

CONVICTION, INCARCERATION, AND RECIDIVISM:
UNDERSTANDING THE REVOLVING DOOR

By

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Conviction, Incarceration, and Recidivism: Understanding the Revolving Door*

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Noncarceral conviction is a common outcome of criminal court cases: for every individual incarcerated, there are approximately three who were recently convicted but not sentenced to prison or jail. We extend the binary-treatment judge IV framework to settings with multiple treatments and use it to study the consequences of noncarceral conviction. We outline assumptions under which widely-used 2SLS regressions recover margin-specific treatment effects, relate these assumptions to models of judge decision-making, and derive an expression that provides intuition about the direction and magnitude of asymptotic bias when a key assumption on judge decision-making is not met. We find that noncarceral conviction (relative to dismissal) leads to a large and long-lasting increase in recidivism for felony defendants in Virginia. In contrast, incarceration (relative to noncarceral conviction) leads to a short-run reduction in recidivism, consistent with incapacitation. Our empirical results suggest that noncarceral felony conviction is an important and overlooked driver of recidivism.

JEL codes: K42, J24

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I INTRODUCTION

The U.S. criminal justice system is commonly referred to as a “revolving door” due to the high rate of recidivism among those who come into contact with it.¹ A key question for policy makers is whether the criminal justice system itself contributes to these patterns or whether they are driven by external factors such as addiction, mental health, neighborhood disadvantage, or limited labor market opportunities. Much of the available quantitative research has focused on how *incarceration* affects recidivism. However, *noncarceral conviction* (a conviction that does not result in incarceration) is a frequent outcome in the criminal court system.² For instance, in 2010, 2.7 individuals were on probation for every person who was incarcerated (Phelps, 2013). A noncarceral conviction could directly affect recidivism through several channels. It may induce crime by reducing its opportunity cost. For example, a conviction record could make it harder to find employment, making crime relatively more attractive. A conviction could also increase future criminal justice contact even if it has no impact on criminal behavior. For example, prosecutors may be more likely to pursue charges against someone with a recent conviction on their record, and judges may sentence them more harshly. Conversely, a conviction could act as a deterrent if it increases the expected penalties for future crime.

In this paper, we provide new evidence on how both felony noncarceral conviction and incarceration affect future criminal justice involvement. Our main approach follows existing research by using quasi-random assignment of cases to judges as a source of exogenous variation, but our discussion formalizes an extension of this research design from the binary-treatment case to the multiple-treatment case. Our goal is to learn about *margin-specific* treatment effects: causal impacts of noncarceral conviction relative to dismissal of all charges, and causal impacts of incarceration relative to noncarceral conviction. These quantities allow us to isolate the role of mechanisms that come into play when someone is convicted without a carceral sentence (such as the mark of a criminal record) from the role of mechanisms that matter for incarceration (such as incapacitation).

We study a newly-constructed panel of felony cases in Virginia, spanning approximately two decades. Our outcomes are new felony charges, new convictions, and new carceral sentences. Following the literature, we henceforth refer to these outcomes as “recidivism.” Our results point to noncarceral conviction as an important, long-lasting driver of recidivism, consistent with a criminogenic effect of a felony conviction record. In contrast, we find that incarceration leads to only a short-term decline in recidivism, consistent with incapacitation.

Our discussion proceeds in three parts. First, we develop an empirical framework for interpreting judge-stringency 2SLS estimands in a multiple-treatment setting with full treatment effect heterogeneity. Prior applied work using 2SLS with multiple treatments has often

¹According to the Bureau of Justice Statistics, 44% of people released from prison in the U.S. in 2005 were rearrested within one year. Nine years later, 83% had been rearrested at least once (Alper, Durose, and Markman, 2018).

²We will at times refer to “noncarceral conviction” as “conviction” for brevity.

relied on instruments that are reasonably thought of as varying the net payoff to taking up a “focal” treatment (e.g., Kline and Walters, 2016; Kirkeboen, Leuven, and Mogstad, 2016; Mountjoy, 2022). For such instruments, it may be justifiable to assume that they are *treatment-specific*, i.e., they either encourage or discourage take-up of the focal treatment and do not cause any switches between other “non-focal” treatments. This property, combined with the usual IV assumptions, ensures that the estimand from a standard 2SLS regression identifies a causal effect of the focal treatment, relative to a mixture of alternatives.³ However, judge stringency instruments vary the shares of cases that are allocated to particular court outcomes. Therefore, they cannot generally be thought of as varying the net payoff to taking up a particular focal treatment, as in the examples cited above.

We argue that this property of judge stringency instruments implies that insights from prior work do not directly carry over. On the one hand, we show that treatment specificity is sufficient for 2SLS with judge stringency instruments to identify *margin-specific* causal effects, unlike in the previously-cited applications. On the other hand, requiring stringency instruments to be treatment-specific could be considered a strong restriction on judge behavior, while it was considered reasonable for other types of instruments. We provide intuition for the restrictiveness of this assumption by examining how it constrains models of judge decision-making. We consider three commonly-used discrete-choice models, applied to judge decision-making over three court outcomes: dismissal, noncarceral conviction, and incarceration. Specifically, we consider ordered, sequential, and multinomial choice models. The judge-stringency instruments are treatment-specific only in the ordered model. For the sequential and unordered models, which are more realistic in our setting, at least one of the instruments is not treatment-specific. However, all three choice models satisfy a weaker assumption that we label conditional pairwise monotonicity (CPM). This assumption, related to the “no defiers” assumption from the binary case, states that an instrument induces flows in only one direction across each margin.

We then derive an expression for the asymptotic bias in the 2SLS estimand under CPM. The bias term is additive and easy to interpret. It provides intuition about the direction and magnitude of asymptotic bias when CPM holds, but treatment specificity does not. Moreover, it clarifies how restrictions on treatment effect heterogeneity, or on the relative effects for compliers on different margins, can sign or eliminate the asymptotic bias without adopting a more restrictive model of judge behavior. Such restrictions may be motivated by specific institutional details, theory, or prior research.

In the second part of the paper, we turn to our main empirical contributions: estimating the impacts of noncarceral conviction and incarceration on future criminal justice involvement. We use 2SLS with the conviction propensity of judges as an instrument for conviction, while controlling for their incarceration propensity.⁴ Analogously, we use judges’ incarceration

³Here, we follow the literature in referring to an estimand as “causal” if it is a non-negatively weighted average of local average treatment effects (LATEs).

⁴This approach mirrors a common strategy used to study the impact of incarceration on recidivism. See

propensity as an instrument for incarceration and control for their dismissal propensity. Under the assumptions described in the first part of our discussion, our estimates imply that noncarceral conviction relative to dismissal leads to large and long-lasting increases in future criminal justice involvement, while incarceration relative to noncarceral conviction decreases recidivism in the first year, consistent with incapacitation.⁵

Our finding that noncarceral conviction increases recidivism (relative to dismissal) is consistent with both increased criminal behavior and an escalation in subsequent criminal justice responses. We examine how impacts differ by prior records, types of offenses, and measures of recidivism, but do not find evidence that supports one mechanism over the other. Both channels imply that a felony conviction can lead individuals to cycle back into the criminal justice system, leading to increased charges, convictions, and future incarceration.

To probe whether it is reasonable to interpret our 2SLS estimates as causal and margin-specific effects, we propose a test that evaluates whether the instruments are treatment specific. Since each model has different implications for treatment-specificity of the instruments, the test also lets us adjudicate between different models of judge decision-making. Our findings suggest that neither instrument is treatment specific. We can therefore empirically reject the ordered and sequential models of judge decision-making. Therefore, our 2SLS estimates could be biased. However, the magnitude of the bias can vary by context. We use our expression for the asymptotic bias, along with theory and empirical evidence, to argue that, in our setting, the bias is unlikely to overturn our qualitative conclusion regarding the effect of noncarceral conviction.

To assuage any remaining concerns about bias in the 2SLS estimates, we provide an alternative approach for identifying and estimating margin-specific treatment effects under the unordered choice model, which is not rejected by our test. We develop a novel approach that builds on [Mountjoy \(2022\)](#). This approach requires treatment-specific instruments, which we have argued judge stringencies generally are not. Following methods from the discrete choice literature, we impose additional structure on the judge’s choice problem to construct treatment-specific instruments from judge stringencies. We then use these newly-constructed instruments to obtain estimates of margin-specific treatment effects. The results are similar to our 2SLS estimates, although they are smaller and less precise.

This research contributes to both applied and methodological literatures. First, our work is related to a small set of recent studies that explore the impact of criminal convictions. [Mueller-Smith and Schnepel \(2021\)](#) and [Augustine et al. \(2022\)](#) show that felony diversion causes large and sustained reductions in future criminal justice contact. Felony diversion helps avoid conviction, but can also affect recidivism through other channels. For instance, there may be enhanced deterrence, since rearrest leads to reinstated charges. In the context of misdemeanors, [Agan, Doleac, and Harvey \(2023\)](#) show that the decision to file charges

[Loeffler and Nagin \(2022\)](#) and [Doleac \(2023\)](#) for recent reviews of this literature.

⁵We also examine the effects of incarceration using a regression discontinuity design based on sentencing guidelines, yielding conclusions that are consistent with our main findings.

increases future contact with the criminal justice system. However, only 26% of those charged receive a misdemeanor conviction, and the authors argue that the mark of a conviction is not the main channel explaining this effect. In related work, [Kamat, Norris, and Pecenco \(2024\)](#) adopt a partial-identification approach and find that misdemeanor conviction increases the number of future charges, but they cannot rule out large effects of felony conviction in either direction. Additionally, there is a socio-legal literature providing theoretical arguments, as well as qualitative and descriptive evidence about the adverse effects of both felony and misdemeanor convictions (e.g., [Chiricos et al., 2007](#); [Natapoff, 2011](#); [Phelps, 2017](#); [Irankunda et al., 2020](#)). We contribute to the existing literature by disentangling conviction from other aspects of the criminal justice process and by assessing the relative importance of felony conviction and incarceration in driving future criminal justice involvement within the same empirical setting.

Second, this paper contributes to the large body of work investigating the consequences of incarceration for recidivism. A recent review shows that post-conviction incarceration generally is not found to have long-term effects on recidivism, while pretrial detention increases recidivism after the incapacitation period ([Loeffler and Nagin, 2022](#)). Our study suggests one way to reconcile these findings: since pretrial detention increases the likelihood of conviction ([Gupta, Hansman, and Frenchman, 2016](#); [Leslie and Pope, 2017](#); [Stevenson, 2018](#); [Dobbie, Goldin, and Yang, 2018](#)), adverse effects of pretrial detention may be operating through conviction rather than the experience of incarceration itself. Studies that identify the impacts of post-conviction incarceration, meanwhile, are often comparing incarceration to noncarceral conviction, with both the treatment and control groups being convicted.

We build on a methodological literature about the identification and estimation of treatment effects in the presence of multiple treatment alternatives. The prior and contemporaneous literature has outlined many of the challenges associated with multiple treatments (e.g., [Heckman and Pinto, 2018](#); [Heinesen et al., 2022](#); [Bhuller and Sigstad, 2024](#); [Kamat, Norris, and Pecenco, 2024](#)). However, not all of the insights developed in the prior literature apply to the judge IV setting, given the special nature of judge stringency instruments as shares of cases that are allocated to particular court outcomes. Identification issues specific to judge IV in a multiple-treatment setting have received sustained consideration in two prior papers studying the impacts of incarceration. [Mueller-Smith \(2015\)](#) provides one of the first in-depth discussions of the challenges inherent in this design and proposes controlling for judge stringency along “non-focal” dimensions (such as fine amount or probation length). [Arteaga \(2021\)](#) discusses multiple-treatment identification issues and shows how to identify causal effects along the incarceration vs noncarceral conviction margin within a sequential model, which is a special case of our framework.

Our paper contributes to the methodological literature in several ways. First, we lay out identifying assumptions sufficient for 2SLS judge IV to yield a causal and margin-specific estimand when there are multiple treatments. In contemporaneous work, [Bhuller and Sigstad \(2024\)](#) present an alternative set of identifying conditions for 2SLS with multiple treatments. Their regression model is different: it instruments for all treatments simultaneously, and

thus requires stronger functional form assumptions than our approach. The monotonicity conditions they propose are weaker than ours, but ours have straightforward and tractable relationships with economic models of judge behavior. One of our contributions is to show how our econometric assumptions relate to three commonly used discrete choice models, which helps illuminate the econometric implications associated with different ways of modeling the court system. We also derive an expression for asymptotic bias under a weaker set of monotonicity assumptions that all of the choice models we consider satisfy. We suggest an empirical test for instrument treatment-specificity, and we demonstrate how to reason about the sign and magnitude of the bias term if the assumption is rejected. Finally, we show how to construct treatment-specific instruments from judge stringency instruments under an alternative set of assumptions, thus allowing the researcher to apply the identification approach presented in [Mountjoy \(2022\)](#) or other approaches that require such instruments (e.g., [Lee and Salanié, 2018](#)).

Lastly, our paper is related to a broad body of applied work that uses judge instruments. We offer a practical guide for research designs using such instruments when judges choose between more than two options.⁶ Researchers can use their institutional knowledge to reason about which choice model fits best and apply the tests that we suggest to see if the data is consistent with their model. Our paper suggests that if both institutional expertise and the tests support an ordered model, 2SLS is a good choice. If either institutional knowledge or the empirical test reject the ordered model, then 2SLS estimands may have an additional bias term. In that case, theory and empirical results from prior literature can help the researcher reason about the sign and magnitude of the bias, as we demonstrate in our setting. Lastly, our alternative approach to identification can be used if institutional knowledge and empirical tests support an unordered model, and if the additional assumptions for constructing treatment-specific instruments are met. It can also be used as a robustness check to IV specifications.

Our paper proceeds as follows. Section [II](#) describes the institutional setting and our data. Section [III](#) extends the random judge design to multiple treatments and presents a set of sufficient conditions for 2SLS to recover causal and margin-specific treatment effects. We show how the treatment-specific instruments assumption rules out some commonly used models of discrete choice, and then derive an expression for the asymptotic bias if this assumption fails. Section [IV](#) presents our empirical results based on 2SLS estimates and introduces an empirical test for treatment-specific instruments. Section [V](#) describes an alternative approach to identification and estimation, as well as corresponding empirical results.

⁶Judge stringency instruments have been used in the criminal justice setting, but also in other settings, such as foster care ([Doyle, 2008](#); [Gross and Baron, 2022](#)), disability claims ([Maestas, Mullen, and Strand, 2013](#)), bankruptcy ([Dobbie and Song, 2015](#)), eviction ([Collinson et al., 2024](#)), or patent decisions ([Sampat and Williams, 2019](#)). In many settings, decision-makers have multiple alternatives: pretrial detention, electronic monitoring, or release ([Rivera, 2023](#)); opioid prescription, other pain medication, no prescription; foreclosure, loan modification, no court action.

II INSTITUTIONAL DETAILS AND DATA

II.A Felony case processing in Virginia

This section describes felony criminal case processing in Virginia, focusing on adjudication within the Circuit Court, which is the primary data source for this paper.

Between arrest and Circuit Court. After a person is arrested, they are brought to the local police station, booked, and held for their bail hearing. Bail is set by a magistrate, a member of the judiciary who will not preside over further hearings on the case. Charges are first filed in District Court, where the preliminary hearing will be held. At this hearing, the prosecutor must convince the judge that there is probable cause that the defendant committed a felony. This hearing is also the first stage in which plea negotiations might occur. Felony charges might be negotiated down to misdemeanors, or the charges might be dropped or dismissed entirely. If the judge finds probable cause for a felony, the case will then proceed to a grand jury hearing in which a panel of citizens conducts an additional review of the evidence. If the grand jury finds probable cause that the defendant committed a felony, charges will be filed in Circuit Court, where the remainder of the criminal proceedings will occur.⁷ Our analyses include only cases that make it to Circuit Court (roughly 90% of felony charges).

Assignment of cases to judges. Once charges have been filed in Circuit Court, the case is assigned to a judge. The exact assignment procedure varies by jurisdiction, as we learned during phone interviews with court clerks. A few examples include: (1) the clerk drawing colored stickers out of a can to assign judges; (2) a rotating schedule where a judge will see all cases scheduled for that court during that rotation; (3) assignment of judges to cases based on availability; and (4) cases assigned to judges based on whether the case number is odd or even. Appendix E shows that our results are robust to which case assignment mechanisms we include.

Adjudication within Circuit Court. Once a judge has been assigned, the defendant must decide whether she wants to plead guilty or take the case to trial. Since the decision about how to plead depends partly on her expectations of success at trial, we describe the trial process first. Trials in Virginia can be either in front of a judge, which is called a bench trial, or in front of a jury. Approximately 15% of felony convictions in our sample come from trials, almost all of which are bench trials. The remainder come from guilty pleas. In a bench trial, the judge decides whether to convict and, if so, what sentence to give. Judges also exert substantial indirect influence on adjudication and sentencing. For instance, judges decide what evidence is admissible, what charges can proceed, what must be struck from the record,

⁷There are some potential variations of this process. For instance, defendants can waive their right to a preliminary hearing or a grand jury hearing, and prosecutors can bypass the preliminary hearing and directly indict the case with the grand jury.

and what instructions the jury receives. Many of these decisions are made prior to trial. Since judges influence the expected outcome of a trial case, they also influence the willingness to offer or accept a plea deal. The more motions are resolved in favor of the defense, the stronger her bargaining position will be. Plea negotiations may result in a stipulated sentence and/or an agreement that the prosecutor will request a particular sentence. Virginia uses a sentence guidelines system, but the judge makes the final decision about the sentence: they have latitude to reject any negotiated plea deal and to deviate from the sentence guidelines if they provide a written explanation. For all these reasons, judges can influence both the likelihood of conviction and incarceration.⁸

Virginia’s criminal justice system compared to other states. Appendix A compares aggregate statistics for Virginia’s criminal justice system to both national averages and statistics for states considered in other recent studies of the impacts of incarceration. Virginia is similar in terms of incarceration and probation rates, and has a similar racial and ethnic composition of its incarcerated population. However, it has lower than average parole rates because Virginia adopted “truth in sentencing” for felony convictions starting in 1995, which requires people with felony convictions to serve at least 85% of their prison term. As a result, the initial carceral sentence is much more closely linked to time spent incarcerated than in other places.

II.B How noncarceral conviction and incarceration may affect recidivism

Noncarceral conviction. Receiving a felony conviction instead of a dismissal could increase or decrease recidivism through several channels. It could decrease recidivism via deterrence. For example, a person who is convicted but not incarcerated is often placed on probation, which entails additional surveillance and scrutiny, increasing the probability of apprehension. It could also raise future sentences conditional on conviction, since prior convictions are used to determine recommended sentences. Both of these channels suggest that noncarceral conviction increases the expected punishment for future offenses, thereby raising the costs of crime and potentially dampening recidivism (Drago, Galbiati, and Vertova, 2009; Philippe, 2024).

Alternatively, felony convictions may increase recidivism due to the stigma and destabilization associated with such records.⁹ For instance, employers or landlords conducting background checks may be dissuaded from hiring or renting to someone with a felony conviction, raising the cost of finding work in the formal sector, depressing future wages, and driving those with felony conviction to move into neighborhoods with higher overall crime

⁸We provide more institutional details related to the relevance of judge stringency for case outcomes as well as empirical evidence in Appendix D.

⁹Our paper focuses on felony charges. While misdemeanor charges are more common (Mayson and Stevenson, 2020), they generally carry fewer legal and extra-legal consequences (Agan et al., 2024).

rates (see e.g. [Pager, 2003](#); [Holzer, Raphael, and Stoll, 2007](#); [Agan and Starr, 2018](#); [Craigie, 2020](#)).¹⁰

A prior conviction may also increase our measures of recidivism by changing the outcomes of future criminal justice interactions, even with no changes to future criminal behavior. Our recidivism measures are based on new felony charges, convictions, and carceral sentences, all of which involve discretionary decisions by various criminal justice actors. A prior conviction may influence these decisions, leading to a “ratcheting up” of penal responses, where each subsequent interaction with the criminal justice system results in more severe consequences. For example, a prior conviction could influence the likelihood that someone will be detained pretrial, or the prosecutor’s willingness to offer diversion or bargain the charges down to a misdemeanor. Criminal justice actors have access to a defendant’s full criminal record at nearly all stages of decision-making. While even prior arrests that were not sustained influence decisions ([Kohler-Hausmann, 2018](#)), convictions are generally considered more serious indicators of prior criminal behavior.

Incarceration. Incarceration could affect recidivism through several channels. It could reduce future criminal justice contact through incapacitation ([Avi-Itzhak and Shinnar, 1973](#)).¹¹ Incarceration could also decrease recidivism through specific deterrence ([Zimring, Hawkins, and Vorenberg, 1973](#); [Drago, Galbiati, and Vertova, 2009](#)). Under this theory, the negative experience of incarceration discourages future criminal behavior. Alternatively, incarceration could increase recidivism because the trauma, disruption, and loss of human capital involved with time behind bars erode a person’s capacity to make a living in the labor market ([Sykes, 1958](#); [Blevins et al., 2010](#)). Crime becomes more attractive as the outside option becomes less lucrative or less accessible. Prison might also expand the criminal network, thus making illicit activity more profitable ([Hagan, 1993](#); [Bayer, Hjalmarsson, and Pozen, 2009](#); [Stevenson, 2017](#)).

II.C Data sources, sample construction, and summary statistics

This subsection provides a brief overview of our data as well as sample and variable construction. A much more detailed description can be found in Appendix B. This subsection also presents summary statistics.

Data. Our primary data source for the judge IV analysis in Section IV comes from [Virginia’s](#)

¹⁰Both arrests and convictions are visible on background checks and both may influence employers’ and landlords’ decisions. However, since convictions have met a higher burden of proof, convictions are likely considered more serious than arrests that do not lead to conviction, in particular by employers ([Agan et al., 2024](#)). Note also that employment background checks submitted to the Virginia criminal records database do not show arrests that did not lead to a conviction (see VA Code §19.2-389).

¹¹This doesn’t mean that incarceration prevents crime, since crime is common in jails and prisons ([Wolff et al., 2007](#)). However, most within-prison crime is either not reported or is punished using an internal disciplinary system. Generally, only very serious crimes result in new charges.

[Circuit Courts \(2021\)](#). The data were scraped from a publicly accessible website. The Circuit Court data are available from 2000–2020 and cover all of Virginia except Alexandria and Fairfax counties. This data contains information on charges (type and date), on the defendant (gender, race, and FIPS code of residence), and on court proceedings (hearing type, outcome, and judge). We also use it to construct defendants’ recidivism outcomes. We then supplement this data with information on prior felony convictions from the [Virginia Criminal Sentencing Commission \(2021\)](#), which covers everyone convicted of a felony in Virginia during the period 1996–2020.

Sample and variable construction. We drop courts where cases are assigned to judges based on judge specialization or some other non-random schema. We also drop courts with substantial missing data or only one judge. Observations are at the case level. We say that a person is “incarcerated” if at least one charge resulted in a carceral sentence. We define a person to be “convicted” if at least one charge led to a sentence, but none resulted in a carceral sentence (i.e., noncarceral conviction). Lastly, we say that a person was “dismissed” if all of their charges led to a dismissal or an acquittal. Our main measure of recidivism is whether a person has a new felony charge in Circuit Court for an offense that allegedly occurred after the focal disposition date.¹² Our main recidivism measure does not include probation revocations unless these are accompanied by a new felony charge for a new crime. We calculate recidivism in the first year, years two to four, years five to seven, and the first seven years after a person’s initial conviction. We also consider two alternative measures of recidivism: a new conviction resulting from felony Circuit Court charges, or a new carceral sentence resulting from felony Circuit Court charges.

Summary statistics. Table [I](#) provides summary statistics for those dismissed, with a noncarceral conviction, or incarcerated, respectively. In our sample, 55% of cases ended with incarceration, about 30% ended with noncarceral conviction, and 15% ended with dismissal. The three groups are similar in terms of zip code-level poverty but differ demographically. Cases ending in a noncarceral conviction are more likely to have female and non-Black defendants. Cases ending in incarceration are more likely to have defendants with prior felony convictions (22%) compared to the noncarceral conviction and dismissed samples (10% and 14%, respectively). Drug charges are the most common charges for all groups, followed by larceny, assault, and fraud. Appendix Figure [E.1](#) presents disposition types for four common offenses: drugs, larceny, assault, and fraud. While there is variation in the breakdown, all three disposition types exist within each offense type.

¹²Crimes committed during incarceration are usually addressed with internal sanctions and are unlikely to result in new felony charges. Hence, our main recidivism measures are likely to overlook crimes committed behind bars.

III EXTENDING BINARY-TREATMENT JUDGE IV TO MULTIPLE TREATMENTS

In this section, we extend the “random judge” framework from the binary-treatment setting to the setting with three possible court outcomes. We outline assumptions under which widely-used 2SLS regressions recover margin-specific treatment effects, provide intuition for their restrictiveness by relating them to models of judge decision-making, and derive an expression that can be used to reason about the likely sign and direction of bias when some of the assumptions are not met.

III.A Notation and common regression specifications

We consider a setting where cases can end in one of three mutually exclusive and collectively exhaustive alternatives: dismissal (d), noncarceral conviction (c), or incarceration (i). We denote treatment by $T \in \{d, c, i\}$. To simplify the discussion below, we further define $T_k \equiv \mathbb{1}\{T = k\}$ as an indicator for the outcome of the case being $k \in \{d, c, i\}$ and $T_{\setminus d} \equiv \mathbb{1}\{T \in \{c, i\}\}$ as an indicator that is equal to one if an individual is convicted or incarcerated (i.e., their case is not dismissed). Finally, we let Y be a measure of recidivism.

Both T_c and T_i are likely to be affected by unobserved factors that also influence recidivism, such as the strength of the evidence or the details of the offense or criminal record. Therefore, in a regression of Y on these court outcomes, there is concern about selection bias. To deal with selection, a common approach is to use judge propensities for specific case outcomes as instruments. Let J denote the identity of the judge randomly assigned to a case. Define incarceration stringency $Z_i \equiv E[T_i \mid J]$ and let $z_i^j \equiv E[T_i \mid J = j]$, where $j \in \{1, \dots, \mathcal{J}\}$ indexes the judges. Similarly define Z_k and z_k^j for $k \in \{c, d\}$.

Using the notation above and abstracting away from covariates, the following regression model is commonly used to study the impacts of incarceration (see, e.g., [Mueller-Smith, 2015](#); [Bhuller et al., 2020](#); [Arteaga, 2021](#); [Norris, Pecenco, and Weaver, 2021](#)):

$$T_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 Z_d + \epsilon \tag{1}$$

$$Y = \beta_0 + \beta_1 T_i + \beta_2 Z_d + \nu. \tag{2}$$

Estimated using two-stage least squares (2SLS), researchers instrument incarceration with the assigned judge’s incarceration stringency, and controls for dismissal stringency Z_d to prevent exclusion violations stemming from the judge’s likelihood of conviction.¹³

¹³Another common specification uses a second stage with the two endogenous treatments, T_c and T_i , instrumented with both stringencies, Z_c and Z_i . Under assumptions A1–A4, defined below, this specification produces the same 2SLS estimand as (1)–(2) (see Appendix C.6). Alternatively, researchers may consider including a single binary treatment indicator (e.g., T_i) and instrument with judge stringency in that dimension, omitting controls for other dimensions of sentencing. Under the standard [Imbens and Angrist \(1994\)](#) LATE assumptions, this approach does not recover a well-defined causal effect when there are multiple treatments and the stringencies are correlated, which is likely given that $Z_i = 1 - (Z_c + Z_d)$ (see Appendix C.7).

Analogously, one approach to learning about the impacts of a noncarceral conviction is to use the following specification, which instruments for conviction but controls for incarceration stringency:

$$T_c = \gamma_0 + \gamma_1 Z_c + \gamma_2 Z_i + \epsilon \quad (3)$$

$$Y = \delta_0 + \delta_1 T_c + \delta_2 Z_i + \nu. \quad (4)$$

In the next subsection, we discuss conditions under which δ_1 has a causal and margin-specific interpretation—i.e., when it can be interpreted as the impact of noncarceral conviction relative to dismissal for some well-defined subgroup of the population.¹⁴

III.B Judge IV assumptions in the multiple-treatment case

We define, for each individual, the potential case outcomes $T(z_c, z_i) \in \{d, c, i\}$, and the potential recidivism outcomes $Y(t, z_i, z_c)$, $t \in \{d, c, i\}$. We further define $T_k(z_c, z_i) \equiv \mathbb{1}\{T(z_c, z_i) = k\}$, for $k \in \{d, c, i\}$. Using this notation, we can state the standard IV assumptions of exclusion, random assignment, and relevance for the multiple-treatment case:

A1. Exclusion: $Y(t, z_i, z_c) = Y(t) \forall t, z_i, z_c$.

A2. Random assignment: $Y(t), T(z_c, z_i) \perp\!\!\!\perp Z_i, Z_c \forall t, z_i, z_c$.

A3. Relevance: $\gamma_1 \neq 0$ in equation (3).

We additionally make the following functional form assumption, following [Blandhol et al. \(2022\)](#):

A4. Rich covariates: The linear projection of Z_c on Z_i is equal to $E[Z_c | Z_i]$.

Equations (3)-(4) instrument for conviction using Z_c while controlling for Z_i rather than instrumenting for conviction and incarceration jointly in the same 2SLS regression. An advantage of this approach is that concerns about the validity of Assumption A4 can be alleviated in a straightforward way by controlling for Z_i more flexibly (see Appendix C.5).

Throughout the paper, unless specified otherwise, we assume A1–A4 are satisfied. A1–A3 represent straightforward analogs to the standard [Imbens and Angrist \(1994\)](#) assumptions, and A4 implements a functional form assumption related to using Z_i as a control. Extending the monotonicity assumption from the binary- to the multiple-treatment setting is less straightforward. In other applications, researchers have assumed that instruments induce

¹⁴Our discussion in the remainder of this section is organized around the interpretation of δ_1 in specification (3)–(4), but an analogous argument holds for β_1 in specification (1)–(2).

compliers to take up a specific treatment, without inducing anyone to switch into other “non-focal” treatments, i.e. that the instruments are treatment-specific. For example, [Kline and Walters \(2016\)](#) study the impact of enrolling in Head Start in a setting with two outside options, using randomly-assigned offers of enrollment as an instrument. The Head Start offer is assumed to not induce switches between the outside options. Similarly, [Kirkeboen, Leuven, and Mogstad \(2016\)](#) study the returns to college majors and use offers of admission to specific majors as instruments. Their irrelevance condition states that access to a major does not induce switches between other choices (e.g., increased access to an economics major won’t induce students to switch between history and mathematics). In a similar vein, [Mountjoy \(2022\)](#) assumes that reducing the distance to a two-year college (while holding distance to four-year college fixed) lowers its relative costs, but does not induce switches between four-year college and not enrolling.

The Unordered Partial Monotonicity (UPM) assumption in [Mountjoy \(2022\)](#) formalizes the treatment-specific instruments assumption. In our notation, this assumption can be stated as:

A5. Unordered Partial Monotonicity (UPM($Z_c \mid Z_i$)):

For all z_c, z'_c, z_i with $z'_c > z_c$ and holding z_i fixed:

- i. $T_c(z'_c, z_i) \geq T_c(z_c, z_i)$,
- ii. $T_i(z'_c, z_i) \leq T_i(z_c, z_i)$,
- iii. $T_d(z'_c, z_i) \leq T_d(z_c, z_i)$.

Treatment specificity of an instrument for conviction, as formalized by UPM, imposes three restrictions on substitution patterns when Z_c increases and Z_i is held fixed. First, it guarantees that individuals only move into (and not out of) noncarceral conviction. Second, it guarantees that individuals only (weakly) move in one direction across any margin. Third, it rules out flows between dismissal and incarceration.¹⁵ The UPM assumption thus incorporates a property similar to the “no defiers” assumption in the binary setting ([Imbens and Angrist, 1994](#)), but additionally rules out switches between incarceration and dismissal.

When using judge stringencies as instruments, the UPM assumption restricts substitution patterns more than in the three studies discussed above. In those examples, the instruments reduce costs or increase access to specific choices. In contrast, judge stringency instruments are the judge-specific probabilities of a case ending with a particular outcome. The stringency instruments will add up to one ($z_d^j + z_c^j + z_i^j = 1$) since our case outcomes are mutually exclusive. As such, judge stringency instruments vary the probabilities of taking up particular treatments. If we condition on the judge stringency for one particular treatment, we fix its net probability of take-up.

¹⁵Note that UPM can hold when varying one instrument and holding the other fixed, but not hold when switching the roles of the instruments. We therefore use the notation UPM($Z_c \mid Z_i$) for the definition above and UPM($Z_i \mid Z_d$) when incarceration is the focal treatment.

This feature of judge instruments is important for understanding judge IV with multiple treatments. If we increase conviction stringency Z_c while holding Z_i fixed, we increase the net probability of conviction while holding the net probability of incarceration constant. Thus, if increasing Z_c results in an $i \rightarrow c$ shift, there must also be a compensating same-sized $d \rightarrow i$ shift to keep the net probability of incarceration constant. However, $\text{UPM}(Z_c | Z_i)$ rules out flows from dismissal to incarceration. Since the net probability of incarceration Z_i is held fixed, there can be no $i \rightarrow c$ flows. Therefore, UPM implies that judge stringency instruments are not only *treatment-specific*, as in the examples described above, but also *margin-specific*: they induce complier flows across only one margin, e.g., dismissal to noncarceral conviction. In the multiple-treatment judge IV setting, UPM therefore ensures that we recover margin-specific treatment effects, but it is also a less plausible assumption than in many other multiple-treatment IV settings. In Section III.C we illustrate the restrictiveness of the UPM assumption by showing that it rules out certain reasonable models of judge decision-making.

Given that UPM may be a particularly strong assumption with judge stringency instruments, we next introduce a weaker monotonicity assumption, which we call conditional pairwise monotonicity (CPM).¹⁶

A6. Conditional Pairwise Monotonicity ($\text{CPM}(Z_c | Z_i)$):

For case outcomes c , i , and d , for all z_c, z'_c, z_i with $z'_c > z_c$ and holding z_i fixed:

- i. $T_c(z'_c, z_i) \geq T_c(z_c, z_i)$ for all individuals
- ii. if $T_i(z'_c, z_i) = T_d(z_c, z_i) = 1$ for any individual, then $T_i(z_c, z_i) = 1$ implies $T_d(z'_c, z_i) = 0$ for all individuals
- iii. if $T_d(z'_c, z_i) = T_i(z_c, z_i) = 1$ for any individual, then $T_d(z_c, z_i) = 1$ implies $T_i(z'_c, z_i) = 0$ for all individuals.

CPM imposes two of the three restrictions imposed by UPM. It guarantees that, in response to increasing Z_c while holding Z_i fixed, individuals only move into (and not out of) $T = c$ and that individuals only (weakly) move in one direction across any margin.¹⁷ CPM does not rule out flows across margins that are not adjacent to noncarceral conviction. For example, an increase in Z_c holding Z_i constant can induce $d \rightarrow c$ and $i \rightarrow c$ flows, but also $d \rightarrow i$ flows. Throughout this paper, we assume CPM holds. Next, we discuss the implications for 2SLS estimands when CPM holds but UPM does not.¹⁸

¹⁶Another way to relax the UPM assumption would be to extend the concept of average monotonicity (Frandsen, Lefgren, and Leslie, 2023) to the multiple-treatment setting. We present a definition of “average UPM” in Appendix C.4. Bhuller and Sigstad (2024) provide a more general way to extend average monotonicity with an arbitrary number of treatments. They provide conditions that are both sufficient and necessary for a 2SLS estimand to have “proper weights.”

¹⁷Note that conditions (ii) and (iii) in A6 can be replaced with $T_d(z'_c, z_i) \leq T_d(z_c, z_i)$ within our setting with stringency instruments, which makes CPM equivalent to (i) and (iii) from the UPM definition.

¹⁸While CPM is weaker than UPM, it still imposes restrictions on judge behavior that may not hold: it

III.C Connecting assumptions to models of judge decision-making

In this subsection, we provide economic intuition for the assumptions in the previous subsection, by examining how they restrict models of judge decision-making. We consider three index-crossing models of judge decision-making based on canonical models of multinomial discrete choice—an ordered choice model, a sequential choice model, and an unordered choice model—and discuss how they relate to the legal and institutional practices of criminal proceedings.¹⁹ All three models satisfy the CPM assumption. Only the ordered choice model satisfies the UPM assumption for both instruments. The sequential model illustrates that UPM may be satisfied for one of the instruments but not the other.

III.C.1 Ordered choice

First, we consider a straightforward extension to a trinary model from the binary threshold-crossing model. This extension is an ordered choice model with a single dimension of case-specific unobserved heterogeneity W . Each judge has their own thresholds for the values of W that would result in dismissal, noncarceral conviction, and incarceration:

$$\begin{aligned} T_d &= \mathbb{1}\{W < \pi_c(Z_d)\} \\ T_c &= \mathbb{1}\{\pi_c(Z_d) \leq W < \pi_i(Z_i)\} \\ T_i &= \mathbb{1}\{W \geq \pi_i(Z_i)\}. \end{aligned} \tag{5}$$

Panel (a) in Figure I visualizes, for two different judges, the regions of W under which each judge dismisses, convicts, and incarcerates. In this example, judge 1 has higher thresholds for both noncarceral conviction and for incarceration than judge 2.

In an ordered choice model, we can estimate margin-specific treatment effects for both the conviction-dismissal margin and the incarceration-conviction margin. Consider panel (b) of Figure I, where both judges have the same incarceration threshold, but judge 2 has a lower noncarceral conviction threshold, meaning that they convict more and dismiss less than judge 1. Fixing Z_i and increasing Z_c will hold $\pi_i(Z_i)$ fixed and decrease $\pi_c(Z_d)$. The only people who will switch treatment status are those who move from dismissal to conviction. When conditioning, the instruments are treatment-specific, since fixing Z_i and increasing Z_c will induce flows into only one choice ($T = c$) and not into any other treatment. Moreover, the instruments only move individuals across a single margin ($d \rightarrow c$). Similarly, we can learn about the effect of incarceration vs noncarceral conviction using variation in Z_i and fixing Z_d . Thus, this choice model satisfies the unordered partial monotonicity assumption for both

rules out defiers by requiring the instrument moves everyone in the same direction across a margin (see e.g. Chaisemartin, 2017; Chan, Gentzkow, and Yu, 2022; Frandsen, Lefgren, and Leslie, 2023; Sigstad, 2023, for more elaborate discussions). Here, we focus on the novel issues that arise with judge stringency instruments and multiple treatments.

¹⁹Throughout this subsection we use “models of judge decision-making” as a shorthand; in practice, court outcomes reflect a combination of decisions by multiple actors.

margins (i.e., $\text{UPM}(Z_c | Z_i)$ and $\text{UPM}(Z_i | Z_d)$ hold).

This model would be appropriate if all judges considered a single dimension of unobserved heterogeneity in their decision, and they agreed on how cases are ranked according to this dimension. The only way judges can differ in their decision making is by setting different thresholds for assigning cases to each outcome. In practice, however, judges could take into account more than one measure of unobserved heterogeneity. In the remainder of this section, we consider models that allow for multiple dimensions of unobserved differences between defendants.

III.C.2 Sequential choice

Next we consider a sequential choice model in which the court process consists of two decisions: (1) a dismissal decision and, if not dismissed, (2) an incarceration decision. This model reflects the two-step process of criminal cases: a trial to adjudicate guilt or innocence, followed by a sentencing hearing if the person is found guilty. It allows judges to consider different, though potentially correlated, unobserved factors in each decision. For example, conviction decisions may depend on the strength of the evidence, while incarceration decisions may depend on other aspects, such as the propensity to re-offend or severity of the crime.

We can write this as a threshold-crossing model:

$$\begin{aligned} T_d &= \mathbb{1}\{U_c < \pi_c(Z_d)\} \\ T_c &= \mathbb{1}\{U_c \geq \pi_c(Z_d), U_i < \pi_i(Z_i, Z_d)\} \\ T_i &= \mathbb{1}\{U_c \geq \pi_c(Z_d), U_i \geq \pi_i(Z_i, Z_d)\}. \end{aligned} \tag{6}$$

In this model, the first choice is between $T \in \setminus d$ (not dismissed) and $T = d$ and depends on the value of case-specific unobservable U_c relative to judge-specific threshold π_c . For cases that switch from dismissed to “not dismissed,” there is then a second choice: noncarceral conviction or incarceration. This choice depends on the value of case-specific unobservable U_i , which can be correlated with U_c , relative to judge-specific π_i .²⁰ This model is consistent with different factors being relevant at each stage of the decision. For example, evidence might be more relevant to conviction while the criminal record might be more relevant to sentencing. It is also consistent with new information arriving at the incarceration stage, such as letters of support for the person convicted of the crime or victim impact statements.

Under the sequential model and A1-A4, it is possible to use 2SLS and the stringency instruments to recover margin-specific treatment effects for the incarceration-conviction margin, but not for the conviction-dismissal margin or the dismissal-no dismissal margin. Figure II illustrates this point. Panel (a) visualizes the decision regions of one judge, which are based on U_c and U_i . Panel (b) compares two judges who have the same probability

²⁰See Heckman, Humphries, and Veramendi (2016) for details on identifying treatment effects in this type of sequential choice model, and Arteaga (2021) for a criminal court application studying the impacts of incarceration using a model similar to the sequential model described above.

of dismissal, but where the second judge has a higher probability of incarceration. Here, variation in Z_i holding Z_d fixed only induces $c \rightarrow i$ changes in court outcomes for a set of compliers.

In contrast, panel (c) compares two judges who have the same probability of incarceration (Z_i), but where judge 2 has a lower probability of dismissal (Z_d). Recall that Z_i is the proportion of cases resulting in incarceration. In this figure, Z_i is represented by the fraction of people in the top right section. For two judges to have the same incarceration shares but different dismissal probabilities, both π_i and π_c must differ across these judges. Comparisons across these two judges induces three sets of compliers: $d \rightarrow c$ compliers, $i \rightarrow c$ compliers, and $d \rightarrow i$ compliers. This example satisfies CPM since there is only a one-way flow across any margin, and no flows out of treatment. However, the flow from $T = d$ to $T = i$ implies that the instrument is not treatment-specific, and $\text{UPM}(Z_c | Z_i)$ is not satisfied.

Although the sequential model captures the two-step nature of the criminal proceeding, it may not be a good model if the outcome of the case is determined by a *joint consideration* of the two dimensions, as may be the case when plea bargaining occurs. We thus also consider a multinomial choice model, which has two dimensions of unobserved heterogeneity, but allows for both unobservables to affect both conviction and incarceration.

III.C.3 Unordered multinomial choice

We now consider an unordered multinomial choice model, where outcomes can be thought of as being determined by judges maximizing over their “returns”:²¹

$$\begin{aligned} R_c &\equiv V_c - \pi_c(Z_c, Z_i) \\ R_i &\equiv V_i - \pi_i(Z_c, Z_i) \\ R_d &\equiv 0. \end{aligned} \tag{7}$$

The outcome of the case depends on the judge’s threshold for noncarceral conviction ($\pi_c(Z_c, Z_i)$), the judge’s threshold for incarceration ($\pi_i(Z_c, Z_i)$), and two unobserved case-specific characteristics (V_c and V_i). Thus, this model assumes that case outcomes are determined by a joint consideration across the two unobserved dimensions, which may better capture the intertwined decisions that are common in Virginia and other US jurisdictions due to plea bargaining. In a plea deal, a defendant typically agrees to plead guilty in exchange for a lower sentence, making conviction and sentencing determinations closely connected; unobserved determinants of the sentencing decision may affect the decision to plead guilty.

Using the unordered multinomial choice model above, we can write the three treatment

²¹See, e.g., Heckman, Urzua, and Vytlačil (2006) for a discussion of treatment effects in an unordered multinomial choice model and Mountjoy (2022) for an application in the context of college choice.

indicators as:

$$\begin{aligned} T_d &= \mathbb{1}\{V_c < \pi_c(Z_c, Z_i), V_i < \pi_i(Z_c, Z_i)\} \\ T_c &= \mathbb{1}\{V_c \geq \pi_c(Z_c, Z_i), V_c - V_i \geq \pi_c(Z_c, Z_i) - \pi_i(Z_c, Z_i)\} \\ T_i &= \mathbb{1}\{V_i \geq \pi_i(Z_c, Z_i), V_i - V_c \geq \pi_i(Z_c, Z_i) - \pi_c(Z_c, Z_i)\}. \end{aligned} \tag{8}$$

The propensity of a judge to convict depends on both π_i and π_c , neither of which is directly observed. Panel (a) of Figure III visualizes the court outcomes and how they depend on judge thresholds and the two unobservables.

Under this model, the instruments are not treatment-specific. Consider panel (b) of Figure III, which shows how treatment assignment changes when holding Z_i fixed and increasing Z_c . In this case, individuals shift from incarcerated to convicted and from dismissed to convicted but, to hold the probability of incarceration (Z_i) constant, individuals also need to shift from dismissed to incarcerated. This flow from dismissal to incarceration violates UPM and demonstrates that instruments neither move individuals into a single treatment nor across a single margin. Similar conclusions are drawn when holding Z_c (or Z_d) fixed and varying Z_i . Hence, under this model, 2SLS with stringency instruments does not recover margin-specific or treatment-specific treatment effects without further assumptions.

These observations illustrate how judge stringency instruments with multiple treatments differ from those in Mountjoy (2022), which also considers an unordered choice model. The difference stems from the fact that stringency instruments are generally not treatment-specific, while the distance instruments in Mountjoy (2022) plausibly are. The judge stringency for conviction does not correspond to π_c ; it corresponds to the fraction of court cases in the conviction section of the graph. By contrast, the distance instrument in Mountjoy (2022) directly shifts π_c , holding π_i constant. In our setting, this would result in flows into conviction from the other two treatments and no flows between incarceration and dismissal, as shown in panel (c) of Figure III. Given that we do not observe π_c or π_i , we can only shift or condition on Z_c and Z_i , resulting in variation that violates UPM and does not solely shift people into or out of a particular treatment.

III.D Asymptotic bias under different monotonicity assumptions

The prior subsection showed how UPM rules out some reasonable models of judge behavior, while the weaker CPM condition is not sufficient for 2SLS to recover margin-specific or treatment-specific effects. Here we derive the Wald estimand under CPM, which is satisfied by all three models. As in the prior section, we will consider the impacts of conviction vs dismissal and study the case where Z_c takes two values and Z_i is fixed. Analogous results for the incarceration-conviction margin can be obtained by rearranging subscripts.

Consider increasing conviction stringency from z_c to z'_c while holding incarceration stringency fixed at z_i . Let $\omega_{i \rightarrow c}$ represent the proportion of cases switching from incarceration to conviction in response to the instrument shift. Similarly, allow $\omega_{d \rightarrow c}$ and $\omega_{c \rightarrow i}$ to represent

the proportions of cases responding by switching across the other margins. Next, let $\Delta_{i \rightarrow c}^{Y_c - Y_i}$ represent the local average of $T = c$ vs $T = i$ treatment effect for $i \rightarrow c$ compliers when the instrument shifts from z_c to z'_c , holding Z_i fixed. More generally, $\Delta_{k \rightarrow l}^{Y_m - Y_n}$ denotes the treatment effect of $T = m$ vs $T = n$ for $k \rightarrow l$ compliers.²²

Proposition 1. *Under A1–A4 and CPM, the Wald estimand of increasing conviction stringency Z_c from z_c to z'_c , while holding incarceration stringency fixed at $Z_i = z_i$, is given by*

$$\begin{aligned} \frac{E[Y(T(z'_c, z_i)) - Y(T(z_c, z_i))]}{E[T_c(z'_c, z_i) - T_c(z_c, z_i)]} = \\ \underbrace{\frac{\omega_{d \rightarrow c} \Delta_{d \rightarrow c}^{Y_c - Y_d} + \omega_{i \rightarrow c} \Delta_{d \rightarrow i}^{Y_c - Y_d}}{\omega_{d \rightarrow c} + \omega_{i \rightarrow c}}}_{\text{Positively-weighted avg. of } Y_c - Y_d \text{ treatment effects}} + \underbrace{\frac{\omega_{i \rightarrow c}}{\omega_{d \rightarrow c} + \omega_{i \rightarrow c}} [\Delta_{d \rightarrow i}^{Y_i - Y_c} - \Delta_{i \rightarrow c}^{Y_i - Y_c}]}_{\text{Bias term}}. \end{aligned} \quad (9)$$

Proof: See Appendix C.1.

Proposition 1 states that the Wald estimand can be decomposed into two terms. The first term is a weighted average of two LATEs for noncarceral conviction vs dismissal, corresponding to two different groups of compliers. The second term represents asymptotic bias relative to this weighted average. The bias term is the difference between the LATE for incarceration vs conviction for two equally-sized groups of compliers, weighted by the share of compliers moving from incarceration to noncarceral conviction. A direct consequence of Proposition 1 is that, when we replace the CPM assumption with the UPM assumption, the bias term in equation (9) is eliminated.

Corollary 1. *Under A1–A4 and UPM, the Wald estimand of increasing conviction stringency Z_c from z_c to z'_c , while holding incarceration stringency fixed at $Z_i = z_i$, is given by*

$$\begin{aligned} \frac{E[Y(T(z'_c, z_i)) - Y(T(z_c, z_i))]}{E[T_c(z'_c, z_i) - T_c(z_c, z_i)]} &= E[Y(T(z'_c, z_i)) - Y(T(z_c, z_i)) \mid T_c(z'_c, z_i) = T_d(z_c, z_i) = 1] \\ &= \Delta_{d \rightarrow c}^{Y_c - Y_d}. \end{aligned} \quad (10)$$

This corollary stems from the fact that the bias term is zero if $\omega_{i \rightarrow c}$ equals zero, i.e. no compliers shift from incarceration to conviction. As discussed previously, UPM combined with judge stringency instruments ensures that this condition is met. Thus, under UPM, the Wald estimand will be $\Delta_{d \rightarrow c}^{Y_c - Y_d}$, which is the LATE for noncarceral conviction vs dismissal for those shifted across that margin by the instrument.

Proposition 1 and Corollary 1 allow us to reason about conditions under which asymptotic bias will be quantitatively important for our 2SLS estimands. Under A1–A4 and UPM, the 2SLS specification in equations (3)–(4) yields a positively-weighted sum of unbiased

²²For simplicity, we suppress notation indicating instrument values; for example, we write $\omega_{d \rightarrow c}$ rather than $\omega_{d \rightarrow c}(z'_c, z_c \mid z_i)$ and $\Delta_{j \rightarrow k}^{Y_m - Y_n}$ rather than $\Delta(z'_c, z_c \mid z_i)_{j \rightarrow k}^{Y_m - Y_n}$.

Wald estimands.²³ If CPM holds but UPM does not, then the 2SLS estimands will represent a positively-weighted sum of the *biased* Wald estimands from equation (9) unless we impose additional assumptions. One possibility is to restrict treatment effect heterogeneity.

Treatment effect homogeneity assumptions under which the bias term is zero. The bias term will be zero if $\Delta_{d \rightarrow i}^{Y_i - Y_c} - \Delta_{i \rightarrow c}^{Y_i - Y_c} = 0$. Thus, if the average treatment effects of incarceration vs conviction are the same for the $d \rightarrow i$ compliers and $i \rightarrow c$ compliers, the bias will be zero. For this result, we do not need the stronger assumption that treatment effects are homogeneous across all cases. Nor do we need to assume treatment effect homogeneity across the conviction-dismissal margin.²⁴ A special case occurs when the impact of incarceration vs conviction is zero for these two groups. This case is of specific interest in our context, because prior studies find null effects across this margin after the incapacitation period (see, e.g., Loeffler and Nagin, 2022; Garin et al., 2024). We return to this point in Section IV.E.2.

Reasoning about sign and magnitude of the bias. Equation (9) also allows us to reason about the likely sign and magnitude of the bias when we are unwilling to make the homogeneity assumptions discussed above. We know that the bias term is less than and proportional to $\Delta_{d \rightarrow i}^{Y_i - Y_c} - \Delta_{i \rightarrow c}^{Y_i - Y_c}$, i.e., the difference in the impact of incarceration (relative to noncarceral conviction) between $d \rightarrow i$ compliers and $i \rightarrow c$ compliers. Thus the sign and the magnitude of the bias depend on the differential impact across these two groups. We illustrate how it is possible to reason about this differential impact in our context of criminal court cases in Section IV.E.

IV CONVICTION, INCARCERATION, AND RECIDIVISM: 2SLS ESTIMATES

IV.A Regression specifications for estimation

Using leave-one-out estimates of judge stringency as our instruments, we consider the following 2SLS regression model, which is common in the literature (stated here for noncarceral conviction; the specification for incarceration is analogous):

$$T_c = \delta_0 + \delta_1 Z_c + \delta_2 Z_i + \delta_3' X + \epsilon \quad (11)$$

$$Y = \gamma_0 + \gamma_1 T_c + \gamma_2 Z_i + \gamma_3' X + \nu, \quad (12)$$

²³Note that assumptions A1–A5 imply the assumptions needed in Blandhol et al. (2022) for 2SLS to recover causal estimands. In particular, A5 implies their “Ordered strong monotonicity” (OSM). See Appendix C.3 for details, and see Appendix C.5 for how to interpret the 2SLS estimand when additional covariates are included in the 2SLS regression.

²⁴Also note that homogeneous treatment effects rule out selection on gains, but still allow for selection on level (e.g., individuals more prone to recidivism can be more likely to be incarcerated).

where Y is one of the measures of recidivism described in Section [II.C](#). The vector X includes court-by-year, court-by-month, and day-of-the-week fixed effects, as well as controls for offense type, race, gender, and a flag for prior felony convictions. For our main measure of judge stringency, we use the three-year leave-one-out conviction and incarceration rates for the judge handling the case.²⁵ We run these 2SLS regressions on the sample described in Section [II.C](#).²⁶

In Appendix [D](#), we discuss how assumptions A1–A3 are supported by features of the institutional environment and provide results from empirical tests of these assumptions. For both the conviction and incarceration regressions, we have a strong first stage with F-statistics of 165 and 288 (Table [II](#)), suggesting that relevance holds in our setting. Panels A and B of Figure [IV](#) plot the variation in residualized judge conviction and incarceration stringency, showing that there is substantial variation in each. Panel C of Figure [IV](#) provides a scatter plot of residualized conviction and incarceration stringency and shows that there is also substantial variation in Z_c conditional on Z_i , and vice versa. For balance, Table [III](#) shows that, while case characteristics are strong predictors of conviction and incarceration, they do not predict judge stringencies. For the few covariates with statistically significant loadings, the predicted difference in stringency tends to be very small (0.016 to 0.036 standard deviations of the residualized stringency measure, see Appendix Table [D.1](#)). In addition, Appendix Tables [D.2](#) and [D.3](#) show that our main results are broadly similar when systematically dropping certain case types, such as assault. For the exclusion restriction, we discuss potential violations and provide tests suggesting that these would not have qualitative impacts on our results. For instance, we show in Figures [E.3–E.6](#) that estimates remain largely unchanged when including sentence-length stringencies as additional controls. Finally, we provide a test of the “no defiers” assumption that is part of both CPM and UPM, with Table [D.5](#) reporting split-sample monotonicity tests and finding the same sign for the first stage across various splits of the data. We postpone the discussion and implementation of an additional test of the UPM assumption to Section [IV.E](#).

IV.B Noncarceral conviction

Table [IV](#) presents 2SLS estimates of the model in equations [\(11\)–\(12\)](#). We consider three measures of future criminal justice contact: new felony charges in Circuit Court, a new conviction resulting from felony Circuit Court charges, or a new carceral sentence resulting from felony Circuit Court charges. We use various time windows to measure recidivism, all measured from the time of disposition: year 1, years 2–4, years 5–7, and cumulatively for the

²⁵We choose a three-year window to ensure that the stringency measures are computed based on an adequate number of cases per judge, without requiring that judges behave identically for their entire tenure. We exclude cases assigned to judges who see fewer than 100 cases in the three-year period.

²⁶As discussed in Section [III](#), under A1–A5, these regression estimates can be interpreted as causal and margin-specific. See Appendix [C.5](#) for additional discussion of what 2SLS identifies when including controls based on [Blandhol et al. \(2022\)](#), and details on the assumption of sufficiently rich controls.

first 7 years. For each of these outcomes, we present OLS and 2SLS regressions.²⁷

As discussed in Section II.B, noncarceral conviction (instead of a dismissal) could increase or decrease recidivism through multiple channels, and the sign of the net effect is not clear a priori. If given a causal and margin-specific interpretation, our 2SLS estimates suggest that noncarceral conviction increases future criminal justice contact relative to dismissal. The estimates for future charges within the first year after conviction are large: around 10.5 percentage points (95% CI, 0.02 to 0.20), which is a 67% increase relative to the control complier mean. The impact on cumulative recidivism 1–7 years later is also statistically significant, with an estimate of 24 percentage points (95% CI, 0.05 to 0.43), a 48% increase relative to the control complier mean. The effects for years 1–7 are approximately twice as large as the effects in year 1, with positive but statistically insignificant effects in years 2–4 and 5–7. The results are similar for the other measures of recidivism we consider.

These point estimates are similar in magnitude to estimates found in the related literature. For instance, [Mueller-Smith and Schnepel \(2021\)](#) find that diversion cuts reoffending rates in half, and [Agan, Doleac, and Harvey \(2023\)](#) find that nonprosecution reduces the likelihood of a new criminal complaint by 53%. [Mueller-Smith, Pyle, and Walker \(2023\)](#) find that adult conviction increases the total number of future felony charges by roughly 75%. While our point estimates could be considered fairly large, the confidence intervals leave room for a wide range of values, as is typical for judge IV research designs.

Our 2SLS estimates are similarly signed but substantially larger than the OLS estimates. However, the OLS estimates likely suffer from omitted variable bias. One important omitted variable is the strength of the evidence, which often consists primarily of witness testimony. [Graef et al. \(2023\)](#) show that witness appearance in court is by far the most predictive factor in whether the defendant will be convicted. Thus, the sign of the bias in the OLS estimates depends in part on the relationship between witness appearance and the defendant’s risk of recidivism. These could be positively correlated if, e.g., witnesses are more invested in securing punishment for high-recidivism defendants. Or they could be negatively correlated if, e.g., witnesses are scared of testifying against high-recidivism defendants. The fact that victims and bystander witnesses often come from the same socioeconomic groups as defendants also suggests a negative correlation. The same factors that give someone a high-recidivism potential—for example, poverty or social marginalization—may also make it harder for the witnesses to take time off work for a court date, or make them less willing to cooperate with a system they distrust. If so, OLS estimates will be downward biased.²⁸ Alternatively, IV compliers may be more impacted by conviction than the average defendant. In Appendix

²⁷Appendix Table E.1 presents reduced-form estimates. The OLS estimate is from a regression of recidivism on a conviction indicator that is one if the individual is convicted or convicted and incarcerated, and controls for an incarceration indicator.

²⁸Witness cooperation is just one potential omitted variable among many that could bias the OLS estimates. For example, if people with skilled lawyers are both less likely to be convicted and to recidivate, OLS would be upward biased; conversely, if those with untreated substance issues are less likely to be convicted but more likely to recidivate, OLS would be downward biased.

Table E.2, we show that the racial composition of the complier group is similar to the overall sample, but that on average this group is less likely to be in court for violent offenses and is less likely to have a prior conviction. Our OLS estimates for noncarceral conviction are somewhat larger when reweighting with complier weights, while the estimates for incarceration do not notably change (see Appendix Table E.3).

We next consider several mechanism. Noncarceral conviction (relative to dismissal) could affect recidivism due to fines and probation conditions. However, a small but growing literature shows that court fines and fees do not affect recidivism (Pager et al., 2022; Finlay et al., 2024; Lieberman, Luh, and Mueller-Smith, 2023). Similarly, several large-scale RCTs have shown that probation and parole conditions do not affect recidivism (Doleac, 2023). We therefore focus on asking whether our results are coming from an increase in criminal behavior or an escalation in subsequent responses by the criminal justice system (“ratcheting up”)—mechanisms we discussed in Section II.B. While we cannot answer this question definitively, we conduct two tests to provide suggestive evidence.

First, if conviction makes it harder to find employment due to the mark of a felony record, we might expect to see a more pronounced increase in income-generating crime. We test for this in Appendix Table E.4 and find similar point estimates across income-generating and non-income-generating crime; the confidence intervals are too large to draw a firm conclusion.²⁹ Second, if the ratcheting-up effect is operative, conviction may have a larger effect on the more downstream measures of future criminal justice contact, such as future conviction or incarceration. The logic here is that if a felony conviction increases the likelihood of a negative outcome at each discretionary stage, the negative impact of a conviction will accumulate. Downstream outcomes, like incarceration, will be impacted more than upstream outcomes, like the charging decision. Comparing the three measures of recidivism in Table IV, the point estimates are larger relative to the control complier means for outcomes with more discretionary decisions, providing suggestive evidence that the ratcheting up mechanism is present.

While we cannot conclusively say whether increased recidivism is driven primarily by increased criminal behavior or a ratcheting-up effect, both mechanisms imply that felony conviction contributes to the revolving door of criminal justice, increasing not just future charges and convictions, but also future incarceration.

IV.C Incarceration

Table V presents 2SLS estimates of the model analogous to those in equations (11)–(12), but instrumenting for incarceration with incarceration stringency and controlling for dismissal stringency.

When given a causal and margin-specific interpretation, our 2SLS estimates suggest a 10 percentage point reduction in future charges in the first year (95% CI, -0.15 to -0.04).

²⁹Likewise, there are no consistent differential patterns for drug vs. non-drug crimes, as shown in Appendix Table E.5.

This reduction is likely due, at least partially, to incapacitation. We find no evidence that incarceration affects future criminal justice interactions beyond the first year. The 2–4 year and 5–7 year estimates are small and statistically insignificant. The cumulative estimate across all seven years implies a seven percentage point reduction in new felony charges (95% CI, -0.19 to 0.05). We can reject increases in recidivism larger than 2.6 percentage points at the .05 level. Results are similar for future convictions and future incarceration.

Our qualitative conclusions regarding incarceration effects are further strengthened by the fact that we find similar results using another research design within the same institutional setting. We leverage the fact that judges’ sentencing decisions are influenced by sentence guidelines. The guidelines-recommended sentence is calculated using a scoring system in which various characteristics of the offense and criminal record are assigned points which are then summed to create the sentence guidelines score. Exploiting two different discontinuities in the sentence guidelines recommendations within a regression discontinuity design framework, we estimate the effects of incarceration on the intensive margin (sentence length) and on the extensive margin (short jail sentences vs probation). As when exploiting quasi-random assignment of cases to judges, we find that incarceration leads to short-term decreases in criminal justice contact. We find no evidence of longer-term impacts of exposure to incarceration. We refer the reader to Appendix G for details on our empirical approach and findings.

Our analysis has some limitations. First, incarceration may affect outcomes among subgroups that we are underpowered to detect (Aizer and Doyle, 2015; Mueller-Smith, 2015; Jordan, Karger, and Neal, 2023). Second, our empirical setting does not allow us to isolate the effects of long carceral sentences (e.g., five or ten years) vs noncarceral conviction. A higher “dosage” of incarceration may have more impact. Third, some people with noncarceral convictions could have been incarcerated prior to trial and thus may have already experienced some incarceration, reducing the difference in carceral exposure between those incarcerated post-trial and those receiving noncarceral convictions.

In a similar vein, some people who receive noncarceral conviction become incarcerated in the future, either because of new criminal convictions or because of probation violations. The difference in carceral exposure between these two groups thus becomes smaller over time. However, our evidence suggests that there remains a substantial difference in exposure to incarceration across these two groups. Appendix Figure E.2 shows how much “incarceration catch-up” occurs for those who receive noncarceral sentences compared to those who receive carceral sentences, both for new crimes and for technical violations resulting in probation revocation. These results suggest that, while there is some catch-up, more than 50% of those receiving a noncarceral sentence are not incarcerated over the next seven years.

Overall, the results from Sections IV.B and IV.C imply that incarceration’s influence on the revolving door is limited, while noncarceral conviction may hold greater importance. Our findings on the effects of incarceration align with the recent literature review by Loeffler and Nagin (2022). Most of the studies that find incarceration to be criminogenic are looking at

pretrial detention, rather than post-sentencing incarceration. Since pretrial detention also increases the probability of conviction (Gupta, Hansman, and Frenchman, 2016; Leslie and Pope, 2017; Stevenson, 2018; Dobbie, Goldin, and Yang, 2018), these studies are effectively estimating the joint effect of conviction and incarceration. In contrast, most studies evaluating the impact of post-conviction incarceration do not find evidence of effects lasting beyond the incapacitation period. In line with our findings, most studies find no evidence that it is an important contributor to the revolving door.

IV.D Robustness and subgroup analyses

In this subsection, we provide a brief overview of robustness checks that we discuss in more detail in Appendix E.1. The results from the previous section are robust to the choice of sample restrictions and controls, as shown in Appendix Figures E.3–E.6. In particular, the results are similar when we drop specific crime types, for example drug cases, for which diversion is more likely to happen than for other offenses. Appendix Figures E.3–E.6 also show that the 2SLS estimates and standard errors remain similar when we more flexibly control for non-focal stringency.³⁰ Appendix Table E.9 shows that the results are robust to varying the definition of recidivism, and considering counts of new offenses and charges. Appendix E.4 shows that the results are robust to correcting for measurement error in stringency using Empirical Bayes methods. Additionally, Appendix Figure E.7 demonstrates no differential mobility out of Virginia based on incarceration outcomes.³¹

To examine treatment effect heterogeneity, we first break out the results based on whether a person has a prior felony conviction (Appendix Table E.6), since avoiding a first felony conviction might play an especially pivotal role. We find that people without a recent felony conviction have large and sustained increases in recidivism as a result of a felony conviction. Yet, we cannot reject that these estimates are equal to estimates for those with a recent felony conviction, for whom estimates are imprecise—likely because they make up only 20% of the sample. Sample size limitations again preclude conclusive inference about heterogeneity in the impacts of incarceration across those with and without a recent felony, although point estimates are similar for the two groups.³²

We additionally explore heterogeneity across race and zip code income level. We provide more details in Appendix E.1. We find qualitatively similar patterns across Black and non-Black defendants. We find suggestive evidence that the impacts of noncarceral conviction are larger for people living in zip codes with above-median poverty rates. Felony convictions might have greater consequences for poorer individuals, perhaps because convictions block access to housing, employment, or public assistance.

³⁰See Table C.2, which provides further robustness to the choice of controls.

³¹We are unable to study differential mobility out of Virginia due to conviction, as less information about defendants is collected for cases ending in dismissal, prohibiting linkage to data on out-of-state moves.

³²We define our prior felony indicator as a prior felony within the last five years. Unlike Jordan, Karger, and Neal (2023), who can isolate first felony convictions using age restrictions, our data does not include age.

IV.E Testing for and characterizing bias in the 2SLS results

In Section III.D, we showed that the 2SLS estimates may be asymptotically biased if the UPM assumption fails. In this subsection, we describe and implement an empirical test for this assumption. We then use theory and external evidence to discuss the likely magnitude and direction of the bias in our setting.

IV.E.1 Testing the UPM assumption

The UPM assumption has testable implications. If instrumental variation is only causing flows between two treatments, there should be no movement in or out of the third treatment. In our setting, this implies:

- (1) Under $\text{UPM}(Z_c | Z_i)$, the observable characteristics of those with $T = i$ should not change when holding Z_i constant and varying Z_c .
- (2) Under $\text{UPM}(Z_i | Z_d)$, the observable characteristics of those with $T = d$ should not change when holding Z_d constant and varying Z_i .

To build intuition for the first implication, consider those incarcerated in the ordered model from Section III.C. When holding incarceration stringency fixed, varying conviction stringency will move people between dismissal and conviction, but will not move people into or out of incarceration. If the instruments are treatment-specific, the observed characteristics of incarcerated individuals should not change. If the characteristics of incarcerated individuals do change, then there must be flows into and out of incarceration, which implies that the instrument is moving people across more than one margin. More generally, this would imply that $\text{UPM}(Z_c | Z_i)$ is violated, as the UPM assumption plus stringency instruments (and A1–A4) ensures compliers move across only one margin. A similar argument holds for the second testable implication.

These conditions allow us to adjudicate between models of judge decision-making introduced in Section III.C. In particular, (1) and (2) must hold for the ordered model, and (2) must hold for the sequential model.

We implement our test using predicted recidivism: an index constructed by regressing recidivism on individual and case characteristics.³³ We test implication (1) by regressing predicted recidivism on the noncarceral conviction instrument, restricting the sample to those incarcerated and controlling for the incarceration instrument and court-by-time fixed effects. Similarly, we test implication (2) by regressing predicted recidivism on the incarceration instrument, restricting to the dismissed sample and controlling for the dismissal instrument and court-by-time fixed effects. Table VI reports the results, where Panel A presents tests

³³Predicted recidivism variables are created by regressing recidivism post-release if incarcerated, or post-conviction/dismissal otherwise, on offense type, socio-demographic controls, and month, court, and day-of-the-week fixed effects. Using these regressions, we construct measures of predicted recidivism within one year, two to four years, five to seven years, and within seven years after case disposition.

for (1) and Panel B tests for (2).³⁴ Appendix Table E.10 shows results for both tests using specific defendant characteristics (criminal record, offense type, and demographics) instead of predicted recidivism.

Using the predicted recidivism index, we reject $UPM(Z_c | Z_i)$ and $UPM(Z_i | Z_d)$, which means we reject both the ordered and sequential models. For (1), we find that predicted recidivism for the incarcerated group increases with the judge’s conviction propensity, holding incarceration propensity constant. For (2) we find that the predicted recidivism for the dismissed group decreases with the judge’s incarceration propensity, holding fixed the dismissal propensity. These results suggest the UPM assumption does not hold in our setting, and so our 2SLS estimates are potentially asymptotically biased.

IV.E.2 Sign and magnitude of asymptotic bias

Proposition 1 states that when UPM does not hold (but A1–A4 and CPM do), 2SLS estimands will be positively-weighted averages of the Wald estimands in equation (9). In this section, we demonstrate how the expression in equation (9) can be combined with theory and external evidence to reason about the direction and magnitude of bias in 2SLS estimands. We consider each margin of interest separately. Throughout this discussion, we will assume that CPM holds, as it does in each of the three judge decision-making models we considered. We also assume A1–A4 from Section III hold.

Noncarceral conviction vs dismissal. For simplicity, we discuss the bias term in the context of the special case where two judges have the same incarceration rate but differing rates of noncarceral conviction. In this case, equation (9) shows that the bias term in the Wald estimand is less than but proportional to $\Delta_{d \rightarrow i}^{Y_i - Y_c} - \Delta_{i \rightarrow c}^{Y_i - Y_c}$, which is the difference in the impact of incarceration vs conviction between those near the incarceration-dismissal margin and those near the incarceration-conviction margin. This allows us to reason about the bias’s sign and magnitude based on how incarceration may affect recidivism differently for these groups, compared to conviction. We separately consider the long- and short-run effects—with “long-run” referring to the post-incapacitation period.

Table VI shows that the average predicted recidivism rate among the incarcerated group increases in response to increasing Z_c while controlling for Z_i . This pattern suggests that individuals shifting from dismissal to incarceration exhibit higher predicted recidivism rates than those shifting from incarceration to conviction. This finding aligns with reasonable expectations about case characteristics close to the different decision margins. Cases near the incarceration-dismissal margin likely involve defendants with serious charges and/or extensive criminal histories (“high severity” cases), who would be incarcerated if sufficient evidence existed for conviction. In contrast, cases at the incarceration-conviction margin typically

³⁴When implementing this test, we are maintaining other assumptions we make throughout the paper, such as the assumption that judge stringencies do not idiosyncratically depend on defendant characteristics and CPM. Results are similar when including flexible controls for the other stringency measure.

involve lower severity offenses but with stronger evidentiary support. Since criminal history is among the strongest predictors of recidivism, these high-severity cases at the incarceration-dismissal margin would likely present higher recidivism risk, consistent with our empirical bias test results. If, in the short run, incarceration affects recidivism primarily through incapacitation, then shifting prison beds towards people at a higher risk of recidivism will reduce recidivism ($\Delta_{d \rightarrow i}^{Y_i - Y_c} < \Delta_{i \rightarrow c}^{Y_i - Y_c}$). In this case, the bias term in equation (9) is negative.

However, the magnitude of the composition change shown in Table VI is relatively small: a ten percentage point increase in noncarceral conviction stringency increases one-year predicted recidivism among the incarcerated group by 0.1 percentage points. This suggests that either the proportion of $i \rightarrow c$ compliers is small, or the two groups have similar observable characteristics and therefore potentially similar treatment effects. Both would imply that the magnitude of the bias is small.

Turning to the long run (i.e., the post-incapacitation period), if incarceration *only* has incapacitation effects, then the impact of incarceration vs conviction is zero after the incapacitation period. In that case, the 2SLS estimates are asymptotically unbiased. Several pieces of evidence indicate that incarceration may not affect recidivism beyond the incapacitation period. In our setting, using an alternative research design, the regression-discontinuity evidence presented in Section IV.C demonstrates that incarceration reduces recidivism only in the short run (for a duration approximately matching the incapacitation period) among individuals on the margin of conviction and incarceration. This finding aligns with broader U.S. evidence, where most studies conclude that the impact of post-conviction incarceration on recidivism is negligible (Loeffler and Nagin, 2022; Garin et al., 2024).

While this evidence suggests limited effects beyond incapacitation, incarceration could theoretically influence recidivism through other channels. For instance, Jordan, Karger, and Neal (2023) finds that prison may serve as a stronger post-release deterrent for individuals with fewer prior convictions. These individuals, who likely have a lower propensity to recidivate, are disproportionately represented near the incarceration-conviction margin compared to the incarceration-dismissal margin.³⁵ In such cases, $\Delta_{d \rightarrow i}^{Y_i - Y_c} > \Delta_{i \rightarrow c}^{Y_i - Y_c}$, potentially introducing positive bias. However, two key findings in our setting suggest this upward bias is unlikely. First, our analysis reveals no evidence of heterogeneous treatment effects of incarceration by prior conviction status (Panel B of Appendix Table E.6), although these estimates lack precision. Second, as established earlier, the preponderance of empirical evidence (including evidence from our own empirical context) indicates that the post-incapacitation effects of incarceration vs conviction on recidivism are negligible.

Overall, the arguments above suggest that a violation of UPM would likely result in a modest negative bias in our 2SLS estimates of noncarceral conviction effects in the short run, with negligible bias in the long run. Hence, it is unlikely that our qualitative conclusions about the impact of noncarceral conviction vs dismissal would be overturned as a result of a

³⁵Our implementation of test (1) using prior convictions instead of predicted recidivism reveals that $c \rightarrow i$ compliers have a lower prior conviction rate than $d \rightarrow i$ compliers (Appendix Table E.10).

violation of the UPM assumption.

Incarceration vs noncarceral conviction. As before, we discuss bias in the context of the simple case where two judges have the same noncarceral conviction rate but differing rates of incarceration. A derivation similar to the proof of Proposition 1 shows that the bias term for the impact of incarceration will be smaller than but proportional to $\Delta_{d \rightarrow i}^{Y_c - Y_d} - \Delta_{c \rightarrow d}^{Y_c - Y_d}$, and zero in the absence of $d \rightarrow i$ compliers. As evidenced by the results of our empirical test in Section IV.E.1, cases near the incarceration-dismissal margin have higher predicted recidivism (they are “high-severity” cases) than those on the conviction-dismissal margin.

Thus, to evaluate the bias for our estimates of the impact of incarceration vs conviction, we need to know whether a felony conviction (vs dismissal) will affect recidivism more for high-severity cases than for low-severity cases. One possibility is that a felony conviction increases recidivism more for low-severity cases, which seems reasonable because low-severity cases are less likely to have a prior felony on their criminal record and the first felony conviction is likely to have greater marginal impact than subsequent ones. If this conjecture is true, the bias term would be negatively signed. However, we find no discernible difference in the impact of conviction vs dismissal across crime types or priors (Appendix Tables E.4 - E.6). In addition, the compositional changes shown in Table VI and Appendix Table E.10 are relatively small. If the compositional shifts are minimal, then either the proportion of $d \rightarrow i$ compliers is small, or the $c \rightarrow d$ and $d \rightarrow i$ compliers have similar observable characteristics. In the first case, the bias would also be small. In the latter, the bias would be small if similar predicted recidivism implies similar treatment effects.

V AN ALTERNATIVE APPROACH TO IDENTIFICATION AND ESTIMATION OF MARGIN-SPECIFIC TREATMENT EFFECTS

In the prior section, we found that our empirical test rejects the UPM assumption. Although we argued that the bias resulting from a violation of UPM is likely small given the specifics of our setting, this section explores alternative approaches based on assumptions that are not rejected by our test. This exercise serves as a robustness check on our qualitative conclusions and as a proof of concept for other researchers studying settings where examination of the expression in Section III.D raises more substantial concerns about bias.

Below we present a method for identifying margin-specific treatment effects in unordered choice settings. This approach does not require judge stringencies to satisfy UPM, nor does it place restrictions on treatment effect heterogeneity. Rather, we build on the approach in Mountjoy (2022), adopting the underlying assumptions. Furthermore, since this approach requires treatment-specific instruments, we suggest several alternative assumptions that can be used to construct such instruments.

V.A Recovering treatment-specific instruments

Equation (7) sets up the unordered choice model and defines judge-specific thresholds (π_c and π_i), which are treatment-specific instruments if they can be exogenously varied while holding the other threshold fixed. The thresholds are not directly observed, but we observe the shares of cases ending in dismissal, conviction, and incarceration for each judge. We aim to recover π_c and π_i from the observed shares. Our setup has similarities to models in industrial organization where product shares are observed for different markets.³⁶ We therefore leverage identification results from the IO literature and adapt them to our context of judge decision-making. [Berry, Gandhi, and Haile \(2013\)](#) outline assumptions under which thresholds can be inverted from shares, and [Berry and Haile \(2024\)](#) show that judge-specific thresholds can be identified without invoking identification at infinity arguments.³⁷

While these papers show that the thresholds are nonparametrically identified, we make additional parametric assumptions for tractability in estimation and show that the results are broadly similar under a set of parametric assumptions.³⁸ Our main specification assumes the unobserved shocks are each the sum of a shock with a standard logistic distribution and a random effect with a correlated multivariate normal distribution (η and ε in the equation below). We can then write the returns in the unordered choice model as

$$\begin{aligned} R_{ncj} &= \beta_c - \pi_c^j + \gamma_c' X_n + \eta_{nc} + \varepsilon_{nc} \\ R_{nij} &= \beta_i - \pi_i^j + \gamma_i' X_n + \eta_{ni} + \varepsilon_{ni}, \end{aligned}$$

where n represents the case, c and i indicate conviction or incarceration, j the judge, X_n are characteristics about the defendant or case, and R_{ncj} and R_{nij} represent the returns to a specific outcome for a specific case assigned to judge j . We assume $f(\varepsilon_{nc}, \varepsilon_{ni})$ has a standard logistic distribution and $g(\eta_{nc}, \eta_{ni}) \sim N(0, \Sigma)$.³⁹

We estimate the model above by judicial circuit and three-year bin, which allows the model parameters to differ across circuits and over time. We then use the estimated model parameters to recover estimates of π_j^c and π_j^i .

³⁶Unlike many applications in the industrial organization literature, our setting features quasi-random assignment of cases to judges, implying that the judge thresholds ($\pi_c(Z_c, Z_i)$ and $\pi_i(Z_c, Z_i)$) are independent of the characteristics of the case (V_c and V_i).

³⁷Applying [Berry and Haile \(2024\)](#)'s identification argument to our setting requires the existence of three continuous covariates whose loadings do not vary across judges. See Appendix F.1 for details. [Kamat, Norris, and Pecenco \(2024\)](#) provide a partial identification strategy that does not require such conditions on covariates, but uses a sequential model combined with a "latent monotonicity" assumption, and recovers bounds rather than point estimates.

³⁸Making additional parametric assumptions for estimation is common and often necessary in this literature. See [Berry and Haile \(2024\)](#) for a detailed discussion.

³⁹We also consider two alternative specifications that are less flexible but easier to implement: (1) no random effect and the unobserved shocks follow standard logistic distributions and (2) Σ is a diagonal matrix.

V.B Recovering margin-specific effects

We refer to the newly constructed treatment-specific instruments—the estimated judge-specific thresholds—as \tilde{Z}_c and \tilde{Z}_i , to distinguish them from the stringency instruments Z_c and Z_i .

Even with treatment-specific instruments, 2SLS estimands are difficult to interpret, as they are weighted averages of treatment effects that correspond to different margins, as visualized in Panel (c) of Figure III. In our context, shifting from \tilde{z}_c to \tilde{z}'_c while holding \tilde{Z}_i fixed would yield a weighted average of the LATE for $i \rightarrow c$ compliers and the LATE for $d \rightarrow c$ compliers. The central objective of Mountjoy (2022) is to decompose the 2SLS estimand, obtained using a treatment-specific instrument, into two treatment-specific effects.

We closely follow Mountjoy (2022) in estimating the impacts on the two margins discussed above. This method relies on assumptions A1–A4, defined for \tilde{z}_c and \tilde{z}_i , plus one additional assumption: “comparable compliers.” This assumption requires that the $i \rightarrow c$ compliers from decreasing \tilde{z}_i have the same potential outcome when convicted as $i \rightarrow c$ compliers from increasing \tilde{z}_c at their limits (see Appendix F for a formal definition). Under this set of assumptions, Mountjoy (2022) shows how to identify and estimate $E[Y(c) - Y(d) \mid d \rightarrow c \text{ complier w.r.t } (\tilde{z}_c, \tilde{z}_i) \rightarrow (\tilde{z}'_c, \tilde{z}_i)]$ and $E[Y(i) - Y(c) \mid i \rightarrow c \text{ complier w.r.t } (\tilde{z}_c, \tilde{z}_i) \rightarrow (\tilde{z}'_c, \tilde{z}_i)]$. We follow Mountjoy (2022) in our approach and provide additional details in Appendix F.

While we do not invoke the UPM assumption in this section, we introduce additional assumptions in both the construction of treatment-specific instruments and in estimating treatment-specific effects.⁴⁰ The assumptions we consider in this section are not necessarily weaker or stronger than those supporting a causal interpretation of the 2SLS estimates.

V.C Results

Panel A of Table VII reports estimates for the noncarceral conviction vs dismissal margin. The point estimates are similar to the 2SLS estimates reported in Section IV, with somewhat smaller estimates for year 1 and somewhat larger estimates for later years. For example, the 2SLS estimate for a future felony charge within the first seven years is 0.24 (95% CI: 0.05–0.43), while the estimate from this alternative approach is 0.27 (95% CI: 0.10, 0.47). For ten of the 12 estimates, the 2SLS estimates fall within the confidence interval of the new estimates. Panel B reports estimates for the incarceration vs noncarceral conviction margin. Again, these results are similar to the 2SLS estimates, with somewhat smaller estimates in year 1.⁴¹

Overall, the 2SLS estimates and the estimates based on this alternative approach tell a similar story: noncarceral conviction increases future criminal justice contact in the long run, while for incarceration the evidence only supports short-term incapacitation effects.

⁴⁰For identification, we assume the unordered model, “comparable compliers,” and the existence of additive covariates whose loadings do not vary across judges. For estimation, we additionally make distributional assumptions about the error terms.

⁴¹We include additional results under the two alternative assumptions outlined in footnote 39 in Appendix Tables F.1 and F.2.

VI CONCLUSION

We study the role of noncarceral conviction in driving future criminal justice contact, and compare it to the role of incarceration. Our analyses consistently demonstrate that noncarceral conviction increases future criminal justice contact (relative to dismissal). In contrast, our analysis of incarceration (relative to noncarceral conviction) only finds evidence for a shorter-term decrease in recidivism, which coincides with the typical period of incapacitation. While our findings support the existence of a “revolving door” effect in the criminal justice system, it primarily operates through the channel of noncarceral conviction rather than through incarceration.

We also discuss methodological challenges stemming from multiple treatment alternatives in the commonly-used random judge research design. We develop an empirical framework for interpreting 2SLS estimands using judge stringency instruments under heterogeneous treatment effects. We provide assumptions that allow the estimands to be interpreted as causal and margin-specific. In particular, we show that requiring judge instruments to be treatment-specific is sufficient (in addition to straightforward extensions of exclusion, random assignment, relevance, and rich controls). We characterize models of judge decision-making that are consistent with treatment specificity, and propose an empirical test for this assumption. For cases where treatment specificity fails, we derive an expression for the asymptotic bias, enabling researchers to assess likely direction and magnitude of bias using features of the institutional setting, theoretical arguments, or prior empirical evidence. Finally, we propose and implement an empirical approach that allows us to identify causal and margin-specific treatment effects under an alternative set of assumptions that does not include treatment-specificity.

Several policy approaches could reduce either the frequency of felony convictions or their lasting impact. The number of felony convictions could be reduced by expanding felony diversion programs, decriminalizing certain offenses, or downgrading certain charges to misdemeanors. To diminish the impact of existing felony convictions, policymakers could limit the accessibility or permissible uses of criminal records. For instance, limiting how long criminal records are publicly available could mitigate the employment effects of having a criminal record, potentially reducing recidivism by increasing formal employment options (Cullen, Dobbie, and Hoffman, 2023). Likewise, reducing automatic escalations within the penal system, such as charge upgrades or sentence enhancements for those with a prior felony conviction, could mitigate the impact of a criminal record (Rose, 2021).

Our analysis suggests that such reforms could help address the penal system’s revolving door problem. While other policy considerations remain relevant—including legitimate uses of felony conviction records in hiring decisions and sentencing—the scale of felony convictions in the U.S. demands careful attention to their downstream effects. With an estimated 8% of adults and 33% of Black adult men holding felony conviction records (Shannon et al., 2017), the impact of these convictions on future criminal justice contact should be an important part of policy discussions.

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Table I: Summary statistics

	Dismissed	Convicted	Incarcerated
	(1)	(2)	(3)
<i>Offenses</i>			
Drugs	0.35	0.33	0.29
Larceny	0.17	0.29	0.25
Assault	0.19	0.08	0.18
Fraud	0.09	0.16	0.10
Traffic	0.04	0.05	0.13
Burglary	0.06	0.07	0.08
Robbery	0.05	0.02	0.06
Sexual assault	0.03	0.02	0.03
Kidnapping	0.03	0.01	0.02
Murder	0.01	0.00	0.01
<i>Defendant characteristics</i>			
Black	0.57	0.51	0.60
Female	0.22	0.32	0.16
% of ppl in zip earning <25K	0.46	0.44	0.46
Has misdemeanor	0.06	0.09	0.08
Prior conviction within 5 years	0.14	0.10	0.22
<i>Sentencing</i>			
Incarceration length	0.00	0.00	27.45
Probation length	0.00	31.50	39.34
Median incar. leng.	0	0	12
Median prob. leng.	0	12	12
Percent of sample	16	30	55
Observations	28,589	54,640	100,152

Note: This table shows means and select medians of relevant variables for the data used in the 2SLS analysis split into the three subsamples. The first column shows estimates for those whose cases were dismissed or who were found not guilty. The second column shows estimates for those whose cases ended with a noncarceral conviction. The final column shows estimates for those whose cases ended with incarceration. The incarceration and probation length medians and means are in months. Probation length is top-coded at 20 years. Our primary data source is Virginia Circuit Court Records from 2000-2012.

Table II: Relevance: first stage coefficients for the 2SLS analysis

	Non-carceral conviction			Incarceration		
	(1)	(2)	(3)	(4)	(5)	(6)
Conviction stringency	0.63*** (0.033)	0.60*** (0.032)	0.59*** (0.046)			
Incarceration stringency			-0.011 (0.041)	0.62*** (0.033)	0.59*** (0.032)	0.60*** (0.035)
Dismissal stringency						0.032 (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	339.5	165.3	346.7	350.7	287.8
N	183,381	183,381	183,381	183,381	183,381	183,381

Note: This table reports the coefficient on the instruments from the first stage of the 2SLS regressions. Columns (1)-(3) report these coefficients for the conviction analysis, where the outcome is an indicator for the case ending in noncarceral conviction. The first column includes the instrument along with court-by-year fixed effects, court-by-month fixed effects, and day-of-week fixed effects. The second column adds individual and case-level controls, and the third column adds the leave-one-out judge incarceration stringency. Columns (4)-(6) repeat this analysis, but with incarceration as the outcome; the final column controls for judge dismissal stringency. Standard errors are clustered at the judge-year level. Stars denote * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table III: Balance

	Convicted	Conv. stringency	Incarceration	Incar. stringency
	(1)	(2)	(3)	(4)
Any prior conv.	-0.1380*** (0.0029)	-0.0000 (0.0002)	0.1704*** (0.0032)	0.0002 (0.0002)
Female	0.1207*** (0.0032)	-0.0003* (0.0002)	-0.1241*** (0.0031)	0.0002 (0.0002)
Black	-0.0413*** (0.0025)	0.0002 (0.0002)	0.0457*** (0.0026)	-0.0002 (0.0002)
Has misdemeanor	0.0434*** (0.0047)	0.0001 (0.0003)	-0.0148*** (0.0050)	0.0003 (0.0003)
Drugs	-0.0282*** (0.0037)	0.0003 (0.0002)	0.0705*** (0.0041)	-0.0000 (0.0003)
Larceny	-0.0093*** (0.0035)	0.0003 (0.0002)	0.0995*** (0.0037)	0.0003 (0.0002)
Assault	-0.1542*** (0.0035)	-0.0011*** (0.0002)	0.1576*** (0.0043)	0.0012*** (0.0003)
Fraud	0.0251*** (0.0040)	0.0004 (0.0003)	0.0515*** (0.0042)	0.0006* (0.0003)
Traffic	-0.1858*** (0.0042)	-0.0003 (0.0003)	0.3307*** (0.0048)	0.0006* (0.0004)
Burglary	-0.0408*** (0.0043)	-0.0001 (0.0003)	0.0782*** (0.0047)	0.0005 (0.0003)
Robbery	-0.0949*** (0.0048)	-0.0002 (0.0004)	0.1647*** (0.0059)	0.0004 (0.0004)
Sexual assault	-0.1681*** (0.0062)	-0.0007 (0.0005)	0.2070*** (0.0074)	0.0012** (0.0006)
Kidnapping	-0.0631*** (0.0066)	-0.0005 (0.0006)	-0.0023 (0.0085)	0.0006 (0.0006)
Murder	-0.1537*** (0.0076)	-0.0012 (0.0008)	0.1762*** (0.0119)	0.0010 (0.0010)
F-stat joint F-test	574.563	3.759	820.355	2.652
P-value joint F-test	0.000	0.000	0.000	0.001
Observations	183,381	183,381	183,381	183,381

Note: This table shows estimates from regressions of either case outcomes (noncarceral conviction or incarceration indicators) or judge stringency measures on case characteristics. Regressions include court-by-year fixed effects, court-by-month 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. Stars denote * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. To see the balance table in standard deviation units, see Appendix Table [D.1](#)

Table IV: Noncarceral conviction and recidivism

	Year 1		Years 2-4		Year 5-7		Year 1-7	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Fut. charge	-0.002 (0.002)	0.105** (0.046)	0.004 (0.003)	0.087 (0.075)	0.006** (0.002)	0.078 (0.075)	0.011*** (0.004)	0.235** (0.097)
Fut. conviction	0.001 (0.002)	0.135*** (0.043)	0.008*** (0.003)	0.115 (0.073)	0.007*** (0.002)	0.055 (0.071)	0.014*** (0.004)	0.300*** (0.095)
Fut. incarceration	0.001 (0.002)	0.113*** (0.037)	0.006** (0.002)	0.059 (0.063)	0.005** (0.002)	-0.024 (0.057)	0.012*** (0.003)	0.215*** (0.083)
Ctrl. comp. mean: fut. chrg.	0.157	0.157	0.301	0.301	0.237	0.237	0.493	0.493
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.	0.137	0.137	0.263	0.263	0.226	0.226	0.459	0.459
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	0.287	0.287	0.276	0.276	0.522	0.522
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 shows regression estimates of the impact of noncarceral conviction on recidivism, measured by any future felony charge, any future felony conviction, and any future felony incarceration. Recidivism is defined relative to the date of sentencing within the time window shown at the top of each column: 1 year, 2-4 years, 5-7 years and up to 7 years. For each outcome, we report ordinary least squares (OLS) and instrumental variable (IV) estimates. All IV regressions control for judge incarceration stringency. For the OLS estimates, we regress recidivism on having a conviction (regardless of incarceration status), controlling for incarceration. The estimates presented are the coefficient on the conviction variable. The middle portion of the table reports the control complier mean and control mean for each of the outcomes we consider. Control means are calculated for cases that end in dismissal. See Appendix E.5 for details on the calculation of control complier means. All regressions control for race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month 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 V: Incarceration and recidivism

	Year 1		Years 2-4		Years 5-7		Years 1-7	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Fut. charge	-0.022*** (0.002)	-0.097*** (0.029)	0.013*** (0.002)	-0.016 (0.047)	0.025*** (0.002)	0.004 (0.040)	0.022*** (0.003)	-0.071 (0.059)
Fut. conviction	-0.018*** (0.001)	-0.112*** (0.029)	0.013*** (0.002)	-0.038 (0.047)	0.023*** (0.002)	0.021 (0.039)	0.022*** (0.003)	-0.106* (0.058)
Fut. incarceration	-0.010*** (0.001)	-0.071*** (0.024)	0.017*** (0.002)	0.007 (0.041)	0.021*** (0.002)	0.052 (0.032)	0.027*** (0.003)	-0.029 (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	0.311	0.311
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

Note: This table shows regression estimates of the impact of incarceration on recidivism, measured by any future felony charge, any future felony conviction, and any future felony incarceration. Recidivism is defined relative to the date of sentencing within the time window shown at the top of each column: 1 year, 2-4 years, 5-7 years and up to 7 years. For each outcome, we report ordinary least squares (OLS) and instrumental variable (IV) estimates. All IV regressions control for the leave-one-out judge dismissal stringency. For the OLS estimates, we regress our measures of recidivism on incarceration, controlling for having a conviction (regardless of incarceration status). The middle portion of the table reports the control complier mean and control mean for each of the outcomes we consider. Control means are calculated for cases that end in noncarceral conviction. All regressions control for race, gender, prior conviction, offense type dummies, year-by-court fixed effects, court-by-month 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 VI: Testing for treatment-specificity

	Predicted recidivism			
	Year 1	Years 2-4	Years 5-7	Years 1-7
Panel A: UPM($Z_c \mid Z_i$) – ordered model				
Conviction stringency (Z_c)	0.013*** (0.0039)	0.030*** (0.0092)	0.023*** (0.0072)	0.048*** (0.014)
Mean dep. var.	0.093	0.202	0.153	0.346
N	100,152	100,152	100,152	100,152
Panel B: UPM($Z_i \mid Z_d$) – sequential and ordered model				
Incarceration stringency (Z_i)	-0.012*** (0.0044)	-0.026** (0.010)	-0.019** (0.0082)	-0.040** (0.017)
Mean dep. var.	0.090	0.183	0.138	0.320
N	28,589	28,589	28,589	28,589

Note: This table tests for treatment specificity following the method outlined in Section [IV.E.1](#). For Panel A, we restrict to the incarcerated sample and regress predicted recidivism on conviction stringency, controlling for incarceration stringency and court-by-time fixed effects. For Panel B, we restrict to the dismissed sample and regress predicted recidivism on incarceration stringency, controlling for dismissal stringency and court-by-time fixed effects. Predicted recidivism variables are created by taking the fitted values from a regression of recidivism after release on controls for demographics, charge, criminal record, and month, year-by-court, court-by-month, and day-of-week fixed effects. Standard errors are clustered at the judge-year level. Stars denote * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

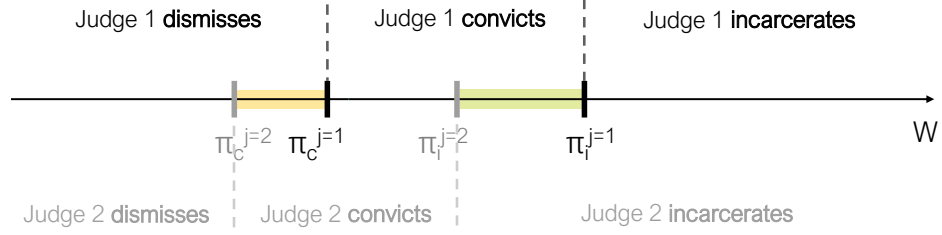
Table VII: Margin-specific treatment effects: an alternative approach

	Mixed logit with correlated normal random effects			
	Year 1	Years 2-4	Years 5-7	Years 1-7
Panel A: Noncarceral conviction vs dismissal (C vs D)				
Felony charge:	0.080** [0.002,0.164] {0.073}	0.209*** [0.093,0.375] {0.148}	0.145*** [0.030,0.302] {0.127}	0.269*** [0.097,0.472] {0.305}
Felony conviction:	0.086** [0.010,0.169] {0.060}	0.225*** [0.108,0.355] {0.124}	0.115** [0.018,0.229] {0.126}	0.317*** [0.159,0.497] {0.252}
Felony incarceration:	0.062* [-0.002,0.129] {0.055}	0.161*** [0.041,0.286] {0.104}	0.075* [-0.012,0.169] {0.092}	0.223*** [0.065,0.412] {0.237}
Panel B: Incarceration vs noncarceral conviction (I vs C)				
Felony charge:	-0.047*** [-0.076,-0.011] {0.081}	0.039 [-0.018,0.101] {0.163}	0.001 [-0.058,0.059] {0.139}	-0.033 [-0.117,0.058] {0.325}
Felony conviction:	-0.039** [-0.068,-0.008] {0.073}	0.033 [-0.024,0.086] {0.150}	0.009 [-0.047,0.062] {0.124}	-0.028 [-0.110,0.059] {0.300}
Felony incarceration:	-0.015 [-0.041,0.011] {0.052}	0.048* [-0.005,0.095] {0.097}	0.011 [-0.037,0.058] {0.092}	-0.031 [-0.111,0.059] {0.229}
Controls	Yes	Yes	Yes	Yes

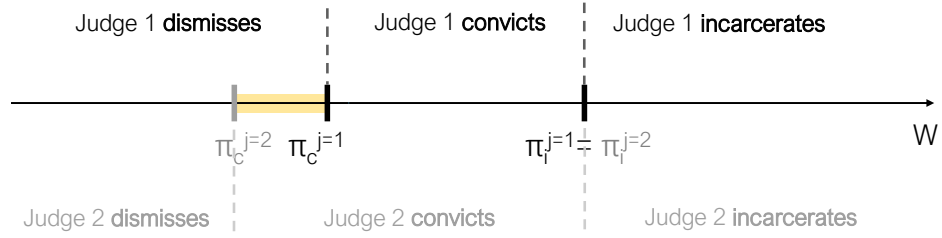
This table reports margin-specific estimates of the impact of noncarceral conviction vs dismissal (Panel A) and incarceration vs noncarceral conviction (Panel B) using an unordered multinomial model based on the methodology developed in [Mountjoy \(2022\)](#). The treatment-specific instruments are recovered as described in Section [V.A](#) using a mixed-logit specification for the choice model where the intercept includes a correlated multivariate normal random effect and controls for female and Black indicators, an indicator for whether any charges are for violent crimes, an indicator for whether any charges are for property crimes, and indicator for whether any charges are for drug crimes, the number of charges, the time since last offense, and the number of misdemeanor charges associated with the case. The choice model is fit by circuit and three-year time window. The estimates then use the recovered treatment-specific instruments in the method developed by [Mountjoy \(2022\)](#), where we include the same controls plus circuit and year fixed effects. The curly brackets report control-group complier means. In the top panel, this is the mean outcome for compliers whose cases were dismissed, while for the bottom panel, it is for those convicted but not incarcerated. 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.

Figure I: Ordered choice model

(a)

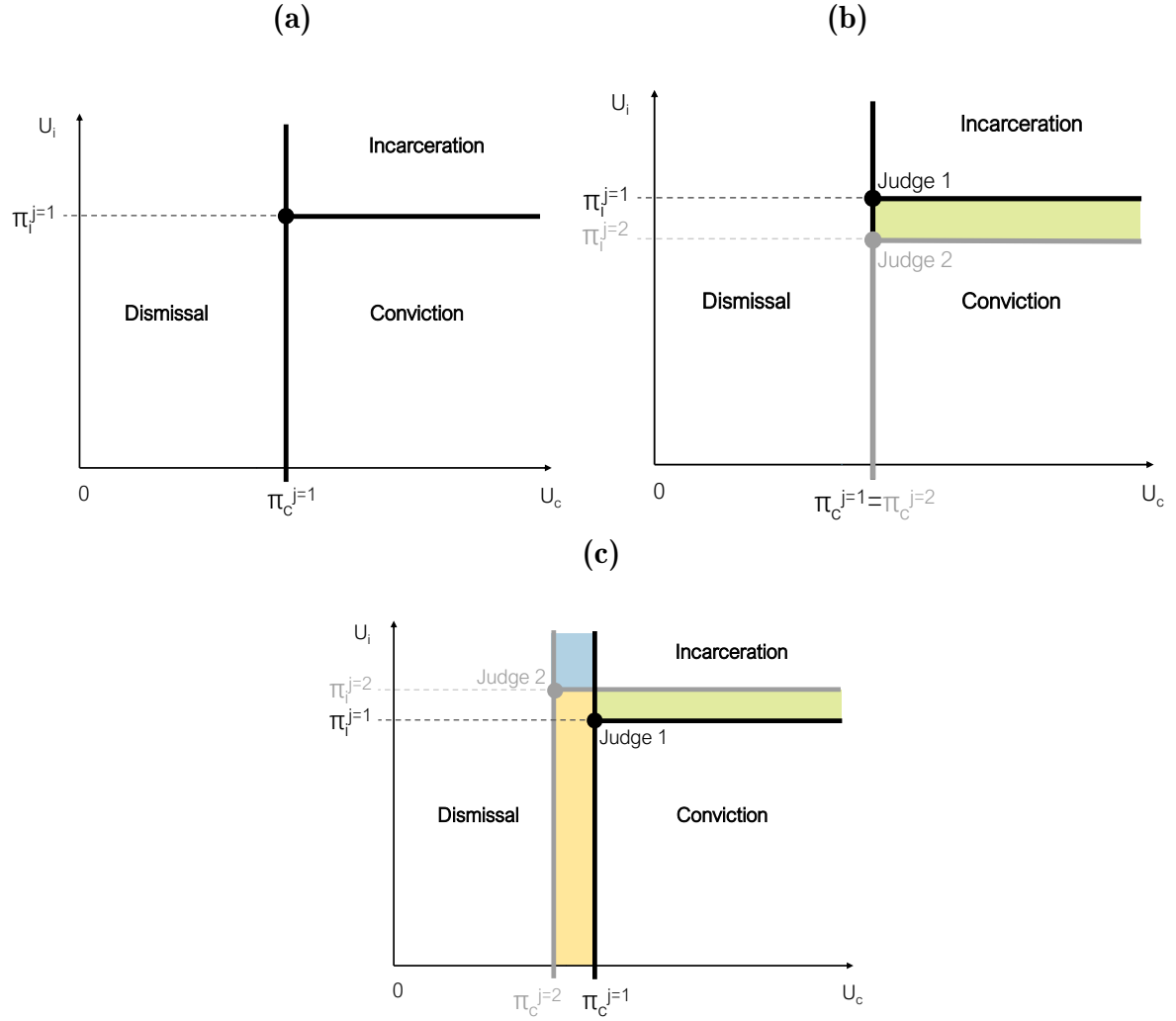


(b)



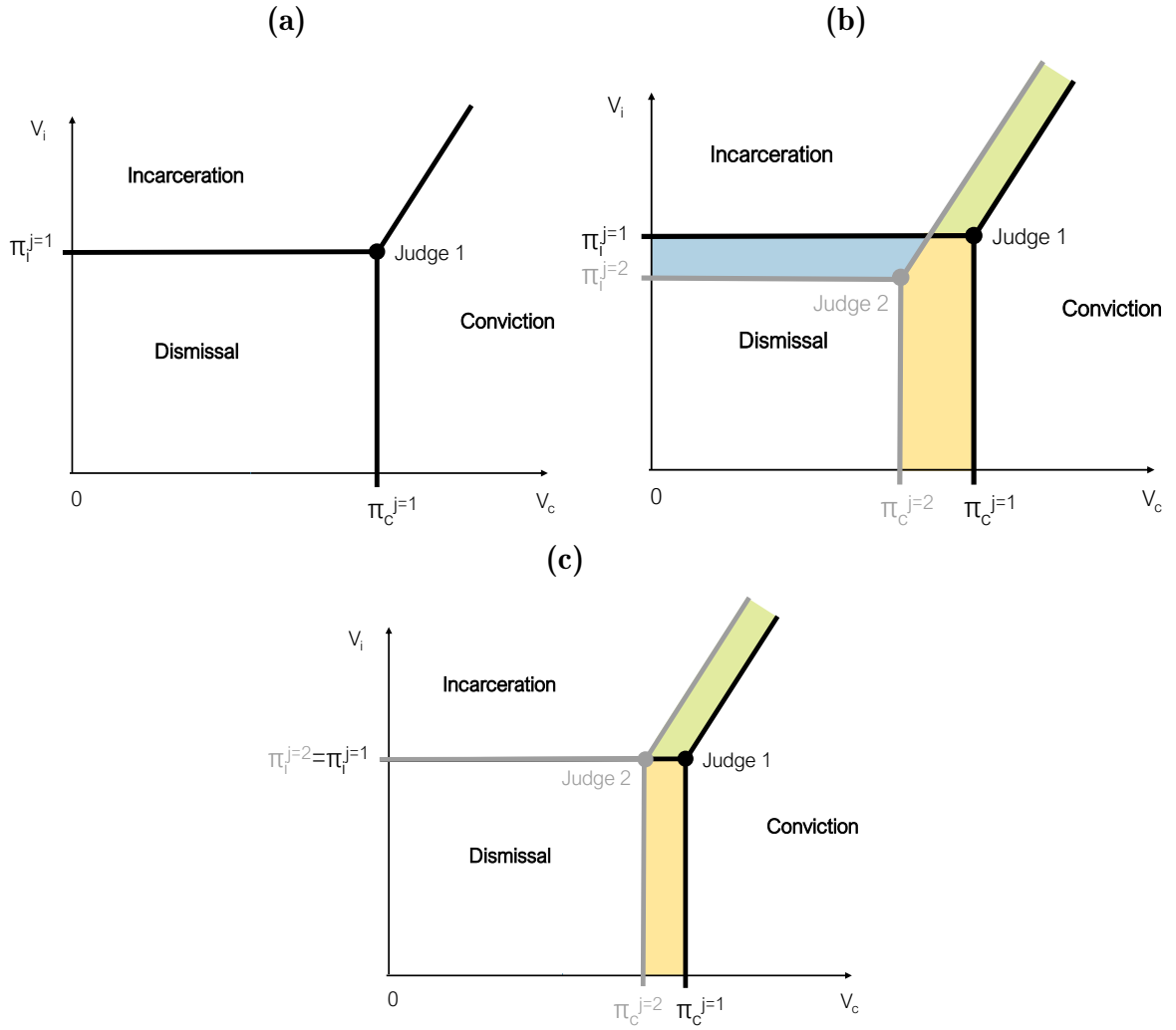
Note: This figure visualizes how, under the ordered choice model discussed in Section [III.C.1](#), judges classify individuals into incarceration, conviction, and dismissal depending on the cases' unobservable W . Panel (a) visualizes this for two arbitrary judges, and Panel (b) does so for two judges with the same incarceration stringency but different conviction stringencies.

Figure II: Sequential choice model



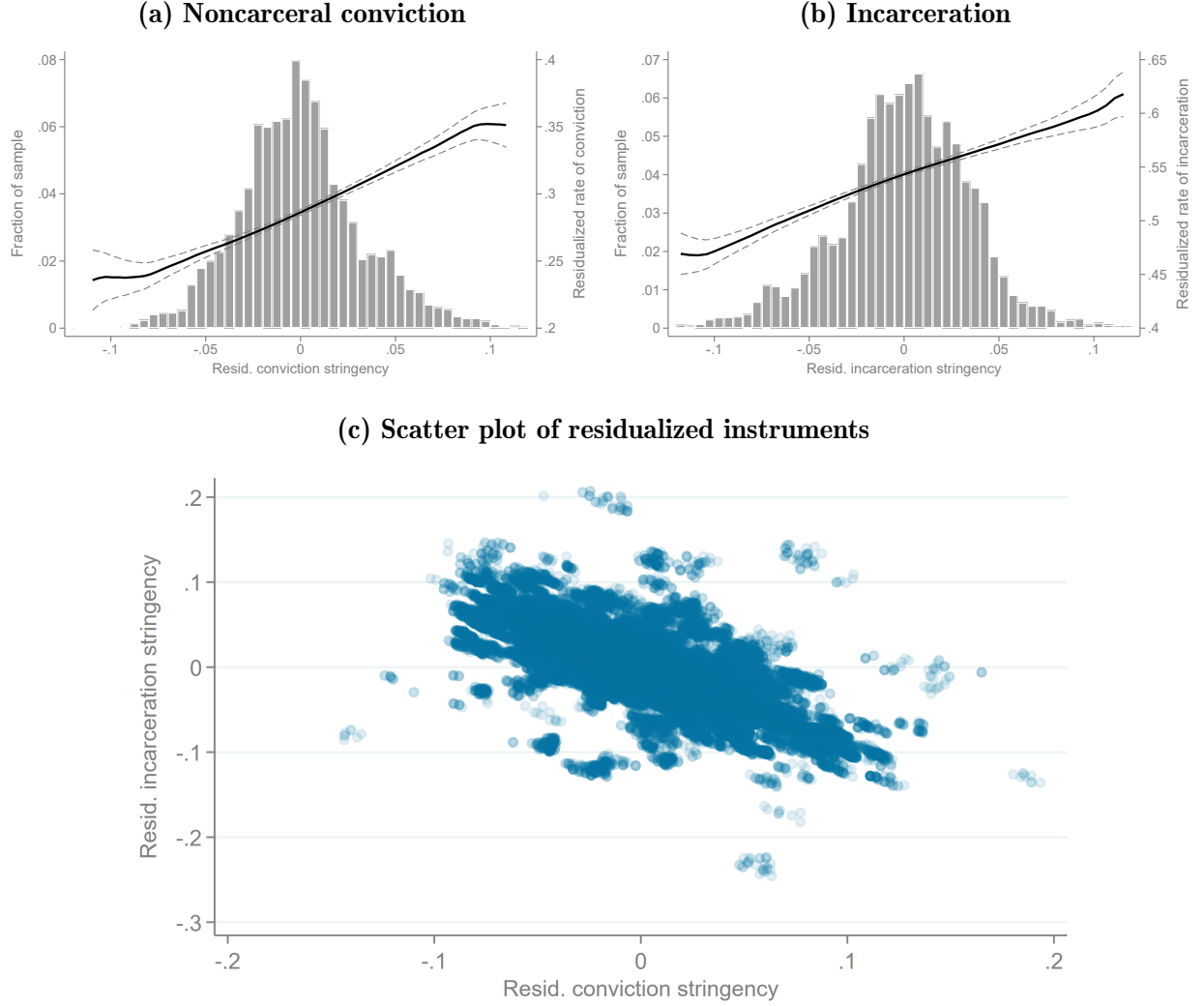
Note: This figure visualizes how, under the sequential choice model discussed in Section [III.C.2](#), judges classify individuals into incarceration, conviction, and dismissal depending on the cases' unobservable U_i and U_c . Panel (a) visualizes this for an arbitrary judge, Panel (b) does so for two judges with the same dismissal stringency and different conviction stringencies, and Panel (c) for two judges with the same incarceration stringency but where judge 2 has a higher conviction stringency.

Figure III: Unordered multinomial choice model



Note: This figure visualizes how, under the unordered multinomial choice model discussed in Section [III.C.2](#), judges classify individuals into incarceration, conviction, and dismissal depending on the cases' unobservable V_i and V_c . Panel (a) visualizes this for an arbitrary judge, Panel (b) does so for two judges with the same incarceration stringency but where judge 2 has higher conviction stringency, and Panel (c) for two judges with the same threshold for incarceration but where judge 2 has a higher conviction stringency.

Figure IV: Distribution of the stringency instruments



Note: This figure presents our first stages in graphical format for noncarceral conviction (Panel a) and incarceration (Panel b). The histograms plot the density of our residualized measures of conviction or incarceration stringency, and the line plots estimates of the first stage regression with conviction (Panel a) and incarceration (Panel b) as the dependent variable. Panel c is a scatter plot of the residualized incarceration and conviction instruments. In all three panels, the corresponding instrument is residualized against day-of-the-week, court-by-month, and court-by-year fixed effects.