

## Does Cash Bail Deter Misconduct?<sup>†</sup>

By AURÉLIE OUSS AND MEGAN STEVENSON\*

*Courts routinely use low cash bail as a financial incentive to ensure 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 legal actor discretion, showing that nonbinding reforms may have limited impact on jail populations. (JEL K41, K42)*

Financial penalties are used in various areas of criminal justice, with the goal of deterring misconduct.<sup>1</sup> One iconic example is the requirement that defendants pay cash bail to secure pretrial release. In recent years, however, hundreds of jurisdictions across the United States have begun to reduce their reliance on cash bail. 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 21, 2018, Philadelphia's newly elected prosecutor

\*Ouss: University of Pennsylvania (email: [aouss@sas.upenn.edu](mailto:aouss@sas.upenn.edu)); Stevenson: University of Virginia School of Law (email: [mstevenson@law.virginia.edu](mailto:mstevenson@law.virginia.edu)). Ilyana Kuziemko was coeditor for this article. Many thanks to the various individuals who provided help in this research, including David Abrams, Iwan Barankay, Jennifer Doleac, Oren Gur, Paul Heaton, Michael Hollander, Mark Houldin, Jacob Kaplan, Jens Ludwig, John MacDonald, Alex Malek, Alex Tabarrok, Bryan McCannon, Arnaud Philippe, John Rappaport, Lyandra Retacco, Liam Riley, Ariel Shapell, Nyssa Tayler, and Benjamin Waxman. This research was supported by the Quattrone Center for the Fair Administration of Justice.

<sup>†</sup>Go to <https://doi.org/10.1257/app.20210349> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

<sup>1</sup>Fines are generally found to be effective: for example, in improving driving behaviors (Luca 2015; Bar-Ilan and Sacerdote 2004; Goncalves and Mello 2022) or reducing collusive pricing (Block, Nold, and Sidak 1981).

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 two-thirds 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-difference 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, even though the prosecutor’s office no longer requests it for eligible categories. Since magistrates do not work for the district attorney, and since there are no changes to directly relevant factors such as the defendant’s risk profile, one might not expect the policy to have much impact. Our results on this question are mixed. We find that the No-Cash-Bail policy did affect bail-setting behavior, leading to a sharp 22 percent (11 percentage point) increase in the likelihood of being granted release on recognizance (ROR, or release without monetary or supervisory conditions). However, the No-Cash-Bail policy had no impact on pretrial detention rates. This is because 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).<sup>2</sup> Therefore, while prosecutorial policy does appear to exert some soft influence on judicial behavior, its impact was attenuated by discretion in the implementation.

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 0 and allow us to reject even small increases in failure to appear (FTA) and pretrial crime at the 5 percent 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-difference 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 is widely used under the theory that it incentivizes court appearance and deters crime. Yet Philadelphia was able to substantially

<sup>2</sup>There was no effect on larger bail amounts, which are more likely to lead to pretrial detention.

liberalize the conditions of pretrial release with no detectable adverse consequences. Why was unnecessary bail being set so often prior to the reform? One possible explanation is that magistrates—or the individuals who set bail policy and oversee magistrate practices—have incentive to err on the side of setting restrictive conditions. If a magistrate makes a “type II error” (being too lenient toward someone who reoffends), they may come under public scrutiny. In a recent example, a man released on \$1,000 bond intentionally drove his SUV into a crowded Wisconsin parade, killing six people and injuring many others. The media drew attention to all legal actors involved, and the court commissioner who had set low bail was reassigned away from criminal cases.<sup>3</sup> Even in less extreme examples, legal actors might feel remorse when people for whom they set low cash bail go on to commit new crimes. In contrast, no one knows for certain when a “type I error” has been made (being too harsh on someone who would not have reoffended) since they don’t observe what a detained person would have done if released. If magistrates face asymmetric penalties in errors, they 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? This depends on how many defendants are still receiving unnecessarily restrictive bail. If magistrates have already exhausted the pool of defendants who can be granted ROR without adverse consequence, then greater liberalization would come with trade-offs. To provide suggestive evidence on this, we exploit a second natural experiment in Philadelphia: the fact that defendants are quasi-randomly assigned to magistrates with varying initial levels of leniency and varying responsiveness to reform. We show that even originally lenient magistrates were able to substantially reduce their use of monetary bail without adverse consequences. Since these are the magistrates most likely to have exhausted the pool of defendants who can safely be granted ROR, there may be room for the stricter magistrates to “catch up.” We also show that our results are similar when we limit our analyses to more serious cases—felonies that were eligible for the No-Cash-Bail policy.

This paper contributes to several literatures. To begin, we provide some of the first evidence on the impacts of the current bail reform movement. Like Stevenson (2018a), we find that discretionary bail reform has little impact on pretrial detention rates. In contrast, Albright (2022) evaluates a nondiscretionary bail reform and finds a large reduction in pretrial detention, at least for short stays. In order to substantially reduce jail populations, it might be necessary to limit discretion or impose some system of accountability that makes it costly to deviate from the reform policy.

We also 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

<sup>3</sup> <https://www.jsonline.com/story/news/2021/12/03/commissioner-who-okd-low-bail-waukesha-parade-suspect-reassigned/8857806002/>.

defendants.<sup>4</sup> We find no evidence that cash bail has a deterrent effect on misconduct among those evaluated, which goes against the traditional economic models of crime involvement (Becker 1968). This may be because cash bail provides little marginal deterrence on top of the criminal justice penalties that already exist. 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. Alternatively, 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, Ouss, and Shah 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).<sup>5</sup>

Lastly, we provide new evidence about the influence of prosecutors. The nascent literature has thus far supported claims about outsize prosecutorial power, showing they are influential in both the high rates of incarceration and in racial disparities in sentencing (Rehavi and Starr 2014b; Pfaff 2017; Arora 2018; Krumholz 2020; Sloan 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 prosecutor-led criminal justice reform 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

<sup>4</sup>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, finding that increasing bail bond reduces FTA. The second paper uses propensity score matching, finding that felony defendants released with surety bonds are less likely to miss court appearances than similar defendants released on recognizance. Other studies that evaluate the combined incapacitative (due to pretrial detention) and deterrent effect of monetary bail include Abrams and Rohlfs (2011); Gupta, Hansman, and Frenchman (2016); and Albright (2022).

<sup>5</sup>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.

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,<sup>6</sup> 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, Jones, and Brooker 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 percent were held on cash bail, 38.2 percent were released on cash bail, and only 14 percent were released on recognizance (Reaves 2013). Among felony defendants with monetary bail set, 43 percent had bail less than \$10,000 and 28 percent had bail less than \$5,000 (Reaves 2013). Misdemeanors, however, constitute the large majority (~80 percent) of cases filed. While there are no nationally representative statistics on bail for misdemeanors, Mayson and Stevenson (2020) analyze data across 8 diverse jurisdictions and find that 40 percent to 90 percent of misdemeanor defendants were required to post monetary bond. Among those with monetary bail, the large majority had bail less than \$5,000.

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 detention. 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 system, 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.<sup>7</sup> 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.<sup>8</sup> Discretionary reform can come from the legislature, from the courts, or, as is increasingly common, from prosecutorial policy. A commitment to no longer

<sup>6</sup>E.g., Arnold, Dobbie, and Yang (2018); Kleinberg et al. (2018); Hull (2017).

<sup>7</sup>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.

<sup>8</sup>This is in contrast to policies that directly change the scope of discretion, like mandatory sentencing guidelines (Kuziemko 2013a; Yang 2015a). Unlike in these situations, bail magistrates' choice set is neither expanded or restricted.

request cash bail for many offense categories has been a staple of prosecutor-led criminal justice reform (Bazelon 2019). For instance, on the first day in office, the 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.<sup>9</sup> 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 Stemen 2010). After the pretrial interview, the defendant is ready for the bail hearing. This takes place over video conference: 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 whom 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.

<sup>9</sup>In 85 percent of cases, the arrest happens within the 2 calendar dates after the alleged offense, so in most cases, arrest and offense dates are close to the same.

- Secured monetary bail: The defendant must pay a deposit (10 percent 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 four to eight 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 percent of the total bail amount. If they comply with all release conditions, 70 percent of the deposit will be returned when the case is disposed.<sup>10</sup>

### *C. The Philadelphia No-Cash-Bail Reform*

On November 7, 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.<sup>11</sup> However, and importantly for our research design, the exact timing of different reforms was not announced ahead of time.

On February 21, 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.<sup>12</sup> 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. Note that, as a practical matter, requesting ROR could result in a shift away from supervised release as well as monetary bail.

There was only one other reform to pretrial practices around that time.<sup>13</sup> On February 15, the DA's office announced a change in charging practices for marijuana possession, retail theft, and sex work. Online Appendix Figure A.1 shows that

<sup>10</sup>This has recently been revised, and a compliant defendant will now receive their full bail deposit back.

<sup>11</sup>His agenda can be found here: <https://krasnerforda.com/platform/>.

<sup>12</sup>A list of the most common eligible and ineligible offense categories can be found in online Appendix Table A1.

<sup>13</sup>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.

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.<sup>14</sup>

#### D. Data and Descriptive Statistics

Our primary data source consists of court dockets web-scraped from the Pennsylvania Unified Judicial System (Philadelphia Municipal Courts 2018). It is structured to include one observation per criminal 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 the initial bail hearing of which occurred in the six months before or the five months after the No-Cash-Bail reform. After dropping marijuana possession, sex work, and retail theft cases,<sup>15</sup> duplicate cases (i.e., a defendant is brought for multiple cases on the same day),<sup>16</sup> and cases where covariates are missing,<sup>17</sup> 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 these 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.<sup>18</sup> “Ineligible cases” are cases the lead charge of which does not appear in that list of 25 offenses. Since the lead charge is the most serious charge, some cases that we categorize as “ineligible” also have one or more eligible charges. If legal actors think that the policy could apply to cases in which any offense is eligible, this creates the potential for a slight spillover effect, which we discuss in more detail later.

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, Goldin, and Yang

<sup>14</sup> 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 six months before and five months after the No-Cash-Bail policy.

<sup>15</sup> As discussed previously, there was a concurrent policy that reduced arrest rates for these charges. Together, they constituted ~10 percent of pre-reform caseload.

<sup>16</sup> We omit multiples due to difficulties in defining the bail type for a defendant with multiple types of bail; 8.5 percent of cases are multiples. Our results are very similar if we include duplicate cases.

<sup>17</sup> About 7 percent of cases are missing some covariates, most often information about past offenses.

<sup>18</sup> 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. Eight percent of ineligible cases are in this category. We show that our results are robust to dropping PWID cases in online Appendix Table A12.

2018; Stevenson 2018b).<sup>19</sup> 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.<sup>20</sup> 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 begin 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 21, shown separately for eligible and ineligible cases. First, note that a large portion—roughly 60 percent of the sample—is eligible for the reform.<sup>21</sup> Second, note that the reform targeted a group of defendants who 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 percent of ineligible cases. Only about 17 percent of eligible cases led to at least 3 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 percent) 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’ zip code is 25 percent, and 75 percent 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 panels show time trends in ROR, pretrial detention, recidivism, and FTA. There was a sharp increase in ROR right after the reform, but neither jail, FTA, nor recidivism changed much. Figure 2 presents the same raw evidence for ineligible cases. In contrast to the eligible cases, there is no sharp visible break in any of the trends around the implementation date.<sup>22</sup>

<sup>19</sup> Most defendants who fail to pay bail within the first three days remain detained until the disposition of the case.

<sup>20</sup> At 6 months, 61 percent of cases have been resolved; at 10 months, 80 percent of cases are resolved. We conduct robustness tests in which our recidivism and FTA measures are defined over varying time windows.

<sup>21</sup> Including the case types omitted because of a concurrent change in charging practice (marijuana possession, sex work, and retail theft), approximately 67 percent of all cases filed in Philadelphia before the reform would have been eligible for the No-Cash-Bail policy.

<sup>22</sup> The slight increase in ROR for ineligible cases after the policy is driven by cases where the lead charge is ineligible but secondary charges were eligible. Online Appendix Figure A.2 presents trends for ineligible cases that

TABLE 1—DESCRIPTIVE STATISTICS FOR CASES BEFORE THE NO-CASH-BAIL POLICY

	Eligible cases	Ineligible cases
<i>Defendant characteristics</i>		
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	37,847	35,401
Fraction below poverty in zip code	0.26	0.26
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
<i>Pretrial conditions</i>		
ROR	0.51	0.08
Supervised release	0.06	0.01
Unsecured monetary	0.07	0.07
Secured bail up to 5,000	0.16	0.27
Secured bail over 5,000	0.20	0.56
Denied bail	0.00	0.01
Jail (3+ nights)	0.17	0.46
<i>Misconduct</i>		
FTA	0.25	0.10
Recidivism	0.17	0.11
Observations	7,468	4,281

*Notes:* This table presents descriptive statistics for cases filed during the six months before the No-Cash-Bail policy. ROR 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 six 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

Our preferred research design is difference-in-differences, with ineligible cases as the control group. We select this research design to account for time-varying trends that affect both groups equally. For instance, both eligible and ineligible cases saw a gentle increase in ROR during the months before the reform. This could be due to seasonality or a gradual change in bail-setting culture during the first few months DA Krasner was in office. We include three alternative specifications as our main robustness tests. The first is a difference-in-difference strategy with eligible cases from the previous year as the control group. The second is to drop cases where the lead charge is ineligible but where a secondary charge is eligible, which represents 21 percent of ineligible cases (8 percent of the full sample). These cases

had no eligible charge at all, and we see no change in trends at all. Later, we show that our results are similar when we drop these “hybrid” cases.

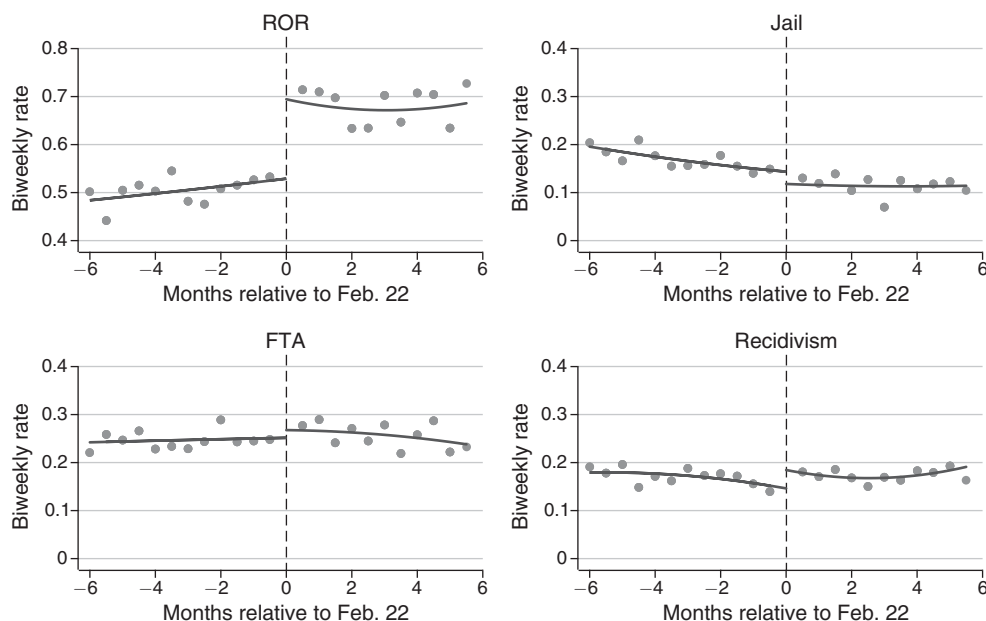


FIGURE 1. TIME TREND IN ROR, PRETRIAL DETENTION, FTA, AND RECIDIVISM FOR ELIGIBLE CASES

*Notes:* Each dot represents the mean value in a two-week time period. The vertical line represents the February 22 date of the No-Cash-Bail policy. The lines are quadratic fits, before and after February 22. ROR means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least three nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within six months after one's initial court hearing.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

could have been treated differently by legal actors, depending on their interpretation of the No-Cash-Bail guidelines. The third is a discontinuity-in-time estimation strategy that exploits sharp changes for eligible cases only.<sup>23</sup> These alternative specifications are motivated by concerns about spillover effects that would lead us to underestimate the policy's impact on bail. By and large, we find very similar estimates using all three strategies.

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  $\mathbf{X}$  include defendant race, age at arrest, gender, prior FTAs, prior convictions, types of offense,<sup>24</sup> grade of offense, whether the defendant was represented by a public

<sup>23</sup> We follow Calonico, Cattaneo, and Titiunik (2014), using time as the running variable—an approach the merits and limits of which relative to the more classic version of running variables have been discussed by Hausman and Rapson (2018a).

<sup>24</sup> Offenses have been aggregated to the 23 most common offenses and a catchall category for the remaining offenses. We cluster standard errors at the offense level, motivated by concerns that effective treatment (i.e., perceptions of the appropriateness of responding to the No-Cash-Bail policy) and outcomes will be correlated within offense type. Our conclusions are unchanged if we do not cluster standard errors or if we cluster standard errors at the judge level using a wild bootstrap (shown in online Appendix Table A8).

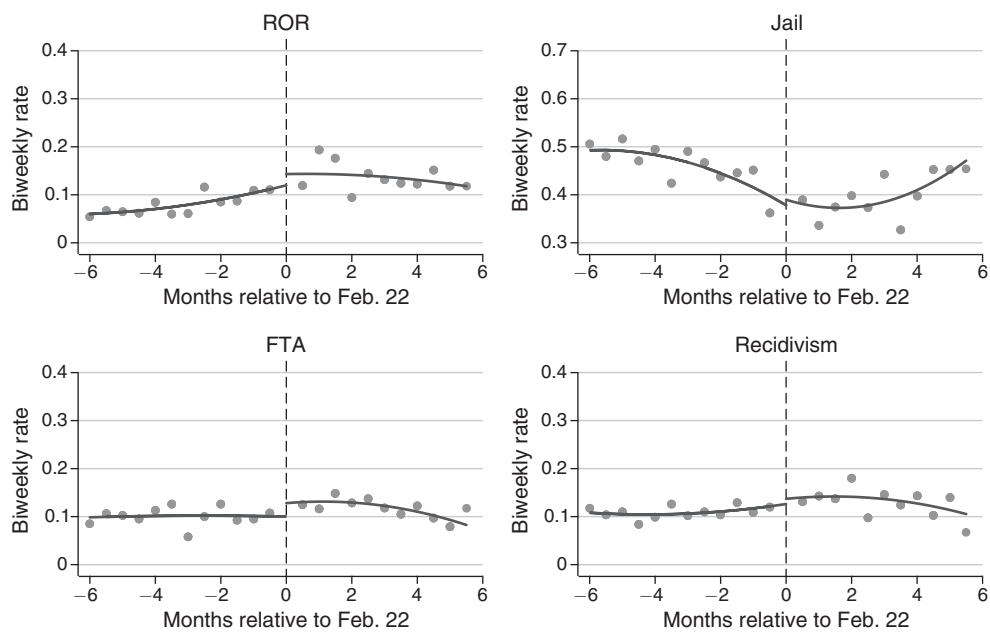


FIGURE 2. TIME TREND IN ROR, PRETRIAL DETENTION, FTA, AND RECIDIVISM FOR INELIGIBLE CASES

Notes: Each dot represents the mean value in a two-week time period. The vertical line represents the February 22 date of the No-Cash-Bail policy. The lines are quadratic fits, before and after February 22. ROR means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least three nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within six months after one’s initial court hearing.

Data source: Court dockets from the Pennsylvania Unified Judicial System

defender, the bail magistrate, day of the week, and magistrate work shift. The main coefficient of interest is  $\delta$ .

(1) 
$$Y_i = \alpha + \beta Post_i + \delta Post_i \times Eligible_i + \lambda Eligible_i + \theta \mathbf{X}_i + \epsilon_i.$$

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 upcharge 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.

Online Appendix Figure A.3 shows a time trend in the number of eligible and ineligible cases filed. (The number of cases filed should closely track the number of arrests

TABLE 2—LEGAL ACTORS’ RESPONSES TO THE NO-CASH-BAIL POLICY: ARRESTS AND CHARGING

	Number of arrests (1)	Percent of declined (2)	Percent of upcharged (3)	Percent of downcharged (4)	Number of cases (5)
<i>Eligible × Post 02/21</i>	−15.2 (16.1)	0.011 (0.0070)	0.0059 (0.0050)	0.012 (0.0063)	−10.8 (14.1)
Mean dep. var.	358	0.015	0.129	0.052	298
Observations	94	26,926	26,926	26,926	94

*Notes:* 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 are 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. Standard errors are in parentheses; they are clustered at the offense level in columns 2–5.

*Data Source:* Court dockets from the Pennsylvania Unified Judicial System

since the declination rate for offenses in our sample is only about 1 percent.) 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 Table 2 and online Appendix Table A2. In column 1 of Table 2, we test for changes in arrest patterns.<sup>25</sup> 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.

Online Appendix Table A2 and online Appendix Figure A.4 test for changes in observable case characteristics: charges per case (which can be a proxy for case severity), probability 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.

<sup>25</sup>For columns 1–4 of this table, the data come from arrest records (Philadelphia Police Department 2018), which, importantly, include 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.

## 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, Dobbie, and Yang 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 3 show difference-in-difference estimates of  $\delta$  (as described in equation (1)) with ROR as the outcome.<sup>26</sup> The odd column does not include controls; the even column does. We estimate that the No-Cash-Bail policy led to an 11 percentage point (22 percent relative to the pre-reform mean for eligible cases) relative increase in the likelihood that defendants will be released on their own recognizance. 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 whom they represent. Even if a bail reform policy is unenforceable, 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 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 3 presents event-study-style coefficient plots in support of the difference-in-difference estimation. Each graph shows coefficients on lead/lag dummy variables interacted with a dummy for eligibility. The lead/lag dummy variables each correspond to one month of bail hearings: six before and five after the policy. Specifically, we estimate the following equation:

$$(2) \quad Y_{it} = \alpha_t + \beta \text{Eligible}_i + \sum_{t \neq -1} \delta_t \text{Eligible}_i + \theta \mathbf{X}_i + \epsilon_i.$$

<sup>26</sup>Online Appendix Table A3 shows the  $\beta$  and  $\lambda$  coefficients from equation (1).

TABLE 3—DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF NO-CASH-BAIL POLICY ON ROR AND JAIL

	ROR		Jail	
	(1)	(2)	(3)	(4)
<i>Eligible × Post 02/21</i>	0.12 (0.028)	0.11 (0.021)	0.0079 (0.021)	0.0072 (0.016)
Controls	No	Yes	No	Yes
Mean dep. var.	0.505	0.505	0.169	0.169
Observations	22,589	22,589	22,589	22,589

Notes: This table presents estimates of  $\delta$  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 three nights after the bail hearing. "Mean dep. var." is the mean of the dependent variable for eligible cases before the No-Cash-Bail policy. Standard errors, clustered at the offense level, are in parentheses.

Data source: Court dockets from the Pennsylvania Unified Judicial System

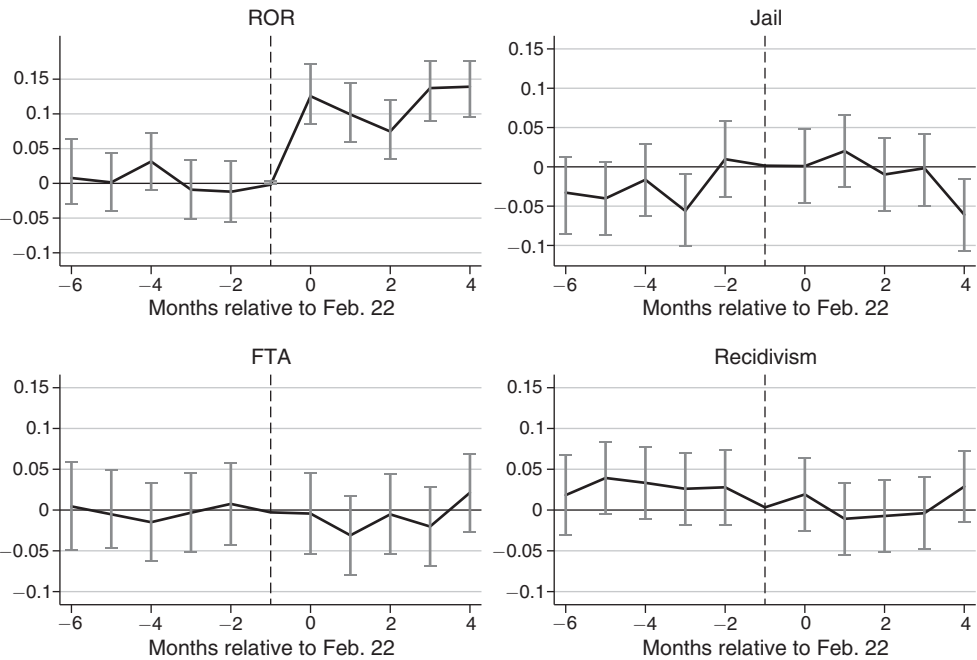


FIGURE 3. DIFFERENCE-IN-DIFFERENCE ESTIMATES WITH LEADS AND LAGS FOR HOW THE NO-CASH-BAIL POLICY AFFECTED ROR, JAIL TIME, FTA, AND RECIDIVISM

Notes: This figure plots the difference-in-difference coefficients obtained from estimating a single equation with monthly leads and lags (equation (2)), with the 95 percent 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 February 22. That month is left out as the comparison category. ROR means that a defendant is released with no monetary or supervisory conditions. Pretrial detention is defined as spending at least three nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within six months after one's initial court hearing.

Data source: Court dockets from the Pennsylvania Unified Judicial System

$\alpha_t$  captures month fixed effects, and  $\delta_t$  captures the leads/lags of the policy, with the month immediately prior to the reform left out as the comparison category. Figure 3 plots the  $\delta_t$  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-difference 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 online Appendix Table A4. 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 donut difference-in-difference 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-difference estimate on the aggregated sample. The estimates remain largely unchanged.

We also find very similar results when we use alternative specifications. As discussed in the empirical strategy section, we use three alternative strategies: (1) using eligible offenses in the prior year as a control group, (2) dropping ineligible cases that have some eligible secondary offenses, and (3) regression discontinuity in time. The results for ROR are presented in column 1, panel A of online Appendix Tables A5–A7.<sup>27</sup> These alternative specifications provide similar (slightly larger) increases in ROR, demonstrating that the increase in ROR is robust across specifications.

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 3, 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 percent level.<sup>28</sup> 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 3).<sup>29</sup> Despite the hopes of reformers, this discretionary policy did not lead to a meaningful decrease in the pretrial detention rate.

<sup>27</sup> In addition, online Appendix Figure A.5 presents event-study graphs using eligible cases in the prior year as the comparison group.

<sup>28</sup> 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.

<sup>29</sup> Our results are not driven by the choice of our definition of being in jail pretrial as having spent at least three nights in jail: as shown in online Appendix Table A9, the results are similar if we vary the definition to having spent at least one to seven nights in jail. Results are also similar across empirical strategies, as shown in column 2, panel A of online Appendix Tables A5–A7.

TABLE 4—DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECTS OF THE NO-CASH-BAIL POLICY ON INITIAL BAIL TYPE

	Supervised release (1)	Unsecured monetary (2)	Secured under 5,000 (3)	Secured over 5,000 (4)
<i>Eligible</i> × <i>Post 02/21</i>	−0.041 (0.019)	−0.021 (0.0077)	−0.046 (0.019)	−0.0070 (0.024)
Controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.061	0.069	0.164	0.200
Observations	22,589	22,589	22,589	22,589

*Notes:* This table presents estimates of  $\delta$  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. Standard errors, clustered at the offense level, are in parentheses.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

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 4 shows difference-in-difference estimates of the impact that the No-Cash-Bail policy had on these types of bail.<sup>30</sup> We see that there was about a 4 percentage point decline in both supervised release and low monetary (secured) bail. Unsecured bail declined by a little over 2 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.<sup>31</sup>

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 who already had relatively high rates of release and (2) discretion

<sup>30</sup> Online Appendix Figure A.6 presents event-study graphs for bail types.

<sup>31</sup> 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.

in implementation meant that only a subset of eligible defendants actually benefited from the reform. 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 jail populations.

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 up-front 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 the use of monetary bail and supervisory conditions could increase misconduct among released defendants if the prospect of monetary penalties acts 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).

Table 5 presents the difference-in-difference estimates with FTA and recidivism as the outcomes ( $\delta$  from equation (1)).<sup>32</sup> 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 percent level, anything larger than a 0.009 percentage point increase in FTA.<sup>33</sup> We can reject any increase in pretrial rearrest.<sup>34</sup> These results are supported by our graphical event-study analysis, which is presented in the bottom two graphs of Figure 3. Trends in FTA and recidivism

<sup>32</sup> Again, online Appendix Table A3 shows the  $\beta$  and  $\lambda$  coefficients from equation (1).

<sup>33</sup> Columns 5–8 of online Appendix Table A4 present a series of robustness tests, similar to those presented in Section IIA, which yield very similar results.

<sup>34</sup> 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.

TABLE 5—DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF NO-CASH-BAIL POLICY ON FTA AND RECIDIVISM

	FTA		Recidivism	
	(1)	(2)	(3)	(4)
<i>Eligible × Post 02/21</i>	−0.0076 (0.011)	−0.0084 (0.011)	−0.017 (0.012)	−0.019 (0.011)
Controls	No	Yes	No	Yes
Mean dep. var.	0.247	0.247	0.171	0.171
Observations	22,589	22,589	22,589	22,589

*Notes:* This table presents estimates of  $\delta$  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 six 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. Standard errors, clustered at the offense level, are in parentheses.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

remain roughly parallel and unchanged both before and after the reform. In online Appendix Table A10, we vary the time windows for FTA and recidivism. There again, we find mostly small and insignificant coefficients. Our alternative specifications—using eligible cases from 2008 as a control group, dropping ineligible cases that had some eligible secondary charges, and regression discontinuity in time—also yield very similar results, as shown in columns 3 and 4 of online Appendix Tables A5–A7. Lastly, we run a placebo test where we randomly assign “treatment” dates between 2013 and 2016. We then run placebo difference-in-difference estimates, similar to those presented in equation (1).<sup>35</sup> Online Appendix Figure A.7 shows the distribution of coefficients obtained in 2,000 random draws of the placebo policy date; the dotted line presents our main coefficients for the true experiment. This figure shows that only the true policy yielded changes in ROR, while the changes in pretrial detention, FTA, and recidivism are within the placebo ranges.

As seen in Table 4, 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 that experienced differential shocks. Table 6 breaks out eligible offenses into those that, prior to the reform, were less/more likely to get supervised release.<sup>36</sup> 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

<sup>35</sup>The only difference is that we don’t control for bail magistrates since the data aren’t available for earlier years. For consistency, we rerun our main results without controls for bail magistrates and include those results in the figure. The estimates change very little.

<sup>36</sup>Pretrial supervision is not equally used for all offenses—it is somewhat common (around 11 percent of cases get supervised release) for drug and property crimes but not for other types of offenses (less than 2 percent).

TABLE 6—BREAKING OUT THE EFFECTS OF CASH BAIL AND SUPERVISED RELEASE

	ROR (1)	Jail (2)	Supervised release (3)	Cash (4)	FTA (5)	Recidivism (6)
<i>Panel A. Cash bail</i>						
<i>Eligible × Post 02/21</i>	0.12 (0.027)	−0.0085 (0.019)	−0.0039 (0.0038)	−0.11 (0.026)	−0.023 (0.0094)	−0.016 (0.0091)
Mean dep. var.	0.287	0.271	0.018	0.695	0.202	0.149
Observations	16,116	16,116	16,116	16,116	16,116	16,116
<i>Panel B. Supervised release</i>						
<i>Eligible × Post 02/21</i>	0.10 (0.025)	0.027 (0.017)	−0.087 (0.011)	−0.014 (0.029)	0.0088 (0.012)	−0.024 (0.020)
Mean dep. var.	0.760	0.049	0.113	0.127	0.300	0.197
Observations	14,817	14,817	14,817	14,817	14,817	14,817
Controls	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* This table presents difference-in-difference estimates of  $\delta$  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 six 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. Standard errors, clustered at the offense level, are in parentheses.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

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 not improve court compliance.<sup>37</sup> The point estimates on misconduct remain small, mostly negative, and mostly statistically insignificant.<sup>38</sup> Figure 4 plots the  $\delta_t$  coefficients from equation (2) for offenses that were most likely to get cash bail before the No-Cash-Bail reform, providing visual confirmation of the results from Table 6. We provide more evidence on the impact of cash bail separate from pretrial supervision in Section IID.<sup>39</sup>

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-difference

<sup>37</sup> Again, these results are not driven by our modeling choices. In panel B of online Appendix Tables A5–A7, we present estimates from the alternative specification on the sample of eligible offenses that experienced the largest change in cash bail. Our results are very similar.

<sup>38</sup> Table 6 shows that ROR leads to a negative and statistically significant reduction in FTA. However, we don’t want to overinterpret this result given that it is not statistically significant in two of the three alternative specifications or in the analysis discussed in Section IID.

<sup>39</sup> While the point estimates from panel B of Table 6 provide suggestive evidence that supervision has little effect on misconduct, the standard errors cannot rule out moderate-sized effects.

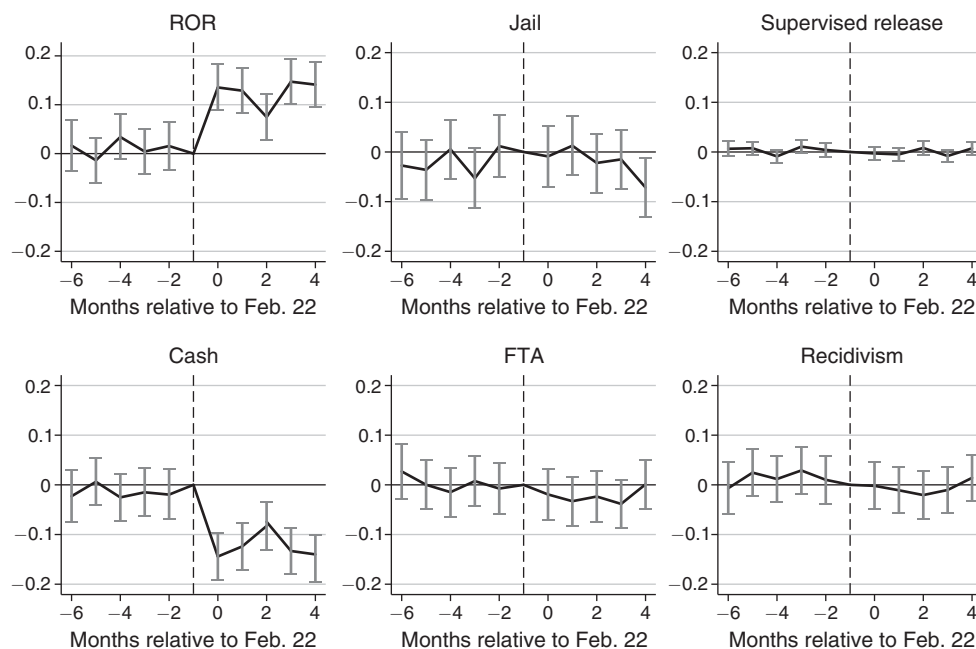


FIGURE 4. HIGH-CASH BAIL OFFENSES: DIFFERENCE-IN-DIFFERENCE ESTIMATES WITH LEADS AND LAGS FOR HOW THE NO-CASH-BAIL POLICY AFFECTED ROR, JAIL TIME, SUPERVISED RELEASE, CASH BAIL, FTA, AND RECIDIVISM FOR OFFENSES THAT ARE MOST LIKELY TO HAVE HAD CASH BAIL (*and not supervised release*)

*Notes:* This figure plots the difference-in-difference coefficients obtained from estimating a single equation with monthly leads and lags (equation (2)), with the 95 percent confidence interval of the coefficient estimate. The treatment group is eligible offenses that were most likely to have had cash bail (and not pretrial supervision) before the No-Cash-Bail reform, as defined in Table 5, and the control group is ineligible offenses. The vertical dashed line indicates the month prior to February 22. That month is left out as the comparison category.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

approach to provide new evidence on this question.<sup>40</sup> 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

<sup>40</sup>In the crime context, this approach has, for example, been used by Draca, Machin, and Witt (2011).

Table 3, 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.

$$(3) \quad ROR_i = \alpha + \delta Post_i \times Eligible_i + \beta_1 Post_i + \lambda_1 Eligible_i + \theta_1 \mathbf{X}_i + \epsilon_i,$$

$$(4) \quad Y_i = \alpha_2 + \gamma \widehat{ROR}_i + \beta_2 Post_i + \lambda_2 Eligible_i + \theta_2 \mathbf{X}_i + \psi_i.$$

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.<sup>41</sup> The modified specification thus adds  $Post_i \times Eligible_i \times Magistrate\_1_i$  in the instruments and  $Magistrate\_1_i$ ,  $Post_i \times Magistrate\_1_i$ , and  $Eligible_i \times Magistrate\_1_i$  as controls in both stages. For this strategy to be valid, we must make a partial monotonicity assumption (Mogstad, Torgovitsky, and Walters 2021)—that is, we assume that people are more likely to get ROR if  $Post_i \times Eligible_i = 1$  or if  $Post_i \times Eligible_i \times Magistrate\_1_i = 1$ .

Results are shown in panel A of Table 7. 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 percent level, we find that ROR leads to at most an 8.8 percentage point increase in FTA and does not increase recidivism. Columns 3 and 4 show the instrumental variables results that allow for heterogeneous magistrate response. In this specification, at the 5 percent level, we show that ROR leads to at most a 3.8 percentage point increase in FTA and a 3 percentage point increase in recidivism.<sup>42</sup>

<sup>41</sup> We discuss and justify this approach further in Section IIC.

<sup>42</sup> Evaluating the relative change with a binary outcome is problematic as the mean outcome approaches zero or one, 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.

TABLE 7—IV ESTIMATES OF THE EFFECTS OF ROR ON FTA AND RECIDIVISM

	IV		Magistrate IV	
	FTA (1)	Recidivism (2)	FTA (3)	Recidivism (4)
<i>Panel A. Full sample</i>				
<i>ROR</i>	−0.076 (0.10)	−0.17 (0.093)	−0.10 (0.084)	−0.11 (0.087)
Upper bound	0.088	−0.017	0.038	0.030
Mean dep. var.	0.247	0.171	0.247	0.171
Observations	22,589	22,589	22,589	22,589
First-stage <i>F</i> -stat	27.4		20.7	
<i>Panel B. Cash bail</i>				
<i>ROR</i>	−0.20 (0.10)	−0.13 (0.070)	−0.20 (0.11)	−0.13 (0.076)
Upper bound	0.011	0.0028	0.0096	0.018
Mean dep. var.	0.202	0.149	0.202	0.149
Observations	16,116	16,116	16,116	16,116
First-stage <i>F</i> -stat	19.1		10.2	
Controls	Yes	Yes	Yes	Yes

*Notes:* This table presents IV estimates in which we use  $Post_i \times Eligible_i$  as an instrument for the change in ROR (release on recognizance). In columns 3 and 4, we also include interactions of  $Post_i \times Eligible_i$  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 9. 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 six 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

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 were charged with offenses that experienced a large change in cash bail but no change in pretrial supervision, as described in panel A of Table 6. Results are shown in panel B of Table 7. Across specifications, our point estimates are negative. At the 5 percent level, we are able to reject the hypothesis that the elimination of cash bail leads to more than a 1 or 2 percentage point increase in FTA or recidivism.

While not significant at conventional levels, our point estimates are most consistent with the claim that monetary bail leads to an increase in misconduct. There are certainly explanations consistent with this. The payment of bail and the time burdens of pretrial supervision could have a destabilizing effect on the lives of indigent defendants (Harris 2016; Mello 2018). The imposition of monetary bail or pretrial supervision without giving the defendant a chance to explain himself or herself may

feel coercive or unfair (Nagin and Telep 2017).<sup>43</sup> This could foster the expectation that the court process will be similarly unfair, thereby decreasing compliance.<sup>44</sup>

Since the negative point estimates are not statistically significant, we can't make a definitive claim about the adverse effects of monetary bail. We do know, however, that monetary bail was not necessary to ensure appearance for the large majority of those evaluated. One possible explanation 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 percent of defendants lived in a zip code where the median income is less than \$30,000, and 75 percent 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.<sup>45</sup> 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, Evans, and Beckett 2010).

Another possibility is 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.<sup>46</sup> Failure to pay court debt can also result in criminal penalties, including incarceration.<sup>47</sup>

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 works 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, Ouss, and Shah (2020) find that simple interventions like increasing the

<sup>43</sup> Defendants are discouraged from speaking during the bail hearing, as they have not yet had a chance to speak to counsel about their case.

<sup>44</sup> 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.

<sup>45</sup> Ideally, we would have liked to determine the percent change in FTA that could be ruled out as a function of changes in cash bail dollar amounts. However, bail amounts are heavily skewed, and our results are somewhat sensitive to how we deal with outliers. With this in mind, we do not include these analyses in the paper.

<sup>46</sup> 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/>.

<sup>47</sup> 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 life.

salience of the court date on a citation and sending text message reminders decreased failures to appear by 13 percent and 21 percent, respectively. Taken together, these results suggest that interventions that marginally increase incentives 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. Impacts by Race and Ethnicity*

So far, we have presented average treatment effects of the No-Cash-Bail policy. In this section, we explore differences in implementation across Black, White, and Hispanic defendants. Given the disproportionate representation of Black and Hispanic groups in the criminal legal system, as well as evidence of racial bias in the setting of bail (Ayres and Waldfogel 1994; Arnold, Dobbie, and Yang 2018), the racial/ethnic impacts of the reform are of particular interest.

In Table 8, we break out our sample by defendant race and ethnicity. Race information is provided in the court data, and Hispanic ethnicity is identified by surname.<sup>48</sup> Panel A presents our main results for non-Hispanic Black defendants, panel B presents results for non-Hispanic White defendants, and panel C presents results for Hispanics.<sup>49</sup> A few things are to note from this table. First, the pre-reform ROR rates are very different: among eligible defendants, 68 percent of White defendants were getting ROR, compared to 41–44 percent of Black and Hispanic defendants. Second, the estimated treatment effect is larger for Black and Hispanic defendants than for White defendants, both in absolute terms as well as relative to the pre-reform ROR rate. (The difference is not statistically significant). Black defendants experienced a 13 percentage point (30 percent) increase in ROR, and Hispanic defendants experienced an 11 percentage point (26 percent) increase. This is compared to a 7.9 percentage point (12 percent) relative increase in ROR for White defendants.<sup>50</sup>

However, this interpretation is somewhat misleading since it ignores base rates in the pool of defendants who were in a position to benefit from the reform. To illustrate this, consider the following example. Imagine the data consist 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

<sup>48</sup> Ethnicity is determined using the `ethnicolr` Python function that attributes the likely ethnicity of a person using census data.

<sup>49</sup> Only about 1 percent of our sample has a race label other than Black or White.

<sup>50</sup> As with our main results, there was no change in pretrial detention and no increase in pretrial misconduct for either group.

TABLE 8—DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECTS OF NO-CASH-BAIL, BY DEFENDANT RACE AND ETHNICITY

	ROR (1)	Jail (2)	FTA (3)	Recidivism (4)
<i>Panel A. Black, non-Hispanic</i>				
<i>Eligible × Post 02/21</i>	0.13 (0.024)	−0.015 (0.023)	−0.010 (0.015)	−0.018 (0.0095)
Mean dep. var.	0.439	0.189	0.206	0.149
Observations	12,602	12,602	12,602	12,602
<i>Panel B. White, non-Hispanic</i>				
<i>Eligible × Post 02/21</i>	0.079 (0.024)	0.032 (0.027)	−0.018 (0.016)	−0.026 (0.022)
Mean dep. var.	0.677	0.091	0.329	0.174
Observations	5,702	5,702	5,702	5,702
<i>Panel C. Hispanic</i>				
<i>Eligible × Post 02/21</i>	0.11 (0.023)	0.035 (0.030)	0.017 (0.015)	−0.0052 (0.028)
Mean dep. var.	0.413	0.233	0.225	0.215
Observations	4,285	4,285	4,285	4,285
Controls	Yes	Yes	Yes	Yes

*Notes:* This table presents estimates of  $\delta$  in equation (1), broken down by defendant race and ethnicity. 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 three 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 six 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. Standard errors, clustered at the offense level, are in parentheses.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

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.

We can evaluate whether the selection mechanism was correlated with race by comparing pre-/postreform changes in the racial/ethnic composition of eligible defendants with cash bail set. Before the policy change, White defendants represented 19 percent of defendants with monetary/supervisory conditions set. After the policy change, this went down to 16 percent. Hispanic representation among the group of eligible defendants with cash bail also decreased, from 26 percent to 24 percent. In contrast, the representation of eligible Black defendants receiving cash bail went up from 54 percent to 59 percent. This suggests that White and Hispanic defendants were slightly more likely to be granted ROR as a result of the

reform than Black defendants.<sup>51</sup> Overall, these analyses suggest that the policy's impact on racial disparities depends on the framework chosen.

#### D. Generalizability

This section considers the extent to which our results generalize to other defendants or other jurisdictions. We focus our discussion on the generalizability of our main results, namely, that cash bail does not meaningfully deter FTA or crime among released defendants. However, we wanted to briefly flag two other issues relevant to bail reform: incapacitation and the supervisory functions of bail bondsmen. Jurisdictions that see a large decrease in the jail population may see a mechanical increase in FTA or pretrial crime, simply since fewer are “incapacitated” by incarceration. Likewise, jurisdictions that move away from the use of bail bondsmen may need to find other methods to fulfill the various roles bondsmen play, such as communicating with defendants about court appearances. A more comprehensive discussion about the consequences of bail reform should take into account these multiple potential channels.

Even after the reform, 30 percent of eligible defendants were still being assigned monetary bail. Could these individuals be granted ROR without adverse consequences? Or do these individuals require cash bail to incentivize appearance in court and deter crime? This type of generalizability question is motivated by a particular model of bail-setting behavior: one in which magistrates grant ROR first to those for whom cash bail is least necessary. If so, then those who received ROR as a result of the No-Cash-Bail policy may be those for whom the deterrent effect of cash bail is particularly low. The remaining defendants—those who still have cash bail set—may require cash bail to prevent misconduct.

We formalize this bail-setting model in equation (5).<sup>52</sup> In this model,  $S$  is the cost of misconduct,  $M_i|ROR$  is expected misconduct if defendant  $i$  was granted ROR, and  $M_i|bail$  is expected misconduct if that defendant was required to pay cash bail.<sup>53</sup> The left-hand side of the equation is therefore the cost-weighted treatment effect of monetary bail.  $C_{j,t}$  represents the costs of setting cash bail for judge  $j$  in time  $t$ , which could include fiscal costs of pretrial detention, costs to the defendant, and personal costs experienced by the magistrate, such as the cost of disregarding the prosecutor's recommendations.

$$(5) \quad S \times (M_i|ROR - M_i|bail) > C_{j,t} \Rightarrow \text{Set cash bail.}$$

Under this model, the No-Cash-Bail policy increased ROR rates by increasing  $C_{j,t}$ . What would happen if a new policy increased  $C_{j,t}$  even further—say, by penalizing magistrates who continue to set cash bail on eligible defendants? Those who were granted ROR as a result of this expanded reform might be meaningfully

<sup>51</sup> We provide a more formal approach to this analysis in a companion paper, Ouss and Stevenson (2022). In short, our method adapts a complier analysis from the instrumental variables literature in order to identify which type of defendants were selected to benefit from a discretionary reform.

<sup>52</sup> Thanks to anonymous referees for their suggestions about this section.

<sup>53</sup> For simplicity, this model does not consider supervised release.

different from those who were granted ROR in the No-Cash-Bail policy. The local average treatment effect we estimated previously might understate the deterrent effects of cash bail for this new group of hypothetical compliers.

We provide suggestive evidence on this by exploiting a second natural experiment in Philadelphia. Defendants in Philadelphia are quasi-randomly assigned to magistrates who vary in both their pre-reform levels of leniency and in the extent to which they respond to the reform. The treatment effects among these six magistrates provide speculative evidence about the impacts of bail reform under different conditions.

Quasi-random assignment to magistrates in Philadelphia arises from their rotating work schedule—a feature that has been extensively documented in Gupta, Hansman, and Frenchman (2016); Dobbie, Goldin, and Yang (2018); and Stevenson (2018b). We confirm that quasi-random assignment persists in our time period of analysis as well.<sup>54</sup> Table 9 divides the sample into cases with bail set by each of six quasi-randomly assigned magistrates and uses the difference-in-difference strategy to test the impact of the No-Cash-Bail policy on each subsample. 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.

Magistrates 5 and 6 were the two most lenient magistrates before the reform. This could be because they view cash bail as relatively more costly than other magistrates did (high  $C_{j,t}/S$ ) or because they were less convinced about the necessity of cash bail to deter misconduct (low  $M_i|ROR - M_i|bail$ ). Either way, as long as these magistrates prioritized granting ROR to those for whom it was least necessary (the core assumption behind the generalizability concern), they are the ones most likely to have exhausted the pool of defendants who can be granted ROR without adverse consequences.

Nonetheless, these magistrates were able to increase ROR by 6–11 percentage points without detectable adverse consequences. This suggests that Philadelphia has not, in fact, exhausted the pool of defendants who are receiving unnecessarily restrictive bail conditions. At the very least, stricter magistrates should be able to “catch up” to these more lenient magistrates without increasing FTA or crime rates. In fact, Magistrate 1, the most strict magistrate before the reform, increased ROR by 30 percentage points as a result of the No-Cash-Bail policy. Even so, their postreform ROR rates were still lower than the postreform ROR rates of Magistrates 5 and 6. And, despite this large shift in bail practices, there were no detectable adverse consequences for Magistrate 1 either.

Note that the increase in ROR from Magistrates 5 and 6 came almost entirely from a reduction in the use of cash bail; there was no change in supervision rates. Similar to the subsetting exercise shown in Table 6, this provides additional evidence on the impact of cash bail separate from pretrial supervision.

<sup>54</sup> We regress case and defendant characteristics on bail magistrates dummies (omitting one bail magistrate), while controlling for day of the week, shift, and quarter—which we include in our analyses. As shown in online Appendix Table A11, we find that case characteristics are very similar across these six bail magistrates. The last row of this table presents *p*-values for a joint *F*-test, testing whether the judge coefficients are jointly equal to zero. For all but one variable (defendant race), we cannot reject the null.

TABLE 9—DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF NO-CASH-BAIL POLICY, BY BAIL MAGISTRATE

	Bail magistrate					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. ROR</i>						
<i>Eligible</i> × <i>Post</i> 02/21	0.30 (0.023)	0.020 (0.020)	0.085 (0.020)	0.12 (0.023)	0.11 (0.023)	0.064 (0.022)
Mean dep. var.	0.324	0.445	0.490	0.543	0.599	0.621
<i>Panel B. Jail</i>						
<i>Eligible</i> × <i>Post</i> 02/21	−0.044 (0.028)	0.044 (0.026)	0.0030 (0.025)	0.012 (0.023)	0.0057 (0.026)	−0.0081 (0.026)
Mean dep. var.	0.200	0.171	0.159	0.175	0.154	0.155
<i>Panel C. Cash</i>						
<i>Eligible</i> × <i>Post</i> 02/21	−0.15 (0.022)	−0.020 (0.020)	−0.039 (0.016)	−0.070 (0.022)	−0.11 (0.023)	−0.064 (0.022)
Mean dep. var.	0.516	0.555	0.416	0.359	0.396	0.379
<i>Panel D. Supervision</i>						
<i>Eligible</i> × <i>Post</i> 02/21	−0.15 (0.015)	— —	−0.046 (0.014)	−0.053 (0.014)	−0.0041 (0.0027)	— —
Mean dep. var.	0.160	0.000	0.094	0.098	0.004	0.000
<i>Panel E. FTA</i>						
<i>Eligible</i> × <i>Post</i> 02/21	−0.034 (0.029)	−0.015 (0.027)	0.015 (0.026)	0.018 (0.025)	−0.018 (0.027)	−0.015 (0.028)
Mean dep. var.	0.253	0.254	0.243	0.235	0.237	0.265
<i>Panel F. Recidivism</i>						
<i>Eligible</i> × <i>Post</i> 02/21	−0.0091 (0.026)	0.0070 (0.025)	0.036 (0.024)	−0.072 (0.023)	−0.036 (0.025)	−0.072 (0.026)
Mean dep. var.	0.165	0.172	0.154	0.189	0.179	0.172
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,187	3,922	3,586	3,449	3,287	4,096

*Notes:* This table presents estimates of  $\delta$  in equation (1), separately for each of the 6 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 three 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 six 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. Standard errors, clustered at the offense level, are in parentheses.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

The magistrate-level analysis is not perfect. For one, it is possible that the stricter magistrates are better at identifying the treatment effects of monetary bail than lenient magistrates or that they care more about this dimension.<sup>55</sup> Furthermore, the standard

<sup>55</sup> The fact that the strictest magistrate was able to increase ROR by 30 percentage points without adverse consequences counsels against.

TABLE 10—DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF NO-CASH-BAIL POLICY ON ROR, JAIL, FTA, AND RECIDIVISM, KEEPING ONLY FELONY CASES

	ROR (1)	Jail (2)	FTA (3)	Recidivism (4)
<i>Eligible × Post 02/21</i>	0.17 (0.019)	−0.020 (0.020)	−0.010 (0.0086)	−0.017 (0.0090)
Controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.172	0.322	0.182	0.184
Observations	12,038	12,038	12,038	12,038

*Notes:* This table presents estimates of  $\delta$  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 three nights in jail immediately after their initial bail hearing. FTA means failure to appear in court. Recidivism (new criminal charges) is measured within six 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. Standard errors, clustered at the offense level, are in parentheses.

*Data source:* Court dockets from the Pennsylvania Unified Judicial System

errors on the magistrate-level analysis are often too large to preclude firm conclusions. Yet despite some drawbacks, we consider the magistrate analysis generally supportive of the argument that many defendants in Philadelphia are still receiving unnecessarily restrictive bail.

We provide one additional analysis related to generalizability questions in Table 10. This table subsets the eligible cases to include only people charged with a felony. (As a reminder, about 43 percent of eligible offenses and 69 percent 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 in pretrial misconduct. This suggests that even among more serious offenses, pretrial conditions of release did not affect misconduct.

Can our results speak to the deterrent effect of bail 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 differences between Philadelphia and the national average. Before the No-Cash-Bail policy, 79 percent of felony defendants in Philadelphia had secured monetary bail, and 40 percent of these were detained until disposition. Nationally, 69 percent of felony defendants had monetary bail set, and 40 percent of these were detained until case disposition (Reaves 2013).<sup>56</sup> 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

<sup>56</sup>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.

who 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 toward leniency, magistrates may have been setting unnecessarily restrictive conditions.

### 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.

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, 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 prosecutor-led criminal justice reform may lead to systemic change. Third, our results provide practical information relevant to the current bail reform movement. It mitigates concerns about losing important deterrent effects as jurisdictions move away from cash bail. It also raises questions about current practices. If nonappearance in court has more to do with inattention than deliberate choice, then interventions targeted toward the root of the problem—such as court reminders—will be more effective than those that target incentives (Fishbane, Ouss, and Shah 2020).

Lastly, our paper poses questions about determinants of judicial behaviors. If cash bail does not act as a deterrent, why were bail magistrates assigning unnecessarily restrictive conditions to so many defendants? One potential explanation is asymmetric penalties in errors. A type II error is when bail is set too low and as a result the defendant commits crime or fails to appear in court. This type of error is visible to the community and may result in negative consequences for the magistrate or for those who set bail policies. Indeed, there have been numerous prominent examples in which bail magistrates and/or bail reformers have received substantial criticism when a person released on low bail goes on to reoffend.<sup>57</sup> In contrast, type I errors—setting monetary bail or pretrial supervision when none is necessary to ensure compliance—is much less visible. 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.

The implication of asymmetric penalties for errors 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. Since many defendants receive bail that is more restrictive than necessary to ensure compliance, one could eliminate restrictive bail conditions for them with little adverse consequences. Careful attention to the motivation of different actors can help in the design of effective policy.

<sup>57</sup> For example, <https://www.foxnews.com/media/new-york-bail-laws-tiffany-harris>.

## REFERENCES

- Abrams, David S., and Chris Rohlfs. 2011. "Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment." *Economic Inquiry* 49 (3): 750–70.
- Albright, Alex. 2022. "No Money Bail, No Problems? Trade-offs in a Pretrial Automatic Release Program." Unpublished.
- Arnold, David, Will Dobbie, and Crystal S. Yang. 2018. "Racial Bias in Bail Decisions." *Quarterly Journal of Economics* 133 (4): 1885–1932.
- Arora, Ashna. 2018. "Too Tough on Crime? The Impact of Prosecutor Politics on Incarceration." Unpublished.
- Ayres, Ian, and Joel Waldfogel. 1994. "A Market Test for Race Discrimination in Bail Setting." *Stanford Law Review* 46 (5): 987–1047.
- Bar-Ilan, Avner, and Bruce Sacerdote. 2004. "The Response of Criminals and Noncriminals to Fines." *Journal of Law and Economics* 47 (1): 1–17.
- Bazon, Emily. 2019. *Charged: The New Movement to Transform American Prosecution and End Mass Incarceration*. New York, NY: Random House.
- Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (2): 169–217.
- Block, Michael Kent, Frederick Carl Nold, and Joseph Gregory Sidak. 1981. "The Deterrent Effect of Antitrust Enforcement." *Journal of Political Economy* 89 (3): 429–45.
- Calónico, Sebastian, Matías D. Cattaneo, and Rocio Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.
- Dobbie, Will, Jacob Goldin, and Crystal S. Yang. 2018. "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review* 108 (2): 201–40.
- Draca, Mirko, Stephen Machin, and Robert Witt. 2011. "Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks." *American Economic Review* 101 (5): 2157–81.
- Fishbane, Alissa, Aurelie Ouss, and Anuj K. Shah. 2020. "Behavioral Nudges Reduce Failure to Appear for Court." *Science* 370 (6517).
- Funk, Kellen. 2019. "The Present Crisis in American Bail." *Yale Law Journal Forum* 128: 1098–1125.
- Gneezy, Uri, and Aldo Rustichini. 2000. "A Fine is a Price." *Journal of Legal Studies* 29 (1): 1–17.
- Goncalves, Felipe, and Steven Mello. 2022. "Should the Punishment Fit the Crime? Discretion and Deterrence in Law Enforcement." Unpublished.
- Gupta, Arpit, Christopher Hansman, and Ethan Frenchman. 2016. "The Heavy Costs of High Bail: Evidence from Judge Randomization." *Journal of Legal Studies* 45 (2): 471–505.
- Harris, Alexes. 2016. *A Pound of Flesh: Monetary Sanctions as Punishment for the Poor*. New York, NY: Russell Sage Foundation.
- Harris, Alexes, Heather Evans, and Katherine Beckett. 2010. "Drawing Blood from Stones: Legal Debt and Social Inequality in the Contemporary United States." *American Journal of Sociology* 115 (6): 1753–99.
- Hausman, Catherine, and David S. Rapson. 2018. "Regression Discontinuity in Time: Considerations for Empirical Applications." *Annual Review of Resource Economics* 10: 533–52.
- Helland, Eric, and Alexander Tabarrok. 2004. "The Fugitive: Evidence on Public versus Private Law Enforcement from Bail Jumping." *Journal of Law and Economics* 47 (1): 93–122.
- Hull, Peter. 2017. "Examiner Designs and First Stage F-Statistics: A Caution." Unpublished.
- Kleinberg, Jon, Himabindu Lakkaraju, Jure Leskovec, Jens Ludwig, and Sendhil Mullainathan. 2018. "Human Decisions and Machine Predictions." *Quarterly Journal of Economics* 133 (1): 237–93.
- Krumholz, Sam. 2020. "The Effect of District Attorneys on Local Criminal Justice Outcomes." Unpublished.
- Kuziemko, Ilyana. 2013. "How Should Inmates Be Released from Prison? An Assessment of Parole versus Fixed-Sentence Regimes." *Quarterly Journal of Economics* 128 (1): 371–424.
- Luca, Dara Lee. 2015. "Do Traffic Tickets Reduce Motor Vehicle Accidents? Evidence from a Natural Experiment." *Journal of Policy Analysis and Management* 34 (1): 85–106.
- Martin, Karin D., Bryan L. Sykes, Sarah Shannon, Frank Edwards, and Alexes Harris. 2018. "Monetary Sanctions: Legal Financial Obligations in US Systems of Justice." *Annual Review of Criminology* 1: 471–95.
- Mayson, Sandra G. 2020. "Detention by Any Other Name." *Duke Law Journal* 69 (7): 1643–80.
- Mayson, Sandra G., and Megan T. Stevenson. 2020. "Misdemeanors by the Numbers." *Boston College Law Review* 61 (3): 971–1044.

- Mello, Steven.** 2018. "Speed Trap or Poverty Trap? Fines, Fees, and Financial Wellbeing." Unpublished.
- Mogstad, Magne, Alexander Torgovitsky, and Christopher R. Walters.** 2021. "The Causal Interpretation of Two-Stage Least Squares with Multiple Instrumental Variables." *American Economic Review* 111 (11): 3663–98.
- Myers, Samuel L., Jr.** 1981. "The Economics of Bail Jumping." *Journal of Legal Studies* 10 (2): 381–96.
- Nagin, Daniel S., and Cody W. Telep.** 2017. "Procedural Justice and Legal Compliance." *Annual Review of Law and Social Science* 13: 5–28.
- Ouss, Aurélie, and Megan T. Stevenson.** 2022. "Who Benefits from a Discretionary Reform? Importing Complier Analysis to a Difference-in-Differences Framework." Unpublished.
- Ouss, Aurélie, and Megan T. Stevenson.** 2023. "Replication data for: Does Cash Bail Deter Misconduct?" American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. <https://doi.org/10.3886/E159582V1>.
- Pfaff, John.** 2017. *Locked In: The True Causes of Mass Incarceration—and How to Achieve Real Reform*. New York, NY: Basic Books.
- Philadelphia Municipal Courts, Clerk.** 2018. "Court Records."
- Philadelphia Police Department, Records.** 2018. "Arrest Records."
- Pretrial Justice Institute.** 2020. "What's Happening in Pretrial Justice?" Pretrial Justice Institute.
- Reaves, Brian A.** 2013. *Felony Defendants in Large Urban Counties, 2009*. Washington, DC: Bureau of Justice Statistics.
- Rehavi, M. Marit, and Sonja B. Starr.** 2014. "Racial Disparity in Federal Criminal Sentences." *Journal of Political Economy* 122 (6): 1320–54.
- Rios, Victor M.** 2011. *Punished: Policing the Lives of Black and Latino Boys*. New York, NY: NYU Press.
- Schnacke, Timothy R., Michael R. Jones, and Claire M.B. Brooker.** 2010. *The History of Bail and Pretrial Release*. Baltimore, MD: Pretrial Justice Institute.
- Shubik-Richards, Claire, and Don Stemen.** 2010. *Philadelphia's Crowded, Costly Jails: The Search for Safe Solutions*. Philadelphia, PA: Philadelphia Research Initiative.
- Sloan, CarlyWill.** 2019. "Racial Bias by Prosecutors: Evidence from Random Assignment." Unpublished.
- Starger, Colin, and Michael Bullock.** 2018. "Legitimacy, Authority, and the Right to Affordable Bail." *William and Mary Bill of Rights Journal* 26 (3): 589–627.
- Stevenson, Megan T.** 2018a. "Assessing Risk Assessment in Action." *Minnesota Law Review* 103 (1): 303–84.
- Stevenson, Megan T.** 2018b. "Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes." *Journal of Law, Economics, and Organization* 34 (4): 511–42.
- Tuttle, Cody.** 2019. "Racial Disparities in Federal Sentencing: Evidence from Drug Mandatory Minimums." Unpublished.
- Wiseman, Samuel R.** 2014. "Pretrial Detention and the Right to be Monitored." *Yale Law Journal* 123 (5): 1118–1625.
- Yang, Crystal S.** 2015. "Free at Last? Judicial Discretion and Racial Disparities in Federal Sentencing." *Journal of Legal Studies* 44 (1): 75–111.

**This article has been cited by:**

1. Jaquelyn L. Jahn, Jessica T. Simes, Jonathan Jay. 2024. Evaluating Firearm Violence After New Jersey's Cash Bail Reform. *JAMA Network Open* 7:5, e2412535. [[Crossref](#)]
2. Jennifer D. Wood, Amy C. Watson, Leah Pope, Amanda Warnock, Veronica Nelson, Nili Gesser, Adria Zern, Aaron Stagoff-Belfort, Jason Tan de Bibiana, Michael T. Compton. 2023. Contexts shaping misdemeanor system interventions among people with mental illnesses: qualitative findings from a multi-site system mapping exercise. *Health & Justice* 11:1. . [[Crossref](#)]
3. Jenny E. Carroll. 2023. The Ever-Shifting Ground of Pretrial Detention Reform. *Annual Review of Law and Social Science* 19:1, 75-91. [[Crossref](#)]
4. E. Jason Baron, Brian Jacob, Joseph Ryan. 2023. Pretrial juvenile detention. *Journal of Public Economics* 217, 104798. [[Crossref](#)]