

The Heavy Costs of High Bail: Evidence from Judge Randomization

Arpit Gupta*
Christopher Hansman†
Ethan Frenchman‡§

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Abstract

Roughly 450,000 people are detained awaiting trial on any given day, typically because bail has not been posted. Using a large sample of criminal cases in Philadelphia and Pittsburgh, we analyze the consequences of bail assessment and pretrial detentions by exploiting the variation in bail setting tendencies among randomly assigned bail judges. Our estimates suggest that the assignment of money bail leads to a 6 percentage point rise in the likelihood of pleading guilty, and a 4 percentage point rise in recidivism. We also find evidence for racial bias in bail setting. Our results highlight the importance of credit constraints in shaping defendant judicial outcomes and point to important fairness considerations in the institutional design of pretrial detention programs.

*Email: ag2968@columbia.edu; Columbia GSB

†Email: cjh2182@columbia.edu; Columbia University

‡Maryland Office of the Public Defender

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1 Introduction

Roughly 450,000 people in the United States are held in jail awaiting trial on any given day.¹ These individuals have not been convicted of a crime, and are presumed to be innocent of the charges for which they have been jailed. For the majority of defendants, the barrier to release is financial: they are unable or unwilling to post bail. Due to limited judicial resources, waiting times for trials are often measured in months or years. Faced with the choice between prohibitive bail assessments or lengthy detention spells, many defendants may decide to avoid trial and take a plea deal, accepting a criminal conviction and any associated consequences.

While there is significant evidence of a correlation between pretrial detention and sentencing, consistent with a direct impact of bail assessment on defendant outcomes (for instance, Lowenkamp, VanNostrand and Holsinger (2013), Phillips (2007), and Phillips (2008)), prior research has struggled with causally estimating the impacts of bail due to the endogenous nature of the detention hearing.² When judges determine whether to release an arrestee and the conditions of such release, they consider, among other things, the facts of the case; the strength of the evidence; the defendants criminal history, ties to the local community; and financial resources. These factors may well be related to actual guilt and render correlations between money bail assessments and convictions difficult to interpret.

This paper investigates the causal impact of money bail on guilty pleas and recidivism using comprehensive court data from the two largest cities in Pennsylvania: Philadelphia and Pittsburgh. By money bail, we refer to the requirement that criminal defendants post a cash amount as bail in exchange for freedom until the trial (other forms of bail allow the defendant to remain free until trial or only post bail in the event of a non-appearance). We exploit the close to random assignment of defendants to court magistrates responsible for assessing bail, focusing in particular on the centralized, 24 hour-a-day arraignment system in Philadelphia. We document that assignment to more severe judges, as defined by the propensity to assign money bail, raises the probability of being assessed money bail for reasons unrelated to other

¹See <http://www.prisonpolicy.org/reports/pie2016.html>

²A notable exception is Abrams and Rohlfs (2011) who exploit an experiment in Philadelphia in the 1980s, although they do not examine the impact of money bail on convictions.

case or defendant characteristics. The nature of our natural experiment allows us to then study the implications of effectively exogenous impositions of money bail on further defendant outcomes.

We find that the assessment of money bail is a significant, independent cause of convictions and recidivism. Criminal defendants who are assessed money bail are 6 percentage points more likely to be convicted. These effects appear to be driven by the subset of cases where arrestees fail to post money bail and are therefore detained, rather than by defendants who are able to make bail. We also examine the lasting negative effects of money bail after the trial, and document that the assessment of money bail increases recidivism in our sample by 4 percentage points.

Our results are primarily driven by the extensive margin—the choice of whether money bail is required or not—rather than the intensive margin of smaller or larger bail amounts. In other words, the assessment of money bail, rather than the bail size, appears to be driving our results. A key implication of this finding is that simply lowering required bail amounts is unlikely to be a sufficient response to ameliorate harms imposed by the bail assessment process. Our findings persist among a number of subgroups—non-white defendants, those assigned a public defender, and male defendants. We find estimates that are even larger among defendants charged with felonies (though we do not reach statistical significance in that sample), suggesting that our effects are not driven by criminal pleas to petty crimes. As a robustness check, we investigate money bail assessment and outcomes in Pittsburgh, where judicial assignment is based on arrest location, and find similar results.

While the coercion of guilty pleas from defendants who wish to avoid further incarceration is one potential mechanism by which money bail may work to cause convictions, we do not attempt to isolate the exact channel. The Supreme Court has recognized the variety of costs imposed by pretrial detention, writing that it “may imperil the suspect’s job, interrupt his source of income, . . . impair his family relationships [and affect his] ability to assist in preparation of his defense.”³ Among many possible explanations for the impact of money bail on guilt are the difficulties for detained defendants to communicate with their counsel

³*Gerstein v. Pugh*, 420 U.S. 103, 114 (1975).

and properly prepare a defense; changes in behavior among various institutional actors such as prosecutors, defense attorneys, judges, and jurors toward defendants who are incarcerated pretrial; the limited opportunity for detained arrestees to participate in diversionary programs and other resolutions not resulting in convictions; and the financial strain of making bail.⁴ Money bail may also directly influence recidivism through the harms of pretrial incarceration imposed upon those those unable to make bail, posttrial incarceration following conviction, or the stigma of conviction.⁵

Despite the multiplicity of channels, we emphasize that our results provide novel evidence for a strong and causal role for bail assessments and pretrial detentions on defendant outcomes. The relationship between money bail, conviction, and recidivism suggests a strong interaction between poverty and the criminal justice system. A large literature has examined the credit constraints facing American households that make even small money bail amounts difficult to post (see Lusardi, Schneider and Tufano (2011)). While it is feasible that money bail could impact convictions amongst those with sufficient liquid assets to post bail, it is more likely that these effects come primarily from the credit-constrained. It is important to note that within our sample, 63 percent of arrestees qualified for representation by the public defender, and therefore are presumably indigent.

The interactions between money bail and subsequent defendant outcomes pose substantive legal issues. From a liberty perspective, these relate to the deprivation of pretrial liberty and the basic assumption that convictions reflect only the merits of the underlying case. Bail also raises equality issues related to the requirement of equal access to justice and the prohibition against wealth discrimination.

Our findings also raise institutional design questions regarding the American money bail system as a whole. One suggested solution to the perceived inequities of pretrial detention is the adoption of empirical pretrial risk assessments. Such tools, based on multivariate models built from large sets of defendant data, create recommendations for release or conditions of release. Despite the use of such assessment tools in Philadelphia and Pittsburgh in the time

⁴Though bail bondsman can front bail amounts in exchange for a collateral value which is typically 10%, even these relatively smaller collateral values may be out of reach for criminal defendants facing liquidity constraints.

⁵See, e.g., Baylor (2015); Appleman (2012); Phillips (2008)

period covered by our analysis, our work suggests that pretrial risk assessments tools do not fully anchor the role for judicial discretion, and call into question the narrow tailoring of pretrial detention to the government interests of public safety and return to court.

Given the intense debate over the role of race the criminal justice system, we additionally study the role of racial bias in the sentencing process. On average, non-white defendants are more likely to be assessed money bail, despite the fact that they are less likely to be ultimately convicted. To attempt to uncover a causal channel, we explore the random assignment of cases to bail judges who exhibit racial bias in bail setting relative to the court average. Non-whites assigned to these judges face a 1–2 percentage point higher chance of being assessed money bail.

To contextualize our findings on guilt and recidivism, we examine whether the assessment of money bail induces defendants to appear at trial, the stated purpose of the money bail system. We find that levying money bail does not seem to increase the probability that a defendant appears at trial. While sentencing decisions typically involve tradeoffs between harms to defendants and benefits to the broader public; our work suggests that bail assessments and the pretrial detention system may be negatively impacting both defendants and the public (via detention costs and recidivism), without producing any compensating public gain.

Our research has a close connection to the legal literature on pretrial justice⁶. There is a large body of evidence suggesting that pretrial custody status is associated with the ultimate outcomes of criminal cases, with released defendants consistently faring better than defendants in detention.⁷ Past work has uncovered the correlation between money bail, pretrial detention, and conviction (e.g. Phillips (2007), Phillips (2008)), and examined other policy considerations regarding the design of pretrial detention systems (See Lowenkamp, VanNostrand and Holsinger (2013), Bechtel et al. (2012) and Phillips (2012)).

In the economics literature, beyond Abrams and Rohlfs (2011), our work is most closely related to the growing set of papers in economics utilizing random assignment of judges

⁶The Pretrial Justice Institute has created an exceptionally detailed bibliography, available at: <http://www.pretrial.org/wpfb-file/pji-pretrial-bibliography-pdf/>

⁷See ABA Standards for Criminal Justice: Pretrial Release 29 (3d. ed. 2007)

within the criminal justice system such as Kling (2006), Doyle Jr (2007), Doyle Jr (2008) and Mueller-Smith (2016), as well as in other contexts, such as Chang and Schoar (2007), Aizer and Doyle (2015) and Dobbie and Song (2015). Our work also relates to papers examining racial bias among judges, such as Depew, Eren and Mocan (2016) and Anwar and Fang (2015). We differ primarily in presenting a novel exploration of the pretrial detention system.

Our paper is structured as follows: Section 2 presents legal background on the bail system and the arraignment process in Philadelphia and Pittsburgh, Section 3 explains our data and empirical strategy, Section 4 contains estimation results, and Section 5 concludes.

2 Legal Background and Bail Hearings

2.1 Legal Background

Any person who is arrested without a warrant is entitled to a hearing within 48 hours of arrest.⁸ At this hearing, a judicial officer must determine whether there is probable cause for the arrest prior to the imposition of “any significant pretrial restraint of liberty.”⁹ Across the country, this initial appearance has evolved into a “hearing at which the magistrate informs the defendant of the charge in the complaint, and of various rights in further proceedings, and determines the conditions for pretrial release.”¹⁰

At a bail hearing, bail magistrates have a number of options available to them:

1. Release on Recognizance (ROR) – Requires defendant only to agree to appear at a later date
2. Non-monetary Conditions – Allows some non-monetary restriction to be placed on the defendant, such as pretrial supervision, or a curfew
3. Unsecured Monetary Condition – Written agreement to be liable for a fixed financial payment

⁸*Cnty. of Riverside v. McLaughlin*, 500 U.S. 44, 56 (1991); *Gerstein v. Pugh*, 420 U.S. 103, 114 (1975)

⁹*Gerstein*, 420 U.S. at 125

¹⁰*Rothgery v. Gillespie Cnty, Tex.*, 554 U.S. 191, 199 (2008)

4. Secured Monetary Condition – Defendant must satisfy a financial condition paid to the court either directly, through a bail bondsman, or other collateral such as real property, in order to secure release
5. No-Bail – Defendant is to be held unconditionally pending trial

A variety of constitutional and legal protections constrain the discretion of judicial officers in determining whether to detain or release a defendant and what conditions to place on such release. First, pretrial liberty is a fundamental right independently guaranteed by the Constitution:¹¹ “In our society liberty is the norm, and detention prior to trial or without trial is the carefully limited exception.”¹² Therefore pretrial detention must be “narrowly focus[ed]” to the governments “compelling” interests in public safety and ensuring that defendants return to court.¹³ In determining whether to release a defendant, and what conditions to place on such release, the judicial officer must make an individualized assessment of the case and defendant.¹⁴

Bail also raises issues covered under the Equal Protection Clause of the Fourteenth Amendment to the Constitution which has been interpreted to prohibit “punishing a person for his poverty.”¹⁵ Persons may not be incarcerated solely due to their inability to make a payment.¹⁶ For this reason such payments must take into account a persons financial resources, since “there can be no equal justice where the kind of trial a man gets depends on the amount of money he has.”¹⁷ Therefore, access to justice and the courts cannot be distributed according to arrestees ability to pay.¹⁸ These federal guarantees find a state parallel in the Pennsylvania Rule of Criminal Procedure 523 which explicitly requires magistrates to consider arrestees financial resources when setting money bail.

¹¹ *Foucha v. Louisiana*, 504 U.S. 71, 80 (1992); *Salerno*, 481 U.S. at 750

¹² *Salerno*, 481 U.S. at 755

¹³ *Salerno*, 481 U.S. at 750-51; *Stack v. Boyle*, 342 U.S. 1, 4 (1951); see also ABA Standards for Criminal Justice: Pretrial Release 37 (3d. ed. 2007)

¹⁴ *Stack*, 342 U.S. at 5

¹⁵ (*Bearden v. Georgia*, 461 U.S. 660, 671 (1983))

¹⁶ *Id.*; see also *Tate v. Short*, 401 U.S. 395 (1971); *Williams v. Illinois*, 399 U.S. 235 (1970); and *Smith v. Bennett*, 365 U.S. 708, 709 (1961)

¹⁷ *Griffin v. Illinois*, 351 U.S. 12, 19 (1956)

¹⁸ *Id.*; see also *Smith v. Bennett*, 365 U.S. 708 (1961)

2.2 Bail Hearings

In Pennsylvania, a judicial magistrate presides over the initial appearance of an arrestee. In the majority of the state, excluding Philadelphia, these magistrates are elected to a six-year term to serve in a particular district court. A single magistrate handles the majority of the arrests that occur within their jurisdiction, although many arrestees are seen by other magistrates during weekends, nights, and other periods when the presiding magistrate is not in service.

Philadelphia, however, features a unique process with a centralized municipal court that operates 24 hours a day. Defendants from across the city appear before one of a team of appointed magistrates who conduct the initial detention hearing. Magistrates generally preside via CCTV over satellite locations in the city where arrestees are held. The centralized location, high case load, constant process, and rotating magistrate calendar result in the effectively random assignment of defendants to magistrates (an assumption we test). Importantly for our purposes, magistrates in Philadelphia only preside over the initial appearance; they do not preside over subsequent hearings or trials. As a result, we are able to interpret the role for judicial discretion by bail magistrates as manifesting only along the dimension of bail assessments.

At the pretrial detention hearing, which typically lasts only a few minutes, magistrates hear information from the defendant (or defendants counsel) and the prosecutor relevant to the defendants flight risk and public safety. This information includes the many factors set forth in Pennsylvania Rule of Criminal Procedure 523, such as: the nature of the offense, the strength of the evidence, and the defendants financial resources, family and community ties, criminal record, and prior failures to appear.

In Philadelphia and Pittsburgh, magistrates also hear from a representative from Pretrial Services, an independent government agency responsible for presenting a recommendation based on the findings of a standardized empirical risk assessment tool. We find that this tool does not eliminate the exercise of wide judicial discretion in determining whether to release or detain defendants, and the assessment of money bail as a condition of release.

Should money bail be set, detainees may only secure their release through the satisfaction of its financial terms. In Philadelphia, detainees may post 10% of the money bail amount directly to the court. In the alternative, detainees can satisfy bail through collateral such as real property or a bail bond. A high bail amount ensures the defendant is required to produce a higher cash sum. Detainees who cannot afford the financial condition of their release remain incarcerated for months or even years awaiting trial. Detainees have the opportunity to move for a reduction in their money bail after the initial hearing. We focus on the first assessed money bail as it is the product of a randomized judicial decision and find this initial decision is influential in determining the final amount the defendant is required to pay, regardless of modifications.

The timeline of defendant actions around the release determination varies from state to state. In Pennsylvania, the detention hearing precedes the entry of the plea. This timeline ensures that the magistrates assessment of money bail is a factor in the defendant’s plea decision from the beginning.

3 Data and Empirical Strategy

3.1 Data Summary

Through contact with the Administrative Office of the Pennsylvania Courts, we obtained comprehensive criminal data on criminal records in the state of Pennsylvania from 2010–2015. These include records taken at both local magistrate levels, as well as subsequent judicial and defendant decisions taken at the higher Common Pleas level. In Philadelphia, a separate Municipal Court system typically handles initial defendant arraignment.

Table 1 summarizes our data for our focal region of Philadelphia, where we are best able to establish judicial randomization, as well as Pittsburgh—the second largest jurisdiction in the state. Our data contain information about the entire history of detention determinations and money bail assessments on criminal defendants (although we focus on the money bail amount resulting from the initial hearing); disposition information on the list of charged offenses (we account for the full case history, but typically focus on the most severe offense);

bench warrant information; as well as final sentencing outcomes for individual defendants. Our first appendix table, Table A1, contains the top 10 most common offenses and basic characteristics of the cases associated with those offenses.

3.2 Empirical Strategy

A simple approach to addressing the role of bail would be to run the OLS regression:

$$Guilt_{it} = \alpha + \beta Bail_{it} + \varepsilon_{it}$$

Where $Bail_{it}$ is an indicator for whether or not individual i is assigned money bail in time t . Table 2 illustrates what would happen if we follow this strategy. Column 1 suggests that being assessed money bail results in a 1.4 percentage point increase in the probability of pleading guilty. This goes up to 4.3 percentage points in column 3 after adding a battery of additional controls, including gender, race, age, and offense fixed effects. Column 4 confirms this relationship when we focus on the log of the bail amount, instead of only the indicator for being assessed money bail. Figure 1 provides a similar illustration of this correlation for a particular offense: the Possession of Marijuana. Defendants charged with this offense are substantially more likely to be found guilty when a money bail is assessed.

While these estimates are consistent with the causal interpretation that higher bail amounts induce greater guilty outcomes, they are also consistent with a spurious correlation resulting from the endogenous bail assessment. Recall that bail assessments are not made randomly, but are exactly intended to be calibrated against the severity of offense; the flight risk of the individual; and even of the strength of the case made by the prosecuting attorney. As these factors driving bail are also likely to be associated with the underlying guilt of the defendant, the results from Table 1 are equally consistent with an optimal judge assessment of bail, with no additional causal role for the bail assessment in driving guilty outcomes.

Concerns about the endogenous assignment of bail are heightened by the red dots in Panel A of Figure 2, which display the coefficients from a regression of money bail on various

covariates. While there is a raw univariate correlation with guilt status, money bail is also associated with gender, being non-white and having a prior case. The correlation of money bail with these covariates is indicative of the strongly endogenous initial assignment of money bail.

The black dots in Panel B of Figure 2 reflect our attempt to address this causal inference problem through judicial assignment. These coefficients reflect the relationship between various covariates and the component of money bail that is due only to judicial severity. They are created by regressing various covariates on the linear prediction of money bail on a judicial severity measure we describe below. None of the covariates appear to be related to the fraction of variation in money bail that is driven by judicial variation, indicating random assignment. By contrast, our outcome variable of guilt *is* associated with our instrument—showing how the judicial assignment of bail can produce causal estimates of the impact of being assessed money bail.

Conceptually, our identification strategy is to isolate the impact of the judge on the probability that an individual is assigned money bail. One approach would be to estimate, for individual i in court c with judge j :

$$Bail_{icjt} = \alpha + \gamma_c + \delta_j + v_{it}$$

Where δ_j are judge fixed effects. However, estimating this equation with fixed effects for judges is biased in finite samples, so we follow the literature (e.g. Dobbie and Song (2015)) in estimating a leave-out mean:

$$Z_{icjt} = \frac{1}{n_{cjt} - 1} \left(\sum_{k=1}^{n_{cjt}} (Bail_k) - Bail_i \right) - \frac{1}{n_{ct} - 1} \left(\sum_{k=1}^{n_{ct}} (Bail_k) - Bail_i \right)$$

which we refer to as judicial severity. Our preferred measure computes judge deviations from the office-offense average to account for possible non-random assignment by offense. Our primary specifications depend on a version of this instrument in which $Bail_{it}$ is defined as the binary decision of whether to assign bail or not. However, we also examine alternative continuous measures, including $\log(1 + \text{bail amount})$.

Panel A of Figure 3 illustrates our estimate of judicial severity against the log bail amount, showing that judge severity is highly predictive of bail amounts faced by criminal defendants. Panel B shows that our judge severity measure is consistent over time, suggesting that judge severity is driven by idiosyncratic personal factors rather than temporary shocks (judge severity is even consistent across different offices when judges move).

In our main specifications, we instrument for the Bail amount $Bail_{ictjo}$ with Z_{ictjo} , our measure of judge severity taken from a within offense measure:

$$\begin{aligned} Guilt_{ictjo} &= \alpha + \beta Bail_{ictjo} + X'_{ictjo} \delta + \eta_{ctjo} + \varepsilon_{ictjo} \\ Bail_{ictjo} &= \alpha + \gamma Z_{ictjo} + X'_{ictjo} \zeta + \rho_{ctjo} + v_{ictjo} \end{aligned}$$

With errors clustered at the office-judge-year level. Our identifying assumption, taken from judge randomization, is that:

$$\text{corr}(Z_{ictjo}, \varepsilon_{ictjo}) = 0$$

In the next section, we provide supporting evidence for this assumption.

3.3 Randomization Check

Though our analysis of the judicial assignment process in Philadelphia leads us to expect close to random assignment of cases across judges, we check this assumption by contrasting our leave-out-mean estimator with various covariates in Table 3. The first column of this table illustrates the means of the dependent variables we analyze. Column 2 regresses our instrument against each covariate in isolation with no additional controls and reports the coefficient. Column 3 regresses our instrument against all covariates and includes fixed effects for the most severe offense of the defendant. Column 4 adds additional month-of-arraignment fixed effects.

Across all specifications, we find strong evidence for random assignment. F-Statistics of the joint significance of covariates we test against our instrument are 0.54 with only offense controls and 0.34 with month fixed effects.

4 Results

4.1 IV Results

Table 4 presents our main results from Philadelphia. The first column shows the first stage—a regression of our instrument of judicial severity against a binary indicator of whether the defendant actually received a money bail. While defendants are on average likely to receive a money bail (62%); we find that judicial factors also play a large role. Our first stage suggests strong instrumental validity: being assigned to a stricter judge results in defendants facing a higher likelihood of being forced to post money bail to guarantee pretrial freedom. Given the centralized Philadelphia magistrate court system and the lack of correlation between our instrument and observable covariates, we interpret this first stage as indicating that judicial severity provides effectively exogenous variation in money bail.

Column two presents the reduced form—a direct regression of our instrument of judge severity against the outcome of case guilty. Though this relationship will be attenuated—since not all people who receive a strict judge are impacted by way of higher bail amounts—the strong and significant relationship in the reduced form indicates a causal relationship between severity and guilt.

The third column scales the reduced form by the first stage to produce our instrumental variables estimate of the relationship between being assigned money bail on a case disposition of guilt. Our estimate suggests that defendants who are required to pay money bail as a result of being assigned to a strict judge are 6 percentage points more likely to plea guilty. Given a baseline guilt level of 50% in our sample, our estimate suggests that the presence of money bail increases the likelihood that a defendant is found guilty by about 12%.

This estimate is large, tightly identified through our measure of judicial severity, and suggests a powerful role for money bail in inducing guilty outcomes. Our data do not permit complete analysis of whether guilty outcomes result from plea bargains or judge convictions. However, we have strong results when focusing on cases in which we can explicitly observe plea behavior, and cases proceeding to trial appear rare in our sample. We believe our estimates are primarily driven by defendant plea behavior.

Table 4 also provides suggestive estimates regarding the role of race on case outcomes. Column one suggests that non-white defendants are 1.4 percentage points more likely to receive an assessment of any bail. Though this assignment may reflect non-racial factors associated with race rather than bias on the part of judges, columns two and three suggests that non-whites are actually *less* likely to be found guilty of crimes. While these results do not exploit judicial randomization and hence cannot be interpreted causally, they are certainly consistent with an interpretation of racial bias on the part of bail magistrates and prosecutors (in the that non-white are disproportionally assigned money bail, despite the fact that they are less likely to be found guilty).

Table 5 shows our primary specification for outcomes for defendants that are split into four categories corresponding to the interaction of being released or detained with being found guilty or not. Each of the four columns presents an IV regression (as in column three of Table 2) with one of those categories as the dependent variable. Defendants are released if they either post bail or receive a bail assessment which enables them to remain free prior to trial. Defendants are detained if they fail to post bail (a very small proportion of defendants are remanded prior to trial without the option to post bail and are also included in this category).

By contrasting the four outcomes: guilty and detained, guilty and released, not guilty and detained, and not guilty and released, we are able to get a better sense of the precise defendant decisions driving our results. Since people not receiving money bail are presumptively released, we can assume that the imposition of money bail is unlikely to *increase* the number who are released. And in fact, we observe that the judicial assignment of money bail reduces the outcome of release both in column two (release and guilty) and column four (release and not guilty).

The reduction in the released population as a result of money bail is matched by increases in the detained populations in both column one and three. The surprising result is that we observe the largest increase in column one, which suggests that the imposition of money bail results in a 16 percentage point increase in the outcome of both pleading guilty and being detained. This increase comes in part from the group of people who would have been

counterfactually released and found guilty anyway (the flow from column two, reflecting defendants who are detained as a result of bail but do not see an change in ultimate case outcome). However, it also reflects a flow from the population in column four, who would counterfactually have been released and found not guilty. The switch from this column reflects the behavior which is the prime interest of our paper: criminal defendants who respond to financial constraints by pleading guilty and facing detention, while they may have been able to be free and avoid a conviction absent the imposition of money bail. The decrease in individuals in this column in total (7.7 percentage points) reflects both individuals induced to plead guilty and be detained, as well as those who wind up in column three (individuals detained and but found not guilty).

Put differently, it is not surprising that criminal defendants respond to an exogenous shock in the assessment of money bail by experiencing greater detention. Our central finding is that criminal defendants are more likely to be both detained *and guilty*. The joint determination of bail posting status and case outcomes are suggestive of the role of liquidity or credit constraints in shaping defendant outcomes.

4.2 Robustness

For robustness, we provide a number of additional checks. Table 6 explores our main IV specification as illustrated in Column 3 of Table 4 for different subsamples—being charged with a felony, having a public defender, being male, and being non-white. While none of these estimates are statistically different from our main estimates, it is noteworthy given our findings on race discussed above that our IV point estimate for non-whites is higher: 8.3 percentage points. Our findings on felonies, an 8.1 percentage points increase, is not precisely estimated but is high in magnitude and suggestive that cases of guilt induced by higher bail are not purely for low-level crimes. Being convicted of a felony typically results in severe long-term impacts on defendant outcomes including opportunities for future employment and voting status.¹⁹

Tables 7 and 8 explore alternate specifications of our judge severity measure. Table 7 uses the log of 1 plus the bail amount, effectively using both the intensive and extensive margins.

¹⁹Though convicted felons can vote in Pennsylvania.

Table 8 uses the log of the bail amount, conditional on being assigned money bail—only the intensive margin. In Philadelphia, we find no evidence that the intensive margin matters, only the extensive margin of being assessed money bail.

Next, we turn to Pittsburgh. As discussed in the section 2, the nature of judicial assignment in Pittsburgh and the rest of the state is not as clean and does not permit a straightforward causal estimate. Rather than a central courtroom which handles all cases, individual magistrate judges are elected to districts in the city and are principally responsible for cases within that jurisdiction. Our judge measure therefore captures the variation arising from the difference between the main judge and other judges which account for 20-30% of cases in districts, typically due to the main judge being absent on a weekend, night, vacation, or for some other reason. Our identifying assumption is that case loads, conditional on observables, do not differ between the main judge and other judges in a given district.

A randomization check in Appendix Table A2 suggests that we have some violations of judicial randomization in Pittsburgh, with a F-Statistic of 4.74 for various defendant characteristics regressed against a measure of judicial severity within Allegheny County. Nonetheless, to establish robustness of our primary finding outside of the city of Philadelphia, we attempt a version of our main specification in Pittsburgh in Table 9. Remarkably, given the extent of non-random assignment, we find estimates that are virtually identical in Pittsburgh—in column three, a 6.4 percentage point increase in guilt as a result of money bail assessment. Given the nature of judicial assignment, we view these results primarily in support of our main analysis in Philadelphia.

In Table 10 we engage in further analysis of racial bias among bail judges. Recall that the coefficient on non-white in Table 5 was consistent with racial bias, but could also be attributed to the correlation of race and other non-observables which predict both bail assessments and a final case outcome of not guilty. To further identify racial bias among judges, we exploit random case assignment and focus on judges who appear to assess money bail on non-white defendants at rates higher than their peers. More formally, we produce a within-race measure of our measure of judicial severity:

$$Z_{icjtr} = \frac{1}{n_{cjtr} - 1} \left(\sum_{k=1}^{n_{cjtr}} (Bail_{kr}) - Bail_{ir} \right) - \frac{1}{n_{ctr} - 1} \left(\sum_{k=1}^{n_{ctr}} (Bail_{kr}) - Bail_{ir} \right)$$

Where r is an indicator that equals one when a defendant is non-white. To identify judges who exhibit racial bias, we focus on judges for whom:

$$Z_{ictr1} > 0, \quad Z_{ictr0} < 0$$

In other words, we focus on judges who exhibit both relative severity against non-white defendants relative to their court average, *and* leniency against white defendants relative to their court average. We produce these measures at the level of: the judge; the judge-year; and the judge-year-offense levels.

The first column of Table 10 is measured at the judge level and includes an indicator for this measure of judicial racial bias, an indicator for non-white, and an interaction between judicial bias and non-white in a regression on money bail assessment. Judges identified as racially biased by our measure do not behave significantly differently towards defendants on average, but are 2.2 percentage points more likely to levy money bail on non-white defendants. These estimates are very similar when we measure racial bias at the judge-year level (2.3 percentage points), and are slightly lower but still significant at the judge-year-offense level (a 1.4 percentage point increase in money bail assessment for non-whites assigned to racially biased judges). In context with earlier results, these results suggest a downside of judicial discretion: it is employed by racially biased judges in ways that impact defendants differentially by race.

4.3 Other Outcomes

Table 11 explores the outcome of recidivism. In order to avoid including individuals who are detained as a result of the trial process and unable to be charged for crimes, we examine recidivism after a six month window from the bail is assessment (varying this window to three months and one year provides similar results). While the first stage is identical in this specification as in our main Table 4, the reduced form in Table 11 examines the direct relation between our judicial severity indicator and an indicator for committing crime in the future. Our estimate of 0.025% is sizable and significant. When scaled with our first stage to

produce our IV estimate, we find that being assigned money bail results in a 4.3 percentage point increase in the likelihood of committing future crime.

This estimate is sizable in the context of a 6 percentage point increase in the likelihood of being convicted as a result of money bail. Our estimate on recidivism can stem from a combination of factors, including: the role of pretrial detentions on future criminal behavior for individuals who could not post bail; the financial hardship of making bail amounts among those who did post bail; and the impact of convictions and carceral sentences on future criminal activity among those who were assessed money bail and found guilty. While we are unable to identify the precise channel, we emphasize that our work provides the first causally rigorous test of the role of pretrial factors on future defendant criminal behavior.

Table 12 attempts an analysis of failure to appear to court. While our data do not permit a complete analysis of failures to appear, we create two indicators which likely capture different aspects of trial non-appearance. The first is a recorded Failure to Appear in the calendar files associated with the case; the second is a bench warrant issued by a judge in association with the case docket (which is typically done in the case of a failure to appear, but could potentially be issued for other reasons). Neither variable is statistically associated with money bail as instrumented for using our judge measure. Though these variables may not perfectly capture all aspects of failures to appear in court events, we stress primarily that our data do not provide strong evidence that bail assessments increase defendants' probability of showing up to court.

5 Conclusion

Our findings raise substantial questions about the nature of the money bail system. We exploit the judicial variation in assessment of money bail in conjunction with randomized assignment of defendants to judges to assess novel causal implications of money bail. We find that defendants assessed money bail have a 6 percentage point higher chance of conviction and a 4 percentage point higher probability of committing future crime. We find that money bail is levied disproportionately on non-white defendants, and use case randomization to

identify a subset of judges who levy money bail 1-2 percentage points more often on non-white defendants. Our results are robust to alternative specifications and examining different subgroups. An important caveat to these results is that they are created using an instrumental variable approach which focuses on criminal defendants induced to pay money bail as a result of judicial severity, and may not apply to criminal defendants facing money bail in general.

These findings have implications for both an assessment of criminal defendants' economic conditions and the institutional design of pretrial detention programs. Existing research showing that a quarter of Americans suggest they cannot come up with \$2,000 in 30 days (Lusardi, Schneider and Tufano (2011)), and we show how these liquidity constraints have real impacts on household outcomes. Criminal defendants assessed bail amounts appear frequently unable to produce the required bail amounts, and receive guilty outcomes as a result. Entered guilty pleas by defendants unwilling to wait months prior to trial and unable to finance bail likely contribute to this result.

We also document how money bail impacts the later outcome of recidivism, potentially through channels of pretrial detention, the financial imposition of bail amounts, or the impact of post-conviction incarceration spells. This fits into other literature documenting how incarceration spells causally influence future criminal behavior (for instance, Mueller-Smith (2016) and Aizer and Doyle (2015)), but differs by providing a link to the pretrial process.

From a legal perspective, our work raises both conceptual and practical issues. Examining the pretrial detention phase of the criminal justice system is particularly topical given the recent policy focus on reducing the incarcerated population in the United States. While sentencing decisions may involve tradeoffs between harms to criminal defendants and countervailing public interests such as deterrence; our analysis indicates a much weaker tradeoff regarding the imposition of bail on criminal defendants. While money bail imposition carries many costs—including those stemming from pretrial detention and those coming from guilty pleas—we find little evidence that judicial severity in assessing money bail results in positive outcomes, such as an increase in defendants' rate of appearance at court.

Aside from liberty considerations, the system of money bail also raises substantive issues related to equal protection. Many defendants appear to be found guilty simply due to an

inability to pay money bail, suggesting that equal access to justice is not provided at different levels of wealth and violating the primary assumption of innocence. Criminal defendants also do not seem to equally treated on the basis of race: we find suggestive evidence of racial bias against the bail setting of criminal defendants in general, and identify a subset of judges who appear racially prejudiced in bail setting. We can expect outcomes of these differential bail assessments on poorer and non-white defendants to be worse on dimensions of case guilt, conviction, and recidivism.

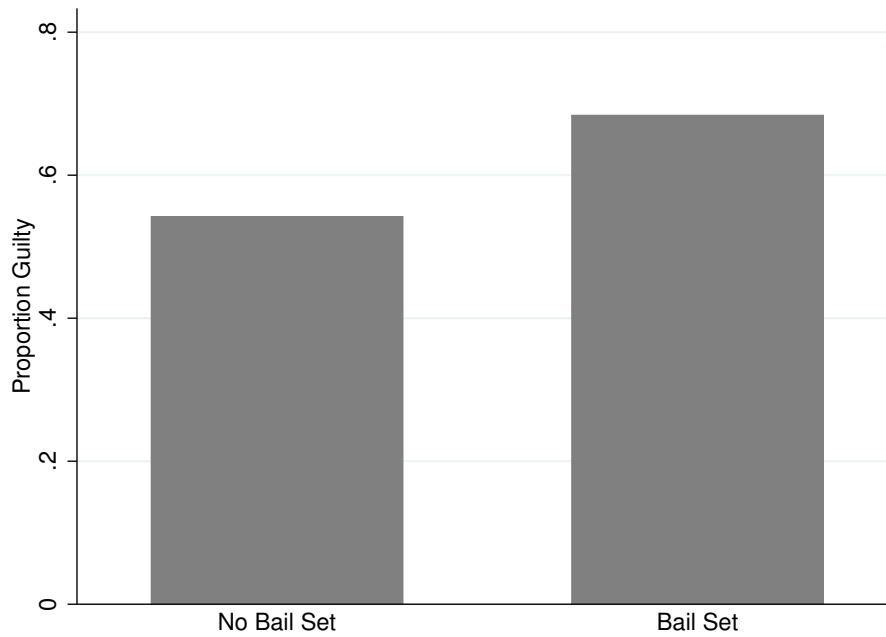
The substantial variation among individual magistrates sustained over time in setting money bail suggests that the imposition of money bail, and therefore pretrial detention, is a function of both case characteristics as well as judicial factors. These findings are particularly striking in light of the fact that Philadelphia, the site of our primary analysis, over this period already employed a pretrial risk assessment tool designed to appropriately target money bail and detention to high-risk defendants. Our work suggests that judicial discretion persists in such settings, and provides supporting evidence that the conditions in these settings for the imposition of money bail are not “narrowly tailored” to flight risk and public safety.

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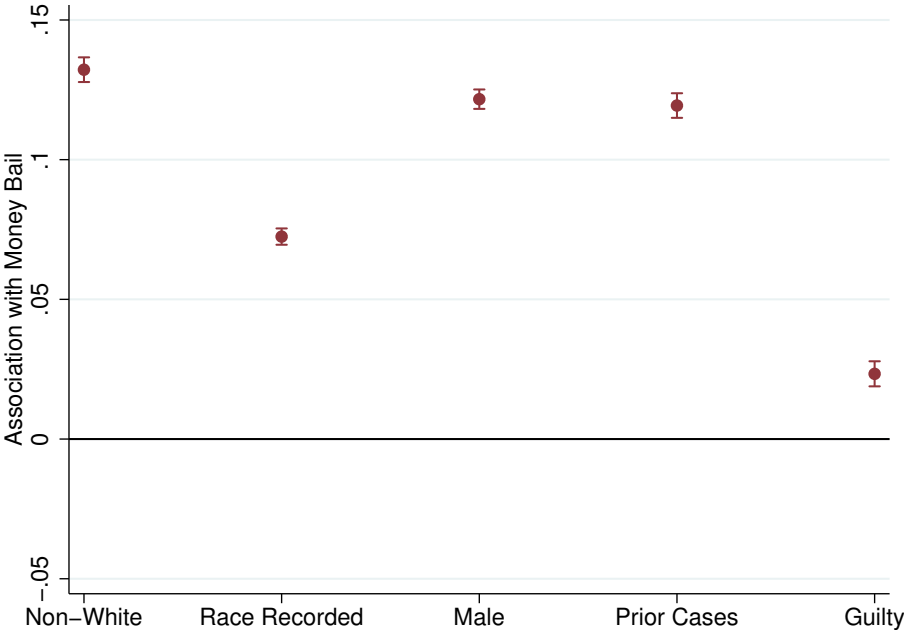
Figure 1
Guilt by Bail Status: Possession of Marijuana



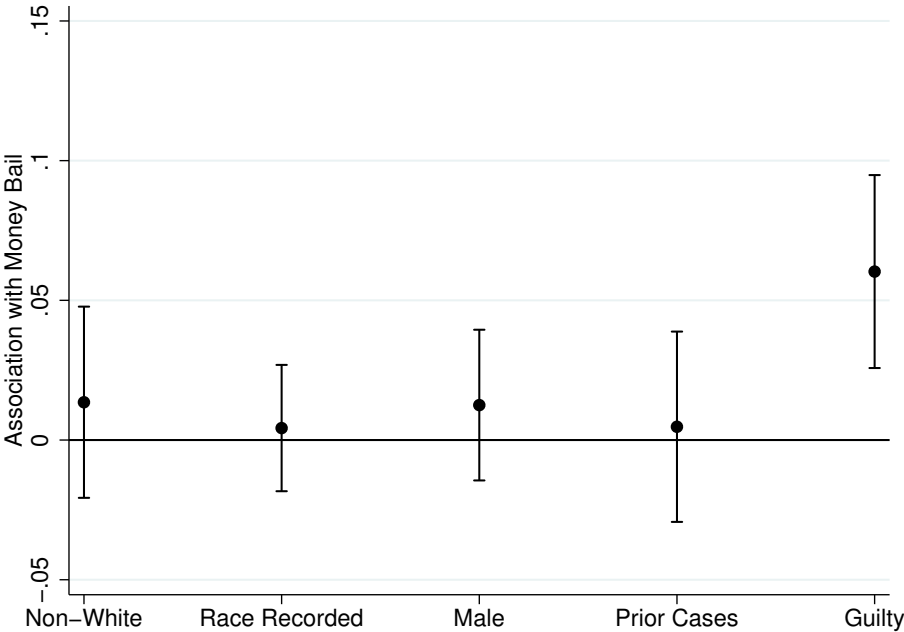
This figure compares the proportion of people with a case outcome of guilty across individuals for whom money bail was required, and those for which money bail was not required. The data subsets to cases in Pennsylvania between 2010–2015 in which criminal defendants were charged with the offense of the possession of marijuana. Absent additional controls, we observe that among criminal defendants charged with this offense, those for whom money bail was imposed over 10 percentage points more likely to be found guilty.

Figure 2
 Randomization Check: Regression Coefficients of Covariates on Money Bail

Panel A: Raw Association With Money Bail



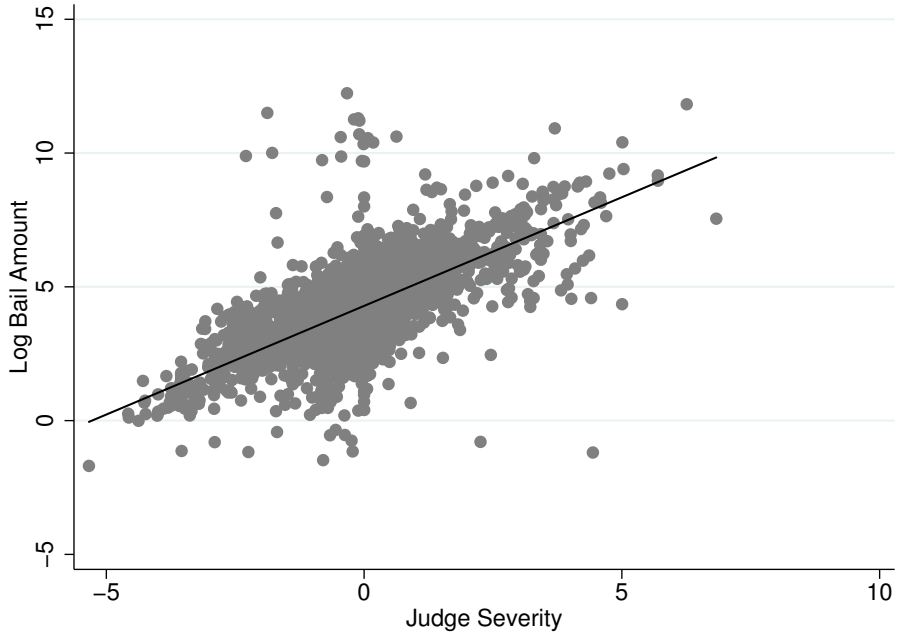
Panel B: Money Bail as Instrumented by Judicial Severity



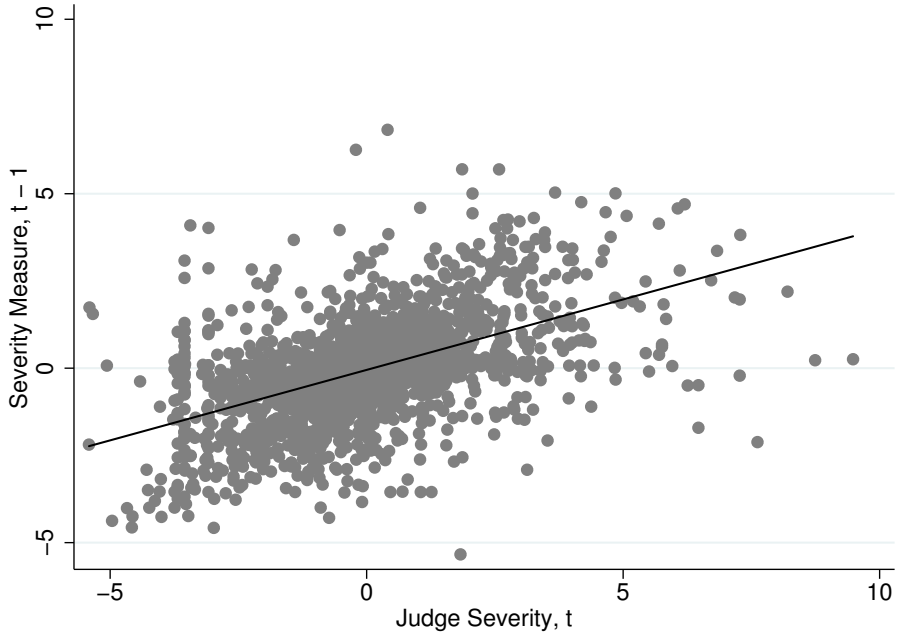
This figure examines the relationship between defendant characteristics and the outcome of being assigned any money bail. Panel A shows coefficients of bivariate regressions of each characteristic on a binary indicator equal to one if the defendant is assigned money bail. Panel B shows coefficients of iv versions of the regressions in Panel A, with our judge severity measure instrumenting for the binary indicator for money bail. The rightmost estimates in both panels show guilt, our primary outcome of interest, rather than a defendant characteristic.

Figure 3

Panel A: Judicial Severity vs. Log Bail Amount



Panel B: Judicial Severity t-1 v. Severity in t



This figure examines our judicial severity measure. We measure judicial severity using a leave-out-mean of the $\log[1 + \text{Money Bail Amount}]$ at the judge-year level, relative to the leave-out-mean average at the court in the same year. These computed judicial measures are then regressed against individual measures of log bail with fixed effects for the month of arraignment. The resulting residuals are averaged at the judge-year level and the average log bail amount is added to each residuals. Panel A contrasts the averaged measure of judicial severity against average log bail amounts at the judge-year level. Panel B compares the averaged measure of judicial severity in one year against the same judge's measure the previous year.

Table 1
Summary Statistics

	Philadelphia		Pittsburgh	
	Mean	SD	Mean	SD
Age	33.5	11.6	33.4	11.7
Non-White	0.56	0.50	0.42	0.49
Race Missing	0.12	0.33	0.027	0.16
Male	0.81	0.39	0.77	0.42
Prior Cases	0.42	0.49	0.33	0.47
Total Offenses	3.42	2.95	4.68	3.48
Case Guilty	0.50	0.50	0.77	0.42
Total Bail	24,083	74,891	12,964	28,697
Money Bail	0.62	0.48	0.53	0.50
Posted Bail	0.60	0.49	0.46	0.50
Bench Warrant	0.019	0.14	0.15	0.35
Commit Future Crime	0.43	0.49	0.33	0.47
Sample Size	201k		38k	

The sample includes criminal cases in Philadelphia and Pittsburgh in the period 2010–2015. Bail information is reported from the magistrate level; case disposition information is taken from the most severe offense for which the defendant was charged; and bench warrant information is taken a merged dataset of all bench warrants filed in association with a particular docket. Prior cases are taken within our sample (and are not able to capture crimes committed in the period prior to our sample).

Table 2
OLS Regressions of Guilt on Assigned Bail

	No Controls	Offense FEs	Full Controls	Log(Money Bail)
Any Money Bail	0.014* (0.008)	0.092*** (0.007)	0.043*** (0.006)	
Log(Money Bail)				0.004*** (0.001)
Proportion Guilty	0.498	0.498	0.498	0.498
N	200643	200643	200617	200617
Case Controls	No	No	Yes	Yes
Offense FEs	No	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

OLS regressions of a binary indicator of a case disposition of guilt on a binary indicator equal to 1 if money bail is initially assigned to the case (Columns 1-3) or the continuous measure $\log[1+\text{money bail amount}]$ (column 4). Case controls include age, age², prior cases, number of offenses, and indicators for race, gender and out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3
Randomization Tests

	Means	Pairwise	Joint Regressions	
			No Controls	Controls
Non-White	0.56	0.00035 (0.000)	0.00037 (0.001)	0.00020 (0.001)
Race Missing	0.12	-0.00026 (0.001)	-0.000015 (0.001)	-0.00014 (0.001)
Male	0.81	0.00053 (0.001)	0.00043 (0.001)	-0.000066 (0.001)
Age	33.5	-0.0000010 (0.000)	-0.00000041 (0.000)	0.000016 (0.000)
Out of State	0.031	0.0018 (0.001)	0.0019 (0.001)	0.0026 (0.002)
Prior Cases	0.42	0.00013 (0.000)	0.00013 (0.000)	0.00037 (0.001)
N. of cases			200617	200617
F-Statistic			0.54	0.34
Offense FEs		No	Yes	Yes
Month FEs		No	No	Yes

OLS regressions of our judge severity measure on case characteristics for the Philadelphia sample. Column 1 presents means of case characteristics. Column 2 presents coefficients of separate bivariate regressions of the judge severity measure on each case characteristic. Column 3 contains the coefficients from a single regression of the judge severity measure on all case characteristics and month fixed effects. Column 4 shows the coefficients from a regression identical to column 3, but additionally including offense fixed effects. F-statistics are reported for the test of joint significance of all shown case characteristics. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4
IV Regressions of Guilt on Money Bail

	First Stage	Reduced Form	IV
	Any Money Bail	Case Guilty	Case Guilty
Severity	0.587*** (0.028)	0.036** (0.017)	
Any Money Bail			0.061** (0.028)
Non-White	0.014*** (0.003)	-0.026*** (0.003)	-0.027*** (0.003)
Male	0.077*** (0.006)	0.026*** (0.003)	0.021*** (0.003)
Mean of Dep. Var.	0.623	0.498	0.499
N	200617	200617	200615
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Philadelphia sample is included. The first column presents the first stage, an OLS regression of Any Money Bail on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
 IV Regressions of Guilt \times Detention Status on Money Bail

	Guilty		Not Guilty	
	Detained	Released	Detained	Released
Any Money Bail	0.161*** (0.059)	-0.098* (0.060)	0.014 (0.050)	-0.077 (0.053)
Non-White	-0.006** (0.002)	-0.021*** (0.003)	0.029*** (0.003)	-0.003 (0.004)
Male	0.029*** (0.005)	-0.008 (0.005)	0.028*** (0.006)	-0.049*** (0.006)
Mean of Dep. Var.	0.226	0.272	0.178	0.323
N	200615	200615	200615	200615
Case Controls	Yes	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

IV regressions of a binary indicator of a new measure of full defendant outcomes on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Philadelphia sample is included. Outcomes for defendants are split into four categories corresponding to the interaction of being detained and a case disposition of guilty. Detained defendants were either remanded without the ability to post bail, or failed to post bail given the assessment of money bail. Released individuals either did not receive money bail or posted money bail. Each of the four columns presents an IV regression with one of those category as the dependent variable. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6
IV Regressions of Guilt on Money Bail by Case Characteristics

	Felony	Public Defender	Male	Non-White
Any Money Bail	0.081 (0.061)	0.054* (0.029)	0.060* (0.032)	0.083** (0.034)
Non-White	-0.045*** (0.003)	-0.026*** (0.003)	-0.026*** (0.003)	
Male	0.020*** (0.006)	0.024*** (0.004)		0.024*** (0.004)
Proportion Guilty	0.541	0.492	0.509	0.515
N	94658	126757	162691	112280
Case Controls	Yes	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

IV regressions of a binary indicator of case dispositions on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Philadelphia sample is included. Each column restricts to the subsample indicated in the column header. Felony refers to defendants who are charged with a felony offenses, public defender refers to defendants represented by public defenders. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7
IV Regressions of Guilt on Log(Money Bail)

	First Stage	Reduced Form	IV
	Log(Money Bail)	Case Guilty	Case Guilty
Severity	0.561*** (0.027)	0.004* (0.002)	
Log(Money Bail)			0.006** (0.003)
Non-White	0.153*** (0.024)	-0.026*** (0.003)	-0.027*** (0.003)
Male	0.829*** (0.058)	0.026*** (0.003)	0.021*** (0.004)
Mean of Dep. Var.	5.695	0.498	0.499
N	200617	200617	200615
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on the continuous measure $\log[1+\text{money bail amount}]$ (Log(Money Bail)) instrumented by our judge severity measure based on Log(Money Bail). Only the Philadelphia sample is included. The first column presents the first stage, an OLS regression of Log(Money Bail) on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8
IV Regressions of Guilt on Log(Money Bail) – Intensive Margin

	First Stage	Reduced Form	IV
	Log(Money Bail Bail>0)	Case Guilty	Case Guilty
Severity	0.489*** (0.035)	-0.006 (0.008)	
Log(Money Bail Bail > 0)			-0.013 (0.016)
Non-White	0.047*** (0.007)	-0.037*** (0.002)	-0.036*** (0.002)
Male	0.344*** (0.021)	0.019*** (0.004)	0.023*** (0.006)
Mean of Dep. Var.	9.143	0.506	0.499
N	124352	124352	124338
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on the continuous measure log[money bail amount], instrumented by our judge severity measure based on log[money bail amount]. Only the Philadelphia sample is included, and defendants with no money bail are excluded. The first column presents the first stage, an OLS regression of log[money bail amount] on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9
IV Regressions of Guilt on Money Bail – Pittsburgh

	First Stage	Reduced Form	IV
	Any Money Bail	Case Guilty	Case Guilty
Severity	0.391*** (0.026)	0.025* (0.013)	
Any Money Bail			0.064** (0.031)
Non-White	0.107*** (0.006)	-0.004 (0.006)	-0.011 (0.007)
Male	0.084*** (0.006)	0.053*** (0.006)	0.047*** (0.006)
Mean of Dep. Var.	0.495	0.777	0.766
N	38149	38149	38141
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Allegheny county (Pittsburgh) sample is included. The first column presents the first stage, an OLS regression of Any Money Bail on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the office-judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10
Estimates of Racial Bias among Bail Judges

	Judge Level	Judge \times Year	Judge \times Year \times Offense
Judicial Racial Bias Indicator	-0.012 (0.009)	-0.011 (0.009)	-0.002 (0.006)
Judicial Racial Bias Indicator \times Non-White	0.022*** (0.006)	0.023*** (0.006)	0.014** (0.006)
F-Test	1.927	1.610	1.261
Proportion With Money bail	0.623	0.623	0.623
N	203159	203159	203159
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

OLS regressions of an assessment of Any Money Bail against various indicators of racial bias among judges. Racial bias is estimated by creating a leave-out-mean average of Any Money Bail within race (white and non-white). Judges who assess money bail more often than their court average for non-white defendants, but less often for white defendants, are assigned a judge racial bias indicator. This measure is created at the level of the judge (column 1), the judge-year combination (column 2), and the judge-year-offense level (column 3). This measure is also interacted with non-white (non-white is a separate, non-reported, covariate). The sample is restricted to Philadelphia, and other controls include: include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11
IV Regressions of Recidivism on Money Bail

	First Stage	Reduced Form	IV
	Any Money Bail	Recidivism	Recidivism
Severity	0.588*** (0.028)	0.023** (0.012)	
Any Money Bail			0.040** (0.020)
Non-White	0.014*** (0.003)	-0.003 (0.004)	-0.004 (0.004)
Male	0.077*** (0.006)	0.036*** (0.003)	0.033*** (0.003)
Mean of Dep. Var.	0.623	0.264	0.264
N	200617	200617	200615
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of recidivism on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Recidivism is a binary indicator equal to one if the defendant is charged with a new offense in Pennsylvania following the case in question. Court appearances within the initial 6 months of arraignment are not coded as recidivism to allow for variation in trial times. Only the Philadelphia sample is included. The first column presents the first stage, an OLS regression of Any Money Bail on our judge severity measure. The second column presents the reduced form: a regression of recidivism on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12
 IV Regressions of Failure to Appear on Money Bail

	Calendar FTA	Bench Warrant FTA
Any Money Bail	0.003 (0.003)	-0.010 (0.016)
Non-White	-0.000 (0.000)	0.001* (0.001)
Male	-0.000 (0.000)	-0.004*** (0.001)
Mean of Dep. Var.	0.00144	0.0192
N	200615	200615
Case Controls	Yes	Yes
Offense FEs	Yes	Yes
Month FEs	Yes	Yes

IV regressions of binary indicators for failing to appear (FTA) at court dates on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. The two columns present two different variables indicating that the defendant failed to appear. Calendar FTA is an indicator equal to one if the defendant is explicitly listed as having failed to appear at a scheduled calendar event in the data. Bench Warrant FTA is an indicator if a bench warrant was issued for the defendant. Only the Philadelphia sample is included. Case controls include age, age², prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

Table A1: Common Offenses

	Count	Any Money Bail	Bail Amount	Non-White	Male
Intentional Possession of a Controlled Substance	22,846	15%	\$643	48%	84%
Manufacture, Delivery, or Possession With Intent to Manufacture or Deliver	18,913	87%	\$17,511	56%	92%
Aggravated Assault	12,417	97%	\$49,645	63%	77%
DUI: 1st Offense	11,436	27%	\$2,166	43%	82%
Retail Theft-Take Merchandise	10,424	36%	\$1,284	58%	63%
Simple Assault	6,293	84%	\$4,449	54%	80%
Possession of Instrument Of Crime W/Intent to Employ	6,081	85%	\$10,928	54%	66%
Receiving Stolen Property	5,865	55%	\$14,205	59%	85%
Possession Of Marijuana	5,641	10%	\$433	72%	92%
Purchase or receipt of Controlled Substance by Unauthorized Person	5,518	11%	\$288	35%	76%

Table A2: Randomization Check in Pittsburgh

	Means	Pairwise	Joint Regressions	
			No Controls	Controls
Non-White	0.42	0.019*** (0.002)	0.019*** (0.002)	0.015*** (0.004)
Race Missing	0.027	0.0050 (0.007)	0.015** (0.007)	-0.013 (0.011)
Male	0.77	0.014*** (0.003)	0.013*** (0.003)	0.0093*** (0.003)
Age	33.4	-0.00011 (0.000)	-0.000042 (0.000)	0.000053 (0.000)
Out of State	0.029	0.015** (0.007)	0.016** (0.007)	0.014* (0.009)
Prior Cases	0.33	-0.0063*** (0.002)	-0.0060** (0.002)	0.0036 (0.003)
N. of cases			38149	38149
F-Statistic			20.0	4.74
Offense FEs		No	Yes	Yes
Month FEs		No	No	Yes