Bail, Jail, and Pretrial Misconduct: The Influence of Prosecutors

Aurélie Ouss*and Megan Stevenson†‡

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Abstract

Courts routinely use low cash bail as a financial incentive to ensure that released defendants appear in court and abstain from crime. This can create burdens for defendants with little empirical evidence on its efficacy. We exploit a prosecutor-driven reform that led to a sharp reduction in low cash bail and pretrial supervision, with no effect on pretrial detention, to test whether such incentive mechanisms succeed at their intended purpose. We find no evidence that financial collateral has a deterrent effect on failure-to-appear or pretrial crime. This paper also contributes to the literature on the role of prosecutors in criminal justice reform. We show that discretionary reforms dilute impacts and can lead to racial disparities in implementation.

*University of Pennsylvania, aouss@sas.upenn.edu

†University of Virginia School of Law, mstevenson@law.virginia.edu

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Financial penalties are used in various areas of criminal justice, with the goal of deterring misconduct.[1] One iconic example is the requirement that defendants pay cash bail to secure pretrial release. However, in recent years, hundreds of jurisdictions across the United States have begun the process of reforming their bail system. Bail reform is motivated by concerns about inadvertent detention for those who cannot afford to pay. But cash bail is not supposed to be a de facto detention order; rather, it's a collateral system that is designed to incentivize *released* defendants to appear in court and refrain from crime. In fact, the modal defendant is able to secure release by paying bail or agreeing to supervisory conditions (Reaves, 2013). This is particularly true among the type of defendants most affected by reform, who tend to be facing less serious charges and often have low bail even absent the reform. The elimination of low-level bail is expected to provide benefit to defendants, since it reduces monetary and time burdens. But getting rid of financial incentives could have adverse consequences in terms of appearance rates and crime. There is little empirical work exploring these dynamics.

In this paper we provide new evidence on the impacts of bail reform and the efficacy of low-level bail conditions. We do so by evaluating a prosecutor-led reform in Philadelphia. On February 21st, 2018, Philadelphia's newly-elected 'progressive-prosecutor' declared that his office would no longer seek monetary bail for defendants charged with a long list of eligible offenses. Nicknamed the 'No-Cash-Bail' policy, this reform applied to nearly 2/3 of all cases filed in the city of Philadelphia, including both misdemeanors and nonviolent felonies. To evaluate the impacts of this policy, we use web-scraped court data and a difference-in-differences design with defendants who were ineligible for the No-Cash-Bail policy as a control group.

Philadelphia's No-Cash-Bail policy, like most bail reform initiatives, is discretionary. That is, the bail magistrates still have full discretion to set monetary bail if they choose, but the prosecutor's office no longer requests it for eligible categories. A first question, then, is whether such discretionary reform even has an impact. Bail magistrates are often assumed to be minimizing misconduct risk, subject to costs of incarceration (Kleinberg et al., 2018; Arnold et al., 2018). In our context, there are no changes to any of the key inputs to the bail decision: the defendants' risk profile or the direct costs of crime or incarceration. Nonetheless, we find that the No-Cash-Bail policy led to a sharp 22% (11 percentage point) increase in the likelihood of being released on recognizance (ROR, or release without monetary or supervisory conditions). We argue that this is due to the role that prosecutors play in setting social norms within criminal justice.

[1] Fines are generally found to be effective, for example in improving driving behaviors (Luca, 2015; Bar-Ilan and Sacerdote, 2004; Goncalves and Mello, 2017a) or to reduce collusive pricing (Block et al., 1981).

Despite the large increase in ROR, the No-Cash-Bail policy had no impact on pretrial detention rates. This may seem surprising, but, evaluating substitution patterns, we find that most of those who received ROR as a result of the reform would have otherwise been released after paying low monetary bail (a deposit of $500 or less), or agreeing to the conditions of pretrial supervision or unsecured bail (in which the defendant does not need to pay for release but owes money to the court should she fail to appear). There was no effect on larger bail amounts, which are more likely to lead to pretrial detention.

Since the No-Cash-Bail policy changed conditions of release without affecting the overall release rate, it provides an ideal opportunity to test the deterrent effects of monetary and supervisory conditions among this group of low-level offenders. Monetary bail is designed as a financial incentive that should act as a deterrent by raising the cost of failing to appear in court (Becker, 1968). Its role in detaining people has received much attention in the literature, but intentionally setting unaffordable monetary bail is controversial and potentially unconstitutional (Starger and Bullock, 2018; Mayson, 2020). Our setting allows us to evaluate the central claim that justifies the use of monetary bail and pretrial supervision: that such conditions incentivize better behavior among those who are released.

We find no evidence that this is the case. Our point estimates are close to zero and allow us to reject even small increases in failure to appear and pretrial crime at the 5% level. Subgroup analysis allows us to isolate impacts of cash bail as distinct from pretrial supervision; we find no evidence that financial incentives increase compliance. We leverage an instrumental variables difference-in-differences approach to directly test the impact of ROR on FTA and crime. Our results suggest that monetary bail is not necessary to prevent misconduct for the large majority of those evaluated.

This poses a puzzle: monetary bail and pretrial supervision are widely used under the theory that such conditions incentivize court appearance and deter crime. Yet Philadelphia was able to substantially liberalize the conditions of pretrial release with no detectable adverse consequences. One possible explanation is that, at least for eligible offenses before the reform, magistrates were setting unnecessarily restrictive conditions of release due to asymmetric penalties in errors: the visibility and salience of 'type II errors' (being too lenient towards someone who reoffends) is much greater than that of 'type I errors' (being too harsh on someone who would not have reoffended). In the absence of interventions that raise the cost of being harsh, magistrates will tend to set bail higher than is necessary to ensure good conduct. This creates low-hanging fruit in bail reform: a pool of defendants for whom monetary and supervisory conditions can be eliminated without adverse consequences.

How big is this pool? Would a more comprehensive bail reform lead to greater

adverse consequences? To provide suggestive evidence on this, we exploit a second natural experiment in Philadelphia: the fact that bail magistrates are quasi-randomly assigned to magistrates with varying initial levels of leniency and varying responsiveness to reform. If magistrates assign ROR based on crime- or FTA-risk, defendants on the margin of receiving ROR from a lenient magistrate will be higher risk than the marginal defendant for a strict magistrate. We find that even originally-lenient magistrates were able to increase ROR rates by 10 percentage points without adverse consequences. Our results suggest that Philadelphia may not have exhausted the pool of defendants receiving unnecessarily restrictive conditions of release, and that other jurisdictions with strict bail practices may be able to substantially liberalize conditions of pretrial release also.

We then evaluate whether there is differential leniency by race in the implementation of the reform. To do so, we adapt a method from the instrumental variables literature that allows us to compare characteristics of those selected to benefit from the reform against the pool of potential beneficiaries. We find that magistrates disproportionately select white defendants as beneficiaries of the reform, relative to the composition of the eligible group. Magistrates did not select beneficiaries based on FTA risk at all, implying that discretion in implementation cannot be credited for the fact that the No-Cash-Bail reform did not increase failures to appear.

This paper contributes to several literatures. To begin, we provide some of the first evidence on the impacts of the current bail reform movement.[2] More specifically, we provide one of the first evaluations of the empirical claim used to justify the use of monetary bail: that it deters failure-to-appear in court for *released* defendants.[3] We find no evidence that monetary or supervisory conditions have a deterrent effect on misconduct among those evaluated, which goes against the traditional economic models of crime involvement (Becker, 1968). We consider several explanations. First, some defendants may simply comply because they were instructed to do so by the court; no further incentives are necessary. Second, for defendants who require additional incentives, sufficient incentive may already be in existence. Failing to appear in court is a crime; it results in a bench warrant and can be used to justify holding someone without bail (or on unaffordable bail) in the future. Cash bail might provide

[2]Stevenson (2018a) finds that a risk assessment based bail reform in Kentucky led to a slight increase in misconduct.

[3]Closest to our work are Myers (1981) and Helland and Tabarrok (2004). The first paper uses regression analysis to look at the correlation between bond amount and FTA in New York in 1971, and finds that increasing bail bond reduces FTA. The second paper uses propensity score matching, and the authors find that felony defendants released with surety bonds are less likely to miss court appearances than similar defendants released on recognizance. The different results across these studies could be explained by variations in the sample, the different roles played by bail bondsman in different jurisdictions, or by methodological approach. Other studies that evaluate the combined incapacitative (due to pretrial detention) and deterrent effect of monetary bail include Abrams and Rohlfs (2011) and Gupta et al. (2016).

little marginal deterrence on top of the criminal justice penalties. Finally, some instances of nonappearance may not be the result of intentional choice. Many arrestees struggle with substance abuse, mental health, and extreme poverty. If they fail to appear in court it may be due to challenges with time management. A recent study shows that sending reminders leads to a large reduction in FTA, suggesting that attention constraints may be a substantial contributor to nonappearance (Fishbane et al., 2020). Regardless of the explanation, our results call into question the widespread use of bail for low-level defendants. It imposes burdens without detectable benefit and raises potential constitutional issues around excessive bail and due process (Wiseman, 2014; Funk, 2019).[4] Beyond bail reform, our paper contributes to a nascent literature studying possible collateral consequences of criminal justice reform that aim to reduce the scale of criminal justice. For example, Rose (2021) studies more lenient responses to parole violations; Agan et al. (2021) investigate the scaling back of misdemeanor prosecution; and Feigenberg and Miller (2020) explore changes in automobile stops. All find that these policies can reduce racial disparities, and / or improve longer-term defendant outcomes, which consistent with our main findings.

Our research also contributes to a broader literature on discretion in criminal justice. This topic has received much attention as a contrast to rule-based decision-making, such as the use of sentencing guidelines or algorithmic risk assessments. Scholars have pointed out some of the weaknesses of discretion, such as the fact that human beings may be poor at evaluating risk (Kleinberg et al., 2018) or make racially biased prediction errors (Arnold et al., 2018). Others have countered that human decision-makers can provide a bulwark against inadvertent consequences of a rule-based decision system (Stevenson and Doleac, 2019). Our results mostly highlight the downside of discretion in reform. We find that discretion can dilute reform's impacts, and lead to racial disparity in the allocation of its benefits. Furthermore, we find no evidence that discretion ameliorated the adverse consequences of bail reform. Even though the legal rationale for bail is that it is supposed to induce appearance, magistrates do not appear to be taking failure-to-appear risk very much into account in the bail setting decision.

Lastly, we provide new evidence about the influence of prosecutors. The nascent literature has thus far supported claims about outsized prosecutorial power, showing they are influential in both the high rates of incarceration and in racial disparities in sentencing (Rehavi and Starr, 2014; Pfaff, 2017; Arora, 2018; Krumholz, 2019; Sloan,

[4]The Supreme Court held that "bail set at a figure higher than an amount reasonably calculated to fulfill this purpose [assuring the presence of the accused in court] is 'excessive' under the Eighth Amendment." *Stack v. Boyle*, 342 U.S. 1. While this has typically been interpreted as applying to high levels of cash bail, our results suggest that low levels of cash bail could also be excessive, since we find that they don't improve court attendance, compared to no bail at all.

2019; Tuttle, 2019). This literature focuses on parts of the criminal justice system that prosecutors have direct control over, such as charging decisions. Our work shows that prosecutors can also be influential in areas where they have no direct control, such as bail. Such influence may stem from the norm-setting role of prosecutors in the courtroom. This is an additional channel of influence by which the progressive prosecutor movement might lead to more systemic change.

The remainder of the paper is organized as follows. In Section I we discuss background on bail reform, the natural experiment in Philadelphia, our data, and our empirical strategy. Section II presents the results of our empirical analysis and Section III concludes.

I. Empirical setting

A. Background on bail reform

The traditional goal of monetary bail is to ensure that those who are released from jail show up in court for their appointed dates (Funk, 2019). Monetary bail acts as collateral; if the defendant fails to appear in court, the bail amount will be forfeited. Although recent economics literature has modeled bail-setting as synonymous with the decision to detain or release,[5] the use of monetary bail as a de facto detention order is highly controversial, and potentially even unconstitutional (Starger and Bullock, 2018; Mayson, 2020). In fact, the legal phrase 'right to bail' is historically understood as a right to release (Schnacke et al., 2010). Nonetheless, monetary bail can often result in pretrial detention, sometimes intentionally, and sometimes inadvertently.

Pretrial release with monetary collateral is extremely common, despite the lack of evidence about whether financial conditions are necessary to reduce pretrial misconduct. The best available national statistics show that, among felony defendants in large urban counties, 33.7% were held on cash bail, 38.2% were released on cash bail, and only 14% were released on recognizance (Reaves, 2013). Among felony defendants with monetary bail set, 43% had bail less than $10,000 and 28% had bail less than $5000 (Reaves, 2013). Misdemeanors, however, constitute the large majority (∼80%) of cases filed. While there are no nationally representative statistics on bail for misdemeanors, Mayson and Stevenson (2020) analyze data across eight diverse jurisdictions and find that 40% to 90% of misdemeanor defendants were required to post monetary bond. Among those with monetary bail, the large majority had bail less than $5000.

In recent years, hundreds of jurisdictions are engaging in bail reform (PJI, 2020). Reform is motivated primarily by concerns about equity and efficiency in pretrial de-

[5] E.g. Arnold et al. (2018); Kleinberg et al. (2018); Hull (2017)

tention. Since monetary bail conditions release on ability-to-pay, poor individuals are disproportionately likely to await trial in jail even if they pose a low risk of nonappearance or crime. Given correlations between race and wealth in the United States, and evidence for racial disparities in many parts of the criminal justice, this concern is especially relevant for minority defendants.

While bail reform initiatives vary, there are several consistent themes. First, reform initiatives aim to eliminate or reduce the use of monetary bail. While this is not the only goal sought by reformers, it has been a centerpiece of the recent movement. Second, reform is often limited to relatively low level offenses such as misdemeanors and nonviolent felonies.[6] This is in part because the state wants to preserve the ability to detain those charged with more serious offenses and the ability to deny bail entirely can be limited by law. Third, most reform initiatives are discretionary, meaning that some body within the jurisdiction declares a presumption of nonmonetary release but leaves the final decision up to the bail magistrate.[7] Discretionary reform can come from the legislature, from the courts, or, as is increasingly common, from prosecutorial policy. A commitment to no longer request cash bail for many offense categories has been a staple of the recent 'progressive-prosecutor' movement (Bazelon, 2019). For instance, on the first day in office, recently elected Los Angeles District Attorney instructed his prosecutors to no longer request monetary bail for misdemeanors and low-level felonies. Similar policies have been announced by prosecutors in Philadelphia, San Francisco, Austin, Chicago, Fairfax, Boston and a variety of cities both small and large.

B. The pretrial process in Philadelphia

Anyone who is arrested in Philadelphia gets brought to a nearby police station, where they are booked and placed in a holding cell.[8] The police officer will then send the report associated with the arrest to the district attorney's office, where a prosecutor reviews the case and determines what charges to file. Once charges have been filed, the defendant is interviewed by a pretrial services officer. The pretrial services officer makes a recommendation for the bail amount, taking into account the defendant's charges, criminal history and life circumstances. Their recommendation is not binding and bail decisions often differ from what was recommended (Shubik-Richards and

[6]For instance, reform in New York City was limited to misdemeanors and nonviolent felonies; Harris County, Texas, eliminated cash bail for misdemeanors; Kentucky presumes release without cash bail for low risk individuals charged with misdemeanors or nonviolent felonies; and so forth.

[7]This is in contrast to policies that directly change the *scope* of discretion, like mandatory sentencing guidelines (Kuziemko, 2013; Yang, 2015). Unlike in these situations, bail magistrates' choice set is neither expanded or restricted.

[8]In 85% of cases, the arrest happens within the two calendar dates after the alleged offense, so in most cases, arrest and offense dates are close to the same.

Stemen, 2010). After the pretrial interview, the defendant is ready for the bail hearing. This takes place over videoconference: the defendant remains in the holding cell and communicates via video with the presiding magistrate. Representatives of both the District Attorney's office (referred to in this paper as the DA rep) and the public defender's office are in the courthouse with the magistrate. While the representatives can make suggestions for the appropriate bail amount, the final decision is made by the magistrate, who is an employee of the judiciary. The DA rep is advised on how much bail to request by line prosecutors who work in the charging unit at the district attorney's office. Neither the magistrate nor the DA rep are, in general, attorneys. Both specialize in bail hearings, and they are not involved in later phases of the case's processing.

The bail hearing typically lasts only a minute or two, during which the magistrate reads the charges, schedules the next court date, determines eligibility for public defense, and decides the conditions of release. These conditions include:

- ROR (Release on own recognizance): The defendant is released solely on their promise to return to court.
- Supervised release: The defendant is released with supervisory conditions, such as drug testing, weekly meetings with the pretrial supervision officer, restrictions on travel, restrictions on who they can interact with, and so forth. Monetary bail is not required.
- Unsecured monetary bail: The defendant does not need to post any money for release, but if they do not show up to their court date, they owe the court their bail amount.
- Secured monetary bail: The defendant must pay a deposit (10% of the bail amount) to be released. If they don't show up in court they forfeit the deposit and owe the court the remaining bail amount.
- Bail denied: The defendant is ordered to be detained pretrial. (Used rarely in Philadelphia.)

For defendants with secured monetary bail, if the person fails to pay the deposit within 4-8 hours of the bail hearing, they will be transported to the local jail. They will remain there until the disposition of the case unless they can procure the bail deposit or obtain a bail reduction.

Professional bail bondsmen are allowed in Philadelphia, but they are less common than in other jurisdictions. This is partly because Philadelphia has a deposit system: the defendant is released if they can pay 10% of the total bail amount. If they comply with all release conditions 70% of the deposit will be returned when the case is

disposed.[9]

C. The Philadelphia No-Cash-Bail reform

On November 7th, 2017, Larry Krasner was elected to the position of Philadelphia's district attorney (DA). He was the first criminal defense lawyer to be elected to that position, and he ran on a platform that included goals like lowering punishments for less serious crimes and reducing the use of pretrial detention.[10] However, and importantly for our research design, the exact timing of different reforms was not announced ahead of time.

On February 21st, 2018, DA Krasner announced that his office would stop seeking monetary bail if the lead charge was among a set of 25 low-level offenses. These offenses include both felonies and misdemeanors, and span from very low-level offenses to more severe offenses, such as burglaries with no person present. They also include several drug charges, such as possession with an intent to deliver.[11] The goal of this reform was to reduce pretrial detention and to avoid incarcerating defendants because they could not afford low bail amounts. Concretely, this meant that the DA's office would instruct their representatives at the bail hearing to ask that defendants with these lead charges be released on their own recognizance, or to not object if ROR was requested by the defendant's legal representative.

There was only one other reform to pretrial practices around that time. [12] On Feb. 15th, the DA's office announced a change in charging practices for marijuana possession, retail theft and prostitution. Appendix Figure A.1 shows that after that date, the number of charges filed for these offenses dropped. We remove them from our analyses. No other concurrent changes affected the prosecution of low-level offenses, or pretrial detention.[13]

D. Data and descriptive statistics

Our primary data source consists of court dockets web-scraped from the Pennsylvania Unified Judicial System. It is structured to include one observation per criminal

[9] This has recently been revised, and a compliant defendant will now receive their full bail deposit back.

[10] His agenda can be found here: https://krasnerforda.com/platform/

[11] A list of the most common eligible and ineligible offense categories can be found in Appendix Table A1.

[12] While DA Krasner hired a number of new prosecutors, and fired some old ones, there were no changes to the group in charge of pretrial processes (charging and bail) until after the end of our sample window – summer 2018.

[13] Over the last several years, Philadelphia has introduced several other changes to their pretrial system, such as early bail review, in which a judge reviews bail for cases in which a defendant is unable to pay, and a pilot project of providing pre-bail-hearing public defense to some defendants. However, these changes were implemented more than a year before the policy evaluated in this paper and should not affect our analysis, which focuses on a time window of 6 months before and 5 months after the No-Cash-Bail policy.

case, and includes all criminal cases filed in Philadelphia from 2007 through April 2019. While we use the entirety of the court data to build criminal history and recidivism variables, our analysis focuses on cases whose initial bail hearing occurred in the six months before or the five months after the No-Cash-Bail reform. After dropping marijuana possession, prostitution, and retail theft cases,[14] duplicate cases (i.e. a defendant is brought for multiple cases on the same day),[15] and cases where covariates are missing,[16] our sample contains 22,589 observations.

The dockets include information on the defendant (first and last name, date of birth, gender, race, ZIP Code, and a unique court identifier), the charges (date of arrest, offense type), the bail hearing (date and time of the bail hearing, bail magistrate name, bail type and amount), whether and at what date and time bail was posted, and notes pertaining to each court appearance (including whether the defendant failed to appear). Using this data, we define several other main variables. First, we define 'eligible cases' as cases that are eligible for the No-Cash-Bail policy; in other words, cases for which the lead charge at the time of the bail hearing appears on the list of 25 offenses for which the DA's office would no longer request cash bail .[17] 'Ineligible cases' are cases whose lead charge does not appear in that list of 25 offenses. Following previous literature, in our main specifications, we consider a person to be detained pretrial if they spend at least three nights in jail (Dobbie et al., 2018; Stevenson, 2018b).[18] We generate a dummy for 'recidivism' which is equal to one if a person with the same unique court identifier is charged with a new offense within six months of the bail hearing.[19] Our FTA variable is equal to one if the defendant fails to appear for at least one court date associated with this case. We define variables for prior FTA and prior charges by searching the data for prior instances with the same defendant identifier. For consistency across cases, and since our data begins in 2007, we limit our time window for priors to nine years before the bail hearing.

Table 1 presents descriptive statistics for cases filed in the six months before the No-Cash-Bail reform was announced on February 21st, shown separately for eligible and ineligible cases. First, note that a large portion – roughly 60% of the sample – is eligible

[14]Marijuana possession, retail theft and prostitution cases constitute ∼10% of pre-reform caseload.

[15]8.5% of cases are multiples; we omit these due to difficulties in defining the bail type for a defendant with multiple types of bail. Our results are very similar if we include duplicate cases.

[16]About 7% of cases are missing some covariates; most often information about past offenses

[17]The one offense category where our definition of eligibility might be somewhat overinclusive is possession with intent to deliver (PWID). For drug types other than marijuana, there are a variety of circumstantial factors that may make a case ineligible. We are able to account for one of these factors – recent prior PWID arrests – but not others. 8% of ineligible cases are in this category. We conduct a variety of tests to ensure that our results are robust to dropping PWID cases. These are described in Section III.D.

[18]Most defendants who fail to pay bail within the first three days remain detained until the disposition of the case.

[19]At six months, 61% of cases have been resolved; at 10 months, 80% of cases are resolved. We conduct robustness tests in which our recidivism and FTA measures are defined over varying time windows.

for the reform.[20] Second, note that the reform targeted a group of defendants that were already being treated more leniently in the initial bail hearing, compared to defendants with ineligible cases. Half of eligible cases already received ROR before the reform, compared to only 7% of ineligible cases. Only about 17% of eligible cases led to at least three nights in jail compared to almost half of ineligible cases. This leniency is likely due to differences in the severity of the case. Even though a substantial portion (45%) of eligible cases carry felony charges, the charges tend to be less serious and defendants have fewer prior charges. Third, note that the FTA and recidivism rates are higher for eligible cases than ineligible cases. This could be because defendants charged with ineligible offenses are more likely to be detained and thus are mechanically prevented from accruing new charges or failing to appear in court. It also could be because eligible defendants are less likely to have monetary or supervisory conditions to incentivize good behavior. Lastly, note that the average poverty rate within defendants' zipcode is 25%, and 75% of eligible defendants had a public defender, which means that they were found indigent by pretrial services. This suggests high levels of resource constraints.

E. Empirical strategy

Before moving to formal analyses, Figure 1 presents some raw graphical evidence of how the No-Cash-Bail policy seems to have affected eligible cases. Clockwise from the top left corner the subfigures show time trends in ROR, pretrial detention, recidivism and FTA. [21] There was a sharp increase in ROR right after the reform, but neither jail, FTA, or recidivism changed much. At first glance this seems surprising, given that theory would predict that a large increase in ROR would affect all three outcomes. Our choice of research design is partially motivated by this figure. We considered both a discontinuity-in-time design as well as difference-in-differences, but select the latter because it is higher powered and would make it less likely for us to falsely reject the null. Also, by including ineligible cases as a control, we can account for time varying trends that affect both groups equally, such as seasonal effects.[22]

Our primary specification is shown in Equation 1, where i indicates case, $Post$ indicates that the initial bail hearing occurred after the No-Cash-Bail reform, and $Eligible$ indicates that the case is eligible for the reform. Unless specified otherwise, covariates X include defendant race, age at arrest, gender, prior FTAs, prior convictions, types of

[20] Including the case types omitted because of a concurrent change in charging practice (marijuana possession, prostitution, and retail theft), approximately 67% of all cases filed in Philadelphia before the reform would have been eligible for the No-Cash-Bail policy.

[21] Appendix Figure A.2 presents the same raw evidence for ineligible cases

[22] As a robustness check, we also include regression discontinuity in time estimates, discussed further in Section III.B.

offense,[23] grade of offense, whether the defendant was represented by a public defender, the bail magistrate, day of the week, and magistrate work-shift. The main coefficient of interest is δ.

$$Y_i = \alpha + \beta Post_i + \delta Post_i * Eligible_i + \lambda Eligible_i + \theta X_i + \epsilon_i \quad (1)$$

Our identifying assumption is that trends in outcomes between eligible and ineligible cases would have remained parallel had it not been for the No-Cash-Bail policy. We discuss challenges to this assumption and provide some initial evidence in support of this assumption here.

As discussed previously, there were no concurrent policy changes that could complicate analysis on our sample. However, it's possible that police and/or line prosecutors responded endogenously to the reform. For instance, police might deprioritize arrests for eligible offenses, or prosecutors may up-charge defendants to make them ineligible for the reform. This would be independently interesting, but would also be a threat to our research design, as it would result in a change in case composition.

Appendix Figure A.3 shows a time trend in the number of eligible and ineligible cases filed. The trend remains roughly parallel with no divergence at the time of the No-Cash-Bail reform. This provides some initial evidence that there were no concurrent changes in behavior that would confound our analysis.

We provide a series of more formal tests in Appendix Table A2 and A3. In Columns 1 of Appendix Table A2, we test for changes in arrest patterns.[24] We find no evidence of differential patterns in arrests for eligible compared to ineligible offenses. In Columns 2-4, we test for changes in prosecutorial charging behavior. We focus on three outcomes: (1) declinations, or the decision to not file charges; (2) upcharging, which we define as a prosecutor charging a case as ineligible, when the police description would have put it in the eligible category; (3) downcharging, which we define as the converse – a prosecutor charging a case as eligible when the police description puts it in the ineligible category. Here again, we don't see any change in charging prosecutors' decisions: they did not appear to be trying to 'game the system' by upcharging, downcharging, or declining any more frequently as a result of the No-Cash-Bail reform. Lastly, Column 5 tests for changes in the number of cases filed. Again, we see no change.

Appendix Table A3 and Appendix Figure A.4 tests for changes in observable case characteristics: charges per case (which can be a proxy for case severity), probability

[23] Offenses have been aggregated to the 23 most common offenses and a catch-all category for the remaining offenses. Standard errors are clustered at the offense level, though our conclusions are unchanged if we do not cluster standard errors, as shown in appendix Table A6.

[24] For Columns 1-4 of this table, the data comes from arrest records, which, importantly, includes what the police thought the offense to be and how the charging prosecutor assessed the case – i.e. if they declined to prosecute that case, and if not, what charge they would seek.

of having a prior, gender, and whether a defendant is black. For all of these analyses, we fail to reject the null, and the coefficients are small relative to the mean.

II. The impact of a prosecutor-led bail reform

A. Bail and pretrial detention

We begin by evaluating whether the No-Cash-Bail policy affected bail. Given that the prosecutor's role in bail is merely advisory, it's unclear whether magistrates will change practices as a result of the district attorney's decree. Bail magistrates are, by law, supposed to set the least restrictive bail conditions that would ensure compliance. Bail amounts are thought to be determined by a trade-off between the costs of both misconduct and incarceration (Arnold et al., 2018; Kleinberg et al., 2018). Prosecutorial policy does not affect any of these key inputs to the bail-setting decision. Furthermore, bail magistrates work for a separate government agency (the judiciary) and are not directly accountable to the district attorney. Despite all this, we find that magistrates *do* respond to the prosecutor-led reform.

The first two columns of Table 2 show difference-in-differences estimates of δ (as described in Equation 1) with ROR as the outcome. The odd column does not include controls; the even column does – a consistent pattern across many of the tables. We estimate that the No-Cash-Bail policy led to an 11 percentage point (22% relative to the pre-reform mean for eligible cases) relative increase in the likelihood that defendants will be released on their own recognizance.[25] The coefficient is stable to the inclusion of covariates, again mitigating concerns about changes in arrest or charging practices that would have led to a change in case composition at the time of the reform.

Why would a change in prosecutorial preferences affect the behavior of bail magistrates? Note that bail requests did not provide more information about defendant riskiness – if anything, since the new policy applied to whole offense categories regardless of a particular person's characteristics, bail magistrates are getting less information from the DA representatives about each individual after the policy change. One potential explanation has to do with social norms. Magistrates plausibly want to make decisions that seem just to their peers, as well as the people who they represent. Even if a bail reform policy is non-enforceable, it may still influence norms, thus changing what it means to 'do justice' well. An elected district attorney is a representative of the people, whose job is to administer justice in the name of the community. If a district attorney says that requiring cash bail for most misdemeanors and nonviolent felonies is unjust, this could be seen as both signal that a change in norms has already occurred,

[25] Appendix Table A4 shows the β and λ coefficients from equation 1.

and a validation of that change. For the magistrate, deviating from community norms can result in challenges during reappointment, disapproval from peers and community members, and other types of soft costs.

Figure 2 presents event-study style coefficient plots in support of the difference-in-differences estimation. In each graph, the dummy for eligibility is interacted with lead/lag dummy variables that each correspond to one month of bail hearings: six before and five after the policy. Specifically, we estimate the following equation:

$$\begin{aligned} Y_i =& \alpha + \sum_{g=2}^{6} \delta_{-g} Month_{-g} * Eligible_i + \sum_{g=0}^{4} \delta_g Month_g * Eligible_i + \\ & \sum_{\substack{g=-6, \\ g\neq -1}}^{4} \beta g Month_g + \lambda Eligible_i + \theta X_i + \epsilon_i \end{aligned} \tag{2}$$

$Month_{-g} * Eligible_i$ equals 1 if a case was an eligible offense and had its initial bail hearing g months before Feb. 21st (lags) and $Month_g * Eligible_i$ equals 1 if a case was an eligible offense and will have its initial bail hearing in g months after Feb. 21st (leads). The dummy for the month prior to the reform is left out as the comparison category. Figure 2 plots the δ coefficients. For instance, the coefficients plotted at -2 in the graphs refer to cases where the bail hearing occurred between one and two months prior to the reform; the coefficients plotted at 1 refer to bail hearings one to two months after the reform. We see that trends in ROR are approximately parallel before the reform for eligible cases. This helps support a central assumption of the difference-in-differences analysis: that trends in outcomes for eligible/ineligible cases would have remained parallel in the absence of reform. The increase in ROR comes immediately after the reform and remains high throughout the time period analyzed.

These results are robust to variations in variable definition, sample, and specification. We present robustness tests in Columns 1-4 of Appendix Table A5. We vary our definition of ROR so that it equals one if the defendant *ever* receives ROR during the pretrial period, as opposed to whether they receive ROR at the *initial* bail hearing. We then limit the sample to 12 weeks before and after the reform; conduct doughnut difference-in-differences regression in which we drop the week just before, the week of, and the week after the reform; and collapse the data to the weekly level for eligible and ineligible cases and conduct the difference-in-differences estimate on the aggregated sample. The estimates remain largely unchanged.

We then move to evaluating the impact on pretrial detention. Despite the sizable change in ROR, there is no statistically detectable differential impact on the likelihood of being detained pretrial. As seen in Columns 3 and 4 of Table 2, the point estimates

are small, stable to the inclusion of covariates, and correspond with a 0.72 percentage point increase in the pretrial detention rate. We can reject a decline of 1.9 percentage points or more at the 5% level.[26] Nor is there any visibly detectable change in the event-study graphical analysis – detention rates for eligible/ineligible defendants are parallel and unchanged both before and after the reform (see Figure 2).[27] Despite the hopes of reformers, this discretionary policy did not lead to a meaningful decrease in the pretrial detention rate.

At first glance, this seems inconsistent with prior claims that a sizable number of defendants are detained pretrial due to an inability to pay monetary bail. However, a closer look at the substitution patterns in bail can help explain why the increase in ROR did not translate into an increase in release. The No-Cash-Bail policy brought about an 11 percentage point relative increase in ROR, the least restrictive type of bail. Concordantly, other bail types declined by a net of 11 percentage points. We examine how the No-Cash-Bail policy affected four bail categories: supervised release without monetary conditions, unsecured bail, secured bail of $5,000 or less and secured bail over $5,000. (As a reminder, a defendant with secured bail of $5,000 would only need to pay $500 to be released, but they will owe the full $5,000 if they fail to appear in court.) Table 3 shows difference-in-differences estimates of the impact that the No-Cash-Bail policy had on these types of bail. We see that there was about a four percentage point decline in both supervised release and low monetary (secured) bail. Unsecured bail declined by a little over two percentage points. Conversely, we see little evidence of a decline in higher bail amounts: the point estimate is about -0.7 percentage points and is not statistically significant. Most of those who received ROR as a result of the reform would otherwise have been able to secure their release by either paying a $500-or-less deposit, accepting the supervisory conditions, or agreeing to the unsecured bail.[28]

The fact that the change in prosecutorial policy did not affect detention rates is noteworthy, given that lowering pretrial detention rates is a primary goal of bail reform. We expect that this is likely because 1) the policy change targeted a group of defendants that already had relatively high rates of release and 2) discretion in implementation meant that only a subset of eligible defendants actually benefited from the reform.

[26]For many of the outcomes measured, our hypotheses are naturally one-sided. In this instance, we are interested in whether the No-Cash-Bail policy led to a *decrease* in pretrial detention rates. We use a one-sided test to provide boundaries on what size effects are inconsistent with our data.

[27]Our results are not driven by the choice of our definition of being in jail pretrial as having spent at least 3 nights in jail: as shown in Appendix Table 2, the results are similar if we vary the definition to having spent at least 1 to 7 nights in jail.

[28]Even relatively low bail amounts can result in pretrial detention, if defendants are too poor to pay (Stevenson, 2018b). One interpretation of our results is that magistrates are able to identify which defendants can afford low monetary bail, and intentionally offer ROR only to those who would otherwise have been able to pay for release. However, given the relatively small changes in secured monetary bail, it would be premature to infer this from our data.

Given that many other bail reform initiatives also target low-level cases and allow judges discretion to continue to set monetary bail, results in Philadelphia provide a cautionary tale about the extent to which bail reform will affect the jail population.

However, even if the No-Cash-Bail policy did not affect pretrial detention, it still led to changes that are likely to be meaningful to a defendant's life. Court debt and pretrial supervision can contribute to net-widening in the reach of criminal justice: seemingly minor criminal justice interventions can lead to large burdens for individuals, and in particular for minority men (Rios, 2011; Martin et al., 2018). Several hundred dollars in secured bail deposits is a large sum for an indigent population. Unsecured bail poses no upfront costs but entails a threatening overhang on the defendant's life. Should she fail to appear in court due to difficulty in understanding when/where she was supposed to appear, accidental oversight in the midst of a chaotic life, inability to get time off of work, or any one of a plethora of reasons, unsecured bail results in court debt. Pretrial supervision requires time-consuming check-ins with pretrial services as well as restrictions on liberty, such as curfews or orders to remain within the jurisdiction. Eliminating the burdens of these conditions has benefit to the defendant. What remains to be seen is whether it has costs in terms of nonappearance or crime.

B. Pretrial misconduct

A concern with reducing the use of monetary bail and supervisory conditions is that misconduct will increase. This could be due to several reasons. If reducing monetary bail means that more defendants are released pretrial, this could result in a mechanical increase in FTA and recidivism simply because more defendants are out on the streets. Since the No-Cash-Bail reform did not affect the pretrial detention rate, this mechanism is not relevant to our context. However, reducing monetary bail and supervisory conditions could increase misconduct among released defendants if the prospect of monetary penalties act as a deterrent, or if supervision improves compliance. This is what classic economic theory would predict, and the reason why cash bail exists (Becker, 1968).

On the other hand, there are also reasons to think that monetary bail and supervision could lead to a slight *increase* in misconduct. For instance, the payment of bail and the time burdens of pretrial supervision could have a destabilizing effect on the lives of indigent defendants. In other contexts, researchers have found that the imposition of minor fines can lead to disproportionate financial distress (Harris, 2016; Mello, 2018). Alternatively, the imposition of monetary bail or pretrial supervision without giving the defendant a chance to explain him or herself may feel coercive or unfair (Nagin and Telep, 2017).[29] This could foster the expectation that the court process will be sim-

[29]Defendants are discouraged from speaking during the bail hearing as they have not yet had a chance to

ilarly unfair, thereby decreasing compliance.[30] While commentators have focused on the concern that bail reform will increase misconduct, competing hypotheses suggest that this is an empirical question. We provide new evidence here.

Table 4 presents the difference-in-differences estimates with FTA and recidivism as the outcomes (δ from Equation 1).[31] We find no statistically detectable impact of the No-Cash-Bail policy on the likelihood of failing to appear in court, or of receiving new charges within six months after the bail hearing for eligible relative to ineligible offenses. We can reject, at the 5% level, anything larger than a 0.009 percentage point increase in FTA.[32] We can reject any increase in pretrial rearrest.[33] These results are supported by our graphical event-study analysis, which is presented in the bottom two graphs of Figure 2. Trends in FTA and recidivism remain roughly parallel and unchanged both before and after the reform. In Appendix Table A9, we vary the time-windows for FTA and recidivism. There again, we find very small and insignificant coefficients.[34]

As seen in Table 3, the No-Cash-Bail policy led to a decrease in supervision as well as in cash bail. If supervision has no effect, or has an opposite-signed effect to cash bail, an analysis that jointly measures the impact of both could be misleading. We use several strategies to better isolate the impact of cash bail. First, we conduct subgroup analysis on groups which experienced differential shocks. Table 5 breaks out eligible offenses into those that, prior to the reform, were less/more likely to get supervised release.[35] For offense categories shown in Panel A, the No-Cash-Bail reform led to a large decrease in the use of cash bail, but no detectable change in the likelihood of pretrial supervision. For offense categories shown in Panel B, the policy led to a large decline in the use of pretrial supervision and a small decline in the use of cash bail. The estimates shown in Panel A provide more direct evidence that cash bail does

speak to counsel about their case.

[30]The defendant may also see monetary bail as a 'price' that has been set for failing to appear. As discussed in the literature on fines as prices (Gneezy and Rustichini, 2000), the defendant may now feel that they have permission to skip the court appearance as long as they're willing to pay the price in terms of forfeited bail.

[31]Again, Appendix Table A4 shows the β and λ coefficients from equation 1.

[32]Columns 5-8 of Appendix Table A5 present a series of robustness tests, similar to those presented in section III.A., which yield very similar results.

[33]Our inquiry is motivated by concerns that bail reform will have adverse consequences. Consistent with this inquiry, we use a one-sided test to identify the magnitude of increase that can be rejected.

[34]In Appendix Table A8, we present regression discontinuity in time estimates for eligible offenses, following Calonico et al. (2014). We use time as the running variable – approach whose merits and limits relative to more classic version of running variables have been discussed by Hausman and Rapson (2018). Our results are similar with this approach – we find no detectable effect of the No Cash Bail reform on court compliance. The slight difference in point estimates could be driven by seasonality in recidivism and failures to appear in court, since the "pre" period is from September to February, and the "post" period is February to July, and spring and summer have higher misconduct.

[35]Pretrial supervision is not equally used for all offenses – it is somewhat common (around 11% of cases get supervised release) for drug and property crimes, but not for other types of offenses (less than 2%).

not improve court compliance.[36] The point estimates on misconduct remain small, mostly negative, and mostly statistically insignificant. We provide more evidence on the impact of cash bail separate from pretrial supervision in Subsection C.

Thus far we have shown that the No-Cash-Bail policy led to a sharp increase in the ROR rate with no evidence of an effect on the likelihood of being detained pretrial. This provides an opportunity to directly test the impact that ROR has on defendant misconduct among released defendants – a topic on which there is little empirical research, in spite of the prevalence of defendants released with monetary or supervisory conditions. To do so, we use an instrumental variables difference-in-differences approach to provide new evidence on this question.[37] Specifically, we use the differential impact that the No-Cash-Bail policy had on eligible defendants as an instrument for ROR. Note that most of the identifying assumptions for the IV method are the same as those for difference-in-differences. The exclusion restriction requires our instrument to potentially affect pretrial misconduct only through changes in pretrial conditions of release. It could be violated in particular if there were contemporaneous policy changes that would have affected the treatment and control group; and/or if the policy changes had affected other things than conditions of pretrial release. However, we have already explained that there were no other policy changes at the charging level over our study period; and we demonstrated that the No-Cash-Bail policy did not appear to influence arrest or charging practices and that there were no other confounding events at that time.

The additional assumptions necessary for an IV specification are that the No-Cash-Bail policy did not affect misconduct through channels other than bail. In Table 2, we show that the policy did not affect pretrial detention rates, which is one of the most obvious potential violations of the exclusion restriction. Furthermore, bail hearings are very short (a few minutes long), yielding few opportunities for magistrate behavior to affect misconduct through channels other than bail. Theoretically, awareness of the policy could have an effect on defendant behavior by fostering greater trust in the criminal justice system. We cannot rule this out, but if this indirect channel existed, we expect it to have a relatively small impact compared to the more direct incentive effects. Furthermore, it arguably would have similar effects across treatment and control group.

Our first and second stage equations are listed below, where Y_i is FTA or recidivism of defendant i and all other variables are as described previously.

$$ROR_i = \alpha + \delta Post_i * Eligible_i + \beta_1 Post_i + \lambda_1 Eligible_i + \theta_1 X_i + \epsilon_i \qquad (3)$$

[36] Again, these results are not driven by our modeling choices. In Panel B of Appendix Table A8, we present regression discontinuity in time estimates for eligible offenses that experienced the largest change in cash bail. Our results are very similar.

[37] In the crime context, this approach has for example been used by Draca et al. (2011).

$$Y_i = \alpha_2 + \gamma \widehat{ROR_i} + \beta_2 Post_i + \lambda_2 Eligible_i + \theta_2 X_i + \psi_i \quad (4)$$

Note that the IV estimates could be recovered by dividing the difference-in-differences for misconduct by those for ROR; but there are two advantages to computing them directly. First, this allows us to compute confidence intervals. Second, we are able to leverage differences across bail magistrates to increase the precision of our estimates. Our second IV estimates use an alternative specification that exploits the fact that magistrates respond differently to the reform, a phenomenon that is discussed in more detail in the next section. This adds some power to our estimates. We use LASSO regression to identify which magistrates have a meaningfully different response to the No-Cash-Bail reform. We find that only Magistrate 1's response differs meaningfully from the others.[38] The modified specification thus adds $Post_i * Eligible_i * Magistrate\_1_i$ in the instruments and $Magistrate\_1_i$, $Post_i * Magistrate\_1_i$, and $Eligible_i * Magistrate\_1_i$ as controls in both stages. For this strategy to be valid, we must make a partial monotonicity assumption (Mogstad et al., 2019) – that is, we assume that people are more likely to get ROR if $Post_i * Eligible_i = 1$ or if $Post_i * Eligible_i * Magistrate\_1_i = 1$.

Results are shown in Table 6. Columns 1 and 2 show the first instrumental variables results, identifying the local average treatment effect for compliers in our analysis. The point estimates are negative. At the 5% level, we find that ROR leads to at most a 9 percentage point increase in FTA and does not increase recidivism. Columns 3 and 4 show the instrumental variables results which allow for heterogeneous magistrate response. In this specification, at the 5% level, we show that ROR leads to at most a 3.9 percentage point increase in FTA and a 3.2 percentage point increase in recidivism.[39]

Again, this method is capturing the joint effect of pretrial supervision and cash bail. To better isolate the impacts of cash bail versus ROR, we rerun our IV specifications solely on the subsample of defendants who saw a large change in cash bail but no change in pretrial supervision, as described in Panel A of Table 5. Results are shown in Panel B of Table 6. Across specifications, our point estimates are negative. At the 5% level, we are able to reject the hypothesis that the elimination of cash bail leads to more than a one or two percentage point increase in FTA or recidivism.

Our estimates are most consistent with the claim that monetary bail leads to an increase in misconduct, which could be due to the destabilizing effects of monetary involvement, or to the 'distrust effect' of the bail process. However, the standard errors don't allow us to say this definitively. We can state, with more confidence, that

[38] We discuss and justify this approach further in section III.C.

[39] Evaluating the relative change with a binary outcome is problematic as the mean outcome approaches 0 or 1, since a simple reframing of the outcome as its inverse (1-X instead of X) can flip the interpretation from a large relative effect to a small one.

monetary and supervisory conditions were not necessary to ensure appearance for the large majority of those evaluated. This is important, because ensuring appearance is the primary justification for the use of monetary bail.

So, why doesn't cash bail appear to deter misconduct among those evaluated? One possible reason is that the amount of monetary penalty was not large enough to deter defendants. That is, it could be the case that higher bail amounts deter FTA but monetary penalties under $5,000 do not. We find this not fully convincing given the indigence of defendants in Philadelphia. Among eligible defendants in the pre-period, 50% of defendants lived in a zip code where the median income is less than $30,000, and 75% were poor enough to qualify for a public defender. The mean bail amount for eligible defendants in the pre-period with secured monetary bail under $5,000 was $3,750; and $4,900 for eligible defendants in the pre-period who got unsecured monetary bail. Even if this is not the amount that defendants have to post to avoid pretrial detention, this is the amount that defendants would be liable for if they fail to appear in court. Thousands of dollars of bail – and hundreds of dollars in the bail deposit – are likely a meaningful sum for these individuals (Harris et al., 2010).

Another possibility could be that defendants don't think that this court debt would be collected, and thus discount it. However, there have been moments in recent history when Philadelphia hired debt collection agencies to aggressively pursue court debt, creating threats, hassle, and damage to credit.[40] Failure to pay court debt can also result in criminal penalties, including incarceration.[41]

Note that the most policy-relevant question is whether low monetary bail provides meaningful *marginal* incentive on top of other criminal justice penalties that exist. FTA is a crime – not just in Philadelphia, but in most jurisdictions. A person who fails to appear in court receives a bench warrant and, if convicted of this crime, may be punished with fines or even incarceration. Our results suggest that these threats provide sufficient incentive, at least for the type of person who might respond to cash bail. Crime policy focused on incentives and deterrence only work for defendants who are aware of and paying attention to the consequences of their choices. This may not be the case for all defendants, many of whom are young and may lack skills in managing time and attention. Consistent with this theory, Fishbane et al. (2020) find that simple interventions like increasing the salience of the court date on a citation and sending text message reminders decreased failures-to-appear by 13% and 21%, respectively. Taken together, these results suggest that interventions that marginally increase incentives

[40]See for example https://www.marketplace.org/2012/12/20/philadelphia-collects-court-debt-decades-later. Note that ultimately, advocates had success in getting FTA related debt forgiveness in 2015. https://clsphila.org/employment/bail-forfeiture/

[41]News reports from other jurisdictions may also have created fear and uncertainty, such as the debates in Florida about whether it was necessary to pay off all court debt before a felony on the criminal record could be cleared – which has consequences on many aspects of a defendant's lives.

may not be as effective as interventions targeting inattention. Criminal justice policy could gain in efficiency by identifying the causes of misconduct, rather than assuming that it was the result of deliberate choice.

C. Generalizability

Even after the reform, 40% of eligible defendants were still being assigned monetary or supervisory conditions. Could these individuals be released without adverse consequences? While not definitive, we can provide suggestive evidence by exploiting a second natural experiment in Philadelphia. Defendants in Philadelphia are quasi-randomly assigned to magistrates who vary both in their pre-reform ROR rates and in their response to the No-Cash-Bail policy. Evaluating impacts across these heterogeneous magistrates provides a thought experiment in which we can speculatively infer what treatment effects would be like under a variety of different conditions.

This analysis is motivated by the idea that, ceteris paribus, magistrates are more likely to be lenient with those who have a low misconduct potential in the absence of restrictive conditions. As leniency expands, those with low misconduct potential are selected first, and the average misconduct potential of the pool of defendants still being assigned monetary or supervisory conditions rises. If this characterization is correct, then an expansion of ROR among previously-lenient magistrates should lead to greater misconduct than an expansion of ROR among previously-strict magistrates, simply because magistrates who are already lenient are more likely to have exhausted the pool of defendants with a low misconduct potential. However, if magistrates don't select primarily based on misconduct potential – either because they are following other objectives or because misconduct potential is hard to predict – then the effects of an increase in ROR should be similar across lenient and strict magistrates.[42]

The question of whether, or to what degree, magistrates are selecting defendants for ROR based on their misconduct potential (or, more precisely, their potential for misconduct in the absence of monetary or supervisory conditions) is central to questions about generalizability. If treatment effects are correlated with treatment assignment, as they would be if magistrates select defendants for ROR because they know there would not be adverse consequences of doing so, then the local average treatment effects we identify would not be broadly representative. Furthermore, the lack of adverse consequences from Philadelphia?s No-Cash-Bail reform could be attributed in part to the fact that it was discretionary. Thus, evaluating effects of an expansion in ROR by magistrates who vary in their initial level of leniency can provide suggestive evidence about the extent to which the estimates identified in our main IV specifications differ from the average treatment effect for eligible defendants. It thus provides suggestive

[42]In spirit, this is similar to the 'associative test' proposed by Gelbach (2021).

evidence about the effects of more comprehensive bail reform, as well as bail reform in other jurisdictions.

We exploit the fact that cases are randomly assigned to bail magistrates, conditional on the shift at which a that magistrate is working – a feature described in Stevenson (2018b) and in Dobbie et al. (2018). We confirm that this is the case in our setting as well.[43] Table 7 divides the sample into cases with bail set by each of six quasi-randomly assigned magistrates, and uses the difference-in-differences strategy to test impact by magistrate. Panels A-F show magistrate-specific results for ROR, pretrial detention, cash bail, pretrial supervision, FTA and recidivism, respectively. Magistrates are ranked across columns by use of ROR before the No-Cash-Bail policy, from lowest to highest usage.

First, an important descriptive fact. Magistrates vary greatly in their use of ROR. The strictest magistrate only granted ROR to 32% of cases before the reform, while the most lenient magistrate granted it to 62%. If ROR led to increased misconduct, we would expect to see higher average FTA and recidivism rates among the more lenient magistrates. We see no such thing. There is no clear association between ROR and misconduct rates (shown in the bottom row of each panel) across bail magistrates.

Second, note that even originally-lenient magistrates were able to increase ROR without detectable adverse consequences. If the originally-lenient magistrates were able to increase leniency without adverse consequences, this suggests that the pool of defendants that were receiving unnecessarily restrictive bail has not yet been exhausted. At a minimum, this suggests that stricter magistrates could increase ROR rates to the post-reform level of the lenient magistrates without increasing FTA/recidivism. Magistrate 1, for example, increased ROR rates by 30 percentage points with no evidence of adverse consequences.

In particular, note that Magistrates 5 and 6, the two most lenient magistrates before the reform, reduced their use of cash bail by 11 and 6 percentage points (respectively), but did not increase supervision rates. Similar to the sub-setting exercise shown in Table 5, this provides leverage to evaluate the impacts of cash bail separate from pretrial supervision. Consistent with our previous analysis, neither magistrate saw any detectable increase in FTA or recidivism.

More generally, the magistrate-level analysis discussed in this section suggests that there is not a strong correlation between treatment effects (the impact of ROR on misconduct) and the likelihood of receiving treatment (the likelihood of being granted ROR). This suggests that the local average treatment effect of bail (as identified by our IV) may not be substantially different from the average treatment effect. This

[43] We regress case and defendant characteristics on bail magistrates dummies (omitting one bail magistrates), whilst controlling for day of the week, shift, and quarter – which we include in our analyses. As shown in Appendix Table A10, we find that case characteristics are very similar across these six bail magistrates.

also suggests that discretion in implementation is unlikely to be credited for the fact that bail reform did not lead to an increase in FTA or recidivism. Magistrates do not appear to be leveraging private information to grant leniency only to those that don't require restrictive bail conditions to ensure compliance.

The magistrate-level analysis is not perfect. Our discussion above focuses on the point estimates, but standard errors are large enough to preclude firm conclusions. Nonetheless, we consider it generally supportive of the argument that many defendants in Philadelphia are still receiving unnecessarily restrictive bail. For other jurisdictions with low ROR rates, of which there are plenty (Mayson and Stevenson, 2020), there may also be a large pool of defendants that could be granted ROR without increasing FTA/crime.

The main objective of this sub-section is to understand how our results may generalize, leveraging quasi-random assignment of cases to magistrates who differ in their practices. But this analysis also raises a theoretical question: why do bail magistrates appear to be assigning unnecessarily restrictive conditions to so many defendants? We expect that this is due, in part, to asymmetric penalties in errors. A type II error is when a magistrate is too lenient, and as a result the defendant commits crime or fails to appear in court. This type of error is visible to the community and has negative consequences for the magistrate's reputation and career. Such errors are particularly salient when the defendant goes on to commit a serious crime, but are still relevant for more minor types of misconduct. A type I error occurs when a magistrate sets monetary bail or pretrial supervision when none is necessary to ensure compliance. This type of error is costly for the defendant, but it is invisible – it's impossible to say whether a particular defendant would have appeared to all court dates and refrained from reoffending if they had been released on recognizance. This asymmetry is sometimes referred to as the 'Willie-Horton effect', based on a high profile politicized case from the 1980s.[44] The implication is that magistrates will tend to err on the side of setting bail too high. This could lead to 'low-hanging fruit' in bail reform. In other words, asymmetric penalties for errors means that many defendants receive bail that is more restrictive than necessary to ensure compliance. One could eliminate restrictive bail conditions for this group with little adverse consequences.

Our analysis in this section has mostly focused on questions of generalizability within the sample of Philadelphia defendants. Can our results speak to bail reform in other jurisdictions? While we can't answer this definitively, we don't see any reason why Philadelphia is unique. In terms of bail setting practices, there are not huge

[44]Willie Horton was a prisoner who was allowed to go home on a weekend leave program in 1987. He absconded from this program and brutally raped a woman. His story was used extensively in political attack ads against presidential candidate Michael Dukakis, who had supported furlough programs as helping with rehabilitation.

differences between Philadelphia and the national average. Before the No-Cash-Bail policy, 79% of felony defendants in Philadelphia had secured monetary bail and 40% of these were detained until disposition. Nationally, 69% of felony defendants had monetary bail set, and 40% of these were detained until case disposition (Reaves, 2013).[45] Philadelphia's reform initiative (discretionary reform targeted at low-level offenses) is also similar to many initiatives across the country, as are basic pretrial practices. FTA is a crime in most, if not all, jurisdictions. The criminal justice penalties in place to deter FTA (most notably, an arrest warrant being issued for failing to appear in court) may already provide sufficient deterrence for those that respond to incentives. Similarly, the asymmetric incentives faced by magistrates are likely to be seen across many jurisdictions, suggesting that in the absence of public pressure towards leniency, magistrates may have been setting unnecessarily restrictive conditions.[46]

D. Differential leniency: racial disparities in implementation

In theory, discretionary reform allows decision-makers to take advantage of private information and allocate the benefits of reform to those for whom the misconduct potential is lowest. However, discretion may also lead to human biases in the allocation of benefits. Racial biases are of particular concern, given the massive racial disparities in bail, pretrial detention, and criminal justice more broadly (Ayres and Waldfogel, 1994; Arnold et al., 2018). In this section, we compare the characteristics of defendants selected to benefit from the reform to characteristics of the pool of potential beneficiaries – those who would have benefitted from the policy had its application been mandatory instead of discretionary. Our goal is to shed light on how magistrates select defendants for enhanced leniency in a discretionary reform.

In order to determine the type of defendants selected for leniency as a result of the No-Cash-Bail reform, we need to account for the pool of people who could potentially benefit from the policy. Heterogeneous treatment effects analysis – a common approach in program evaluation – is not appropriate in our context, since it ignores base rates in the pool of eligible defendants. To illustrate this, consider the following example.

[45] Classification practices could explain why rates in Philadelphia are slightly higher. Pennsylvania classifies offenses as misdemeanors if the sentence is less than five years; in most jurisdictions, misdemeanors only have sentences up to one year. Misdemeanor bail-setting figures are not available nationally.

[46] We provide one additional analysis related to generalizability questions in Appendix Table A11. This table subsets the eligible cases to include only people charged with a felony. (As a reminder, about 43% of eligible offenses and 69% of ineligible offenses are felonies.) Relative to ineligible cases, ROR rates doubled, (+17 percentage points), we find no decrease in pretrial detention, and no evidence of an increase pretrial misconduct. This suggests that even among more serious offenses, pretrial conditions of release did not affect misconduct.

Imagine the data consists of 100 white defendants and 100 black defendants who are eligible for the reform. Before the reform, 90 white defendants received ROR and 10 black defendants received ROR. Both groups saw a 10 percentage point increase in ROR as a result of the reform. A heterogeneous treatment effects analysis would say that both groups benefited equally, and that black defendants experienced a greater percent change in ROR relative to their pre-reform rates. However, white defendants were disproportionately selected for the benefit. That is because the pool of eligible defendants – those who were not already receiving ROR before the reform – was disproportionately (9/10) black. If defendants were selected for ROR out of the pool of eligible defendants in a manner that is orthogonal to race, there would have been, in expectation, 18 black defendants and 2 white defendants chosen. In other words, a selection mechanism that was uncorrelated with race would lead to highly heterogeneous treatment effects for Black defendants.

To identify what types of defendants benefited from the reform, we adapt a method from the instrumental variables literature: complier analysis. In this context, 'compliers' are those who received ROR solely because of the reform. In contrast, 'never-takers' (referred to here as 'never-ROR') are defendants who were eligible for the reform, but who did not receive increased leniency as a result of it. We use this language to be consistent with the IV literature, but we want to ward off a potential confusion: the active agents here are the bail magistrates, not the defendants. We call a group of defendants 'compliers' not because they chose to comply with the reform, but rather because magistrates complied with the reform in granting ROR to this group. The pool of defendants who could potentially benefit from the reform (those whose cases were eligible and who were not already receiving ROR before the reform) consist of compliers and the never-ROR group ('potential beneficiaries', henceforth).

We use the methodology developed by Jäger et al. (2019), which extends complier analysis methods (Imbens and Rubin, 1997; Abadie, 2003) to the context of difference-in-differences. In this method, the characteristics of the never-ROR group are estimated using eligible defendants who did not receive ROR after the reform. The characteristics of compliers are derived by examining differences between eligible defendants who received ROR after the reform and eligible defendants who received ROR before the reform, with adjustments based on the parallel trends assumption. This method is described in more detail in Appendix A.

Note that we are taking a slightly different approach from most applications of this method. Usually, complier analysis is used to determine how different the LATE is from the ATE. We use it to evaluate the differential impacts of a discretionary policy. When a decision-maker has discretion to comply or not-comply with the policy, complier analysis provides a useful tool to evaluate which type of person the decision-maker will

choose to be lenient with. This provides insight both into the decision-making process and the differential impacts of a policy.

Table 8 presents a comparison of characteristics of the beneficiaries of the reform (compliers) against the pool of potential beneficiaries. Column 1 shows average characteristics of beneficiaries, Column 2 shows the average characteristics of potential beneficiaries, and Column 3 shows the ratio of Columns 1 and 2. This ratio captures the disproportionate under- or over-representation of the characteristic among beneficiaries. In this last column, we include bootstrapped standard errors, based on 1000 replications. In the first three rows, we evaluate whether magistrates disproportionately select people of certain races/ethnicities for benefit. We find evidence that they do. While the pool of potential beneficiaries is 53% black, only 37% of those who benefited from the No-Cash-Bail policy were black. In contrast, Hispanics are slightly overrepresented among beneficiaries, and white defendants are substantially overrepresented. 32% of beneficiaries are white, compared to only 21% of potential beneficiaries. Note however that the standard errors are large, and we cannot reject equality of these ratios across groups, so this analysis is suggestive.

In the fourth row, we evaluate whether magistrates are disproportionately selecting people from wealthier neighborhoods. We find that beneficiaries tend to come from ZIP Codes with slightly higher poverty rates, although the difference is not very large.

In the last two rows, we consider whether magistrates are disproportionately selecting people with low predicted recidivism or FTA rates. To do so, we generate two separate risk assessments by taking the fitted values of regressions from recidivism and FTA (respectively) on case and defendant characteristics.[47] Race is not included as a predictor. We find that magistrates are selecting individuals with a slightly lower recidivism risk to be beneficiaries of the reform. Interestingly, the same does not hold true for FTA risk – the predicted FTA rates are identical.

In the previous section, we showed that magistrates may not be selecting individuals for ROR based on the likelihood that ROR would lead to misconduct. The analysis presented here is slightly different – we evaluate whether magistrates are selecting individuals for ROR based on their *likelihood* of misconduct, not the likelihood that ROR would *increase* misconduct. When it comes to FTA risk, however, both analyses tell a similar story: magistrates do not prioritize FTA risk in setting bail. This casts one of the potential benefits of discretionary reform in doubt, particularly, the claim that discretion enabled more efficient criminal justice reform by leveraging individualized

[47]Predictors include whether the defendant was charged with a felony, age at time of arrest, gender, prior charges, prior FTAs, day and time of the bail hearing, magistrate fix effects, whether the defendant had a public defender, and the current charge. To avoid mechanical incapacitation effects, our training data set excludes defendants who were detained pretrial. In our primary model this is defined as detained at least three days; results are similar if this is defined as detained until case disposition.

information about FTA risk. If magistrates pay little attention to FTA risk when setting bail, it seems unlikely that they can be credited with selecting only those individuals for whom ROR is not necessary to ensure appearance.

Evidence on the extent to which magistrates select based on FTA or crime risk also illuminates the racial disparities in selection. Such disparities cannot be explained by differences in FTA risk across races, since magistrates do not appear to select based on FTA risk at all. Magistrates do seem to be selecting beneficiaries based on recidivism risk, so the racial disparities in leniency could be because of assumptions the magistrate makes about recidivism risk. A third possibility is that magistrates are racially biased in their willingness to grant leniency. A few recent papers provide other examples in which discretion to 'give someone a break' is utilized differently for recipients of different races. Goncalves and Mello (2017b) show that police officers under-report the extent to which white drivers were speeding in order to reduce their fine, but were less likely to offer that same courtesy to black drivers. Chen and Philippe (2017) show that white judges sentence more leniently if the sentencing date falls on the defendant's birthday, but only for white defendants.[48]

III. Conclusion

We provide new evidence on discretionary bail reform by evaluating the impacts of the No-Cash-Bail policy in Philadelphia. This setting provides a unique opportunity to evaluate the main justification for the use of monetary bail: that it helps ensure appearance and prevent crime among released defendants. We find no evidence to support this, and can reject even small increases in FTA and rearrest. Prior to the reform, magistrates appear to have been setting unnecessarily restrictive conditions of release. This could be due to asymmetric penalties in errors: the consequences of setting bail too low are salient and costly from the perspective of the magistrate, whereas the consequences setting bail too high are less noticed.

Our paper makes several contributions to existing literature. First, we provide new insight about financial penalties: as a marginal incentive on top of other penalties,

[48]We provide one robustness test for the complier analysis, in which we drop defendants charged with possession with intent to deliver (PWID). Eligibility for PWID cases is measured imperfectly, since a variety of characteristics not fully captured in our data can make a PWID case ineligible for the reform. Thus, the results shown in Table 8 might be due to classification error rather than discretionary choices. In other words, perhaps some black defendants did not benefit because they were not eligible, not because they were not selected. Appendix Table A12 shows our complier analysis for non-PWID cases. Even in the sample where classification error is unlikely, we still see substantial race differences. The fraction of black defendants in the compliers category is one quarter that of the pool of potential beneficiaries. In this sample, there is again no difference in FTA risk, and only small differences in crime risks across the two groups. This lends support to the racial bias in leniency hypothesis. (Note that our main results on ROR, jail, FTA and recidivism remain unchanged when we drop PWID cases, as shown in Appendix Table A13.)

they may not serve as an effective deterrent. Second, our paper shows that prosecutors can influence practices they have no direct control over. This suggests that prosecutors can wield soft influence as norm-setters: an additional channel through which the progressive prosecutor movement may lead to systemic change. Third, our paper provides evidence on the role of discretion in criminal justice. Magistrate discretion did not appear to ameliorate adverse consequences of bail reform, at least in terms of FTA. It did, however, reduce the impacts of reform and lead to racial disparity in implementation. Lastly, our results provide practical information relevant to the current bail reform movement. It mitigates concerns about moving away from cash bail, and raises questions about current practices. If nonappearance in court has more to do with inattention than deliberate choice, then interventions targeted towards the root of the problem – such as court reminders – will be more effective than those that target incentives (Fishbane et al., 2020). Careful attention to the motivation of different actors can help in the design of effective policy.

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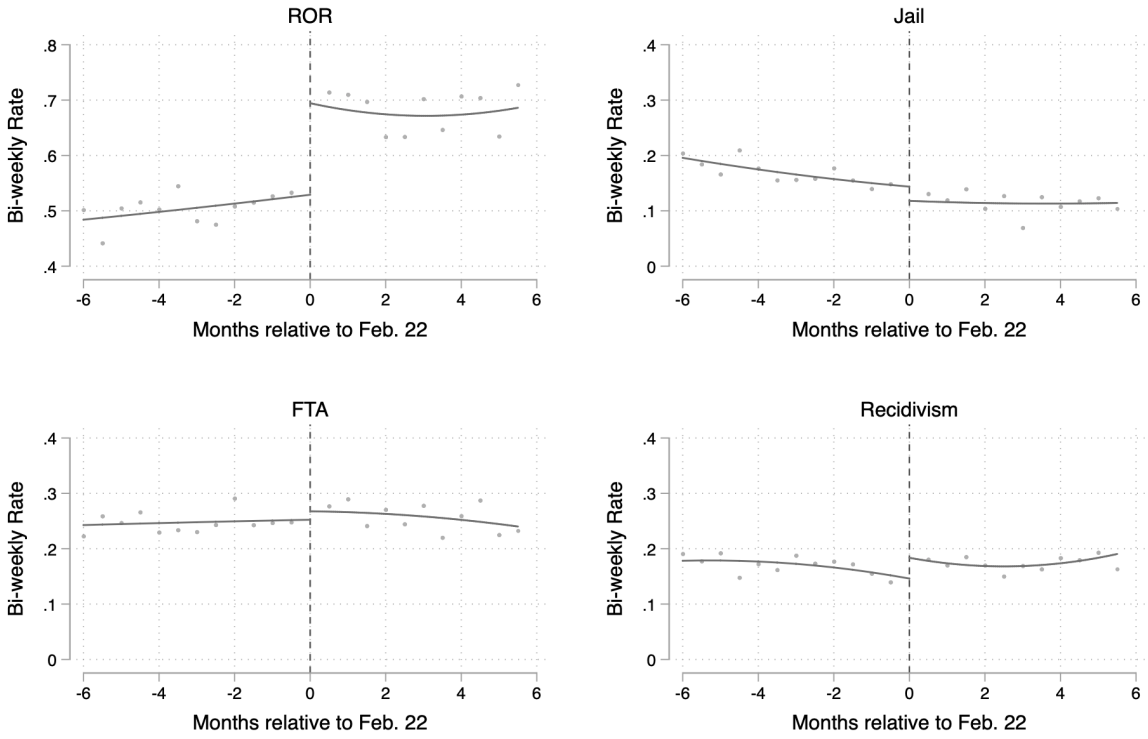
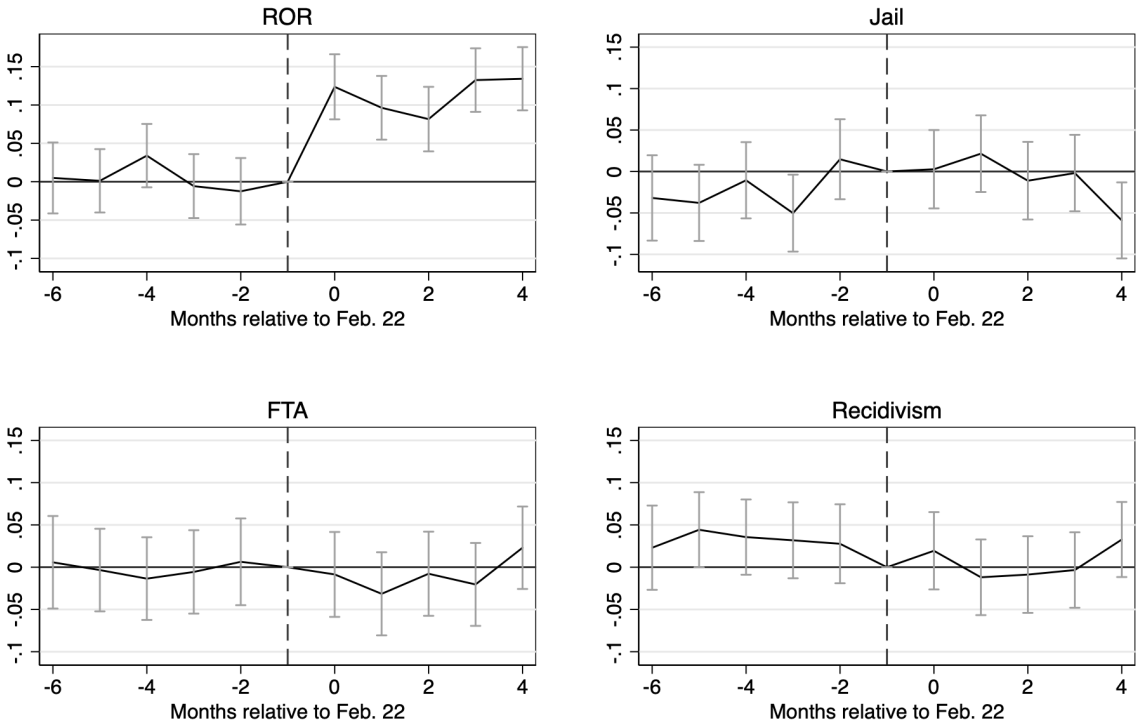
Figures and tables

Figure 1: Time trend in ROR, pretrial detention, FTA and recidivism for eligible cases



Note: Each dot represents the mean value in a two-week time period. The vertical line represents the Feb. 22 date of the No-Cash-Bail policy. The lines are quadratic fits, before and after Feb. 22. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. Data source: court dockets from the Pennsylvania Unified Judicial System.

Figure 2: Difference-in-differences estimates with leads and lags for how the No-Cash-Bail policy affected ROR, jail time, FTA and recidivism



Note: This figure plots the difference-in-difference coefficients obtained from estimating a single equation with monthly leads and lags (Equation 2), with the 95% confidence interval of the coefficient estimate. The treatment group is eligible cases and the control group is ineligible cases. The vertical dashed line indicates the month prior to Feb. 22. That month is left out as the comparison category. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. Data source: court dockets from the Pennsylvania Unified Judicial System.

Table 1: Descriptive statistics for cases before the No-Cash-Bail policy

	Eligible cases	***Ineligible cases***
Defendant characteristics		
Age	34.41	32.76
Male	0.84	0.83
Black	0.48	0.67
Hispanic	0.22	0.16
White	0.29	0.16
Median household income in zip code	37847	35401
Percent below poverty in zip code	26.46	26.49
Public defender	0.75	0.68
Felony	0.45	0.70
Has a prior FTA	0.18	0.13
Has a past conviction	0.57	0.53
Has a past felony conviction	0.27	0.39
Pre-trial conditions		
ROR	0.51	0.08
Supervised release	0.06	0.01
Unsecured monetary	0.07	0.07
Secured bail up to 5000	0.16	0.27
Secured bail over 5000	0.20	0.56
Denied bail	0.00	0.01
Jail (3+ nights)	0.17	0.46
Misconduct		
FTA	0.25	0.10
Recidivism	0.17	0.11
Observations	7468	4281

This table presents descriptive statistics for cases filed during the six months before the No-Cash-Bail policy. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. All variables are dummies except age and median household income. Data source: court dockets from the Pennsylvania Unified Judicial System.

Table 2: Difference-in-difference estimates of the effect of No-Cash-Bail policy on ROR and jail

	ROR		Jail	
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	0.12***	0.11***	0.0079	0.0072
	(0.028)	(0.021)	(0.021)	(0.016)
Controls	No	Yes	No	Yes
Mean Dep. Var.	0.505	0.505	0.169	0.169
N	22589	22589	22589	22589

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. Odd columns don't include controls; even columns do. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. ROR means released on own recognizance. Jail refers to being detained pretrial for at least 3 nights after the bail hearing. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table 3: Difference-in-difference estimates of the effects of the No-Cash-Bail policy on initial bail type

	Supervised release	Unsecured Monetary	Secured Under 5,000	Secured Over 5,000
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	-0.041*	-0.021*	-0.046*	-0.0070
	(0.019)	(0.0077)	(0.019)	(0.024)
Controls	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.061	0.069	0.164	0.200
N	22589	22589	22589	22589

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. Secured bail requires the payment of a deposit before release; unsecured bail does not. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table 4: Difference-in-difference estimates of the effect of No-Cash-Bail policy on FTA and recidivism

	FTA		Recidivism	
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	-0.0076	-0.0084	-0.017	-0.019
	(0.011)	(0.011)	(0.012)	(0.011)
Controls	No	Yes	No	Yes
Mean Dep. Var.	0.247	0.247	0.171	0.171
N	22589	22589	22589	22589

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. Odd columns don't include controls; even columns do. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table 5: Breaking out the effects of cash bail and supervised release

	ROR	Jail	Supervised release	Cash	FTA	Recidivism
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cash bail						
Eligible*Post 02/21	0.12***	-0.0085	-0.0039	-0.11***	-0.023*	-0.016
	(0.027)	(0.019)	(0.0038)	(0.026)	(0.0094)	(0.0091)
Mean Dep. Var.	0.287	0.271	0.018	0.695	0.202	0.149
N	16116	16116	16116	16116	16116	16116
Panel B: Supervised release						
Eligible*Post 02/21	0.10***	0.027	-0.087***	-0.014	0.0088	-0.024
	(0.025)	(0.017)	(0.011)	(0.029)	(0.012)	(0.020)
Mean Dep. Var.	0.760	0.049	0.113	0.127	0.300	0.197
N	14817	14817	14817	14817	14817	14817
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents difference-in-differences estimates of δ in Equation 1. In both Panels A and B, ineligible offenses are the control group. In Panel A, the treatment group is eligible offenses that most often get cash before the No-Cash-Bail reform and rarely get pretrial supervision (crimes other than drug or property). In Panel B the treatment group is eligible offenses that are more likely to get pretrial supervision and less likely to get cash bail before the No-Cash-Bail reform (drug and property crimes). Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases within each panel before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table 6: IV estimates of the effects of ROR on FTA and recidivism

	IV		Magistrate IV	
	(1) FTA	(2) Recidivism	(3) FTA	(4) Recidivism
Panel A: Full sample				
ROR	-0.073	-0.17	-0.099	-0.11
	(0.100)	(0.094)	(0.084)	(0.088)
Upper bound	0.091	-0.014	.039	0.032
Mean Dep. Var.	0.247	0.171	0.247	0.171
N	22589	22589	22589	22589
First-stage F-stat	27.4		20.7	
Panel B: Cash Bail				
ROR	-0.19	-0.13	-0.20	-0.13
	(0.11)	(0.070)	(0.11)	(0.076)
Upper bound	0.016	0.0054	0.013	0.020
Mean Dep. Var.	0.202	0.149	0.202	0.149
N	16116	16116	16116	16116
First-stage F-stat	65.5		43.5	
Controls	Yes	Yes	Yes	Yes

Note: This table presents IV estimates in which we use $Post_i * Eligible_i$ as an instrument for the change in ROR (release on recognizance). In Columns 3 and 4 we also include interactions of $Post_i * Eligible_i$ with a dummy for having one's case examined by Magistrate 1, who had by far the biggest change in ROR, as shown in Table 7. Panel A presents results for the full sample. In Panel B, we subset eligible offenses to only include offenses that are most likely to have had cash bail (and not pretrial supervision) before the No-Cash-Bail reform, as defined in Table 5. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Difference-in-difference estimates of the effect of No-Cash-Bail policy, by bail magistrate

	Bail magistrate					
	1	2	3	4	5	6
Panel A: ROR						
Eligible*Post 02/21	0.30***	0.020	0.085***	0.12***	0.11***	0.064**
	(0.023)	(0.020)	(0.020)	(0.023)	(0.023)	(0.022)
Mean Dep. Var.	0.324	0.445	0.490	0.543	0.599	0.621
Panel B: Jail						
Eligible*Post 02/21	-0.044	0.044	0.0030	0.012	0.0057	-0.0081
	(0.028)	(0.026)	(0.025)	(0.023)	(0.026)	(0.026)
Mean Dep. Var.	0.200	0.171	0.159	0.175	0.154	0.155
Panel C: Cash						
Eligible*Post 02/21	-0.15***	-0.020	-0.039*	-0.070**	-0.11***	-0.064**
	(0.022)	(0.020)	(0.016)	(0.022)	(0.023)	(0.022)
Mean Dep. Var.	0.516	0.555	0.416	0.359	0.396	0.379
Panel D: Supervision						
Eligible*Post 02/21	-0.15***	-	-0.046***	-0.053***	-0.0041	-
	(0.015)	-	(0.014)	(0.014)	(0.0027)	-
Mean Dep. Var.	0.160	0.000	0.094	0.098	0.004	0.000
Panel E: FTA						
Eligible*Post 02/21	-0.036	-0.026	0.00070	0.019	-0.013	-0.037
	(0.025)	(0.023)	(0.023)	(0.021)	(0.023)	(0.024)
Mean Dep. Var.	0.171	0.185	0.176	0.160	0.159	0.194
Panel F: Recidivism						
Eligible*Post 02/21	-0.0085	0.0077	0.036	-0.072**	-0.037	-0.071**
	(0.026)	(0.025)	(0.024)	(0.023)	(0.025)	(0.026)
Mean Dep. Var.	0.165	0.170	0.154	0.189	0.180	0.172
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	3187	3922	3586	3449	3287	4096

Note: This table presents estimates of δ in Equation 1, separately for each of the six bail magistrates who saw more than 100 cases in 2017. Eligible cases are the treatment group, and ineligible cases are the control group. Each panel presents a different outcome, specified at the top of that panel. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. Cash is giving a defendant cash bail as a condition of release – either secured or unsecured. Supervision is giving a defendant pretrial supervision. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table 8: Comparing the characteristics of beneficiaries and of potential beneficiaries of the No-Cash-Bail reform

	Beneficiaries (compliers)	Potential beneficiaries (compliers + never-ROR)	Ratio: beneficiaries / potential beneficiaries
	(1)	(2)	(3)
Black	0.37	0.53	0.69
			(0.28)
Hispanic	0.28	0.25	1.09
			(0.40)
White	0.32	0.21	1.57
			(0.51)
Percent below poverty	0.23	0.27	0.85
			(0.13)
Predicted recidivism	0.15	0.19	0.76
			(0.09)
Predicted FTA	0.27	0.27	0.99
			(0.07)

Note: This table presents the average characteristics of compliers, who are the beneficiaries of the No-Cash-Bail policy, to that of all potential beneficiaries of the No-Cash-Bail policy, which to say compliers of never-ROR. Calculations follow the methodology outlined in appendix A. In Column 3, we report standard errors (in parentheses) based on 1000 bootstrap replications.

For Online Publication

A Determining complier characteristics

We present a brief overview of our estimation procedures to determine complier characteristics, which is based off the approach developed in Jäger et al. (2019).

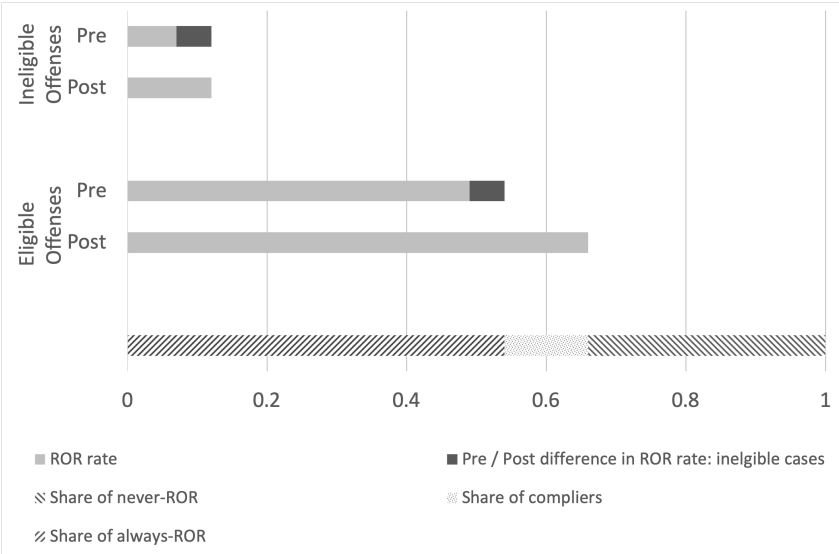
Notations. $R \in \{0, 1\}$ indicates whether a defendant got ROR or not. $T \in \{t_1, t_0\}$ indicates whether the offense if eligible or not; and $P \in \{p_1, p_0\}$ indicates whether the case is in the post period (i.e. after the No-Cash-Bail reform) or not. $Z \in \{0, 1\}$ indicates whether a defendant could potentially benefit from the No-Cash-Bail reform, which depends on whether that defendant's offense is eligible, and whether his case is heard after the No-Cash-Bail reform. $Z = 1$ for (t_1, p_1), and $Z = 0$ for (t_1, p_0), (t_0, p_1) and (t_0, p_0). R_0 and R_1 denote the potential values of R for $Z = 0$ and $Z = 1$.

- Compliers have potential outcomes $R_0 = 0, R_1 = 1$
- Never-ROR have potential outcomes $R_0 = 0, R_1 = 0$
- Always-ROR have potential outcomes $R_0 = 1, R_1 = 1$

Overview. We are interested in comparing the characteristics of compliers to the characteristics of never-ROR, to understand towards what kinds of defendants criminal justice actors are willing apply greater leniency, and for whom they are not. The estimation procedure has two steps:

1. Estimate the proportion of compliers, never-ROR, and always-ROR among eligible cases in the post-period.
2. Estimate the average of variables of interest among compliers and never-ROR among eligible cases in the post-period.

Step 1: Estimating the proportion of each group The figure below illustrates how we define the shares of always-ROR, never-ROR and compliers.



The first part of the figure shows the ROR rates for **ineligible** cases, before and after the No-Cash-Bail reform. The darker part of the bar shows the difference in ROR between the Pre and the Post period for ineligible cases.

The second part of the figure shows the ROR rates for **eligible** cases, before and after the No-Cash-Bail reform. The darker part of the bar reports the difference in ROR between the Pre and the Post period for **ineligible** cases.

The share of always-ROR is made up of defendants who get ROR among eligible cases in the Pre periods; adjusted for the difference between the Pre / Post period of ineligible cases. This captures our parallel trends assumption for difference-in-differences: in the absence of the No-Cash-Bail reform, we assume that eligible cases would have experienced the same change in ROR as ineligible cases. The remaining RORs for eligible cases in the Post period are for compliers. Lastly, never-RORs are the kinds of defendants who did not get ROR even when they have eligible offenses and are in the post-period.

Formally, we can derive the proportion of compliers, always-ROR and never-ROR by using the following difference-in-differences equation:

$$R_i = \alpha + \beta P_i + \delta P_i * T_i + \lambda T_i + \epsilon_i$$

- Compliers have potential outcomes $R_0 = 0, R_1 = 1$. Their proportion, π_c, is $\hat{\delta}$.
- Always-RORs have potential outcomes $R_0 = 1, R_1 = 1$. Their proportion, π_a, is $\hat{\alpha} + \hat{\beta} + \hat{\lambda}$.
- Never-RORs have potential outcomes $R_0 = 0, R_1 = 0$. Their proportion, π_n, is $1 - \hat{\alpha} - \hat{\beta} - \hat{\delta} - \hat{\lambda}$.

Step 2: Estimating average characteristics within each group

Now that we know what portion of eligible defendants in the post-period are in each group, we can estimate their average characteristics in the post-treatment period. For the never-ROR group, this is straightforward: it is the average characteristics of eligible defendants who **did not** get ROR in the post-period. (By monotonicity, we assume that eligible defendants who did not receive ROR in the post-treatment period would also not have received ROR in the pre-treatment period.)

$$E[x|p_1, t_1, R_1 = 0, R_0 = 0] = E[x|p_1, t_1, R_1 = 0]$$

For the always-ROR group, we identified the average characteristics of eligible defendants who **did** get ROR in the pre-treatment period, adjusting for time trends in characteristics using Pre/Post changes in the ineligible group, and again relying on the monotonicity assumption. This adjustment (the second term below) derives from the parallel trends assumption.

$$E[x|p_1, t_1, R_0 = 1] = E[x|p_0, t_1, R_0 = 1] + (E[x|p_1, t_0, R_0 = 1] - E[x|p_0, t_0, R_0 = 1])$$

.

This gives us all the ingredients we need to estimate average characteristics of compliers. We know that eligible defendants who received ROR in the post-period are a mix of compliers and always-ROR. From Step 1, we know what portion of these

defendants are compliers, and what portion are always-ROR. Simple algebra yields the average characteristics of compliers.

The average characteristics of defendants for who get ROR if $Z = 1$ can be decomposed as follows:

$$\begin{aligned} E[x|p,t,R_1=1] =& E[x|p,t,R_1=1,R_0=1] * P(R_0=1|p,t,R_1=1) \\ &+ E[x|p,t,R_1=1,R_0=0] * P(R_0=0|p,t,R_1=1) \end{aligned}$$

Rearranging terms to isolate the average characteristics of compliers:

$$\begin{aligned} E[x|p,t,R_1=1,R_0=0] =& \frac{1}{P(R_0=1|p,t,R_1=1)} E[x|p,t,R_1=1] \\ &- \frac{P(R_0=0|p,t,R_1=1)}{P(R_0=1|p,t,R_1=1)} E[x|p,t,R_0=1,R_1=1] \end{aligned}$$

By monotonicity, $E[x|p,t,R_0=1,R_1=1] = E[x|p,t,R_0=1]$ and so

$$\begin{aligned} E[x|p,t,R_1=1,R_0=0] =& \frac{1}{P(R_0=1|p,t,R_1=1)} E[x|p,t,R_1=1] \\ &- \frac{P(R_0=0|p,t,R_1=1)}{P(R_0=1|p,t,R_1=1)} E[x|p,t,R_0=1] \end{aligned}$$

Using Bayes rule, this can be re-expressed in terms of the shares of always-ROR, π_a, and compliers, π_c:

$$E[x|p,t,R_1=1,R_0=0] = \frac{\pi_c + \pi_a}{\pi_c} E[x|p,t,R_1=1] - \frac{\pi_a}{\pi_c} E[x|p,t,R_0=1]$$

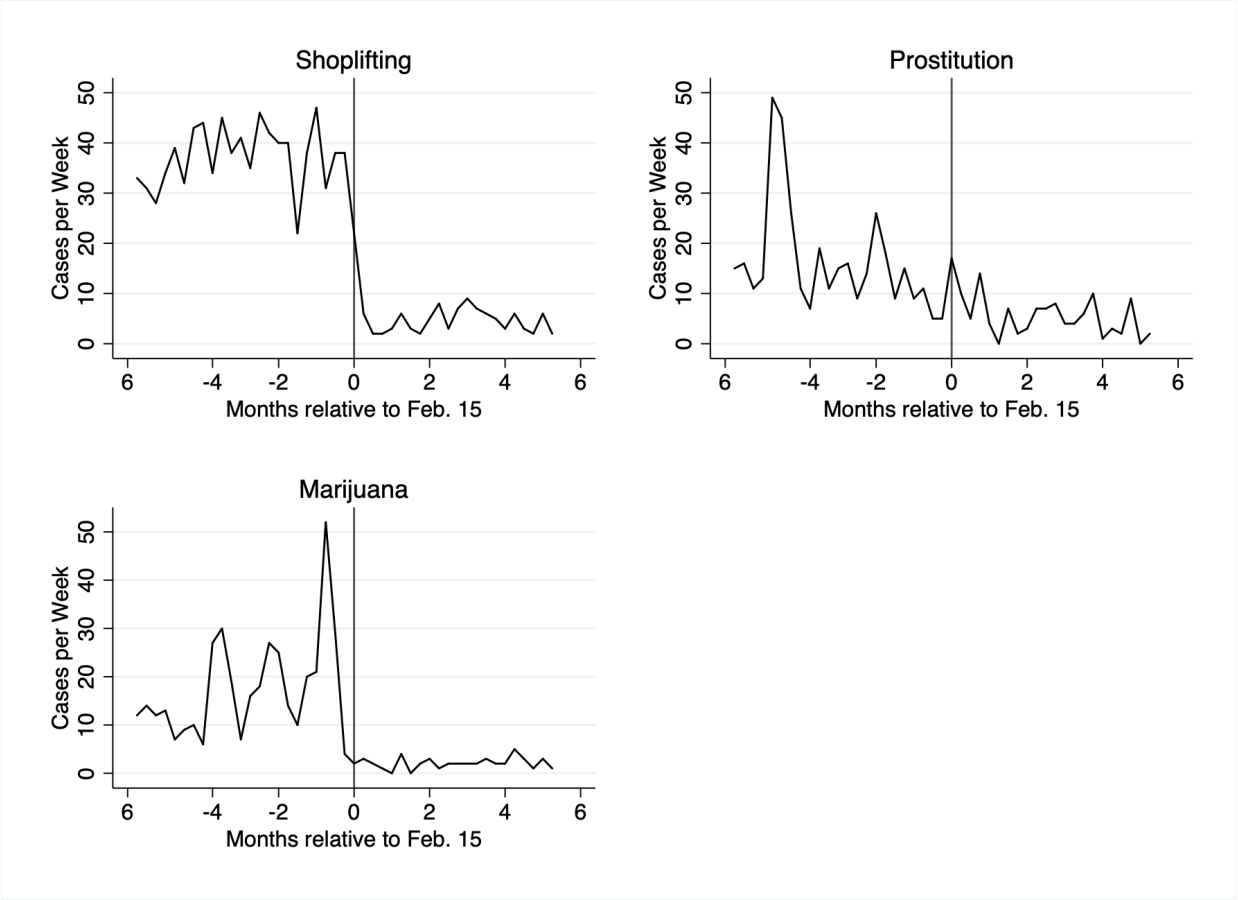
Empirically, we can calculate this for $p = 1$ and $t = 1$. The values $E[x|p_1,t_1,R_1=1]$ and $E[x|p_1,t_1,R_0=1]$ can be derived with observational data:

$$E[x|p_1,t_1,R_1=1] = E[x|p_1,t_1,R=1,Z=1]$$

And the average characteristics of always-ROR in the post-treatment period, $E[x|p_1,t_1,R_0=1]$, was defined above.

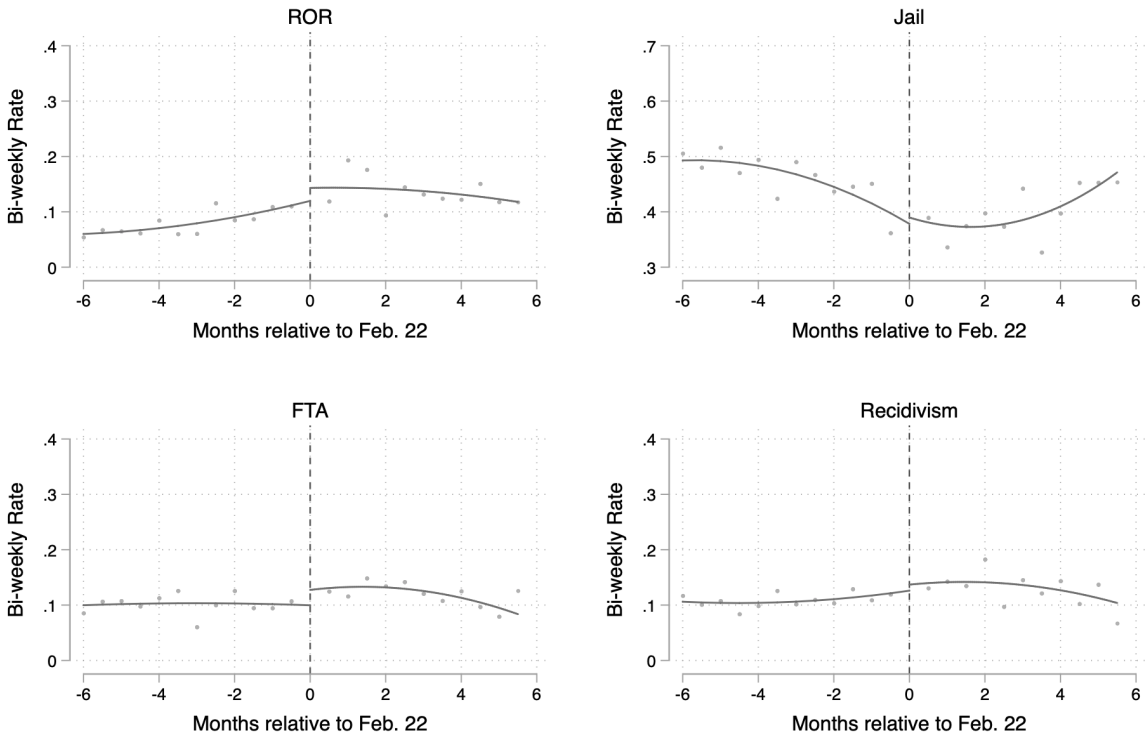
B Additional figures and tables

Figure A.1: Changes in the weekly number of cannabis, prostitution and shoplifting cases after the Feb. 15th announcement not to prosecute these kinds of cases anymore. For all of our analyses, we drop these cases.



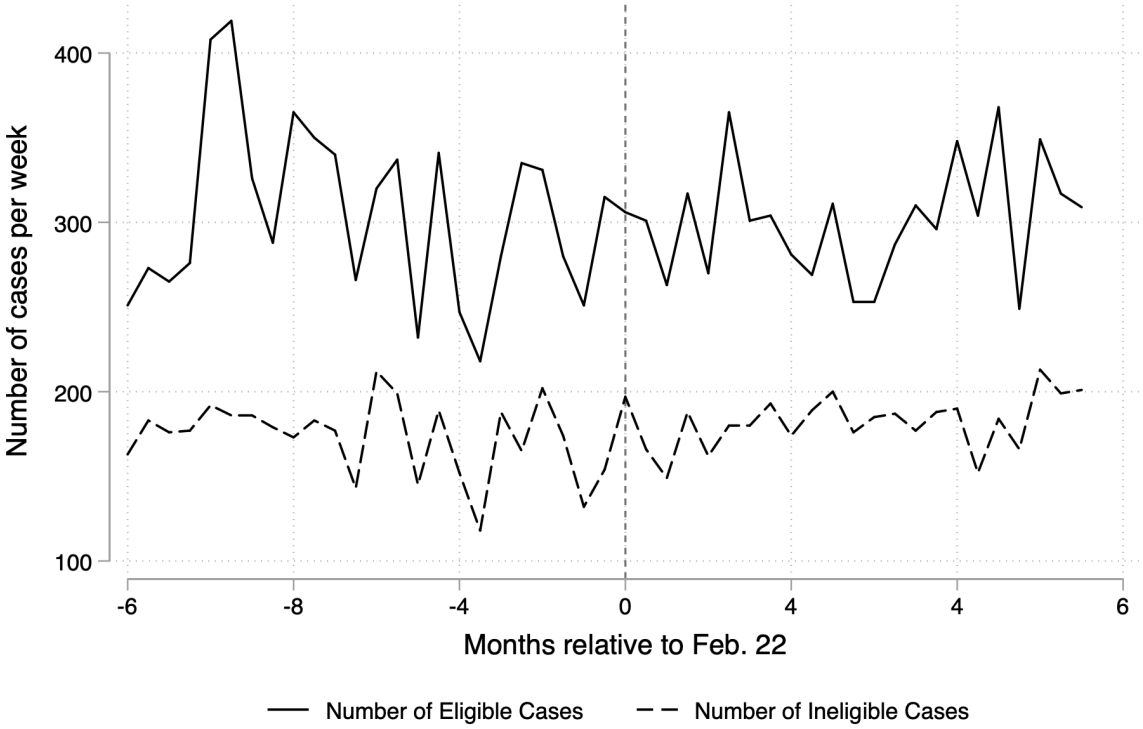
These figures document a decline in the number of cases filed for shoplifting, prostitution and marijuana offenses that occurred due to changes in prosecutorial policy around the same time as the No-Cash-Bail reform. We drop these cases from our analysis. Data source: court dockets from the Pennsylvania Unified Judicial System.

Figure A.2: Time trend in ROR, pretrial detention, FTA and recidivism for ineligible cases



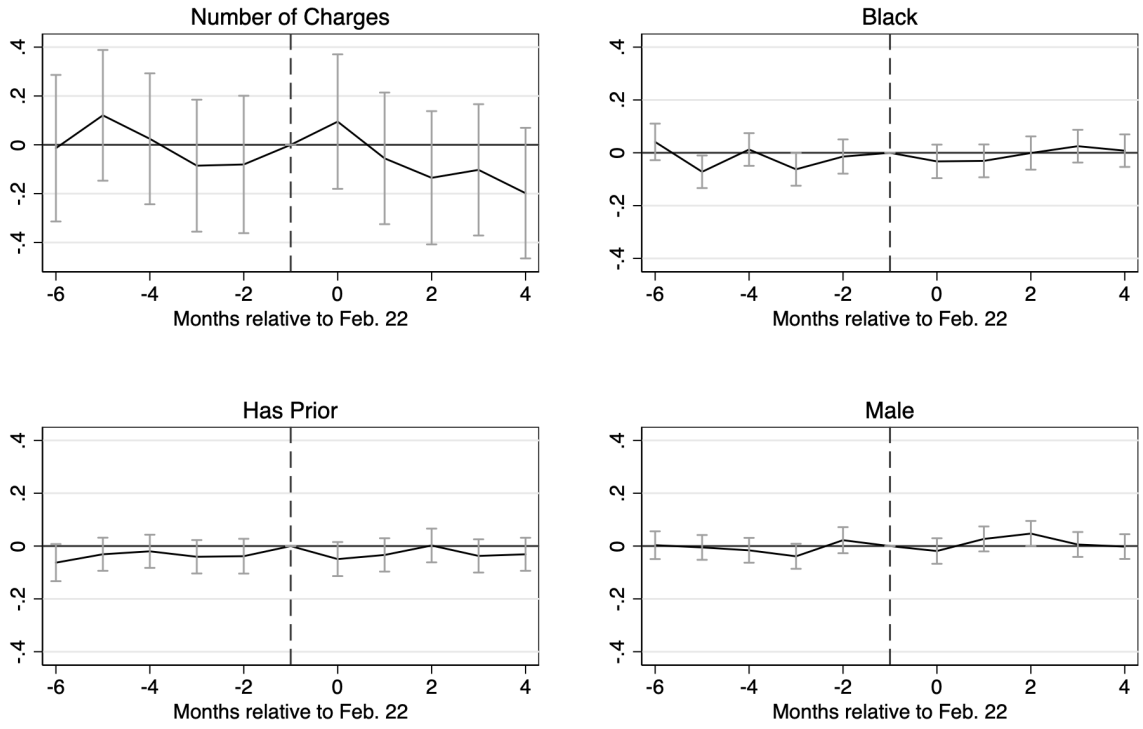
Note: Each dot represents the mean value in a two-week time period. The vertical line represents the Feb. 22 date of the No-Cash-Bail policy. The lines are quadratic fits, before and after Feb. 22. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. Data source: court dockets from the Pennsylvania Unified Judicial System.

Figure A.3: Trends in the weekly number of eligible and ineligible cases s



This figure presents trends in the weekly number of cases filed that were eligible for the No-Cash-Bail reform as well as those that were ineligible. The vertical line represents the Feb. 22 date of the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System.

Figure A.4: Balance check: Difference-in-differences estimates with leads and lags for how the No-Cash-Bail policy affected case composition



Note: This figure plots the difference-in-difference coefficients obtained from estimating a single equation with monthly leads and lags (Equation 2), with the 95% confidence interval of the coefficient estimate. The treatment group is eligible offenses and the control group is ineligible offenses. The vertical dashed line indicates the month prior to Feb. 22. That month is left out as the comparison category. Data source: court dockets from the Pennsylvania Unified Judicial System.

Table A1: Offense category frequency for eligible and ineligible cases

	Panel A: Eligible Cases
Possession with intent to deliver (PWID)	0.28
Drug purchase	0.20
Drug possession	0.18
DUI	0.15
Theft	0.06
Burglary	0.04
Receiving stolen property	0.03
Other	0.03
Observations	7468
	Panel B: Ineligible Cases
Aggravated assault	0.20
Firearm violation	0.12
Robbery	0.10
Simple assault	0.09
Possession with intent to deliver (PWID)	0.09
Possession of weapon	0.06
Domestic violence	0.05
Other	0.17
Observations	4281

Note: This table shows the most frequent offense categories for eligible and ineligible cases. While PWID in general are eligible offenses, PWID cases for which the defendant had a PWID in the past 6 months are not eligible. Data source: court dockets from the Pennsylvania Unified Judicial System.

Table A2: Legal actors' responses to the No Cash Bail Policy: arrests and charging

	# arrests	% declined	% upcharged	% downcharged	# cases
	(1)	(2)	(3)	(4)	(5)
Eligible*Post 02/21	-15.2	0.011	0.0059	0.012	-10.8
	(16.1)	(0.0070)	(0.0050)	(0.0063)	(14.1)
Mean Dep. Var.	358	0.015	0.129	0.052	298
N	94	26926	26926	26926	94

Note: In Columns 1-4, we use Philadelphia arrest data. In Columns 3 and 4, a case is considered as "upcharged" if the police classified it as an ineligible offense and it was initially charged as an ineligible offense, and "downcharged" if a case brought in as an ineligible offense is charged as an eligible offense. In Columns 2-4, we use difference-in-difference estimates. In Columns 1 and 5, the data is collapsed to the weekly level, and estimations include quadratic time trends. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are in parentheses; they are clustered at the offense level in Columns 2-5.

Table A3: Testing for changes in case composition at time of reform

	Charges per case	Has Prior	Male	Black
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	-0.058	0.0054	0.020	0.0078
	(0.047)	(0.016)	(0.011)	(0.015)
Mean Dep. Var.	2.456	0.574	0.839	0.478
N	22589	22589	22589	22589

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. The outcomes are number of charges per case (Column 1), a dummy equal to 1 for having a prior (Column 2), for being male (Column 3) or for being Black (Column 4). 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A4: Difference-in-difference estimates of the effect of No-Cash-Bail policy on ROR, jail, FTA and Recidivism, showing coefficients on Post and Eligible Offenses

	ROR	Jail	FTA	Recidivism
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	0.11***	0.0072	-0.0084	-0.019
	(0.021)	(0.016)	(0.011)	(0.011)
Post 02/21	0.034	-0.045**	0.021	0.029***
	(0.019)	(0.015)	(0.014)	(0.0076)
Eligible Offense	0.070	-0.080	0.0091	-0.031
	(0.086)	(0.089)	(0.057)	(0.030)
Controls	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.505	0.169	0.247	0.171
N	22589	22589	22589	22589

Note: This table presents estimates of δ, β and λ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A5: Robustness checks

	ROR				FTA			
	Ever	12 weeks	Donut	Weekly	Total	12 weeks	Donut	Weekly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible*Post 02/21	0.11***	0.11***	0.11***	0.12***	-0.020	-0.025*	-0.0067	-0.0049
	(0.020)	(0.022)	(0.019)	(0.017)	(0.018)	(0.012)	(0.012)	(0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.512	0.515	0.505	0.510	0.330	0.253	0.246	0.249
N	22175	10680	20736	96	22175	10680	20736	96

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. Columns 1-4 present robustness tests for ROR, and Columns 5-8 present robustness tests for FTA. In Column 1, Ever ROR is a dummy equal to 1 if a person gets an ROR at any point during the pretrial period, instead of just the initial bail hearing. In Column 5, total FTA is the total number of FTA incidents, instead of the likelihood of having an FTA. Columns 2 and 6 limit our sample to 12 weeks before and after Feb. 21. Columns 3 and 7 exclude the week of the policy, and the weeks just before and after. Columns 4 and 8 are collapsed to one observation per week. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. ROR means released on own recognizance. FTA is failure to appear in court. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A6: Robustness checks: no clustering

	ROR		Jail		FTA		Recidivism	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible*Post	0.11***	0.11***	0.0072	0.0072	-0.0084	-0.0084	-0.019	-0.019
	(0.021)	(0.0090)	(0.016)	(0.010)	(0.011)	(0.011)	(0.011)	(0.0097)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Yes	No	Yes	No	Yes	No	Yes	No
Mean Dep. Var.	0.505	0.505	0.169	0.169	0.247	0.247	0.171	0.171
N	22589	22589	22589	22589	22589	22589	22589	22589

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. Standard errors are presented in parentheses. In the odd columns, they are clustered at the offense level; in the even columns, they are not clustered. FTA is failure to appear in court. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7: Difference-in-difference estimates of the effect of No-Cash-Bail policy on jail: different lengths of jail time

	Spent at least x nights in jail, with x equal to...						
	1	2	3	4	5	6	7
Eligible*Post 02/21	0.0049	0.0070	0.0074	0.013	0.014	0.015	0.013
	(0.017)	(0.017)	(0.016)	(0.015)	(0.016)	(0.016)	(0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.190	0.175	0.169	0.164	0.159	0.157	0.155
N	22589	22589	22589	22589	22589	22589	22589

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. The outcome in each column is being detained pretrial for at least x nights after the bail hearing – Column 1 is at least 1 night, Column 2 is at least 2 nights, and so on. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A8: Regression discontinuity in time estimates of the No-Cash-Bail policy, for eligible offenses

	(1) ROR	(2) Jail (3+ nights)	(3) FTA	(4) Recidivism
Panel A: Full sample				
Post 02/21	0.104***	-0.0356	0.0102	0.0394
	(0.0282)	(0.0204)	(0.0268)	(0.0227)
Mean Dep. Var.	0.531	0.154	0.255	0.160
Effective RD observations	3475	4437	4730	4633
Bandwidth for estimation	41	52	57	56
Bandwidth for bias	61	78	86	84
Panel B: Cash Bail				
Post 02/21	0.152**	-0.0428	-0.0327	0.0239
	(0.0494)	(0.0339)	(0.0365)	(0.0271)
Pre-reform mean	0.349	0.239	0.202	0.141
Effective RD observations	1810	2678	2375	3150
Bandwidth for estimation	38	58	50	68
Bandwidth for bias	57	87	82	103

Note: This table presents regression discontinuity in time estimates of the effect of the No-Cash-Bail policy, for eligible offenses, following Calonico et al. (2014). Panel A presents results for the full sample. In Panel B, we subset eligible offenses to only include offenses that are most likely to have had cash bail (and not pretrial supervision) before the No-Cash-Bail reform, as defined in Table 5. 'Mean Dep. Var.' is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A9: Robustness for difference-in-difference estimates of the effect of No-Cash-Bail policy on FTA and recidivism: different time windows

	FTA within...			Recidivism within...		
	1 month	3 months	10 months	1 month	3 months	10 months
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible*Post 02/21	0.0057	0.0029	-0.0041	-0.0026	-0.0081	-0.025*
	(0.0056)	(0.0064)	(0.010)	(0.0049)	(0.0081)	(0.0093)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.035	0.085	0.157	0.046	0.108	0.235
N	22589	22589	20524	22589	22589	20524

Note: This table presents estimates of δ in Equation 1. Eligible offenses are the treatment group, and ineligible offenses are the control group. The outcome in Columns 1-3 is FTA and in Columns 4-6 is recidivism. They are defined as having missed one's court date or failed to appear in court within 1 month (Columns 1 and 4), 3 months (Columns 2 and 5) or 10 months (Columns 3 and 6) within one's initial court hearing. In Columns 3 and 6, we limit our sample to defendants for whom we observe outcomes for at least 10 months after their initial court hearing. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A10: Testing balance in judge case characteristics

	(1) Felony	(2) Black	(3) Age	(4) Male	(5) Past FTA	(6) Past charges
Judge 1	0.0014	-0.023	-0.34	-0.0046	-0.0034	0.0083
	(0.0042)	(0.012)	(0.31)	(0.013)	(0.012)	(0.013)
Judge 2	0.0075	0.0014	0.083	-0.0061	-0.0055	0.0032
	(0.0067)	(0.011)	(0.27)	(0.0081)	(0.0076)	(0.011)
Judge 3	0.0042	-0.0022	-0.033	0.014	0.019*	0.014
	(0.0033)	(0.0074)	(0.26)	(0.010)	(0.0089)	(0.010)
Judge 4	-0.0015	-0.0087	-0.21	-0.0040	-0.00100	0.012
	(0.0012)	(0.011)	(0.33)	(0.0098)	(0.0089)	(0.0079)
Judge 5	0.0070	-0.0041	-0.18	0.0052	-0.0048	-0.00059
	(0.0049)	(0.0088)	(0.29)	(0.0068)	(0.0099)	(0.0093)
Mean Dep. Var.	0.532	0.559	34.061	0.834	0.160	0.558
N	21527	21527	21527	21527	21527	21527

Note: This table tests whether cases are quasi-randomly assigned to bail magistrates. To do so, we regress observable case characteristics on dummies for bail magistrates, while controlling for day of the week, shift, quarter in the year, and offense. We drop cases that are not examined by judges who see at least 100 cases a year. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A11: Difference-in-difference estimates of the effect of No-Cash-Bail policy on ROR, jail, FTA and Recidivism, keeping only felony cases

	ROR	Jail	FTA	Recidivism
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	0.17***	-0.020	-0.010	-0.017
	(0.019)	(0.020)	(0.0086)	(0.0090)
Controls	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.172	0.322	0.182	0.184
N	12038	12038	12038	12038

Note: This table presents estimates of δ in Equation 1, keeping only felony cases. Eligible offenses are the treatment group, and ineligible offenses are the control group. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.

Table A12: Comparing the characteristics of beneficiaries and of potential beneficiaries of the No-Cash-Bail reform for cases other than PWID

	Beneficiaries (compliers)	Potential beneficiaries (compliers + never-ROR)	Ratio: beneficiaries / potential beneficiaries
	(1)	(2)	(3)
Black	0.11	0.43	0.26
			(0.71)
Hispanic	0.35	0.22	1.62
			(1.61)
White	0.50	0.34	1.49
			(0.49)
Percent below poverty	0.23	0.24	0.95
			(0.19)
Predicted recidivism	0.14	0.16	0.86
			(0.14)
Predicted FTA	0.28	0.27	1.06
			(0.10)

Note: This table presents the average characteristics of compliers, who are the beneficiaries of the No-Cash-Bail policy, to that of all potential beneficiaries of the No-Cash-Bail policy, which to say compliers of never-ROR. Calculations follow the methodology outlined in appendix A. We drop PWID (possession with intent to deliver) cases, whose classification into "eligible" and "ineligible" cases may be more ambiguous, since it depends on prior drug cases whose characteristics we do not fully observe in our data. Note that our main results are unchanged when we drop PWID cases (Table A11) In Column 3, we report standard errors (in parentheses) based on 1000 bootstrap replications.

Table A13: Difference-in-difference estimates of the effect of No-Cash-Bail policy on ROR, jail, FTA and Recidivism, dropping PWID cases

	ROR	Jail	FTA	Recidivism
	(1)	(2)	(3)	(4)
Eligible*Post 02/21	0.099***	0.013	-0.014	-0.017
	(0.021)	(0.016)	(0.011)	(0.015)
Controls	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.684	0.084	0.194	0.156
N	17952	17952	17952	17952

Note: This table presents estimates of δ in Equation 1, dropping PWID (possession with intent to deliver) cases. Eligible offenses are the treatment group, and ineligible offenses are the control group. ROR (released on own recognizance) means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least 3 nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within 6 months after one's initial court hearing. Controls are for offense statute and class, age, gender, day of week, shift, presence and number of past offenses and past FTAs, and initial bail commissioner. "Mean Dep. Var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Data source: court dockets from the Pennsylvania Unified Judicial System. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors, clustered at the offense level, are in parentheses.