0.123	0.104	0.290***	-0.100***	-0.048	-0.011	-0.099*
	(0.051)	(0.083)	(0.080)	(0.109)	(0.029)	(0.046)	(0.041)	(0.060)
Fut. conviction	0.134***	0.159**	0.074	0.354***	-0.113***	-0.067	0.005	-0.133**
	(0.048)	(0.079)	(0.076)	(0.106)	(0.028)	(0.046)	(0.039)	(0.058)
Fut. incarceration	0.101**	0.084	-0.005	0.251***	-0.076***	-0.021	0.035	-0.059
	(0.042)	(0.069)	(0.061)	(0.093)	(0.024)	(0.039)	(0.032)	(0.050)
Specification 2								
Fut. charge	0.089	0.151	0.154	0.350***	-0.103***	-0.041	-0.017	-0.105*
	(0.060)	(0.094)	(0.095)	(0.128)	(0.031)	(0.050)	(0.044)	(0.064)
Fut. conviction	0.129**	0.188**	0.123	0.425***	-0.118***	-0.062	0.002	-0.139**
	(0.057)	(0.090)	(0.091)	(0.126)	(0.030)	(0.049)	(0.042)	(0.062)
Fut. incarceration	0.106**	0.107	0.035	0.327***	-0.078***	-0.025	0.031	-0.073
	(0.048)	(0.078)	(0.074)	(0.110)	(0.026)	(0.042)	(0.034)	(0.054)
Specification 3								
Fut. charge	0.116**	0.153*	0.097	0.320***	-0.100***	-0.038	-0.015	-0.094
	(0.054)	(0.089)	(0.081)	(0.115)	(0.031)	(0.047)	(0.042)	(0.062)
Fut. conviction	0.153***	0.192**	0.071	0.403***	-0.116***	-0.058	-0.008	-0.131**
	(0.053)	(0.085)	(0.077)	(0.113)	(0.030)	(0.046)	(0.040)	(0.060)
Fut. incarceration	0.119***	0.120	-0.021	0.292***	-0.075***	-0.018	0.023	-0.068
	(0.044)	(0.074)	(0.063)	(0.099)	(0.025)	(0.039)	(0.033)	(0.053)
Specification 4								
Fut. charge	0.108	0.191*	0.148	0.391***	-0.103***	-0.043	-0.035	-0.119*
	(0.067)	(0.103)	(0.099)	(0.140)	(0.034)	(0.052)	(0.045)	(0.066)
Fut. conviction	0.155**	0.225**	0.119	0.487***	-0.121***	-0.065	-0.025	-0.158**
	(0.065)	(0.099)	(0.095)	(0.140)	(0.033)	(0.050)	(0.043)	(0.064)
Fut. incarceration	0.130**	0.146*	0.013	0.378***	-0.078***	-0.036	0.009	-0.102*
	(0.054)	(0.086)	(0.077)	(0.122)	(0.028)	(0.043)	(0.036)	(0.057)
Observations	183371	183371	183371	183371	183371	183371	183371	183371

Note: This table reports estimates of the impact of conviction and incarceration on our three measures of recidivism. Each specification adds richer controls. Specification 1 includes the fixed effects included in the paper: court-by-year, court-by-month of year, and day of week dummies, plus percentile dummies for residualized judge incarceration or dismissal stringency. Specification 2 matches specification 1 but swaps out court-by-year and court-by-month of year fixed effects with court-by-year-by-month of year fixed effects. Specification 3 includes the main place and location fixed effects plus year-by-decile of residualized incarceration or dismissal stringency dummies. Specification 4 is the same as specification three, but swaps out year-by-court and year-by-month of year dummies with court-by-year-by-month of year dummies. Standard errors are clustered at the judge-year level. The sample is restricted to cases observed for 7 years. Stars denote * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

D Validating assumptions A1-A4

In this section, we discuss whether Assumptions A1-A4 from Section 3 are supported by features of the institutional environment and provide empirical evidence, based on a standard battery of tests, to help assess their validity.

Relevance. Here, we explain the various ways through which judges can influence both conviction and incarceration outcomes, expanding on Section 2.2. We also present empirical evidence of the relevance of judges' influence on these decisions.

Judges influence conviction in several ways. In all cases, they have the latitude to dismiss charges if they find the evidence insufficient. They are directly responsible for adjudicating guilt during bench trials (that is, trials by judge, without lay jurors). They also exert indirect influence on the likelihood of conviction through multiple channels. First, they make the determination on various pretrial motions, which can have a large impact on the likelihood of conviction. For example, they can refuse to grant a continuance if a key witness does not show up to court on a given day. They rule on the admissibility of evidence, including critical pieces like confessions, possession of contraband, or expert testimony. Finally, they can affect jury composition by ruling on motions to strike and by formulating jury instructions.

Judges also influence sentences in several ways. In the case of a bench trial, they directly choose the sentence. In the case of guilty pleas, they can reject the negotiated plea agreement. Moreover, their reputation as a tough or lenient judge might shape what offers prosecutors and defense attorneys are willing to put forward (LaCasse and Payne, 1999). For example, if the judge has a reputation for choosing short sentences, the prosecutor may adjust and offer shorter sentences as part of the plea deal.

Empirically, we find persistent differences in case outcomes across judges. Panels A and B of Figure 4 in the main paper shows the histogram of judge noncarceral conviction stringency (Panel A) and judge incarceration stringency (Panel B). Each panel plots the residualized leave-one-out judge propensity for that case outcome. In both panels there is substantial variation in the instrument.[59] Both panels also plot the local linear regression of the residualized court outcome on the instrument.

Panel C of Figure 4 plots the residualized noncarceral conviction and incarceration stringencies against each other. The two instruments are negatively correlated, which is expected, since the probability of all three case outcomes adds up to one. Importantly for our research design, there is substantial variation in Z_c across most of the support of Z_i and vice versa.

Table 2 in the main paper presents our first-stage estimates, and confirms that judge stringency has a large and statistically significant effect on conviction and incarceration. The first three columns show the results for the first stage on noncarceral conviction. The first column shows the loading on conviction stringency when only including interacted court and time fixed effects as controls. The second column adds detailed case-level controls. The third column additionally controls for incarceration stringency. Across all three specifications, the conviction stringency remains large, with partial F-statistics between 165 and 360. Columns 4 through 6 perform similar first-stage regressions on incarceration stringency, with the sixth column controlling

[59] Conviction stringency was constructed by residualizing an indicator for noncarceral conviction against county-by-year, county-by-month-of-year, and day-of-week fixed effects, then constructing leave-one-out averages at the judge-by-three-year level. Incarceration stringency is similarly constructed.

for dismissal stringency. Again, the loading on incarceration stringency is large and statistically significant, with partial F-statistics between 288 and 351.

Random assignment. As discussed in Section 2.1, within our sample, cases are quasi-randomly assigned to judges within court. There is either actual randomization, or case assignment is done based on scheduling or judge availability.[60] In addition, we confirm empirically that judge stringency is largely not predicted by case characteristics. In Table 3 of the main paper, we show that case characteristics are strong predictors of being convicted and of being incarcerated (columns 1 and 3). We then show that case characteristics largely do not predict with judge conviction stringency (column 2) or incarceration stringency (column 4). For the few instances where covariates have statistically significant loadings, the predicted difference in stringency tends to be very small. Table D.1 replicates columns (2) and (4) from Table 3 but using standardized stringency measures. The odd columns regress non-carceral conviction stringency and incarceration stringency on case characteristics where the stringency measure has been standardized to have a mean of zero and a standard deviation of one. We see that the largest loading is on an indicator for assault cases which predicts assault cases are associated with a 0.015 standard deviation change in stringency. The odd columns do not account for variation in stringency caused by variation over time or across courts. The even columns replicate the regressions from the odd columns but first residualize the stringency instruments for the set of district and time fixed effects used in our analysis before standardizing. Here we find the largest coefficient in absolute value to be 0.036. Overall, this suggests that while there are a few instances where covariates have statistically significant loadings, these loadings imply small predicted differences in stringency.

We additionally provide robustness showing that our results are not sensitive to fully excluding certain types of cases from our analysis, that is, both from the construction of the stringency instruments and from the 2SLS regressions. Table D.2 provides our main OLS and IV estimates for noncarceral conviction for four different subsets of cases. In Panel A we drop all cases involving assault charges when constructing the instrument and running the analyses, as this is the offense type that is most predictive of both noncarceral conviction and incarceration stringency in our balance tables. These results are broadly similar to our main estimates in Table 4 with the same sign and magnitude, with the two main differences being that point estimates are moderately smaller, and standard errors are somewhat larger (likely due to the 15% reduction in sample size).

Panel B and C repeat the prior exercise, but throw out cases with drug offenses and cases with violent offenses, respectively. We focus on drug offenses and violent offenses since these are offense types where we believe judges may be most likely to differ in opinion on appropriate case outcome. We again find that dropping these offense types lead to broadly similar results, with similar point estimates and somewhat larger standard errors. Finally, Panel D drops cases with assault, sexual assault, fraud, or traffic charges (all offense types where there is any evidence of imbalance in Table 3). Estimates again are broadly similar. For this specification, we lose statistical significance on several coefficients that are significant in our main table. This may

[60] In Appendix E, we show that IV estimates are similar when we remove courts where assignment is by judge availability.

be in part due to moderately smaller (though similar in magnitude) estimates in Year 1-7, but is largely driven by larger standard errors, likely because of the 39% reduction in sample size. Table D.3 replicates the analysis in Table D.2, but for incarceration. Similar to results for conviction, results are broadly similar. Overall, Tables D.2 and D.3 suggest that our results are not driven by potential exclusion violations.

In our general robustness analysis in Appendix Section E, we compare how our estimates vary under several different assumptions. There we additionally include results in Figures E.3-E.6 where we use the full sample, but allow judge stringency to differ by (1) if the case has an assault charge or not and (2) if the case has a drug charge or not. These alternative constructions of our instrument are more demanding on our data, but also find statistically significant increases in recidivism from non-carceral conviction 1-7 years after the case, and statistically significant decreases from incarceration only in the first year after the case.

Finally, as additional evidence of exogeneity, first-stage estimates barely change when we add controls to our first-stage regression, as seen by comparing columns 2 and 3 and columns 5 and 6 of Table 2 in the main paper.

Exclusion. Our identification strategy relies on the assumption that the conviction stringency instrument affects recidivism outcomes only through its effects on conviction once we control for judges' incarceration stringency, and vice versa. Here we argue that the risk of potential exclusion violations is low. We consider sentence length to be the most important potential violation. For example, if a high-conviction judge also tends to give longer sentences (holding incarceration probability fixed) it would violate exclusion. We test for this by regressing sentence length on our measure of conviction stringency, controlling for incarceration stringency. As shown in Appendix Table D.4, we find no evidence of a violation of the exclusion restriction for conviction. In addition, when we re-estimate the main IV regressions with an additional control for sentence length stringency or probability of sentence length shorter than 6 months and longer than 1 year and 4 years, we find that the main conclusions are unchanged (see Appendix Figures E.3-E.6).[61]

A judge may influence other aspects of the case, such as probation and parole terms, or fines and fees. While we do not rule these channels out, we do not expect them to be as important. There are a number of large-scale RCTs that have shown probation and parole conditions do not affect recidivism (for a recent review, see Doleac, 2023). There is also a small but growing literature showing that court fines and fees do not affect recidivism (Pager et al., 2022; Finlay et al., 2023; Lieberman et al., 2023). The findings in this literature add confidence that even if judge stringency in conviction and incarceration were correlated with these other factors, they would not bias the results.

We do not expect decisions made at the beginning of the case, such as bail or pretrial detention, to lead to an exclusion violation. These decisions are made by bail magistrates that have no later influence over the case. Furthermore, there is often a month between the date of the arrest and when the defendant arrives at circuit court and the judge is assigned. It follows that the Circuit Court judge has no influence over these early aspects of the defendant's criminal justice experience.

[61]We define sentence length stringency as the tri-yearly leave-one-out average sentence for the judge handling the case, setting sentences to 0 if a person has no carceral sentence and to the sentence length in months if a person is sentenced to a carceral sentence.

Although we are comfortable arguing that conviction and incarceration are likely the most important channels by which criminal justice involvement can affect recidivism, we see expanding beyond a trinary model to include these alternatives as an important area of future research. Given the tradeoffs, we have chosen tractability over complexity.

Lastly, in Appendix Table E.1, we present reduced-form estimates, which regress outcomes on our instruments, and do not require the exclusion assumption to hold.

Monotonicity. As discussed previously, one consequence of CPM (and the stronger condition, UPM) is that there will only be one-way flows across any margin. Here we present some empirical evidence in support of this assumption. Following common practice for binary treatments (see, for example, Bhuller et al., 2020 or Norris et al., 2021), we conduct split-sample regressions where the data is bifurcated using observed characteristics such as race and gender. Judge stringency is then estimated on each subsample, and the first stage regression is then run on its complement, controlling for stringency along the other margin. If the "no defiers" condition holds, we would expect positive coefficients for each sub-sample. Appendix Tables D.5 and D.6 report the coefficient on the instrument from split-sample first-stage regressions. Each row presents a particular case characteristic. For example, the first row breaks our sample into whether a person has a drug charge or does not. The "Zero" column for that row calculates the stringency on the individuals without a drug charge and then estimates the first stage on those with a drug charge, reporting the coefficient on that instrument. The "One" column does the converse of that – calculates the stringency on the individuals with a drug charge and then estimates the first stage on those without a drug charge, reporting the coefficient on that instrument. For both conviction and incarceration, we find positive coefficients on the instrument for all split-sample estimates. Also see Section 4.5 of the paper where we present a test of the UPM assumption.

Table D.1: Balance: outcomes in standard deviations

	Conv. string.	Resid. conv. string.	Incar. string.	Resid. incar. string.
	(1)	(2)	(3)	(4)
Any prior conv.	-0.0004	-0.0011	0.0031	0.0073
	(0.0025)	(0.0060)	(0.0027)	(0.0062)
Female	-0.0043*	-0.0102*	0.0025	0.0058
	(0.0023)	(0.0056)	(0.0024)	(0.0057)
Black	0.0031	0.0075	-0.0028	-0.0065
	(0.0022)	(0.0054)	(0.0022)	(0.0053)
Has misdemeanor	0.0013	0.0031	0.0041	0.0097
	(0.0037)	(0.0089)	(0.0039)	(0.0090)
Drugs	0.0045	0.0108	-0.0003	-0.0006
	(0.0032)	(0.0077)	(0.0035)	(0.0081)
Larceny	0.0035	0.0085	0.0041	0.0096
	(0.0027)	(0.0066)	(0.0029)	(0.0068)
Assault	-0.0148***	-0.0355***	0.0142***	0.0332***
	(0.0031)	(0.0075)	(0.0031)	(0.0072)
Fraud	0.0047	0.0114	0.0068*	0.0160*
	(0.0034)	(0.0082)	(0.0039)	(0.0090)
Traffic	-0.0036	-0.0088	0.0076*	0.0177*
	(0.0043)	(0.0103)	(0.0045)	(0.0104)
Burglary	-0.0016	-0.0039	0.0056	0.0132
	(0.0039)	(0.0093)	(0.0042)	(0.0098)
Robbery	-0.0026	-0.0062	0.0043	0.0101
	(0.0052)	(0.0124)	(0.0055)	(0.0128)
Sexual assault	-0.0085	-0.0205	0.0143**	0.0335**
	(0.0067)	(0.0161)	(0.0070)	(0.0163)
Kidnapping	-0.0063	-0.0151	0.0070	0.0164
	(0.0076)	(0.0182)	(0.0075)	(0.0176)
Murder	-0.0149	-0.0357	0.0118	0.0275
	(0.0108)	(0.0259)	(0.0117)	(0.0273)
F-stat joint F-test	3.757	3.757	2.666	2.666
P-value joint F-test	0.000	0.000	0.001	0.001
Observations	183,381	183,381	183,381	183,381

Note: This table replicates Table 3, but where the left-hand-side variable in the regression (i.e., either noncarceral conviction, incarceration, noncarceral conviction stringency, or incarceration stringency) has been standardized in the sample to have a mean of zero and a standard deviation of one. For each outcome, we regress the standardized outcome on case characteristics. Regression includes court-by-year fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. The offenses are ordered by their prevalence in the data. The balance outcomes shown are for those cases adjudicated in 2012 or earlier, representing our seven-year sample. Star denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.2: Noncarceral conviction and recidivism– robustness to unbalanced offenses

	Year 1		Year 2-4		Year 5-7		Year 1-7	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Panel A: No assault offenses								
Fut. charge	0.004*	0.099*	0.015***	0.062	0.011***	0.045	0.026***	0.180*
	(0.002)	(0.053)	(0.003)	(0.080)	(0.003)	(0.082)	(0.004)	(0.105)
Fut. conviction	0.006***	0.122**	0.017***	0.106	0.012***	0.038	0.028***	0.254**
	(0.002)	(0.050)	(0.003)	(0.079)	(0.003)	(0.078)	(0.004)	(0.105)
Fut. incarceration	0.005***	0.105**	0.014***	0.018	0.009***	-0.035	0.025***	0.169*
	(0.002)	(0.043)	(0.003)	(0.067)	(0.002)	(0.064)	(0.003)	(0.090)
Observations	155100	155100	155100	155100	155100	155100	155100	155100
Panel B: No drug offenses								
Fut. charge	-0.013***	0.147**	-0.010***	0.188	-0.003	0.084	-0.015***	0.334**
	(0.003)	(0.073)	(0.004)	(0.127)	(0.003)	(0.109)	(0.005)	(0.151)
Fut. conviction	-0.010***	0.207***	-0.006	0.238*	-0.001	0.085	-0.011**	0.430***
	(0.003)	(0.069)	(0.003)	(0.125)	(0.003)	(0.107)	(0.004)	(0.157)
Fut. incarceration	-0.007***	0.164***	-0.006**	0.175	-0.002	-0.044	-0.011***	0.323**
	(0.002)	(0.063)	(0.003)	(0.111)	(0.003)	(0.087)	(0.004)	(0.136)
Observations	125602	125602	125602	125602	125602	125602	125602	125602
Panel C: No violent offenses								
Fut. charge	0.004*	0.096*	0.016***	0.072	0.012***	0.045	0.028***	0.193*
	(0.002)	(0.057)	(0.003)	(0.086)	(0.003)	(0.086)	(0.004)	(0.111)
Fut. conviction	0.006***	0.126**	0.018***	0.107	0.013***	0.031	0.031***	0.263**
	(0.002)	(0.054)	(0.003)	(0.084)	(0.003)	(0.082)	(0.004)	(0.112)
Fut. incarceration	0.005***	0.110**	0.016***	0.024	0.010***	-0.046	0.028***	0.176*
	(0.002)	(0.047)	(0.003)	(0.071)	(0.002)	(0.068)	(0.003)	(0.097)
Observations	149473	149473	149473	149473	149473	149473	149473	149473
Panel D: No assault, sexual assault, fraud, or traffic offenses								
Fut. charge	0.005*	0.106	0.018***	0.011	0.009***	-0.025	0.028***	0.152
	(0.003)	(0.073)	(0.004)	(0.102)	(0.003)	(0.101)	(0.004)	(0.135)
Fut. conviction	0.006***	0.157**	0.020***	0.035	0.012***	-0.070	0.031***	0.223*
	(0.002)	(0.070)	(0.003)	(0.099)	(0.003)	(0.096)	(0.004)	(0.133)
Fut. incarceration	0.006***	0.141**	0.017***	0.000	0.009***	-0.109	0.030***	0.154
	(0.002)	(0.059)	(0.003)	(0.088)	(0.002)	(0.081)	(0.004)	(0.117)
Observations	112135	112135	112135	112135	112135	112135	112135	112135

Note: Panel A of this table shows 2SLS estimates of the impact of conviction vs dismissal on future charges, convictions, and incarcerations. Here we recalculate the instrument by assault cases (assault and weapons) and then drop the assault cases from the sample. Panel B is similar except it recalculates the stringency splitting drug and non-drug cases and drops drug cases. Panel C recalculates the instruments using all violent offenses (assault, sexual assault, and murder). Panel D includes all unbalanced offenses which includes assault, sexual assault, fraud, and traffic. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). Each time period restricts the sample to cases observed for all 7 years. All regressions control for stringency on the other margin (i.e., z_i for the conviction specification), race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.3: Incarceration and recidivism– robustness to unbalanced offenses

	Year 1		Year 2-4		Year 5-7		Year 1-7	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Panel A: No assault offenses								
Fut. charge	-0.020***	-0.090***	0.018***	-0.001	0.028***	-0.004	0.030***	-0.053
	(0.002)	(0.031)	(0.003)	(0.048)	(0.002)	(0.042)	(0.003)	(0.059)
Fut. conviction	-0.017***	-0.101***	0.019***	-0.026	0.026***	0.012	0.029***	-0.094
	(0.002)	(0.030)	(0.002)	(0.048)	(0.002)	(0.040)	(0.003)	(0.058)
Fut. incarceration	-0.009***	-0.066***	0.021***	0.008	0.024***	0.036	0.034***	-0.048
	(0.001)	(0.025)	(0.002)	(0.042)	(0.002)	(0.033)	(0.003)	(0.052)
Observations	155100	155100	155100	155100	155100	155100	155100	155100
Panel B: No drug offenses								
Fut. charge	-0.017***	-0.117***	0.015***	0.001	0.026***	0.036	0.028***	-0.038
	(0.002)	(0.040)	(0.003)	(0.064)	(0.002)	(0.053)	(0.003)	(0.080)
Fut. conviction	-0.013***	-0.138***	0.016***	-0.032	0.024***	0.036	0.028***	-0.091
	(0.002)	(0.038)	(0.003)	(0.062)	(0.002)	(0.051)	(0.003)	(0.077)
Fut. incarceration	-0.006***	-0.085***	0.019***	0.002	0.022***	0.085**	0.034***	-0.014
	(0.002)	(0.032)	(0.002)	(0.055)	(0.002)	(0.043)	(0.003)	(0.069)
Observations	125602	125602	125602	125602	125602	125602	125602	125602
Panel C: No violent offenses								
Fut. charge	-0.020***	-0.085***	0.020***	0.011	0.030***	0.003	0.033***	-0.034
	(0.002)	(0.032)	(0.003)	(0.049)	(0.002)	(0.043)	(0.003)	(0.060)
Fut. conviction	-0.017***	-0.098***	0.020***	-0.008	0.027***	0.020	0.032***	-0.075
	(0.002)	(0.031)	(0.003)	(0.049)	(0.002)	(0.041)	(0.003)	(0.058)
Fut. incarceration	-0.009***	-0.065**	0.022***	0.024	0.025***	0.042	0.035***	-0.033
	(0.001)	(0.026)	(0.002)	(0.043)	(0.002)	(0.034)	(0.003)	(0.053)
Observations	149473	149473	149473	149473	149473	149473	149473	149473
Panel D: No assault, sexual assault, fraud, or traffic offenses								
Fut. charge	-0.021***	-0.075*	0.022***	0.037	0.033***	0.037	0.036***	0.001
	(0.002)	(0.041)	(0.003)	(0.059)	(0.003)	(0.055)	(0.003)	(0.076)
Fut. conviction	-0.017***	-0.095**	0.022***	0.028	0.030***	0.080	0.035***	-0.025
	(0.002)	(0.040)	(0.003)	(0.058)	(0.002)	(0.052)	(0.003)	(0.073)
Fut. incarceration	-0.009***	-0.062*	0.024***	0.043	0.026***	0.054	0.037***	0.001
	(0.002)	(0.035)	(0.003)	(0.055)	(0.002)	(0.043)	(0.003)	(0.067)
Observations	112135	112135	112135	112135	112135	112135	112135	112135

Note: Panel A of this table shows 2SLS estimates of the impact of incarceration vs conviction on future charges, convictions, and incarcerations. Here we recalculate the instrument by assault cases (assault and weapons) and then drop the assault cases from the sample. Panel B is similar except it recalculates the stringency splitting drug and non drug cases and drops drug cases. Panel C recalculates the instruments using all violent offenses (assault, sexual assault, and murder). Panel D uses all unbalanced offenses which includes assault, sexual assault, fraud, and traffic. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). Each time period restricts the sample to cases observed for all 7 years. All regressions control for stringency on the other margin (i.e., z_i for the conviction specification), race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.4: 2SLS regressions of sentence length on conviction stringency

	Conviction reg controling for incarceration									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sent length	Any incar	6mo	1y	2y	3y	4y	5y	6y	7y
Pr. convict	7.68	-0.033	0.088*	-0.032	-0.043	-0.027	0.0023	-0.011	-0.0071	-0.00018
	(64.2)	(0.051)	(0.047)	(0.043)	(0.033)	(0.029)	(0.025)	(0.021)	(0.017)	(0.017)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	322.018	0.546	0.374	0.203	0.113	0.078	0.061	0.042	0.035	0.030
N	183381	183381	183381	183381	183381	183381	183381	183381	183381	183381

Note: This table shows a regression of various sentence length variables on z_c. The first column uses sentence length as the outcome, the second any incarceration, third to tenth any incarceration greater than 6 months, 1 year, 2 years, 3 years, 4 years, 5 years, 6 years, and 7 years respectively. All regressions control for z_i, race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. The sample is restricted to cases observed for 7 years. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.5: Split sample monotonicity test: conviction

	Zero	One
Any drug charges	0.548	0.199
Any property charges	0.463	0.238
Any violent charges	0.430	0.099
Black	0.308	0.393
Female	0.875	0.168
Prior conviction	0.274	0.148

Note: This table shows first-stage estimates for the conviction (without incarceration) instrument where, for each regression, the stringency measure is calculated on a specific subpopulation, and the regression is then run on its complement. For example, the "Zero" column of the "Any drug charges" row calculates judge stringency on those without drug charges, then estimates the first stage on those *with* drug charges, and reports the coefficient on the instrument. Regression includes court-by-year fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. The regression also controls for the leave-one-out propensity of the judge to have cases that end in incarceration. The sample is restricted to cases observed for 7 years.

Table D.6: Split sample monotonicity test: incarceration

	Zero	One
Any drug charges	0.538	0.366
Any property charges	0.677	0.342
Any violent charges	0.319	0.191
Black	0.460	0.592
Female	0.658	0.269
Prior conviction	0.750	0.337

Note: This table shows first-stage estimates for the incarceration instrument where, for each regression, the stringency measure is calculated on a specific subpopulation, and the regression is then run on its complement. For example, the "Zero" column of the "Any drug charges" row calculates judge stringency on those without drug charges, then estimates the first stage on those *with* drug charges, and reports the coefficient on the instrument. Regression includes court-by-year fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Regression also controls for the leave-one-out propensity of the judge to dismiss cases. The sample is restricted to cases observed for 7 years.

E Additional figures and tables: IV analyses

In this appendix, we present a series of additional analyses and robustness tests for our main IV analyses.

E.1 Overview of analyses

E.1.1 Disposition types

Disposition type by offense. Figure E.1 shows the breakdown of disposition types for four common offenses: drugs, fraud, larceny, and assault. These offense categories differ in seriousness and, while the exact breakdown varies, all disposition types are present in each offense type considered.

Future exposure to incarceration Appendix Figure E.2 illustrates the extent of "incarceration catch-up" for individuals given noncarceral sentences compared to those given carceral sentences, considering both new crimes and technical violations leading to probation revocation. These results indicate that although some catch-up occurs, over 50% of those receiving noncarceral sentences avoid incarceration over the next seven years.

E.1.2 Reduced-form estimates

Appendix Table Panel A E.1 presents reduced-form estimates, showing the relationship between our outcome variables and the conviction instrument controlling for race, gender, prior conviction, offense type dummies, and year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects as well as the leave-one-out judge incarceration stringency. We find that the instrument positively and significantly affects the year 1 and the year 1-7 outcomes. Appendix Table E.1 Panel B shows comparable reduced-form estimates for the incarceration results.

E.1.3 Compliers

Characterizing compliers. Appendix Table E.2 compares compliers for the conviction and incarceration margins to the full sample. The distribution of offenses is mostly similar for compliers to both instruments, with a few exceptions. Compliers to the conviction instrument are more likely to be female (27% vs 22%) and are more likely to have a property crime charge (42% vs 38%). Moreover, they are less likely to have a prior conviction (10% vs 17%), less likely to have a violent charge than the general sample (8% vs 19%), and less likely to have charges that fall into the other category (6% vs 16%). Compliers to the incarceration instrument are slightly more similar to the full sample, but exhibit some of the same notable differences. First, prior conviction rates and share of women are more similar, 19% vs 17% and 21% vs 22%, respectively. For property charges and violent charges we continue to see disparities with 46% vs 38% having a property charge and 8% vs 19% having a violent charge.

Complier weighted OLS. In Appendix Table E.3 we reweight the OLS for incarceration and conviction margin compliers. The OLS estimates do not change much when re-weighting for compliers. The reweighted estimates for noncarceral conviction are somewhat larger, while the estimates for incarceration are nearly identical.

E.1.4 Heterogeneity

Increased criminal behavior or "ratcheting up"? We take two strategies to provide suggestive evidence on whether the recidivism effects come from increased criminal behavior or the "ratcheting up" effect. First, we look at differences across different stages of the criminal justice process. If each discretionary decision is influenced by the criminal record, then the influence of the conviction will accumulate as someone advances through the criminal proceedings. If the ratcheting up effect is operative, it may have a larger effect on the more downstream measures of future criminal justice contact, like incarceration, than on the more upstream measures, like new charges. Consistent with this mechanism, we note that in all of our estimates presented in Table 4, the percent changes are larger for more downstream measures of future criminal justice contact.[62]

Second, we consider recidivism across crime types. Following Deshpande and Mueller-Smith (2022), we break out new crimes into income generating crimes or other crimes.[63] If our results are driven by increases in income-generating crime, this would be more consistent with the destabilization channel. Appendix Table E.4 shows that our point estimates are similar for both crime types. The impacts are larger in percent change terms for more downstream measures of future criminal justice contact. Results are similar if we break out drug crimes from non-drug crimes (Appendix Table E.5). These analyses are far from definitive, but they provide some suggestive evidence in favor of the "ratcheting up" channel.

2SLS estimates for other subgroups. In Appendix Tables E.6 - E.8, we present 2SLS estimates conditional on various offense categories and sociodemographic characteristics. Appendix Table E.6 separately considers people with or without prior convictions in the last 5 years. We find large effects of conviction for those with no prior felony conviction. Our sample of those with a prior felony conviction is quite small and standard errors are too large to inform us about differences in effect sizes across groups.

For incarceration, we find that both groups have similar patterns: short-term incapacitation effects, but no long-term effects, for either group. This result differs from findings in Jordan et al. (2023). This could partially be caused by two limitations in our data. First, we can only observe prior felony convictions if they appear in our data set. Given this, our indicator for prior felony conviction is "prior felony conviction within the last 5 years" (and would miss all felony convictions outside of the state). Presumably, some subset of our sample with no felony conviction within the last five years have older felony convictions we cannot observe. Jordan et al. (2023) solve this

[62]The fact that conviction increases the probability of future incarceration also indicates that there are direct future financial costs within the criminal justice of these marginal convictions.

[63]Income generating crimes are cases with at least one burglary, drug (excluding drug possession), fraud, larceny, robbery, or prostitution charge.

issue by restricting their analysis to individuals who are younger than 18 at the start of their sample. We are not able to include a similar restriction as we do not know the age or date of birth for many people in our sample. It is possible that we would find different results for incarceration if our data allowed us to fully restrict the sample to first-time offenders.

We find no substantial differences between Black and White defendants (Appendix Table E.7). We do find some evidence that impacts are larger for people living in zip codes with above median poverty rates (Appendix Table E.8). This could be because felony convictions have more consequences in terms of access to relevant social services or housing, or in terms of future criminal justice scrutiny, for poorer people.

E.1.5 Robustness checks

Robustness to sample choice and specification. In Appendix Figures E.3-E.6, we examine how our main 2SLS estimates for conviction and incarceration change when we alter our sample or specifications, for our 1 year, 2-4 year, 5-7 year, and 1-7 year estimates. We consider the following variations:[64]

- Changing the required number of cases seen by a judge in our 3-year window (50 or 150 instead of 100);
- Varying which courts are included. We conducted phone interviews in 2021 with court clerks in all courts in Virginia for which we had data. We asked the clerks how cases were allocated. Our main sample includes courts where cases are quasi-randomly allocated (see Section 2.1 for more details.) We vary which courts we include:
  - Keep all courts, even if there appears to be selection in the kinds of cases that judges handle. This can happen for example if there are specialized courts, in particular drug courts.
  - Drop courts where the clerks said that cases were assigned based on judge availability, which may be more subject to discretion in what cases to work on.
- Clustering our standard errors at the month court level or at the defendant level;
- Changing what offenses are included:
  - Dropping drug cases. Although diversion is rare for felonies in Virginia, it is more likely in drug cases. Thus, dropping drug cases means eliminating the cases where diversion is most probable.
  - Dropping offenses types that are not balanced across judges (see Table 3).
- Varying how we control for non-focal stringency. In our main specification, we control for incarceration stringency, defined as the fraction of cases that end in carceral sentences. Here, we consider including controls for sentence length stringency, probability of sentence length shorter than 6 months or longer than 1 year or 4 years, flexibly controlling for deciles of the non-focal stringency, or no controls at all.
- Reconstructing the judge stringency instrument by crime type (assault or not and drug or not).

[64]The sample and specification changes are detailed in the footnotes of the figures.

- Including all years for which we can construct recidivism. We expand the sample up to 2015 for outcomes in years 2-4 and up to 2018 for outcomes in year 1.

Generally, our estimates are very close to our main specification (colored in green and denoted by the red dotted line). Although we occasionally lose statistical significance, estimates from the majority of the specifications remain significantly different from zero at the 95% level when our main estimate is also significant. Our main estimates also tend to fall towards the middle of the range of point estimates.

Robustness to different definitions of recidivism. In Appendix Table E.9, we show that our results are robust to defining recidivism in a variety of ways. In panel A we count recidivism as the total number of future charges (i.e., if you have 3 future charges in a case 1 year later, we count that as 3.) In panel B we count the total number of future charge events. Meaning that if you have a case in year 1 and another separate case in year 2 we count that as 2. Finally in panels C, D, and E we look at recidivism where there is one charge, two to three charges, and four or more charges respectively. This tests our results using slightly different definitions of recidivism. While overall we see the same general patterns, our estimates occasionally fall in and out of significance. Furthermore, much of our results seem to be coming from recidivism with more than one charge as evidenced from panels D and E.

Empirical Bayes Shrinkage. We correct for potential measurement error in judge stringency instruments using Empirical Bayes methods. We implement an Empirical Bayes procedure where we assume that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution. We consider two specifications: in the first, we assume that judge stringencies are drawn from a single Beta distribution, while the second assumes that the Beta distribution varies by district-year. We provide detailed descriptions of our methodology and results in Appendix F.3. Overall, our results are not sensitive to using shrunken leniency estimates, which is consistent with the fact that judges in our sample see many cases per year.

Differential mobility. Our results could be confounded if conviction or incarceration influence the likelihood of moving outside of Virginia, and therefore change the likelihood that we would capture their recidivism in our data. Due to data limitations, we cannot test for this in the IV setting. However, for our RD analyses, we can test to see if there is any discontinuity in the likelihood of living in Virginia for those right above/below the cutoff in the incarceration length score and the probation/jail score. We build an indicator for Virginia residency that is equal to one if the person is marked as being in the state of VA in year 5 post-sentencing and year 7 post-sentencing. Missing observations are excluded.[65] As we can see in Appendix Figure E.7, there is no discontinuity at our cutoff score. Notably, in the incarceration-length sample, the share of people remaining in Virginia 5-7 years after the sentencing date ranges from 79-83% at every score. This consistency suggests that neither conviction nor incarceration affect migration from Virginia.

[65] If we instead include missings as 0s the results are very similar. Around 7.7% of the sample is missing this information.

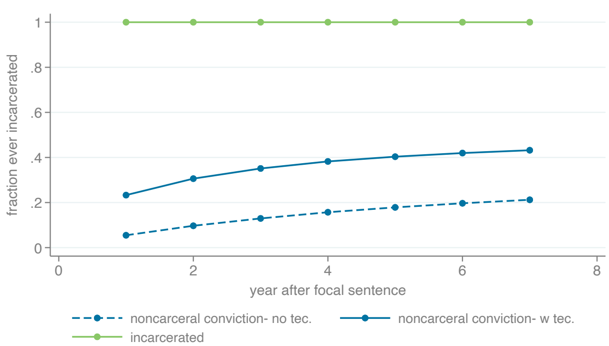
E.2 Appendix figures: 2SLS analyses

Figure E.1: Dismissed, convicted, and incarcerated percentages by offenses

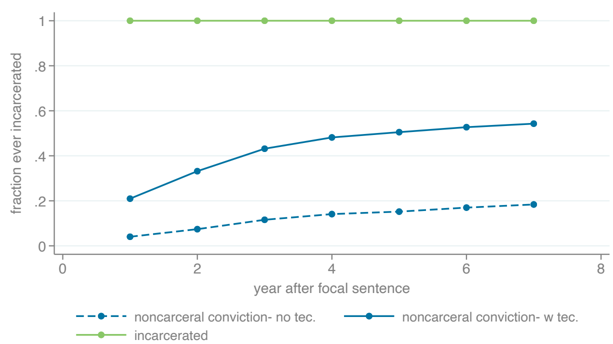
Note: This figure shows the variation in dismissal, conviction, and incarceration by four offense categories. The top left depicts fraud cases, the top right larceny, the bottom left assault, and the bottom right drugs. There is variation in the percent of cases dismissed, convicted, and incarcerated within each offense. The sample is restricted to cases observed for 7 years.

Figure E.2: Dynamics of incarceration

(a) Ever incarcerated with and without technical revocations



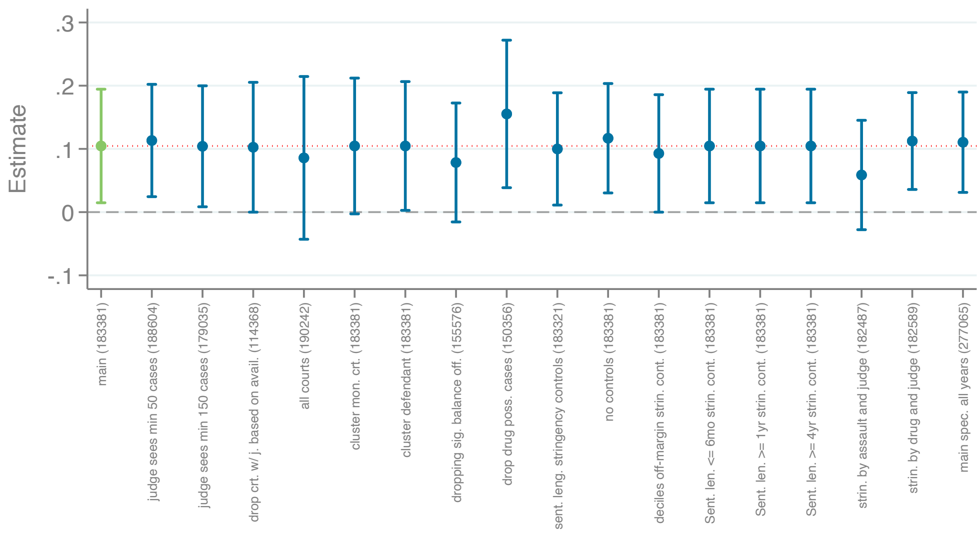
(b) Ever incarcerated with and without technical revocations– complier reweighted



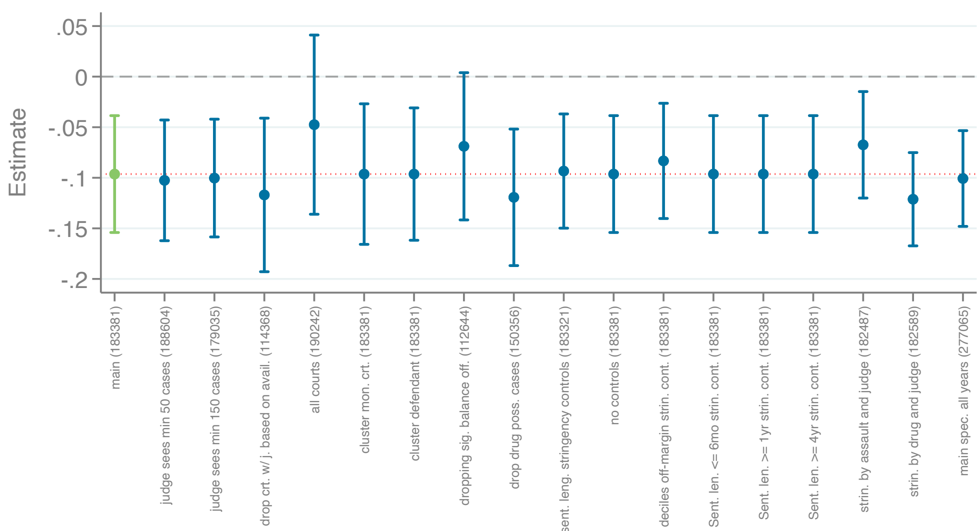
Note: Panel (a) shows the fraction of individuals that ever experience an incarceration in each year post the focal sentence date. This takes into account the current incarceration, prior incarcerations, and any future incarcerations. It is a cumulative measure, so naturally for our incarceration group the estimate is 100%. We have split out including incarceration for technical probation violations in the solid line from incarceration only due to a new crime in the dotted line. The blue lines are for people who initially got a noncarceral conviction sentence while the green line is for those who initially got a carceral sentence. Panel (b) is the same figure but complier weighted, meaning that it is reweighted to match the compliers for each margin.

Figure E.3: Robustness for 2SLS results: recidivism in year 1

(a) Noncarceral conviction



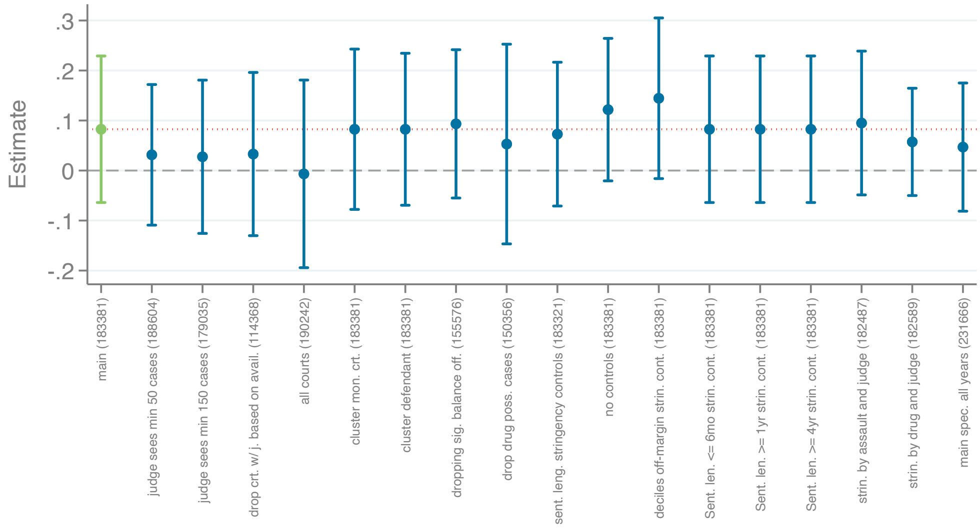
(b) Incarceration



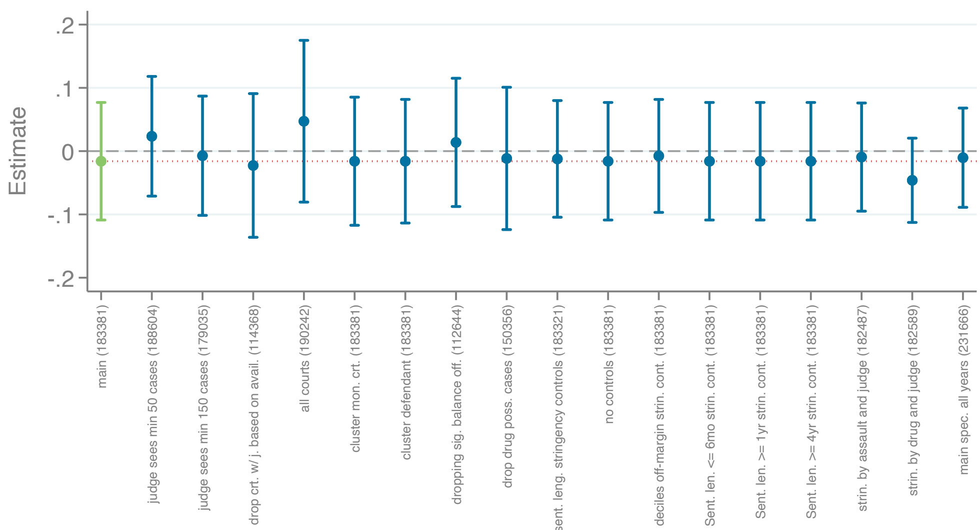
Note: This figure shows various estimates of the impact of conviction (panel a) and incarceration (panel b) on recidivism within the first year after sentencing. The main sample is restricted to cases observed for 7 years. Sample size is noted in parentheses and the main estimate is highlighted in green; 95% confidence intervals are shown. The red dotted line is located at the height of the main estimate and the dashed gray line is located at 0. The sample restrictions on the estimates are the following: (1) Our main specification. (2) Requiring that a judge sees a minimum of 50 cases in our 3 year window. (3) Requiring that a judge sees a minimum of 150 cases in our 3 year window. (4) Dropping courts that use judges based on availability. (5) Keeping courts where clerks described an assignment process that seemed non-random. (6) Clustering standard errors at the court month level. (7) Clustering standard errors at the defendant level. (8) Dropping any offenses that are significant in our balance tests. (9) Dropping any cases that relate to drug possession. (10) Including a sentence length stringency instrument control. (11) Main specification without any of our controls. (12) Including decile bins of our off-margin stringency as controls. (13) Controlling for “probability of sentence length less than 6 months” stringency. (14) Controlling for “probability of sentence length greater than 1 year” stringency. (15) Controlling for “probability of sentence length greater than 4 years” stringency. (16) Controlling for judge stringency instruments recalculated by assault only/non-assault charges. (17) Controlling for judge stringency instruments recalculated by drug/non-drug cases. (18) Using the full sample of available years for the estimate.

Figure E.4: Robustness for 2SLS results: recidivism in years 2-4

(a) Noncarceral conviction



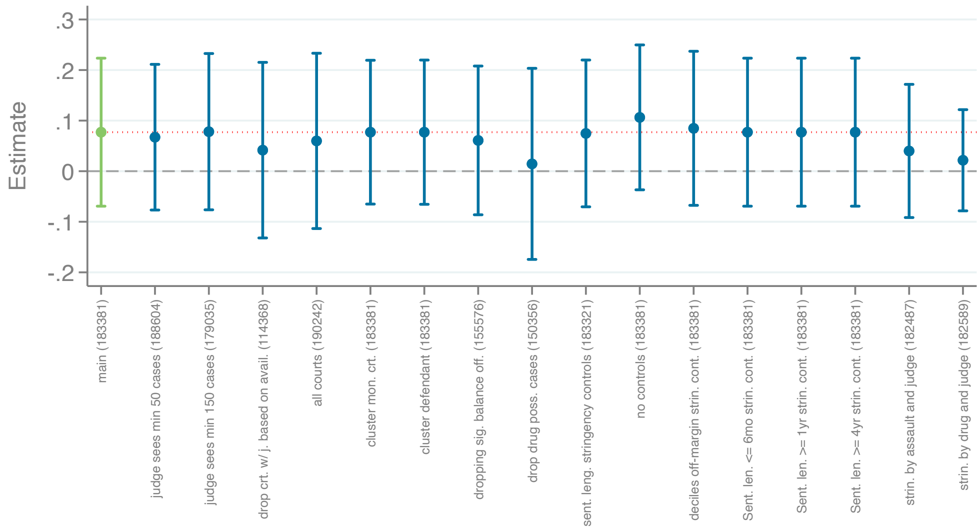
(b) Incarceration



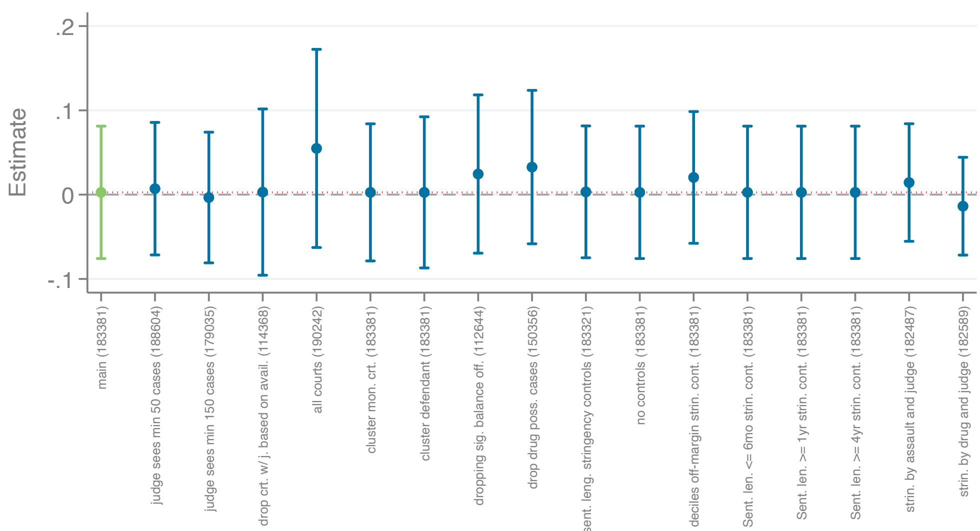
Note: This figure shows various estimates of the impact of conviction (panel a) and incarceration (panel b) on recidivism 2-4 years after sentencing. The main sample is restricted to cases observed for 7 years. Sample size is noted in parentheses and the main estimate is highlighted in green; 95% confidence intervals are shown. The red dotted line is located at the height of the main estimate and the dashed gray line is located at 0. The sample restrictions on the estimates are the following: (1) Our main specification. (2) Requiring that a judge sees a minimum of 50 cases in our 3 year window. (3) Requiring that a judge sees a minimum of 150 cases in our 3 year window. (4) Dropping courts that use judges based on availability. (5) Keeping courts where clerks described an assignment process that seemed non-random. (6) Clustering standard errors at the court month level. (7) Clustering standard errors at the defendant level. (8) Dropping any offenses that are significant in our balance tests. (9) Dropping any cases that relate to drug possession. (10) Including a sentence length stringency instrument control. (11) Main specification without any of our controls. (12) Including decile bins of our off-margin stringency as controls. (13) Controlling for "probability of sentence length less than 6 months" stringency. (14) Controlling for "probability of sentence length greater than 1 year" stringency. (15) Controlling for "probability of sentence length greater than 4 years" stringency. (16) Controlling for judge stringency instruments recalculated by assault only/non-assault charges. (17) Controlling for judge stringency instruments recalculated by drug/non-drug cases. (18) Using the full sample of available years for the estimate.

Figure E.5: Robustness for 2SLS results: recidivism in years 5-7

(a) Noncarceral conviction



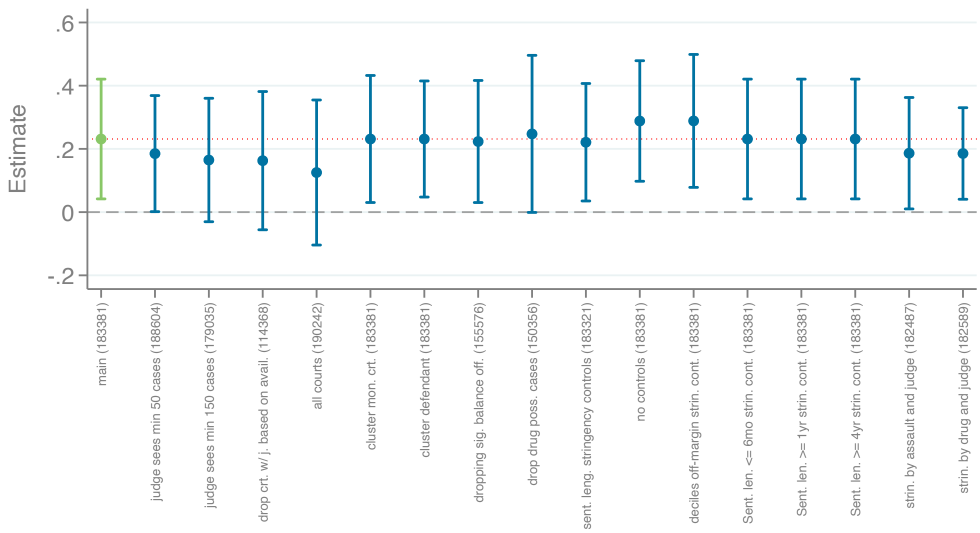
(b) Incarceration



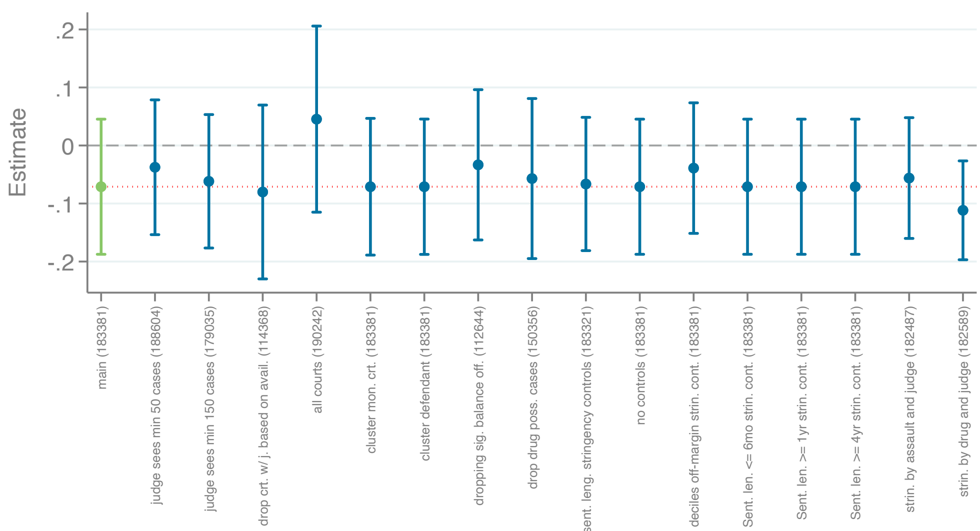
Note: This figure shows various estimates of the impact of conviction (panel a) and incarceration (panel b) on recidivism 5-7 years after sentencing. The main sample is restricted to cases observed for 7 years. Sample size is noted in parentheses and the main estimate is highlighted in green; 95% confidence intervals are shown. The red dotted line is located at the height of the main estimate and the dashed gray line is located at 0. The sample restrictions on the estimates are the following: (1) Our main specification. (2) Requiring that a judge sees a minimum of 50 cases in our 3 year window. (3) Requiring that a judge sees a minimum of 150 cases in our 3 year window. (4) Dropping courts that use judges based on availability. (5) Keeping courts where clerks described an assignment process that seemed non-random. (6) Clustering standard errors at the court month level. (7) Clustering standard errors at the defendant level. (8) Dropping any offenses that are significant in our balance tests. (9) Dropping any cases that relate to drug possession. (10) Including a sentence length stringency instrument control. (11) Main specification without any of our controls. (12) Including decile bins of our off-margin stringency as controls. (13) Controlling for “probability of sentence length less than 6 months” stringency. (14) Controlling for “probability of sentence length greater than 1 year” stringency. (15) Controlling for “probability of sentence length greater than 4 years” stringency. (16) Controlling for judge stringency instruments recalculated by assault only/non-assault charges. (17) Controlling for judge stringency instruments recalculated by drug/non-drug cases.

Figure E.6: Robustness for 2SLS results: recidivism in years 1-7

(a) Noncarceral conviction



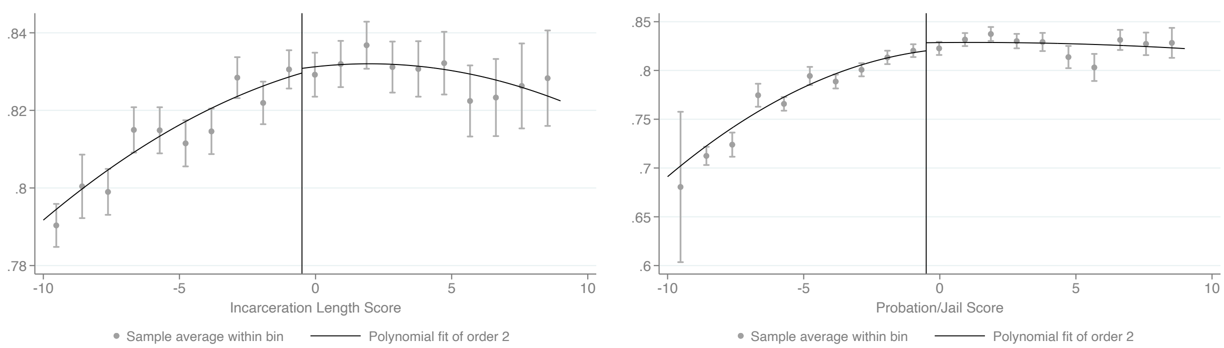
(b) Incarceration



Note: This figure shows various estimates of the impact of conviction (panel a) and incarceration (panel b) on recidivism within the first seven years after sentencing. The main sample is restricted to cases observed for 7 years. Sample size is noted in parentheses and the main estimate is highlighted in green; 95% confidence intervals are shown. The red dotted line is located at the height of the main estimate and the dashed gray line is located at 0. The sample restrictions on the estimates are the following: (1) Our main specification. (2) Requiring that a judge sees a minimum of 50 cases in our 3 year window. (3) Requiring that a judge sees a minimum of 150 cases in our 3 year window. (4) Dropping courts that use judges based on availability. (5) Keeping courts where clerks described an assignment process that seemed non-random. (6) Clustering standard errors at the court month level. (7) Clustering standard errors at the defendant level. (8) Dropping any offenses that are significant in our balance tests. (9) Dropping any cases that relate to drug possession. (10) Including a sentence length stringency instrument control. (11) Main specification without any of our controls. (12) Including decile bins of our off-margin stringency as controls. (13) Controlling for "probability of sentence length less than 6 months" stringency. (14) Controlling for "probability of sentence length greater than 1 year" stringency. (15) Controlling for "probability of sentence length greater than 4 years" stringency. (16) Controlling for judge stringency instruments recalculated by assault only/non-assault charges. (17) Controlling for judge stringency instruments recalculated by drug/non-drug cases.

Figure E.7: Testing for discontinuities in Virginia residency

(a) Incarceration-length sample **(b) Probation/jail sample**



Note: The outcome variable here is a flag indicating that the person is still residing in Virginia 5-7 years after their sentencing date, based on data obtained from a private vendor. Panel (a) is restricted to the RD incarceration-length sample; panel (b) is restricted to the RD probation/jail sample. There is no discontinuity across either threshold. People whose residency information is missing (7.7% of the sample) were excluded from the analysis.

E.3 Appendix tables: 2SLS analyses

Table E.1: Reduced form estimates

	Year 1	Year 2-4	Year 5-4	Year 1-7
	RF	RF	RF	RF
Panel A: Conviction				
Fut. charge	0.062**	0.049	0.046	0.137**
	(0.027)	(0.044)	(0.044)	(0.055)
Fut. conviction	0.080***	0.065	0.032	0.174***
	(0.025)	(0.042)	(0.042)	(0.053)
Fut. incarceration	0.066***	0.032	-0.015	0.124***
	(0.022)	(0.037)	(0.034)	(0.048)
Observations	183381	183381	183381	183381
Panel B: Incarceration				
Fut. charge	-0.058***	-0.010	0.002	-0.043
	(0.018)	(0.029)	(0.024)	(0.036)
Fut. conviction	-0.067***	-0.022	0.012	-0.064*
	(0.017)	(0.028)	(0.023)	(0.035)
Fut. incarceration	-0.042***	0.006	0.031	-0.017
	(0.014)	(0.025)	(0.019)	(0.031)
Observations	183381	183381	183381	183381

Note: This table shows estimates from reduced form regressions of recidivism on z_c in Panel A and regressions of recidivism on z_i in Panel B. The four columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). The sample is restricted to cases observed for 7 years. All regressions control for z_i in the first Panel and z_d in the second as well as, race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. The table reports the estimated impact of conviction and incarceration. The first row is for any future felony charge, the second row is for any future conviction, and the third row is for any future incarceration. Standard errors are clustered at the judge-year level. Stars denote * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table E.2: Complier characteristics (noncarceral conviction)

	Pr(X=x)	Pr(X=x—complier)	$\frac{Pr(X=x\|complier)}{Pr(X=x)}$
Panel A: Conviction			
Prior conviction	0.172	0.101	0.586
	(0.003)	(0.032)	(0.187)
Female	0.218	0.273	1.248
	(0.003)	(0.041)	(0.186)
Black	0.568	0.557	0.981
	(0.015)	(0.049)	(0.085)
Has misdemeanor	0.078	0.080	1.024
	(0.004)	(0.020)	(0.254)
Drugs	0.313	0.316	1.011
	(0.007)	(0.034)	(0.105)
Property	0.377	0.417	1.104
	(0.008)	(0.045)	(0.115)
Violent	0.194	0.084	0.433
	(0.004)	(0.031)	(0.158)
Other	0.160	0.064	0.397
	(0.002)	(0.027)	(0.170)
Panel B: Incarceration			
Prior conviction	0.172	0.186	1.084
	(0.003)	(0.021)	(0.119)
Female	0.218	0.209	0.956
	(0.003)	(0.031)	(0.140)
Black	0.568	0.549	0.967
	(0.015)	(0.029)	(0.047)
Has misdemeanor	0.078	0.061	0.787
	(0.004)	(0.018)	(0.222)
Drugs	0.313	0.264	0.845
	(0.007)	(0.028)	(0.088)
Property	0.377	0.460	1.220
	(0.008)	(0.034)	(0.091)
Violent	0.194	0.084	0.431
	(0.004)	(0.028)	(0.139)
Other	0.160	0.150	0.937
	(0.002)	(0.023)	(0.144)

Note: This table shows the characteristics of compliers for our 2SLS conviction analysis in Panel A and incarceration analysis in Panel B. The first column reports average characteristics for the full 2SLS sample. The second column reports the estimated average coefficients for compliers. The third column reports the ratio of column 2 to column 1. The sample is restricted to cases observed for 7 years. Standard errors are calculated via bootstrap using 500 bootstrap samples.

Table E.3: Complier weighted OLS

	Year 1		Year 2-4		Year 5-7		Year 1-7	
	OLS	OLS weighted	OLS	OLS weighted	OLS	OLS weighted	OLS	OLS weighted
Panel A: Conviction								
Fut. charge	-0.002	0.004*	0.004	0.010***	0.006**	0.010***	0.011***	0.021***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
Fut. conviction	0.001	0.006***	0.008***	0.012***	0.007***	0.011***	0.014***	0.023***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
Fut. incarceration	0.001	0.006***	0.006**	0.010***	0.005**	0.008***	0.012***	0.021***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Ctrl. mean: fut. chrg.	0.081	0.089	0.154	0.170	0.115	0.129	0.270	0.297
Ctrl. mean: fut. conv.	0.069	0.076	0.135	0.148	0.102	0.114	0.243	0.268
Ctrl. mean: fut. incar.	0.048	0.054	0.097	0.109	0.073	0.083	0.182	0.204
Panel B: Incarceration								
Fut. charge	-0.022***	-0.022***	0.013***	0.013***	0.025***	0.025***	0.023***	0.024***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Fut. conviction	-0.018***	-0.018***	0.013***	0.014***	0.023***	0.023***	0.022***	0.023***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Fut. incarceration	-0.010***	-0.010***	0.017***	0.017***	0.021***	0.021***	0.027***	0.028***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Ctrl. mean: fut. chrg.	0.088	0.088	0.177	0.175	0.133	0.132	0.308	0.306
Ctrl. mean: fut. conv.	0.078	0.077	0.160	0.159	0.121	0.120	0.285	0.283
Ctrl. mean: fut. incar.	0.055	0.055	0.116	0.115	0.085	0.084	0.214	0.212
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381

Note: This table shows regression estimates of the impact of conviction on future recidivism in Panel A and the impact of incarceration on future recidivism in Panel B, showing our ordinary least squares (OLS) regressions and complier weighted OLS etimates. The four columns report results for four time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). Each time period restricts the sample to cases observed for 7 years. All regressions control for race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. The first three rows of each panel report the estimated impact of conviction or incarceration on different measures of recidivism. The first row is for any future charge, the second row is for any future conviction, and the third row is for any future incarceration. For the OLS estimates in Panel A, we regress our measures of recidivism on having a conviction (regardless of incarceration status) controlling for incarceration. For for the OLS weighted estimates, we use the same regression but weighted by IV compliers. For the OLS estimates in Panel B, we regress our measures of recidivism on incarceration, controlling for dismissal. For the OLS weighted estimates we use the same regression but weighted by incarceration IV compliers. The estimates presented are the coefficients on the conviction variable. Standard errors are clustered at the judge-year level. Stars denote * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.4: Income-generating vs non-income-generating recidivism

	Income generating recidivsim				Non-income generating recidivsim			
	Year 1	Year 2-4	Year 5-7	Year 1-7	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Conviction								
Fut. charge	0.061*	0.031	0.042	0.131*	0.092**	0.035	0.006	0.128
	(0.034)	(0.060)	(0.054)	(0.077)	(0.039)	(0.063)	(0.062)	(0.082)
Fut. conviction	0.070**	0.095	0.042	0.196**	0.102***	0.032	-0.020	0.118
	(0.032)	(0.059)	(0.052)	(0.077)	(0.036)	(0.058)	(0.060)	(0.081)
Fut. incarceration	0.046*	0.045	-0.016	0.096	0.091***	0.040	-0.031	0.131**
	(0.027)	(0.050)	(0.044)	(0.068)	(0.030)	(0.049)	(0.045)	(0.066)
Ctrl. comp. mean: fut. chrg.	0.023	0.117	0.076	0.181	0.002	0.121	0.100	0.181
Ctrl. mean: fut. chrg.	0.054	0.106	0.079	0.196	0.053	0.108	0.081	0.204
Ctrl. comp. mean: fut. conv.	0.017	0.087	0.073	0.144	-0.013	0.105	0.098	0.159
Ctrl. mean: fut. conv.	0.047	0.092	0.070	0.175	0.044	0.091	0.070	0.178
Ctrl. comp. mean: fut. incar.	0.015	0.057	0.077	0.119	-0.015	0.082	0.076	0.113
Ctrl. mean: fut. incar.	0.034	0.069	0.053	0.135	0.030	0.064	0.048	0.128
Panel B: Incarceration								
Fut. charge	-0.043*	0.004	-0.020	-0.054	-0.066***	-0.022	0.029	-0.032
	(0.024)	(0.036)	(0.031)	(0.048)	(0.024)	(0.041)	(0.032)	(0.051)
Fut. conviction	-0.053**	-0.013	-0.027	-0.073	-0.072***	-0.018	0.054*	-0.016
	(0.023)	(0.036)	(0.030)	(0.048)	(0.023)	(0.039)	(0.031)	(0.049)
Fut. incarceration	-0.033*	-0.002	0.025	-0.019	-0.044**	0.009	0.059**	0.025
	(0.019)	(0.031)	(0.026)	(0.042)	(0.019)	(0.032)	(0.024)	(0.040)
Ctrl. comp. mean: fut. chrg.	0.089	0.117	0.124	0.259	0.066	0.131	0.094	0.242
Ctrl. mean: fut. chrg.	0.056	0.112	0.080	0.204	0.047	0.102	0.080	0.195
Ctrl. comp. mean: fut. conv.	0.092	0.127	0.117	0.259	0.074	0.117	0.072	0.227
Ctrl. mean: fut. conv.	0.049	0.102	0.073	0.188	0.041	0.090	0.071	0.174
Ctrl. comp. mean: fut. incar.	0.070	0.077	0.063	0.175	0.048	0.076	0.038	0.140
Ctrl. mean: fut. incar.	0.035	0.074	0.053	0.141	0.029	0.062	0.047	0.123
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381

Note: The first panel of this table shows 2SLS estimates of the impact of conviction vs dismissal on future recidivism. In the first four columns, recidivism is defined in reference to new income-generating felony charges, in the last four columns recidivism is defined in reference to new non-income generating charges. The second panel is similar except it shows 2SLS estimates of the impact of incarceration vs conviction. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). The sample is restricted to cases observed for 7 years. All regressions control for stringency on the other margin (i.e., z_i for the conviction specification), race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table E.5: Drug vs non-drug recidivism

	Drug charges				Non-drug charges			
	Year 1	Year 2-4	Year 5-7	Year 1-7	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Conviction								
Fut. charge	0.146**	0.001	-0.015	0.161	0.077	0.148	0.141	0.298**
	(0.070)	(0.105)	(0.110)	(0.140)	(0.057)	(0.097)	(0.086)	(0.121)
Fut. conviction	0.115*	0.026	-0.052	0.204	0.144***	0.173*	0.124	0.369***
	(0.062)	(0.098)	(0.104)	(0.134)	(0.054)	(0.093)	(0.083)	(0.118)
Fut. incarceration	0.114**	-0.027	-0.084	0.130	0.110**	0.110	0.019	0.272***
	(0.055)	(0.091)	(0.087)	(0.128)	(0.048)	(0.083)	(0.068)	(0.103)
Ctrl. comp. mean: fut. chrg.	0.149	0.356	0.249	0.554	0.164	0.273	0.229	0.460
Ctrl. mean: fut. chrg.	0.079	0.159	0.123	0.282	0.094	0.176	0.132	0.306
Ctrl. comp. mean: fut. conv.	0.134	0.328	0.232	0.523	0.142	0.231	0.221	0.425
Ctrl. mean: fut. conv.	0.067	0.137	0.108	0.252	0.080	0.154	0.117	0.277
Ctrl. comp. mean: fut. incar.	0.136	0.334	0.281	0.572	0.137	0.268	0.279	0.504
Ctrl. mean: fut. incar.	0.047	0.097	0.076	0.184	0.058	0.116	0.087	0.216
Panel B: Incarceration								
Fut. charge	-0.060	0.000	-0.012	-0.046	-0.114***	-0.026	0.004	-0.090
	(0.061)	(0.089)	(0.081)	(0.109)	(0.034)	(0.055)	(0.046)	(0.069)
Fut. conviction	-0.073	0.001	0.030	-0.082	-0.130***	-0.056	0.014	-0.123*
	(0.055)	(0.089)	(0.078)	(0.109)	(0.033)	(0.053)	(0.045)	(0.066)
Fut. incarceration	-0.062	0.082	0.010	-0.017	-0.079***	-0.021	0.062*	-0.042
	(0.049)	(0.079)	(0.067)	(0.102)	(0.028)	(0.047)	(0.037)	(0.058)
Ctrl. comp. mean: fut. chrg.	0.123	0.177	0.105	0.340	0.119	0.205	0.162	0.377
Ctrl. mean: fut. chrg.	0.100	0.189	0.143	0.336	0.082	0.168	0.126	0.291
Ctrl. comp. mean: fut. conv.	0.084	0.132	0.064	0.260	0.083	0.182	0.133	0.328
Ctrl. mean: fut. conv.	0.088	0.171	0.130	0.311	0.072	0.153	0.114	0.269
Ctrl. comp. mean: fut. incar.	0.043	0.048	0.026	0.136	0.043	0.083	0.064	0.180
Ctrl. mean: fut. incar.	0.063	0.125	0.090	0.234	0.051	0.110	0.081	0.201
Observations	57,249	57,249	57,249	57,249	126,134	126,134	126,134	126,134

Note: The first panel of this table shows 2SLS estimates of the impact of conviction vs dismissal on future recidivism. In the first four columns, recidivism is defined in reference to new drug charges; in the last four columns recidivism is defined in reference to new non-drug charges. The second panel is similar except it shows 2SLS estimates of the impact of incarceration. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). The sample is restricted to cases observed for 7 years. All regressions control for stringency on the opposite margin (i.e., z_i in the conviction specification) race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table E.6: 2SLS estimates for those with/without prior felony convictions

	Priors				No priors			
	Year 1	Year 2-4	Year 5-7	Year 1-7	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Conviction								
Fut. charge	0.092	0.306	-0.204	0.319	0.102**	0.073	0.113	0.229**
	(0.211)	(0.365)	(0.398)	(0.443)	(0.046)	(0.073)	(0.069)	(0.093)
Fut. conviction	0.205	0.239	-0.216	0.354	0.125***	0.109	0.085	0.293***
	(0.200)	(0.346)	(0.379)	(0.447)	(0.044)	(0.071)	(0.065)	(0.090)
Fut. incarceration	0.152	0.356	-0.340	0.444	0.107***	0.034	0.005	0.190**
	(0.178)	(0.315)	(0.327)	(0.430)	(0.036)	(0.060)	(0.052)	(0.078)
Ctrl. comp. mean: fut. chrg.	0.342	0.506	0.544	0.955	0.139	0.272	0.190	0.427
Ctrl. mean: fut. chrg.	0.147	0.294	0.239	0.503	0.080	0.151	0.112	0.265
Ctrl. comp. mean: fut. conv.	0.273	0.422	0.506	0.847	0.124	0.240	0.182	0.402
Ctrl. mean: fut. conv.	0.129	0.264	0.217	0.471	0.067	0.130	0.098	0.236
Ctrl. comp. mean: fut. incar.	0.291	0.503	0.705	1.071	0.120	0.262	0.223	0.455
Ctrl. mean: fut. incar.	0.098	0.209	0.173	0.386	0.047	0.093	0.069	0.176
Panel B: Incarceration								
Fut. charge	-0.101	-0.039	0.099	-0.021	-0.090***	-0.010	-0.013	-0.073
	(0.071)	(0.112)	(0.111)	(0.131)	(0.032)	(0.050)	(0.042)	(0.062)
Fut. conviction	-0.148**	-0.092	0.102	-0.106	-0.099***	-0.025	0.007	-0.098
	(0.069)	(0.107)	(0.107)	(0.126)	(0.031)	(0.049)	(0.040)	(0.060)
Fut. incarceration	-0.091	-0.062	0.152	-0.022	-0.064**	0.024	0.036	-0.023
	(0.064)	(0.097)	(0.094)	(0.120)	(0.025)	(0.042)	(0.034)	(0.053)
Ctrl. comp. mean: fut. chrg.	0.097	0.209	0.239	0.451	0.127	0.201	0.129	0.357
Ctrl. mean: fut. chrg.	0.119	0.302	0.234	0.496	0.084	0.161	0.120	0.285
Ctrl. comp. mean: fut. conv.	0.087	0.194	0.195	0.402	0.085	0.167	0.099	0.297
Ctrl. mean: fut. conv.	0.106	0.280	0.217	0.471	0.074	0.145	0.109	0.261
Ctrl. comp. mean: fut. incar.	0.052	0.130	0.115	0.264	0.043	0.065	0.043	0.154
Ctrl. mean: fut. incar.	0.076	0.216	0.161	0.375	0.052	0.103	0.075	0.194
Observations	31,505	31,505	31,505	31,505	151,878	151,878	151,878	151,878

Note: The first panel of this table shows 2SLS estimates of the impact of conviction on future recidivism for those with/without a prior felony conviction within 5 years. The second panel is similar except it shows 2SLS estimates of the impact of incarceration. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). The sample is restricted to cases observed for 7 years. All regressions control for stringency on the opposite margin (i.e., z_i in the conviction specification) race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table E.7: 2SLS estimates for Black and non-Black defendants

	Black				Non-Black			
	Year 1	Year 2-4	Year 5-7	Year 1-7	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Conviction								
Fut. charge	0.102	0.073	0.112	0.238	0.103	0.115	0.042	0.241**
	(0.069)	(0.117)	(0.109)	(0.147)	(0.063)	(0.090)	(0.094)	(0.114)
Fut. conviction	0.125*	0.112	0.100	0.335**	0.142**	0.128	0.011	0.274**
	(0.067)	(0.110)	(0.102)	(0.146)	(0.058)	(0.088)	(0.092)	(0.111)
Fut. incarceration	0.164***	0.008	-0.012	0.259**	0.058	0.115	-0.027	0.181*
	(0.061)	(0.088)	(0.080)	(0.126)	(0.049)	(0.082)	(0.077)	(0.103)
Ctrl. comp. mean: fut. chrg.	0.150	0.303	0.222	0.493	0.155	0.260	0.216	0.433
Ctrl. mean: fut. chrg.	0.104	0.196	0.148	0.339	0.070	0.135	0.104	0.241
Ctrl. comp. mean: fut. conv.	0.136	0.260	0.212	0.460	0.133	0.235	0.210	0.407
Ctrl. mean: fut. conv.	0.088	0.169	0.129	0.305	0.059	0.120	0.093	0.218
Ctrl. comp. mean: fut. incar.	0.137	0.324	0.289	0.567	0.128	0.233	0.249	0.447
Ctrl. mean: fut. incar.	0.064	0.126	0.094	0.235	0.040	0.087	0.069	0.164
Panel B: Incarceration								
Fut. charge	-0.131***	-0.009	-0.061	-0.113	-0.057	-0.022	0.073	-0.027
	(0.042)	(0.070)	(0.060)	(0.086)	(0.040)	(0.065)	(0.057)	(0.082)
Fut. conviction	-0.125***	-0.025	-0.044	-0.140	-0.099**	-0.048	0.092	-0.072
	(0.041)	(0.068)	(0.057)	(0.085)	(0.040)	(0.064)	(0.056)	(0.079)
Fut. incarceration	-0.105***	0.013	0.016	-0.070	-0.032	0.004	0.089*	0.014
	(0.035)	(0.056)	(0.047)	(0.074)	(0.032)	(0.057)	(0.049)	(0.072)
Ctrl. comp. mean: fut. chrg.	0.164	0.226	0.173	0.435	0.097	0.208	0.147	0.361
Ctrl. mean: fut. chrg.	0.094	0.193	0.144	0.332	0.081	0.157	0.118	0.279
Ctrl. comp. mean: fut. conv.	0.122	0.194	0.136	0.371	0.062	0.171	0.113	0.300
Ctrl. mean: fut. conv.	0.082	0.173	0.130	0.305	0.073	0.144	0.109	0.259
Ctrl. comp. mean: fut. incar.	0.066	0.070	0.062	0.193	0.034	0.098	0.059	0.185
Ctrl. mean: fut. incar.	0.059	0.127	0.091	0.232	0.050	0.102	0.076	0.192
Observations	104,225	104,225	104,225	104,225	79,158	79,158	79,158	79,158

Note: The first panel of this table shows 2SLS estimates of the impact of conviction on future recidivism for Black and non-Black defendants. The second panel is similar except it shows 2SLS estimates of the impact of incarceration. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). The sample is restricted to cases observed for 7 years. All regressions control for stringency on the opposite margin (i.e., z_i in the conviction specification) race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table E.8: 2SLS estimates for those from zip codes above and below median poverty level

	Above median poverty zip				Below median poverty zip			
	Year 1	Year 2-4	Year 5-7	Year 1-7	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Conviction								
Fut. charge	0.173*	0.186	0.048	0.351**	0.019	0.002	0.041	0.055
	(0.096)	(0.138)	(0.130)	(0.171)	(0.055)	(0.100)	(0.109)	(0.123)
Fut. conviction	0.166*	0.245*	0.000	0.383**	0.070	-0.009	0.043	0.122
	(0.086)	(0.131)	(0.123)	(0.157)	(0.051)	(0.095)	(0.104)	(0.121)
Fut. incarceration	0.111	0.181	-0.105	0.296**	0.062	-0.064	0.004	0.041
	(0.068)	(0.119)	(0.104)	(0.148)	(0.048)	(0.080)	(0.086)	(0.102)
Ctrl. comp. mean: fut. chrg.	0.164	0.319	0.188	0.471	0.129	0.253	0.210	0.442
Ctrl. mean .	0.110	0.204	0.151	0.350	0.078	0.150	0.115	0.268
Ctrl. comp. mean: fut. conv.	0.144	0.284	0.182	0.438	0.117	0.231	0.203	0.419
Ctrl. mean: fut. conv.	0.091	0.177	0.133	0.316	0.067	0.133	0.102	0.242
Ctrl. comp. mean: fut. incar.	0.145	0.301	0.251	0.515	0.108	0.245	0.245	0.462
Ctrl. mean: fut. incar.	0.065	0.131	0.096	0.241	0.046	0.097	0.075	0.183
Panel B: Incarceration								
Fut. charge	-0.100**	0.006	0.079	0.010	-0.071*	0.041	-0.011	-0.018
	(0.046)	(0.073)	(0.066)	(0.086)	(0.042)	(0.067)	(0.061)	(0.080)
Fut. conviction	-0.092**	-0.023	0.102	-0.001	-0.097**	0.024	-0.004	-0.063
	(0.045)	(0.071)	(0.062)	(0.081)	(0.040)	(0.067)	(0.059)	(0.080)
Fut. incarceration	-0.055	-0.020	0.150***	0.041	-0.054	0.079	0.005	0.002
	(0.036)	(0.062)	(0.056)	(0.076)	(0.035)	(0.059)	(0.052)	(0.072)
Ctrl. comp. mean: fut. chrg.	0.145	0.194	0.137	0.364	0.094	0.204	0.170	0.374
Ctrl. mean: fut. chrg.	0.101	0.202	0.153	0.352	0.084	0.169	0.125	0.295
Ctrl. comp. mean: fut. conv.	0.099	0.155	0.094	0.279	0.066	0.181	0.139	0.329
Ctrl. mean: fut. conv.	0.088	0.181	0.138	0.324	0.075	0.155	0.114	0.274
Ctrl. comp. mean: fut. incar.	0.056	0.083	0.056	0.180	0.031	0.067	0.051	0.157
Ctrl. mean: fut. incar.	0.063	0.133	0.099	0.248	0.053	0.110	0.079	0.202
Observations	73,473	73,473	73,473	73,473	73,533	73,533	73,533	73,533

Note: The first panel of this table shows 2SLS estimates of the impact of conviction on future recidivism for those who live in zip codes where the percent earning under 25K (percent in poverty) is above/below median. The second panel is similar except it shows 2SLS estimates of the impact of incarceration. The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). The sample is restricted to cases observed for 7 years. All regressions control for stringency on the opposite margin (i.e., z_i in the conviction specification) race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table E.9: New charges and conviction

	Noncarceral conviction sample				Incarceration sample			
	Year 1	**Year 2-4**	**Year 5-7**	**Year 1-7**	**Year 1**	**Year 2-4**	**Year 5-7**	**Year 1-7**
Panel A: Total number of future charges								
Fut. charge	0.200	0.287	0.050	0.537	-0.166	-0.040	0.121	-0.084
	(0.201)	(0.414)	(0.300)	(0.575)	(0.114)	(0.215)	(0.167)	(0.312)
Fut. conviction	0.285	0.863**	0.306	1.454***	-0.169	-0.241	0.154	-0.256
	(0.182)	(0.356)	(0.254)	(0.494)	(0.103)	(0.202)	(0.152)	(0.283)
Ctrl Mean: fut. charge	0.222	0.498	0.362	1.083	0.213	0.483	0.353	1.050
Ctrl Mean: fut. conv.	0.000	0.000	0.000	0.000	0.209	0.473	0.344	1.025
Panel B: Number of future charge events								
Fut. charge	0.095	0.114	-0.092	0.117	-0.128***	-0.064	0.073	-0.120
	(0.060)	(0.128)	(0.121)	(0.212)	(0.038)	(0.078)	(0.062)	(0.117)
Fut. conviction	0.192***	0.371***	0.078	0.641***	-0.127***	-0.089	0.085	-0.132
	(0.056)	(0.115)	(0.101)	(0.193)	(0.036)	(0.075)	(0.061)	(0.113)
Ctrl Mean: fut. charge	0.115	0.251	0.183	0.549	0.108	0.247	0.185	0.540
Ctrl Mean: fut. conv.	0.000	0.000	0.000	0.000	0.106	0.241	0.179	0.526
Panel C: Future recidivism with 1 charge								
Fut. charge	0.044	-0.055	-0.004	0.044	-0.042*	0.058	0.020	0.019
	(0.038)	(0.055)	(0.062)	(0.076)	(0.025)	(0.036)	(0.032)	(0.045)
Fut. conviction	0.075**	0.066	0.064	0.182***	-0.032	0.050	0.029	0.024
	(0.034)	(0.052)	(0.051)	(0.070)	(0.022)	(0.035)	(0.031)	(0.045)
Ctrl Mean: fut. charge	0.063	0.122	0.092	0.213	0.060	0.119	0.089	0.208
Ctrl Mean: fut. conv.	0.000	0.000	0.000	0.000	0.059	0.116	0.086	0.203
Panel D: Future recidivism with 2 to 3 charges								
Fut. charge	0.051**	0.067*	0.050	0.126***	-0.035**	-0.063***	-0.008	-0.078**
	(0.022)	(0.037)	(0.034)	(0.045)	(0.015)	(0.024)	(0.022)	(0.031)
Fut. conviction	0.069***	0.115***	0.070**	0.193***	-0.037**	-0.062***	-0.009	-0.087***
	(0.020)	(0.035)	(0.032)	(0.043)	(0.014)	(0.022)	(0.021)	(0.029)
Ctrl Mean: fut. charge	0.019	0.037	0.028	0.064	0.021	0.042	0.032	0.074
Ctrl Mean: fut. conv.	0.000	0.000	0.000	0.000	0.020	0.041	0.031	0.072
Panel E: Future recidivism with 4 or more charges								
Fut. charge	0.003	0.069***	0.027	0.065**	-0.014*	-0.018	-0.006	-0.015
	(0.013)	(0.020)	(0.020)	(0.028)	(0.008)	(0.013)	(0.012)	(0.017)
Fut. conviction	0.016	0.088***	0.035*	0.100***	-0.013*	-0.020	-0.003	-0.016
	(0.013)	(0.019)	(0.019)	(0.026)	(0.007)	(0.013)	(0.011)	(0.016)
Ctrl Mean: fut. charge	0.005	0.011	0.008	0.019	0.006	0.013	0.010	0.023
Ctrl Mean: fut. conv.	0.000	0.000	0.000	0.000	0.006	0.013	0.010	0.022
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381

Note: The first panel of this table shows 2SLS estimates of the impact of conviction vs dismissal on the number of future charges and convictions. The second panel is similar except it shows 2SLS estimates of the impact of incarceration vs conviction on the number of future charges. This table shows the results using both the noncarceral conviction sample (columns 1-4) and the incarceration sample (columns 5-8). The columns report results for four recidivism time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). Each time period restricts the sample to cases observed for all 7 years. All regressions control for stringency on the other margin (i.e., z_i for the conviction specification), race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

Table E.10: Testing the models with descriptive characteristics

	Prior Conviction	Female	Black	Misdemeanors	Assualt	Burglary	Drugs	Fraud	Kidnapping	Larceny	Misc	Murder	Robbery	Sexual Assualt
Panel A: Ordered														
Conviction stringency (Z_c)	0.13***	-0.10**	0.0015	0.084**	-0.087*	0.036	0.026	0.055	-0.0020	0.047	-0.032**	-0.054*	0.022	0.0070
	(0.046)	(0.046)	(0.055)	(0.036)	(0.049)	(0.032)	(0.058)	(0.034)	(0.017)	(0.052)	(0.015)	(0.030)	(0.022)	(0.040)
Mean dep. var.	0.228	0.176	0.583	0.097	0.185	0.075	0.299	0.097	0.020	0.260	0.014	0.059	0.033	0.112
N	153692	153692	153692	153692	153692	153692	153692	153692	153692	153692	153692	153692	153692	153692
Panel B: Sequential and ordered														
Incarceration stringency (Z_i)	-0.051	0.11	-0.059	0.0081	0.19**	-0.0060	0.14	-0.13**	0.016	-0.039	0.039*	0.028	0.048	-0.036
	(0.064)	(0.074)	(0.085)	(0.039)	(0.073)	(0.041)	(0.091)	(0.055)	(0.033)	(0.071)	(0.021)	(0.043)	(0.033)	(0.032)
Mean dep. var.	0.136	0.220	0.570	0.065	0.192	0.057	0.352	0.093	0.027	0.175	0.011	0.045	0.034	0.037
N	28589	28589	28589	28589	28589	28589	28589	28589	28589	28589	28589	28589	28589	28589

Note: This table replicates the test of the UPM assumption conducted in Table 6, but using individual covariates as the dependent variables rather than predicted recidivism. For Panel A, we restrict to the incarcerated sample and regress case characteristics on conviction stringency, controlling for incarceration stringency and court-by-time fixed effects. For Panel B, we restrict to the dismissed sample and regress case characteristics on incarceration stringency, controlling for dismissal stringency and court-by-time fixed effects. The sample is restricted to cases adjudicated in 2012 or earlier. Standard errors are clustered at the judge-year level. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01.

F Additional derivations and results

F.1 2SLS with two endogenous variables

Here we briefly discuss why our specification – which instruments for a binary treatment indicator (such as T_c) with one stringency (such as Z_c) while controlling for another stringency (such as Z_i) – should have the same estimand as running a single 2SLS regression with two endogenous treatment variables and both stringencies. In the main paper, we consider the following population regression:

$$\begin{aligned} T_c &= \delta_0 + \delta_1 Z_c + \delta_2 Z_i + U \\ Y &= \gamma_0 + \gamma_1 T_c + \gamma_2 Z_i + V \end{aligned}$$

In the population, we should have $\delta_0 = 0$, $\delta_1 = 1$, and $\delta_2 = 0$. Thus, γ_1 should be equal to γ_1' in the following regression:

$$Y = \gamma_0' + \gamma_1' Z_c + \gamma_2' Z_i + V'$$

Consider now a specification in which both endogenous variables, T_c and T_i, are instrumented for in the same second-stage regression:

$$\begin{aligned} T_c &= \delta_0 + \delta_1 Z_c + \delta_2 Z_i + U \\ T_i &= \omega_0 + \omega_1 Z_c + \omega_2 Z_i + U \\ Y &= \gamma_0'' + \gamma_1'' T_c + \gamma_2'' T_i + V'' \end{aligned}$$

By similar logic, $\omega_0 = 0$, $\omega_1 = 0$, and $\omega_2 = 1$. Thus, $\gamma_1 = \gamma_1' = \gamma_1''$ and $\gamma_2 = \gamma_2' = \gamma_2''$.

In our sample, the first-stage coefficients are not precisely zero or one, as is common in the applied literature. Yet, these two approaches produce similar estimates. Table F.1 shows that, when running 2SLS with two instruments and two endogenous variables, our estimates are similar to those in the main paper and we reach similar conclusions. Note that in these 2SLS and OLS regressions we replace T_c with $T_{\setminus d}$ (i.e., the conviction instrument dummy that remains equal to one for those incarcerated) so that the loading on T_i can be interpreted as the change relative to $T = C$ rather than $T = D$.

Table F.1: **Two instruments and two endogenous variables**

	Year 1		Year 2-4		Year 5-7		Year 1-7	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Convict: fut. charge	-0.002	0.105**	0.004	0.086	0.006**	0.082	0.011***	0.241**
	(0.002)	(0.048)	(0.003)	(0.078)	(0.002)	(0.078)	(0.004)	(0.100)
Incar: fut. charge	-0.022***	-0.097***	0.013***	-0.016	0.025***	0.003	0.023***	-0.071
	(0.002)	(0.029)	(0.002)	(0.047)	(0.002)	(0.040)	(0.003)	(0.059)
Convict: fut. conv.	0.001	0.136***	0.008***	0.115	0.007***	0.059	0.014***	0.306***
	(0.002)	(0.044)	(0.003)	(0.075)	(0.002)	(0.074)	(0.004)	(0.098)
Incar: fut. conv.	-0.018***	-0.112***	0.013***	-0.037	0.023***	0.020	0.022***	-0.106*
	(0.001)	(0.029)	(0.002)	(0.047)	(0.002)	(0.039)	(0.003)	(0.058)
Convict: fut. incar.	0.001	0.114***	0.006**	0.058	0.005**	-0.023	0.012***	0.220**
	(0.002)	(0.039)	(0.002)	(0.066)	(0.002)	(0.059)	(0.003)	(0.086)
Incar: fut. incar.	-0.010***	-0.071***	0.017***	0.009	0.021***	0.052	0.027***	-0.028
	(0.001)	(0.024)	(0.002)	(0.041)	(0.002)	(0.032)	(0.003)	(0.051)
Ctrl Mean: fut. charge	0.088	0.088	0.175	0.175	0.132	0.132	0.306	0.306
Ctrl Mean: fut. conv.	0.077	0.077	0.159	0.159	0.120	0.120	0.283	0.283
Ctrl Mean: fut. incar.	0.055	0.055	0.115	0.115	0.084	0.084	0.212	0.212
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381

Note: This table shows regression estimates of the impacts of conviction and incarceration on future recidivism. The four columns report results for four time ranges (1 year, 2-4 years, 5-7 years, and 1-7 years). For each panel we report ordinary least squares (OLS) and instrumental variable (IV) estimates with two instruments and two endogenous variables. Each time period restricts the sample to cases observed for seven years. All regressions control for race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month-of-year fixed effects, and day-of-week fixed effects. The first six rows report the estimated impact of conviction or incarceration on different measures of recidivism. The first two rows are for any future charge, the second two rows are for any future conviction, and the third two rows are for any future incarceration. For the OLS estimates, we regress our measures of recidivism on having a conviction (regardless of incarceration status) controlling for incarceration. The estimates presented are the coefficient on the conviction variable. For the IV estimates, this provides an estimate of the impacts of conviction compared to dismissal for the set of compliers at that margin and incarceration compared to conviction for the set of compliers at the other margin. Standard errors are clustered at the judge-year level. Stars denote * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

F.2 Binary treatment

Consider an attempt to estimate the impacts of incarceration vs non-incarceration using the following 2SLS specification:

$$T_i = \delta_0 + \delta_1 Z_i + U$$
$$Y = \gamma_0 + \gamma_1 T_i + V$$

This specification is similar to equations (1) and (2) from the main text, but does not include judge dismissal stringency as a control. Under the standard LATE assumptions, γ_1 will not yield a weighted average of LATEs of incarceration vs non-incarceration, since an increase in Z_i could generate flows between dismissal and conviction in the non-incarcerated group if Z_i and Z_c are correlated, which is likely given that $Z_i = 1 - (Z_c + Z_d)$ by construction.

F.3 2SLS estimates with Empirical Bayes Shrinkage

We estimate judge stringency using leave-one-out means. To help ensure stringency measures are not too noisy, we restrict our analysis to judges who see at least 100 cases over the three-year windows we use to calculate stringency. We can further correct for potential measurement error using Empirical Bayes methods. Empirical Bayes was developed in the context of the teacher valued added literature (Chetty et al., 2014; Kane and Staiger, 2008), where the population distribution of teacher value added is typically assumed to be normally distributed, but measured with noise, also typically assumed to be normally distributed. This approach has also been applied to judge stringency measures in some papers (Arnold et al., 2022; Norris, 2019), using standard Empirical Bayes shrinkage procedures (Morris, 1983).

Here we similarly perform parametric Empirical Bayes, but we assume that the population distribution judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution. We believe these parametric assumptions are better than assuming normality since judge stringencies are probabilities.

We take two approaches. The first assumes judge stringencies are drawn from a single Beta distribution, while the second assumes the Beta distribution varies by district and year.

Empirical Bayes with a single Beta prior: First, we assume that judge stringencies are drawn from a $Beta(\alpha, \beta)$ distribution, and we estimate $\hat{\alpha}$ and $\hat{\beta}$ via maximum likelihood based on our sample of judge stringencies, which are calculated in 3-year bins by judge, restricting to judges who handle at least 100 cases.[66] Let's consider noncarceral conviction stringency (the same derivations apply for incarceration or dismissal stringencies). Let C_j be the number of cases ending in a noncarceral conviction for judge j and N_j be the total number of cases they handle. Based on the estimated Beta prior, the posterior conviction stringency is given by:

$$\frac{C_j + \alpha}{N_j + \alpha + \beta}.$$

We then adjust construct the leave-one-out posterior stringency as:

$$\frac{C_j - C_{j,i} + \alpha}{N_j - 1 + \alpha + \beta}.$$

where i represents that particular case.

Figure F.1 plots our main stringency measures (x-axis) against the estimates Empirical Bayes estimates (y-axis). The measures are similar; they largely fall close to the 45 degree line.

Panel (a) of table F.2 reports our main first stage estimates; panel (b) reports the first-stage estimates using the shrunk stringency estimates. The results are very similar. The first-stage coefficients and F-statistics are slightly larger when we use the Empirical Bayes estimates. Panel (a) of table F.3 reproduces our main estimates, and panel (b) reports our 2SLS estimates for noncarceral conviviction using the Empirical

[66] To simplify, we use "judge" and j subscripts though, as in the rest of the paper, these are three-year rolling averages.

Bayes stringencies. Results are nearly identical. Table F.4 produces a similar table for incarceration, with similar conclusions.

Empirical Bayes with priors that vary by district-year: So far, we have used the same Beta prior for all judges. Here, we estimate priors that vary parametrically by district-year. We can express $\alpha = \gamma/\sigma$ and $\beta = (1-\gamma)/\sigma$, where γ is the average stringency and σ is the spread. We then estimate $\gamma_j = \gamma_0 + \gamma_{d,y}$ where $\gamma_{d,y}$ shifts the average stringency by district-year. We estimate this regression using a Bayesian Beta-Binomial regression, then with estimates of γ_0 and $\gamma_{d,y}$, we construct α_j and β_j for each judge-district-year. We construct the leave-one-out posterior stringency as:

$$\frac{C_j - C_{j,i} + \alpha_j}{N_j - 1 + \alpha_j + \beta_j}.$$

This is very similar to the previous approach; the difference is that we now shrink judge stringencies towards the average stringency within district-year, rather than the overall average in our sample. This approach is appealing, as it allows the prior distribution to vary by district-year, but requires estimating many more parameters to recover our empirical priors.

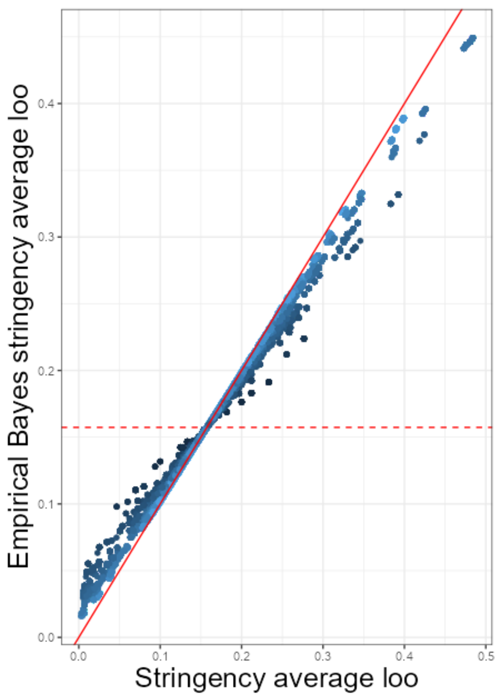
Analogous to panel (b), panel (c) of Table F.2 presents first-stage estimates using Empirical Bayes stringency with district-year priors. Here, we obtain first-stage coefficients that are closer to one, and larger F-statistics. A plausible interpretation is that this approach more effectively addresses measurement error in stringency measures.

Panel (c) of Table F.3 reports our main 2SLS estimates for noncarceral conviction using Empirical Bayes stringency with district-year priors. The results are very similar to our main specification, though estimates are somewhat smaller, particularly for the Year 1-7 time window where estimates are 12.5% to 23.3% smaller and the estimate on future charge is statistically significant at the 0.1 rather than 0.05 level. Table F.4 produces similar results for the 2SLS estimates of incarceration with similar conclusions.

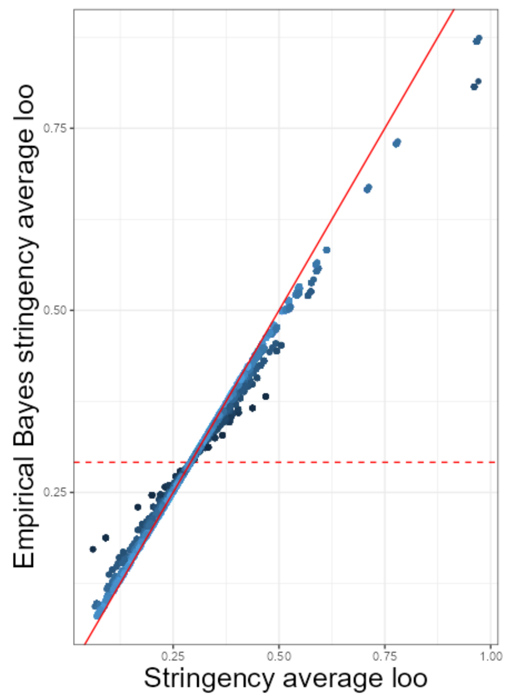
Overall, these results show that accounting for measurement error with either of the methods above does not qualitatively change our conclusions and does not lead to large quantitative differences.

Figure F.1: Leave-one-out raw stringency vs. leave-one-out empirical Bayes stringency

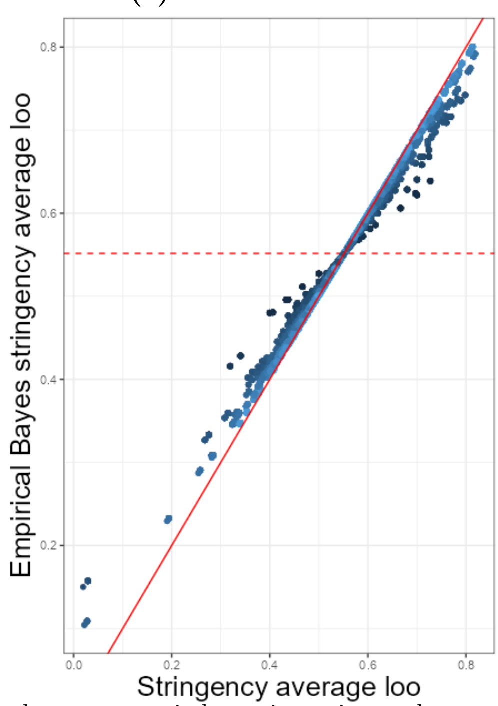
(a) Dismissal



(b) Conviction



(c) Incarceration



Notes: This figure compares the leave-one-out judge stringencies used on our main analysis to leave-one-out stringencies calculated via empirical Bayes with a single Beta prior. The brighter the blue points, the higher the total number of cases for judge j.

Table F.2: Relevance: first stage coefficients for the 2SLS analysis (Empirical Bayes shrinkage)

	Conviction			Incarceration		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: no shrinkage						
Conviction stringency	0.63***	0.60***	0.59***			
	(0.033)	(0.032)	(0.046)			
Incarceration stringency			-0.010	0.62***	0.59***	0.60***
			(0.041)	(0.033)	(0.032)	(0.035)
Dismissal stringency						0.033
						(0.051)
Controls	No	Yes	Yes	No	Yes	Yes
Mean dep. var.	0.298	0.298	0.298	0.546	0.546	0.546
F-stat	360.3	339.7	165.3	346.7	350.4	287.7
N	183,381	183,381	183,381	183,381	183,381	183,381
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: single Beta-prior (EB loo)						
Conviction stringency	0.69***	0.66***	0.65***			
	(0.035)	(0.034)	(0.048)			
Incarceration stringency			-0.011	0.68***	0.65***	0.66***
			(0.044)	(0.035)	(0.033)	(0.037)
Dismissal stringency						0.030
						(0.055)
Controls	No	Yes	Yes	No	Yes	Yes
Mean dep. var.	0.298	0.298	0.298	0.546	0.546	0.546
F-stat	397.0	373.7	177.9	369.0	372.9	308.4
N	183,381	183,381	183,381	183,381	183,381	183,381
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: priors varying by district-year (BB loo)						
Conviction stringency	1.02***	0.97***	0.94***			
	(0.048)	(0.047)	(0.073)			
Incarceration stringency			-0.033	0.97***	0.94***	0.95***
			(0.067)	(0.050)	(0.047)	(0.051)
Dismissal stringency						0.044
						(0.094)
Controls	No	Yes	Yes	No	Yes	Yes
Mean dep. var.	0.298	0.298	0.298	0.546	0.546	0.546
F-stat	444.5	424.9	165.5	379.3	393.5	348.8
N	183,381	183,381	183,381	183,381	183,381	183,381

Note: This table compares the coefficients on the instruments from the first stage of the 2SLS regressions in our main analysis (Panel A) with the coefficients derived using Empirical Bayes with a single Beta prior (Panel B) and with the coefficients derived using Empirical Bayes with priors that vary by district-year (Panel C).

Table F.3: Noncarceral conviction and recidivism (Empirical Bayes shrinkage)

	Year 1		Year 2-4		Year 5-7		Year 1-7	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Panel A: no shrinkage								
Fut. charge	-0.002	0.105**	0.004	0.083	0.006**	0.077	0.011***	0.231**
	(0.002)	(0.046)	(0.003)	(0.075)	(0.002)	(0.075)	(0.004)	(0.097)
Fut. conviction	0.001	0.135***	0.008***	0.110	0.007***	0.055	0.014***	0.295***
	(0.002)	(0.043)	(0.003)	(0.072)	(0.002)	(0.071)	(0.004)	(0.095)
Fut. incarceration	0.001	0.111***	0.006**	0.055	0.005**	-0.025	0.012***	0.209**
	(0.002)	(0.037)	(0.002)	(0.063)	(0.002)	(0.057)	(0.003)	(0.083)
Ctrl comp. mean: fut. chrg.	0.158	0.158	0.302	0.302	0.237	0.237	0.494	0.494
Ctrl mean: fut. chrg.	0.089	0.089	0.170	0.170	0.129	0.129	0.297	0.297
Ctrl comp. mean: fut. conv.	0.138	0.138	0.264	0.264	0.225	0.225	0.460	0.460
Ctrl mean: fut. conv.	0.076	0.076	0.148	0.148	0.114	0.114	0.268	0.268
Ctrl comp. mean: fut. incar.	0.135	0.135	0.288	0.288	0.276	0.276	0.523	0.523
Ctrl mean: fut. incar.	0.054	0.054	0.109	0.109	0.083	0.083	0.204	0.204
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381
Panel B: single Beta prior (EB loo)								
Fut. charge	-0.002	0.109**	0.004	0.084	0.006**	0.075	0.011***	0.231**
	(0.002)	(0.046)	(0.003)	(0.072)	(0.002)	(0.073)	(0.004)	(0.094)
Fut. conviction	0.001	0.137***	0.008***	0.113	0.007***	0.056	0.014***	0.295***
	(0.002)	(0.043)	(0.003)	(0.070)	(0.002)	(0.069)	(0.004)	(0.092)
Fut. incarceration	0.001	0.108***	0.006**	0.055	0.005**	-0.028	0.012***	0.203**
	(0.002)	(0.037)	(0.002)	(0.061)	(0.002)	(0.055)	(0.003)	(0.080)
Ctrl comp. mean: fut. chrg.	0.156	0.156	0.299	0.299	0.236	0.236	0.492	0.492
Ctrl mean: fut. chrg.	0.089	0.089	0.170	0.170	0.129	0.129	0.297	0.297
Ctrl comp. mean: fut. conv.	0.136	0.136	0.263	0.263	0.224	0.224	0.458	0.458
Ctrl mean: fut. conv.	0.076	0.076	0.148	0.148	0.114	0.114	0.268	0.268
Ctrl comp. mean: fut. incar.	0.133	0.133	0.285	0.285	0.273	0.273	0.518	0.518
Ctrl mean: fut. incar.	0.054	0.054	0.109	0.109	0.083	0.083	0.204	0.204
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381
Panel C: priors varying by district-year (BB loo)								
Fut. charge	-0.002	0.102**	0.004	0.039	0.006**	0.088	0.011***	0.186*
	(0.002)	(0.047)	(0.003)	(0.075)	(0.002)	(0.075)	(0.004)	(0.097)
Fut. conviction	0.001	0.132***	0.008***	0.076	0.007***	0.067	0.014***	0.259***
	(0.002)	(0.045)	(0.003)	(0.072)	(0.002)	(0.072)	(0.004)	(0.097)
Fut. incarceration	0.001	0.112***	0.006**	0.019	0.005**	-0.026	0.012***	0.172**
	(0.002)	(0.039)	(0.002)	(0.063)	(0.002)	(0.058)	(0.003)	(0.084)
Ctrl comp. mean: fut. chrg.	0.131	0.131	0.269	0.269	0.213	0.213	0.448	0.448
Ctrl mean: fut. chrg.	0.089	0.089	0.170	0.170	0.129	0.129	0.297	0.297
Ctrl comp. mean: fut. conv.	0.115	0.115	0.238	0.238	0.200	0.200	0.417	0.417
Ctrl mean: fut. conv.	0.076	0.076	0.148	0.148	0.114	0.114	0.268	0.268
Ctrl comp. mean: fut. incar.	0.108	0.108	0.240	0.240	0.222	0.222	0.437	0.437
Ctrl mean: fut. incar.	0.054	0.054	0.109	0.109	0.083	0.083	0.204	0.204
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381

Note: This table compares the OLS and 2SLS regression estimates depicting the impact of noncarceral conviction on future recidivism in our main analysis (Panel A) with the estimates obtained using Empirical Bayes with a single Beta prior (Panel B) and with the coefficients derived using Empirical Bayes with priors that vary by district-year (Panel C).

Table F.4: Incarceration and recidivism (Empirical Bayes shrinkage)

	Year 1		Year 2-4		Year 5-7		Year 1-7	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Panel A: no shrinkage								
Fut. charge	-0.022***	-0.096***	0.013***	-0.016	0.025***	0.003	0.023***	-0.071
	(0.002)	(0.029)	(0.002)	(0.047)	(0.002)	(0.040)	(0.003)	(0.059)
Fut. conviction	-0.018***	-0.111***	0.013***	-0.037	0.023***	0.020	0.022***	-0.106*
	(0.001)	(0.029)	(0.002)	(0.047)	(0.002)	(0.039)	(0.003)	(0.058)
Fut. incarceration	-0.010***	-0.071***	0.017***	0.009	0.021***	0.052	0.027***	-0.028
	(0.001)	(0.024)	(0.002)	(0.041)	(0.002)	(0.032)	(0.003)	(0.051)
Ctrl comp. mean: fut. chrg.	0.122	0.122	0.199	0.199	0.147	0.147	0.370	0.370
Ctrl mean: fut. chrg.	0.088	0.088	0.175	0.175	0.132	0.132	0.306	0.306
Ctrl comp. mean: fut. conv.	0.084	0.084	0.168	0.168	0.113	0.113	0.310	0.310
Ctrl mean: fut. conv.	0.077	0.077	0.159	0.159	0.120	0.120	0.283	0.283
Ctrl comp. mean: fut. incar.	0.043	0.043	0.071	0.071	0.051	0.051	0.166	0.166
Ctrl mean: fut. incar.	0.055	0.055	0.115	0.115	0.084	0.084	0.212	0.212
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381
Panel B: single Beta prior (EB loo)								
Fut. charge	-0.022***	-0.100***	0.013***	-0.016	0.025***	0.003	0.023***	-0.066
	(0.002)	(0.029)	(0.002)	(0.046)	(0.002)	(0.039)	(0.003)	(0.058)
Fut. conviction	-0.018***	-0.115***	0.013***	-0.037	0.023***	0.019	0.022***	-0.104*
	(0.001)	(0.028)	(0.002)	(0.046)	(0.002)	(0.038)	(0.003)	(0.056)
Fut. incarceration	-0.010***	-0.069***	0.017***	0.008	0.021***	0.049	0.027***	-0.029
	(0.001)	(0.024)	(0.002)	(0.040)	(0.002)	(0.032)	(0.003)	(0.050)
Ctrl comp. mean: fut. chrg.	0.124	0.124	0.200	0.200	0.147	0.147	0.373	0.373
Ctrl mean: fut. chrg.	0.088	0.088	0.175	0.175	0.132	0.132	0.306	0.306
Ctrl comp. mean: fut. conv.	0.087	0.087	0.170	0.170	0.113	0.113	0.314	0.314
Ctrl mean: fut. conv.	0.077	0.077	0.159	0.159	0.120	0.120	0.283	0.283
Ctrl comp. mean: fut. incar.	0.045	0.045	0.072	0.072	0.051	0.051	0.169	0.169
Ctrl mean: fut. incar.	0.055	0.055	0.115	0.115	0.084	0.084	0.212	0.212
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381
Panel C: priors varying by district-year (BB loo)								
Fut. charge	-0.022***	-0.106***	0.013***	0.001	0.025***	-0.004	0.023***	-0.055
	(0.002)	(0.028)	(0.002)	(0.045)	(0.002)	(0.038)	(0.003)	(0.056)
Fut. conviction	-0.018***	-0.116***	0.013***	-0.022	0.023***	0.012	0.022***	-0.094*
	(0.001)	(0.028)	(0.002)	(0.045)	(0.002)	(0.037)	(0.003)	(0.054)
Fut. incarceration	-0.010***	-0.073***	0.017***	0.016	0.021***	0.044	0.027***	-0.025
	(0.001)	(0.023)	(0.002)	(0.038)	(0.002)	(0.031)	(0.003)	(0.048)
Ctrl comp. mean: fut. chrg.	0.112	0.112	0.193	0.193	0.142	0.142	0.353	0.353
Ctrl mean: fut. chrg.	0.088	0.088	0.175	0.175	0.132	0.132	0.306	0.306
Ctrl comp. mean: fut. conv.	0.081	0.081	0.165	0.165	0.115	0.115	0.302	0.302
Ctrl mean: fut. conv.	0.077	0.077	0.159	0.159	0.120	0.120	0.283	0.283
Ctrl comp. mean: fut. incar.	0.045	0.045	0.085	0.085	0.061	0.061	0.180	0.180
Ctrl mean: fut. incar.	0.055	0.055	0.115	0.115	0.084	0.084	0.212	0.212
Observations	183,381	183,381	183,381	183,381	183,381	183,381	183,381	183,381

Note: This table compares the OLS and 2SLS regression estimates depicting the impact of incarceration on future recidivism in our main analysis (Panel A) with the estimates obtained using Empirical Bayes with a single Beta prior (Panel B) and with the coefficients derived using Empirical Bayes with priors that vary by district-year (Panel C).

F.4 Calculating control means for compliers

To calculate control-group complier means, we follow Dahl et al. (2014) and Agan and Starr (2018). First we show how to derive control-group complier means for the simple case of binary treatment and a binary instrument. We then expand this to our setting.

In the simple case where $Z \in 0,1$ and $D \in 0,1$, we aim to calculate $E[Y(0) \mid D(1) > D(0)]$. Here $Y(0)$ is the potential outcome when $D = 0$, $D(1)$ is the potential treatment when $Z = 1$, and $D(0)$ is the potential treatment when $Z = 0$. Note that

$$\underbrace{E[Y|D=0,Z=0]}_{\text{data}} = \frac{\pi_c}{\pi_c+\pi_n} \underbrace{E[Y(0)|D(1)>D(0)]}_{\text{unknown}} + \frac{\pi_n}{\pi_c+\pi_n} E[Y(0)|D(1)=D(0)=0]$$

$$\begin{aligned}
&\text{where} \\
&\pi_n = \underbrace{Pr(D=0|Z=1)}_{\text{data}} \\
&\pi_a = \underbrace{Pr(D=1|Z=0)}_{\text{data}} \\
&\pi_c = 1-\pi_n-\pi_a.
\end{aligned}$$

In the expression above, the terms with "data" below them can be estimated directly from the data. The term $E[Y(0)|D(1) = D(0) = 1] = E[Y|D = 0, Z = 1]$, where the right-hand term can also be estimated directly from the data. This leaves only one unknown term: $E[Y(0)|D(1) > D(0)]$, which is the term of interest. Re-arranging the equations and plugging in, we get:

$$E[Y(0)|D(1) > D(0)] = \frac{\pi_c+\pi_n}{\pi_c} E[Y|D=0,Z=0] - \frac{\pi_n}{\pi_c} E[Y|D=0,Z=1],$$

where all the terms on the right side of the equality can be estimated from the data.

Our setting differs from this setting above as we have a continuous instrument, and D can take on 3 values. We follow Dahl et al. (2014) and Agan and Starr (2018) in adapting the math above to the case with continuous instruments. We use code from the replication file of Agan and Starr (2018), which is adapted from Dahl et al. (2014). This adaptation involves calculating the minimum and maximum values of the instrument (z_{min} and z_{max}). Following the papers above, we can then adapt the equations to be:

$$\begin{aligned}
\underbrace{E[Y|D=0,Z=z_{min}]}_{\text{data}} &= \frac{\pi_c}{\pi_c+\pi_n} \underbrace{E[Y(0)|D(z_{max})>D(z_{min})]}_{\text{unknown}} \\
&\quad + \frac{\pi_n}{\pi_c+\pi_n} E[Y(0)|D(z_{max})=D(z_{min})=0] \\
&\text{where} \\
\pi_n &= \underbrace{Pr(D=0|Z=z_{max})}_{\text{data}} \\
\pi_a &= \underbrace{Pr(D=1|Z=z_{min}])}_{\text{data}} \\
\pi_c &= \beta * (z_{max}-z_{min})
\end{aligned}$$

where β is from the regression of D on the instrument. Similar to the binary case, we have $E[Y(0)|D(z_{min}) = D(z_{max}) = 1] = E[Y|D = 0, Z = z_{max}]$. We use the first and 99th percentiles of the residualized instrument for z_{min} and z_{max}, respectively.

To address the fact that we consider multiple treatments, we include non-focal judge stringency as an additional control. For example, if D is the indicator for conviction, we use judge conviction stringency as the instrument, controlling for judge incarceration stringency. Under UPM and the other IV assumptions laid out in the main paper, the only compliers will be those shifting from $T = d$ to $T = c$ and, therefore, capture the margin-specific compliers of interest.

G Additional details for multinomial model with heterogeneous effects

This appendix discusses how we apply Mountjoy (2022) in our setting. First, we describe the identification and estimation of margin-specific treatment effects. Then, we report additional empirical results.

G.1 Additional details on identification and estimation

This subsection summarizes how we adapt the identification and estimation strategies from Mountjoy (2022) to obtain the results in Section 5. To begin, we state the "comparable compliers" assumption of Mountjoy (2022) in our notation:

A7. Comparable Compliers (CC)

For all $\tilde{z}_c$ and $\tilde{z}_i$,

$$\begin{aligned}&\lim_{\tilde{z}_c' \uparrow \tilde{z}_c} E[Y(c)|T(\tilde{z}_c', \tilde{z}_i) = c, T(\tilde{z}_c, \tilde{z}_i) = i] \\ =&\lim_{\tilde{z}_i' \downarrow \tilde{z}_i} E[Y(c)|T(\tilde{z}_c, \tilde{z}_i') = c, T(\tilde{z}_c, \tilde{z}_i) = i].\end{aligned}$$

This assumption says that $i \to c$ compliers from decreasing $\tilde{z}_i$ have the same potential outcome when convicted as $i \to c$ compliers from increasing $\tilde{z}_c$ at their limits, where $\tilde{z}_i$ and $\tilde{z}_c$ are the treatment-specific instruments.

Given a treatment-specific instrument for conviction, it is possible to identify a weighted average of two LATEs that are specific to two different margins as captured by the following expression:

$$LATE_c = \omega LATE_{d \to c} + (1 - \omega) LATE_{i \to c}.$$

This decomposition is visualized in Panel (c) of Figure 3, which shows that such variation induces two sets of compliers, those moving from $T = d$ to $T = c$ (in yellow) and those moving from $T = i$ to $T = c$ (in green).

Mountjoy (2022) shows that it is possible to recover the two margin-specific LATEs, as well as ω, by using variation in two treatment-specific instruments to construct the relevant expected potential outcomes for the two groups. His identification results directly apply once we have recovered choice-specific instruments.

We also follow Mountjoy (2022) in estimation. For example, we similarly assume the relevant conditional expectations are well approximated by a local linear regression centered around the chosen evaluation point of the instruments. These regressions include additive controls as specified in the notes of Table 7. We use an Epanechnikov kernel with a bandwidth of 3 and report estimates evaluated at the mean value of the instruments. This approach produces similar estimates when using smaller or larger bandwidths. Inference is based on 500 bootstrap samples. We report 95% confidence intervals based on the bootstraps and significance stars based on the 90%, 95%, and 99% two-sided confidence intervals.

We refer the reader to Mountjoy (2022) for a full discussion of identification and estimation.

G.2 Additional results

Tables G.1 and G.2 provide additional results under alternative assumptions used to construct the treatment-specific instruments. The first set of results comes from assuming a standard multinomial logistic model. While restrictive, this allows for a simple closed-form solution for constructing thresholds from shares, as explained in the main paper. The second mirrors the mixed model reported in Table 7, but assumes the random effects follow an independent multivariate normal distribution. Confidence intervals for all three approaches are calculated using 500 bootstrap samples.

Overall, the results in Tables G.1 and G.2 are similar in magnitude to Table 7, although the estimates are somewhat larger and tend to be closer to the 2SLS estimates reported in the paper.

Table G.1: Margin-specific treatment effects: alternative approach (robustness, multinomial logit)

	simple log-ratio			
	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Noncarceral conviction vs dismissal (C vs D)				
Felony charge:	0.067	0.172**	0.187**	0.264**
	[-0.024,0.183]	[0.015,0.353]	[0.047,0.390]	[0.035,0.519]
	{0.077}	{0.173}	{0.136}	{0.344}
Felony conviction:	0.090*	0.216**	0.156**	0.342***
	[-0.017,0.205]	[0.064,0.403]	[0.011,0.327]	[0.133,0.636]
	{0.068}	{0.132}	{0.146}	{0.282}
Felony incarceration:	0.056	0.149**	0.074	0.212**
	[-0.024,0.150]	[0.009,0.305]	[-0.038,0.236]	[0.018,0.400]
	{0.070}	{0.107}	{0.123}	{0.298}
Panel B: Incarceration vs noncarceral conviction (I vs C)				
Felony charge:	-0.043**	0.036	0.002	-0.037
	[-0.074,-0.007]	[-0.024,0.113]	[-0.061,0.072]	[-0.139,0.061]
	{0.084}	{0.178}	{0.138}	{0.334}
Felony conviction:	-0.035**	0.031	0.019	-0.027
	[-0.071,-0.003]	[-0.034,0.101]	[-0.050,0.092]	[-0.130,0.075]
	{0.074}	{0.163}	{0.120}	{0.306}
Felony incarceration:	-0.012	0.045	0.014	-0.037
	[-0.044,0.020]	[-0.020,0.106]	[-0.041,0.070]	[-0.141,0.077]
	{0.054}	{0.109}	{0.099}	{0.241}
Controls	Yes	Yes	Yes	Yes

Note: This table reports margin-specific estimates of the impact of noncarceral conviction vs dismissal (Panel A) and incarceration vs noncarceral conviction (Panel B) using an unordered multinomial model based on the methodology developed in Mountjoy (2022). The methodology is described in the notes of Table 7, except that here judge-specific latent preferences are calculated under the stronger assumption that case outcomes are determined by a multinomial logit. The curly brackets report control-group complier means. 95% confidence intervals are reported in brackets and are based on 500 bootstrap samples. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01 based on the 90%, 95%, and 99% confidence intervals.

Table G.2: Margin-specific treatment effects: alternative approach (robustness, independent mixed logit)

	mixed logit with independent normal random effects			
	Year 1	Year 2-4	Year 5-7	Year 1-7
Panel A: Noncarceral conviction vs dismissal (C vs D)				
Felony charge:	0.077*	0.185***	0.124**	0.209***
	[-0.007,0.159]	[0.069,0.309]	[0.004,0.234]	[0.060,0.368]
	{0.059}	{0.140}	{0.120}	{0.297}
Felony conviction:	0.086**	0.198***	0.110**	0.260***
	[0.011,0.151]	[0.092,0.319]	[0.007,0.220]	[0.106,0.447]
	{0.049}	{0.117}	{0.116}	{0.247}
Felony incarceration:	0.059*	0.137**	0.061	0.175**
	[-0.006,0.121]	[0.035,0.248]	[-0.035,0.153]	[0.006,0.342]
	{0.048}	{0.098}	{0.095}	{0.234}
Panel B: Incarceration vs noncarceral conviction (I vs C)				
Felony charge:	-0.054***	0.008	-0.021	-0.082*
	[-0.089,-0.020]	[-0.043,0.068]	[-0.076,0.036]	[-0.176,0.010]
	{0.090}	{0.181}	{0.150}	{0.354}
Felony conviction:	-0.044***	0.004	-0.012	-0.079*
	[-0.076,-0.008]	[-0.055,0.061]	[-0.065,0.042]	[-0.165,0.012]
	{0.079}	{0.168}	{0.133}	{0.330}
Felony incarceration:	-0.019	0.024	-0.006	-0.079**
	[-0.047,0.008]	[-0.026,0.076]	[-0.056,0.046]	[-0.158,-0.001]
	{0.057}	{0.111}	{0.103}	{0.257}
Controls	Yes	Yes	Yes	Yes

Note: This table reports margin-specific estimates of the impact of noncarceral conviction vs dismissal (Panel A) and incarceration vs noncarceral conviction (Panel B) using an unordered multinomial model based on the methodology developed in Mountjoy (2022). The methodology is described in the notes of Table 7, except that here judge-specific latent preferences are calculated under the stronger assumption that the intercepts include a random effect that is an uncorrelated multivariate normal. The curly brackets report control-group complier means. 95% confidence intervals are reported in brackets and are based on 500 bootstrap samples. Stars denote * p< 0.10, ** p< 0.05, *** p< 0.01 based on the 90%, 95%, and 99% confidence intervals.

H Impacts of incarceration: additional evidence from sentencing guidelines

In this Appendix, we provide supporting evidence on the effects of incarceration, exploiting an independent source of variation: discontinuous changes in recommended sentences in the Virginia sentencing guidelines. 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 two different discontinuities, we estimate the effects of incarceration on the intensive margin (sentence length) and on the extensive margin (short jail sentences vs probation). We are also able to provide evidence on the extensive margin for those who had never previously been incarcerated.

H.1 Empirical setup

Sample and data. For these analyses, we focus on people who were convicted of a felony in Virginia and use data from the Virginia Criminal Sentencing Commission (VCSC). See Appendix B for more details on the data and sample construction, and Table H.1 for summary statistics on our 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 diagram in Figure H.1 describes the order in which the different sentencing worksheets are filled out. 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.

Based on the cutoff in Worksheet A, either Worksheet B (for those below the cutoff) or Worksheet C (for those above the cutoff) is used to calculate the final guidelines-recommended sentence. Worksheet B also has a discontinuity that is useful for our analysis. Defendants who score above a particular cutoff on the “probation/jail score” are recommended for short jail sentences, while defendants who score below that cutoff are recommended for probation.

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. An example of Worksheet A can be found in Appendix H.7; the other worksheets follow a similar organization.

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. In Section H.4 below, we provide evidence that there is no change in characteristics at the cutoff, along with tests for bunching in the running variable on either side of 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:

$$y_{i,s} = \beta * \mathbb{1}(s \geq 0) + f(s) * \mathbb{1}(s \geq 0) + g(s) * \mathbb{1}(s < 0) + \epsilon \tag{14}$$

where $y_{i,s}$ is the outcome of the person in case i having obtained a sentencing score of s.[67] 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

[67] As a reminder, the sentencing score is either the incarceration-length score or the probation/jail score.

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.[68] 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.

H.2 Intensive margin: effects of longer carceral sentences.

As expected from the way worksheets are designed, we find that small differences in the incarceration-length score translate into large changes in people's sentences. Columns 1 and 2 of Table H.2 show the regression discontinuity results and Appendix Figure H.4 presents graphic evidence. Scoring above the threshold generates large (42 ppt) changes in the probability of having a sentence greater than one year, and sentences are on average eight months longer, compared to the control-group mean of 4 months.[69]

By comparing people on either side of the threshold, we can estimate the causal effect on new criminal justice contact of going from a sentence of approximately four months to approximately one year. Columns 3-9 of Table H.2 present outcomes in various time periods, from year 1 to year 8-10 after a person's sentencing date.

Our results are consistent with those estimated using quasi-random assignment of cases to judges. In the first year after sentencing, people above the cutoff are less likely to recidivate. This is likely due to an incapacitation effect: those right below the cutoff have an average sentence of four months, while those right above have an average sentence of 12 months. However, in the longer run, this effect disappears, with no significant difference in recidivism. In our ten-year cumulative measure, we can reject anything larger than a 1.2 percentage point increase in new felony charges over a control group mean of 46%.

H.3 Extensive margin: effects of exposure to incarceration

We found no evidence that tripling the sentence length (from approximately four to 12 months) affected future criminal justice contact. This may be because the impacts of incarceration accrue rapidly in the first several months. For example, a few months in jail might lead a person to lose their job, or to experience ruptures in their family lives (Dobbie et al., 2018). We can test the impact of initial exposure by looking at variation in outcomes for people who score just above or just below the cutoff in the

[68] We focus on the left of the cutoff, since we have more observations there.

[69] Control-group means are calculated for people whose score is below the relevant cutoff, and whose score is within the bandwidth used in that RD estimate.

probation/jail score. The first two columns of Panel A of Table H.3 show that scoring above the threshold translates into a 43 ppt increase in the likelihood of receiving a carceral sentence, and the average sentence length increases by 0.73 months (Figure H.5 presents graphic evidence for this extensive margin). Estimates from the probation/jail sample therefore capture the effect of a short jail sentence relative to probation only.[70] Columns 3-5 of Panel A of Table H.3 present results for recidivism. Given that sentences around the cutoff are so short in the sample, we look at short-term results using the six months after sentencing, and longer-term results looking 2-3 years after sentencing. Here, we find no evidence of a short-term incapacitation effect—likely because the difference in sentences is only about a month.[71] As previously, we find no evidence of longer-term effects. In our 1-3 year cumulative measure we can reject anything larger than a 0.007 percentage point increase over a control mean of 20%.

It is also possible that a person's very first incarceration spell may be particularly destabilizing or traumatic. To get at that question, we re-run our analysis on the portion of the probation/jail sample who had not been incarcerated previously, and who had not been detained pretrial.[72] This lowers our sample size substantially, particularly since data on pretrial detention is only available after 2010. As seen in Panel B of Table H.3, there is still a strong discontinuity in the likelihood of receiving a carceral sentence for those right above the cutoff, but no evidence of a change in outcomes once the original carceral sentence is complete. However, the estimates are noisy and we can't reject moderate changes in either direction.

These results are very similar to those obtained exploiting quasi-random assignment of cases to judges: we find short-term decreases in criminal justice contact, likely due to incapacitation, but we do not identify any longer-term impacts of exposure to incarceration. Table E.2 Panel B and Table H.6 present complier characteristics for the IV analyses, and characteristics of defendants who score just above or just below the relevant cutoffs. There are similarities across these groups, but also some small differences. For example, marginal defendants in the RD analysis are more likely to have been convicted with a drug crime compared to the IV compliers—especially for the extensive margin analyses.

H.4 Balance and marginal cases

Balance tests. Figure H.2 (H.3) and Table H.4 (H.5) present balance tests for the intensive margin experiment based on Worksheet A (extensive margin experiment, Worksheet B). We first perform analyses of defendant characteristics, such as demographics or criminal history, and find no notable discontinuities. We then turn to legal actor decisions. Since inputs to the worksheets and how they translate into sentences is common knowledge, it is possible that some savvy legal actors might try to manipulate inputs. For example, a better defense attorney might push harder to drop certain charges if their client has a score close to the cutoff, in order to push them just below the cutoff and avoid longer recommended sentences. If defense attorneys were trying

[70] Short sentences such as those experienced right above the cutoff are not atypical. For example, in Pennsylvania, the average amount of time spent in jail post-sentencing upon release is 2.4 months (PASC, 2013).

[71] We do find short-term incapacitation effects when looking at quarterly data.

[72] Our data is limited to Virginia; it is possible that they had experienced incarceration in another state.

to push their clients to the left of the cutoff, one way this could manifest is by having more charges dropped just before the cutoff. That is because some of the points are linked to number of offenses for which a person is convicted. This does not seem to be happening. We also look at measures of defendant poverty, which can affect quality of representation (Agan et al., 2021).[73] We do not find evidence of a discontinuity at the cutoff, suggesting that quality of representation does not change at this point.

We do find one difference: defendants in the incarceration-length sample are about 2.8 percentage points more likely to have their case resolved by plea just before than just after the cutoff (Panel B of table H.4). This result could be because the longer sentences offered to those just above the threshold make people more willing to "risk it" in court. Since taking the case to trial increases the likelihood of dismissal by 10 percentage points, a 2.8 percentage point increase in the trial rate would lead to losing 0.28% of the sample right above the threshold. Given how small the differences in conviction is at the threshold, and the fact that we see no detectable differences in observable characteristics, we think that this is unlikely to affect our research design too much. We also note that we do not find this discontinuity for the probation/jail sample, so these concerns do not apply to that set of analyses.

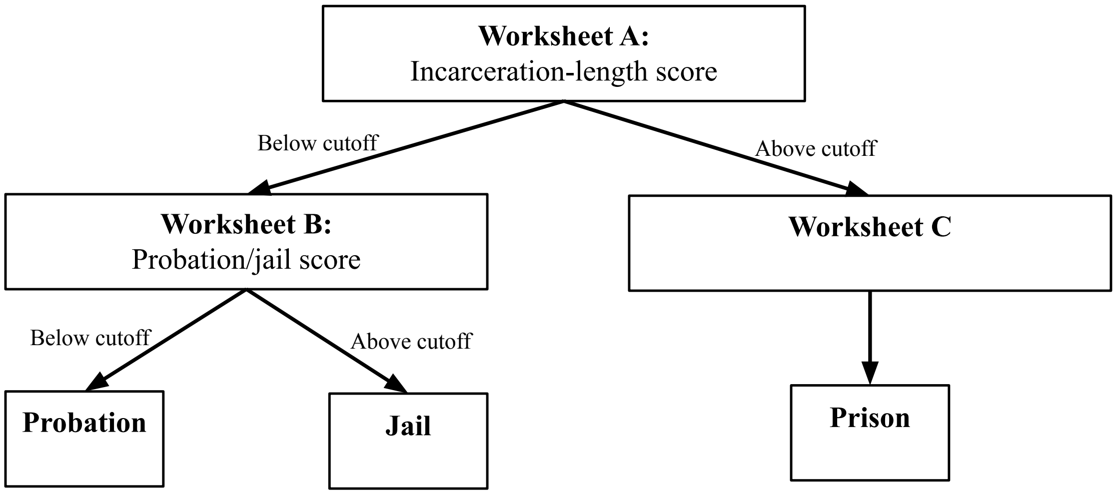
Lastly, we examine the distribution of the running variables to evaluate whether there is excess mass right above or below the cutoff. Such excess mass would be consistent with strategic manipulation of the scores to nudge defendants above or below the discontinuity in guidelines-recommended sentence. These analyses are shown in Figures H.2 (a) and H.3 (a) for the incarceration-length score and the probation/jail score, respectively. Visual inspection reveals possible excess mass below the cutoff for the incarceration-length score. Though, the distribution is not smooth, making it hard to infer whether this bunching is just a natural byproduct of a lumpy distribution or the result of strategic manipulation. There is no visible bunching around the cutoff for the probation/jail score.

Marginal case. Appendix Table H.6 compares the characteristics of marginal cases to those of the full sample in the relevant experiment, where marginal cases are defined as those scoring right below or right above the cutoff. The biggest difference between marginal cases and the full sample for Worksheet A is that marginal cases are much more likely to have prior incarceration: 87% had been incarcerated in the past, compared to 65% for the sample overall. This set aside, marginal cases are similar across offenses, but tend to be slightly younger. For worksheet B, there are differences across offense types: people convicted of a drug offense are more likely to be moved by the policy, while people convicted with property crimes are less so. Marginal cases are also more likely to have been incarcerated in the past (65% compared to 54%). Note that the marginal cases in the RD and IV experiments are different (as an example, 21% of the IV incarceration marginal cases had a prior felony conviction in the last 5 years, compared to 85% of the RD marginal cases). Yet, our results are similar across both experiments, suggesting that the differences in composition are not yielding different findings.

[73] We proxy poverty by whether a defendant comes from zip codes where the percent of people reporting less than $25,000 (less than $10,000) per year to the IRS was above the median within our sample.

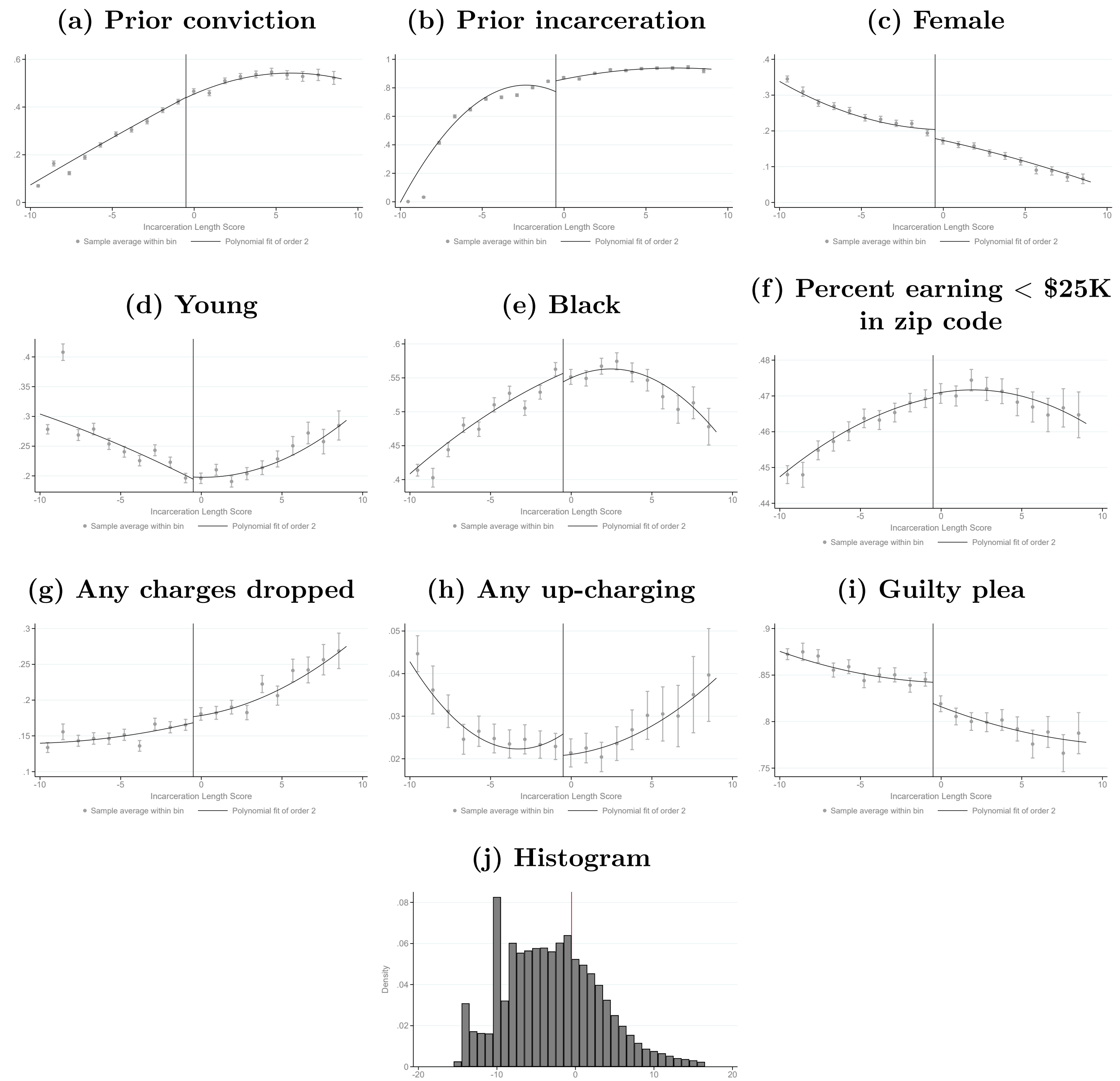
H.5 Appendix figures: RD analyses

Figure H.1: Flowchart of felony sentencing determination in Virginia



Note: This figure presents a flowchart describing the sentencing process in Virginia after a felony conviction, and how and when different Worksheets are used.

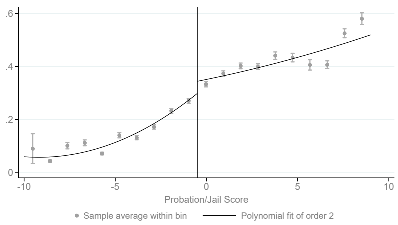
Figure H.2: Balance tests – incarceration-length sample



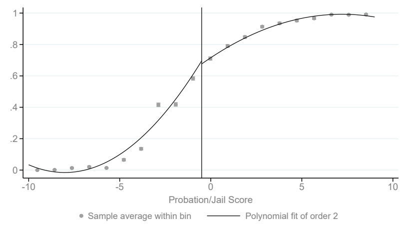
Note: Panels (a) - (i) show RD plots for various demographic variables and case characteristics. Panel (j) shows the distribution of incarceration-length scores around the cutoff. The incarceration-length score is normalized so that the cutoff is at zero.

Figure H.3: Balance tests – probation/jail sample

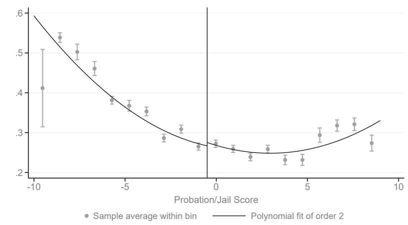
(a) Prior conviction



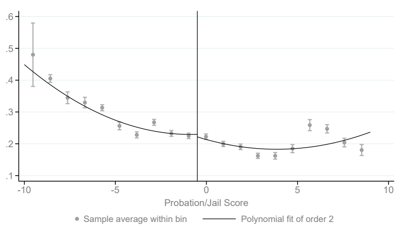
(b) Prior incarceration



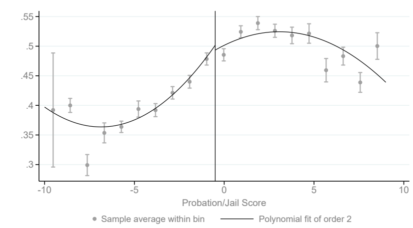
(c) Female



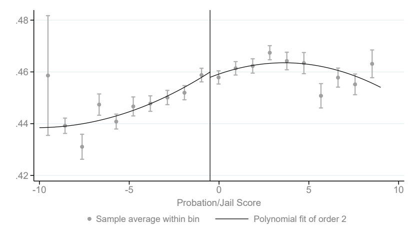
(d) Young



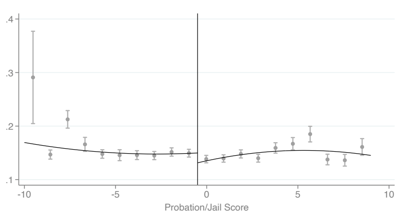
(e) Black



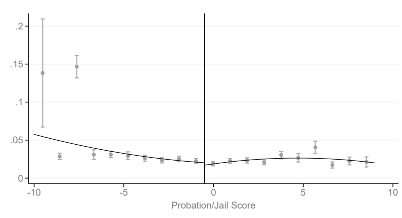
(f) Percent earning < $25K in zip code



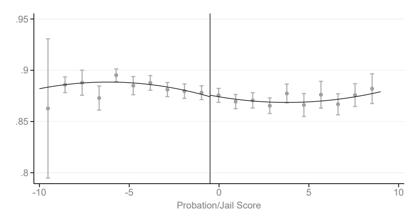
(g) Any charges dropped



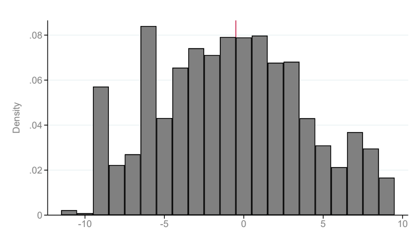
(h) Any up-charging



(i) Guilty plea



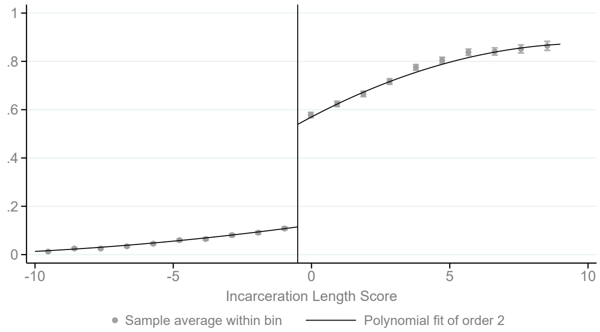
(j) Histogram



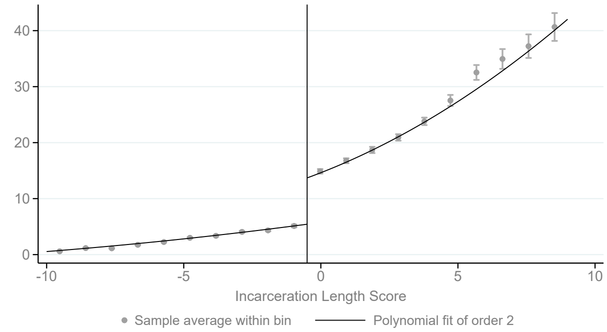
Note: Panels (a) - (i) show RD plots for various demographic variables and case characteristics. Panel (j) shows the distribution of probation/jail scores around the cutoff. The probation/jail score is normalized so that the cutoff is at zero.

Figure H.4: RD first stage and outcome graphs – incarceration-length sample

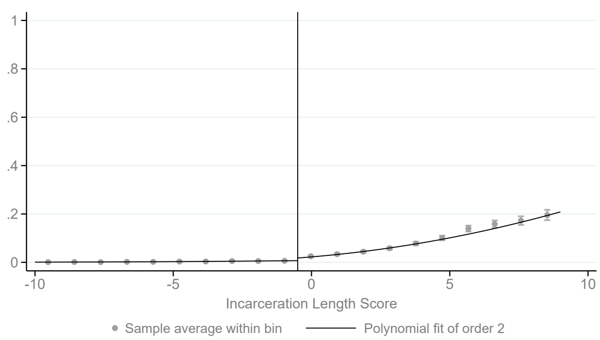
(a) Incarcerated for at least 1 year



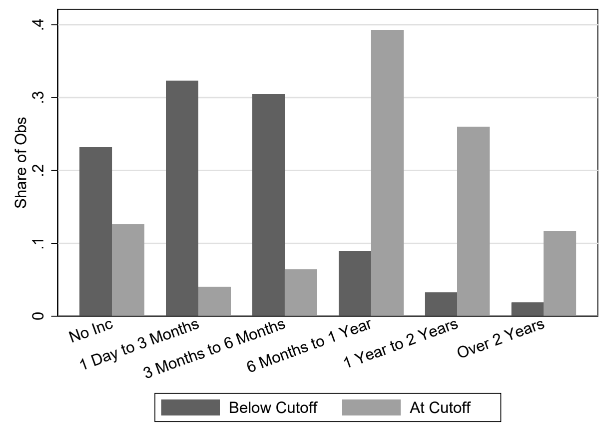
(b) Sentence in months



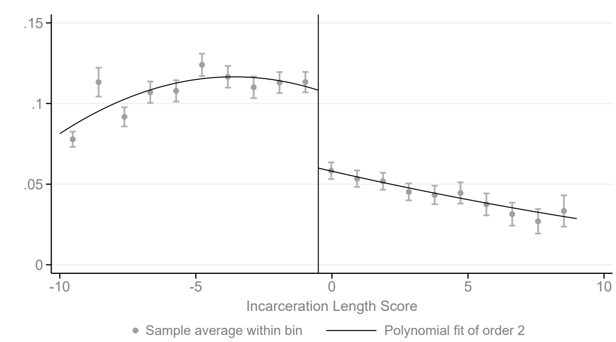
(c) Incarcerated for at least five years



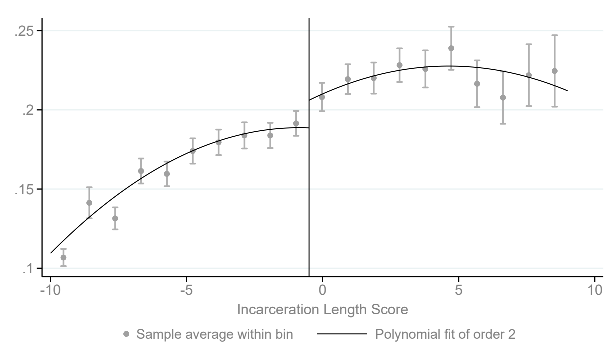
(d) Sentencing distributions for those right above/below the cutoff



(e) New charge within 1 year of sentencing



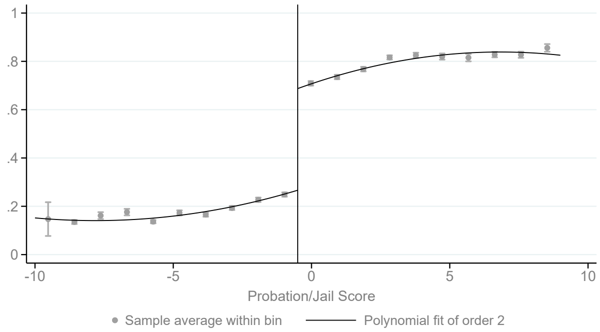
(f) New charge in years 5-7 after sentencing



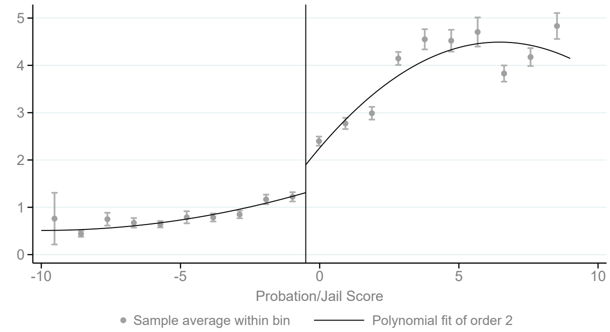
Note: Panel (a) shows the RD plot for being incarcerated for at least one year around the discontinuity in the incarceration-length score. Panel (b) shows the same plot for months sentenced and panel (c) shows the same plot for being sentenced to at least five years. Panel (d) shows the distribution of sentence lengths for those just above and just below the cutoff. Panel (e) shows RD plots for recidivism-defined as a binary variable for having at least one new charge one year post-sentencing and panel (f) shows recidivism within 5-7 years post-sentencing.

Figure H.5: RD First stage and outcome graphs – probation/jail score

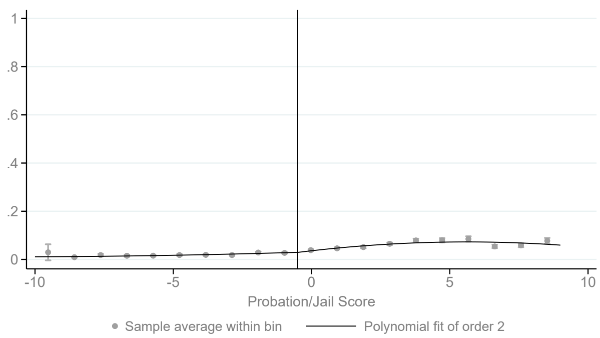
(a) Incarcerated at all



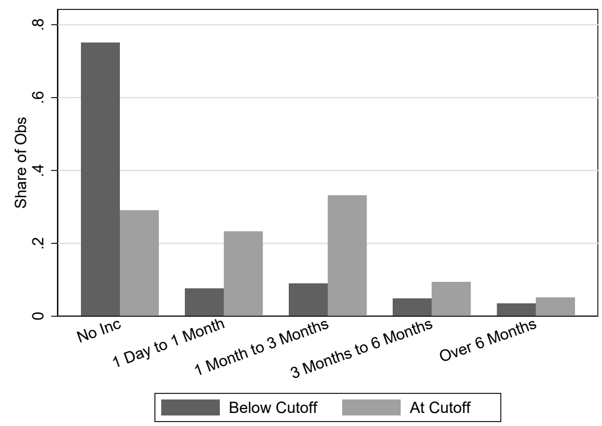
(b) Sentence in months



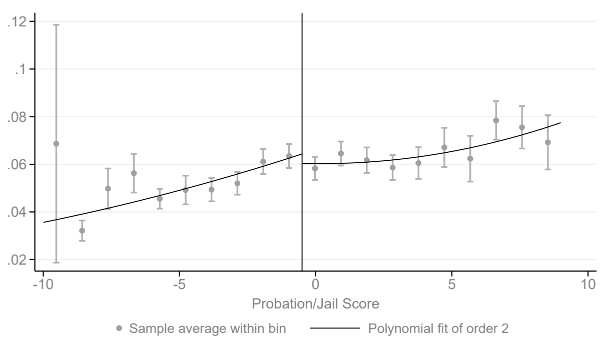
(c) Incarcerated for at least one year



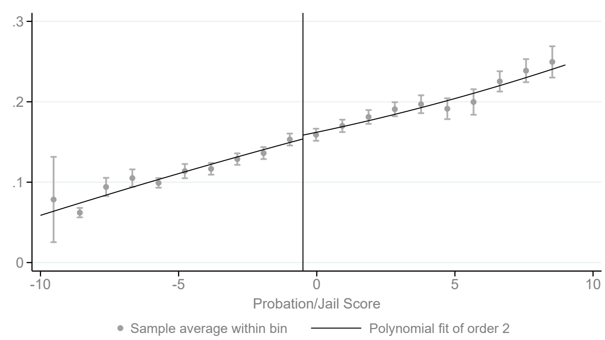
(d) Sentencing distributions for those right above/below the cutoff



(e) New charge within 6 months of sentencing



(f) New charge in years 2-3 after sentencing



Note: Panel (a) shows the RD plot for being incarcerated at all. Panel (b) shows the same plot for months sentenced and panel (c) shows the same plot for being sentenced to at least one year. Panel (d) shows the distribution of sentence lengths for those just above and just below the cutoff. Panel (e) shows RD plots for recidivism-defined as a binary variable for having at least one new charge six months post-sentencing and panel (f) shows recidivism within 2-3 years post-sentencing.

H.6 Appendix tables: RD analyses

Table H.1: Summary statistics: RD sample

	Incarceration length worksheet	Probation/jail worksheet
	mean	mean
Offenses		
Assault	0.05	0.00
Burglary	0.11	0.00
Drug	0.41	0.57
Larceny	0.35	0.42
Miscellaneous	0.02	0.01
Robbery	0.02	0.00
Sexual assault	0.03	0.00
Defendant characteristics		
Black	0.50	0.45
Female	0.23	0.32
Under 23	0.26	0.24
% of ppl in zip earning <25K	0.46	0.45
Incarceration		
Recommended for prison	0.34	0.00
Prior incarceration	0.63	0.54
Prior circuit crt. felony convic.	0.33	0.27
Carceral sentence	0.61	0.47
Jail sentence	0.34	0.45
Prison sentence	0.28	0.04
Sentence >= 5 years	0.04	0.00
Months of sentence	10.50	2.15
Post-release		
New felony charge within 1 year	0.08	0.11
Observations	151,778	115,300

Note: This table shows the means of relevant variables for the incarceration-length sample from Worksheet A and the probation/jail sample from Worksheet B.

Table H.2: Incarceration and recidivism: RD estimates for the intensive margin

	Sentence		Recidivism			
	(1) **Incar > 1 yr**	(2) **Months**	(3) **1 year**	(4) **2-4 years**	(5) **5-7 years**	(6) **1-7 years**
Treatment	0.440	8.451	-0.052	-0.009	0.015	-0.023
	[0.422,0.459]	[7.873,9.029]	[-0.065,-0.038]	[-0.028,0.011]	[-0.005,0.034]	[-0.048,0.002]
N	81,439	81,439	81,439	81,439	81,439	81,439
Control mean	0.08	4.00	0.12	0.23	0.18	0.41

Note: This table first shows the RD estimates of how the cutoff affects sentences (probability of getting a carceral sentence greater than 1 year and sentence length (columns 1-2) and recidivism (columns 3-6). We measure recidivism as the likelihood of receiving a new charge for various time windows: the first post-sentencing year, in which incapacitation is most likely, years 2-4, in which some incapacitation may still be present, as well as years 5-7, during which incarceration rates across treatment and control are equal. It also shows cumulative time windows of 1-7 years to compare to our IV estimates. Below the estimates, we present in brackets confidence intervals obtained following Kolesár and Rothe (2018). Our estimations are for a bandwidth of 5 above and below the cutoff. See Appendix H for a discussion of parameter choices.

Table H.3: Incarceration and recidivism: RD estimates for the extensive margin

	Sentence		Recidivism		
	(1) **Any incar**	(2) **Months**	(3) **6 months**	(4) **1-3 years**	(5) **2-3 years**
Panel A: probation/jail sample					
Treatment:	0.428	0.754	-0.006	-0.005	-0.003
	[0.391,0.465]	[0.523,0.986]	[-0.014,0.001]	[-0.019,0.008]	[-0.015,0.009]
N	80,304	80,304	80,304	80,304	80,304
Control mean	0.21	0.99	0.06	0.21	0.13
Panel B: no prior incar. probation/jail sample					
Treatment:	0.422	0.887	0.015	0.022	-0.002
	[0.340,0.504]	[0.254,1.520]	[-0.013,0.043]	[-0.036,0.080]	[-0.045,0.041]
N	7,851	7,851	7,851	7,851	7,851
Control mean	0.18	0.80	0.05	0.20	0.13

Note: This table first shows the RD estimates of how the cutoff affects sentences (probability of getting a carceral sentence and sentence length (columns 1-2) and recidivism (columns 3-5). We measure recidivism as the likelihood of receiving a new charge for various time windows: the first is 6 months post-sentencing year, in which incapacitation is most likely. It also shows cumulative 1-3 year estimates to compare more closely to our IV results. The third is years 2-3, during which incarceration rates across treatment and control are equal. The first panel is our probation/jail score sample while our second panel is for those in our probation/jail sample without prior incarceration post-2010. Below the estimates, we present in brackets confidence intervals obtained following Kolesár and Rothe (2018). Our estimations are for a bandwidth of 5 above and below the cutoff. See Appendix H for a discussion of parameter choices.

Table H.4: Balance: RD estimates for incarceration-length sample

	(1) In Virginia 5-7yrs	(2) Any prior chrg.	(3) Prior incarc.	(4) Female	(5) Young	(6) Black
Panel A: demographic balance						
RD estimate:	-0.004	0.006	-0.004	-0.016	0.002	-0.016
	[-0.020,0.012]	[-0.011,0.023]	[-0.173,0.165]	[-0.033,0.001]	[-0.038,0.042]	[-0.043,0.011]
N	81,439	81,268	81,439	81,439	81,022	81,439
Control mean	0.80	0.35	0.77	0.22	0.22	0.53
	Plea	Dropped chrg.	Upgrade chrg.	Zip <10K	Zip <25K	
Panel B: income & legal actor balance						
RD estimate:	-0.023	0.007	-0.002	-0.000	-0.000	
	[-0.041,-0.005]	[-0.013,0.028]	[-0.009,0.006]	[-0.003,0.002]	[-0.006,0.006]	
N	81,439	76,399	76,399	58,899	58,899	
Control mean	0.85	0.16	0.02	0.19	0.47	

Note: Panel A shows RD estimates of discontinuities in various predetermined characteristics across the cutoff in the incarceration-length score. Panel B tests for discontinuities at the cutoff in outcomes of the criminal proceedings, such as whether the case resolved in a guilty plea, whether there were any dropped charges, whether there were any charges that were upgraded from misdemeanor to felony, and various measures of indigency within the defendant's zip Code. Below the estimates, we present in brackets confidence intervals obtained following Kolesár and Rothe (2018). Our estimations are for a bandwidth of 5 above and below the cutoff.

Table H.5: Balance: RD estimates for the probation/jail sample

	(1) In Virginia 5-7yrs	(2) Any prior chrg.	(3) Prior incarc.	(4) Female	(5) Young	(6) Black
Panel A: demographic balance						
RD estimate:	-0.006	0.033	0.040	0.017	0.007	-0.000
	[-0.033,0.021]	[-0.014,0.079]	[-0.104,0.184]	[-0.025,0.058]	[-0.033,0.046]	[-0.050,0.050]
N	80,304	80,099	80,304	80,304	79,968	80,302
Control mean	0.79	0.20	0.36	0.31	0.24	0.43
	Plea	Dropped chrg.	Upgrade chrg.	Zip <10K	Zip <25K	
Panel B: zip income & legal actor balance						
RD estimate:	-0.001	-0.012	-0.003	-0.001	-0.003	
	[-0.016,0.013]	[-0.025,0.002]	[-0.009,0.003]	[-0.003,0.001]	[-0.009,0.003]	
N	80,304	72,503	72,503	57,005	57,005	
Control mean	0.88	0.14	0.02	0.18	0.45	

Note: Panel A shows RD estimates of discontinuities in various predetermined characteristics at the cutoff in the probation/jail score. Panel B tests for discontinuities across the cutoff in outcomes of the criminal proceedings, such as whether the case resolved in a guilty plea, whether there were any dropped charges, whether there were any charges that were upgraded from misdemeanor to felony, and various measures of indigency within the defendant's zip Code. Below the estimates, we present in brackets confidence intervals obtained following Kolesár and Rothe (2018). Our estimations are for a bandwidth of 5 above and below the cutoff.

Table H.6: **Marginal cases in the RD study**

	Incarceration length worksheet		Probation/jail worksheet	
	P(X=x)	P(X=x| Marginal)	P(X=x)	P(X=x| Marginal)
Prior conviction	0.636	0.852	0.521	0.565
	(0.481)	(0.355)	(0.500)	(0.496)
Female	0.245	0.204	0.320	0.277
	(0.430)	(0.403)	(0.466)	(0.447)
Black	0.458	0.507	0.438	0.459
	(0.498)	(0.500)	(0.496)	(0.498)
Prior incarceration	0.651	0.871	0.535	0.651
	(0.477)	(0.335)	(0.499)	(0.477)
Drugs	0.412	0.393	0.576	0.815
	(0.492)	(0.488)	(0.494)	(0.388)
Property	0.496	0.491	0.413	0.173
	(0.500)	(0.500)	(0.492)	(0.378)
Violent	0.073	0.098	0.000	0.000
	(0.260)	(0.298)	(0.000)	(0.000)
Other	0.040	0.047	0.011	0.012
	(0.197)	(0.212)	(0.105)	(0.111)
Observations	230357	27556	152663	20609

Note: This table compares socio-demographic characteristics of compliers to that of the full sample for the RD sample.

H.7 Example of sentencing worksheet

Drug/Schedule I/II ❖ Section A

Offender Name: ____________________

◆ Primary Offense

A. Possess Schedule I or II drug

1 count	1
2 counts	3
3 counts	8

B. Sell, Distribute, Possession with Intent Schedule I or II drug

1 count	12
2 counts	13
3 counts	14
4 counts	15

C. Sell, etc. Schedule I, II drug to minor (1 count) 11

D. Accommodation - Sell, Distribute, Possession with Intent Schedule I or II drug

1 count	5
2 counts	7

E. Sell, etc. imitation Schedule I or II drug (1 count) 4

Score [ ]

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

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

Score: 0

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

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

Score: 0

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

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

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

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

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

Score: 0

◆ Prior Incarcerations/Commitments
If YES, add 2 → 0

◆ Prior Felony Drug Convictions/Adjudications

Number	
1 - 2	1
3 - 4	2
5	3
6 or more	4

Score: 0

◆ Prior Juvenile Record
If YES, add 1 → 0

◆ Legally Restrained at Time of Offense

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

Score: 0

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

◆ Two or More Prior Felony Convictions/Adjudications
For Possession, Possession with Intent, Distribution, Manufacture or Sale of Schedule I or II Drug

If YES, add 2 → 0

Total Score → [ ]

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

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