The Heavy Costs of High Bail: Evidence from Judge Randomization

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ABSTRACT

In the United States, roughly 450,000 people are detained awaiting trial on any given day, typically because they have not posted bail. Using a large sample of criminal cases in Philadelphia and Pittsburgh, we analyze the consequences of the money bail system 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 12 percent increase in the likelihood of conviction and a 6–9 percent increase in recidivism. Our results highlight the importance of credit constraints in shaping defendant outcomes and point to important fairness considerations in the institutional design of the American money bail system.

1. INTRODUCTION

Roughly 450,000 people in the United States are held in jail awaiting trial on any given day (Minton and Zeng 2015). These individuals have not been convicted of any 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. Because of limited judicial resources, defendants often remain incarcerated for months or years awaiting trial.[1] Many defendants who are detained on money bail before trial eventually choose to plead guilty in

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1. In our data, the median time between bail arraignment and trial is 200 days.

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exchange for release rather than risk continued detention or an uncertain trial outcome.

There is significant evidence of a correlation between pretrial detention and both conviction and recidivism, which is consistent with a direct impact of bail assessment on defendant outcomes (for instance, Lowenkamp, VanNostrand, and Holsinger 2013a, 2013b; Phillips 2007, 2008). However, prior research struggles with causally estimating the impact of money bail because of the endogenous nature of detention hearings.[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, and the arrestee's criminal history, ties to the local community, and financial resources. These factors may be related to factual guilt and render correlations between money bail assessments and outcomes such as convictions and recidivism difficult to interpret.

This paper investigates the causal impact of money bail on convictions 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 a criminal defendant post a cash amount as bail in exchange for freedom before trial.[3] In Philadelphia, defendants are assigned bail at a centralized, 24-hour-a-day court presided over by arraignment court magistrates, to whom we refer as judges for convenience. These judges differ in what we call severity, or the propensity to assess bail. All else being equal, some judges assess money bail frequently, while others do so sparingly. The Philadelphia system assigns defendants to bail judges in an effectively random manner, which creates a natural experiment that we exploit to determine the role of money bail in determining defendant outcomes. We document that defendants' assignment to more severe judges raises the probability of being assessed money bail for reasons unrelated to other case factors, including defendant characteristics. This 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. In Philadelphia, criminal defendants who are assessed money bail are 12 percent (6 percentage points) more likely to be convicted. These effects appear to be driven by the subset of

2. A notable exception is Abrams and Rohlfs (2011), who exploit an experiment in Philadelphia in the 1980s.

3. Other forms of bail may require nonmonetary conditions or require the defendant to pay only in the event of a nonappearance.

cases in which arrestees are detained because of their inability to post bail. We also investigate money bail assessment and outcomes in Pittsburgh, where judicial assignment is based on arrest location, and find similar results. We combine the Philadelphia and Pittsburgh samples to gain statistical precision in examining the lasting negative effects of money bail after the conclusion of the underlying criminal case. We document that the assessment of money bail increases recidivism in our sample period by 6–9 percent yearly (.7 of a percentage point).

Our results are primarily driven by whether money bail is required and not by the amount of money bail. In other words, the assessment of money bail, rather than the bail size, appears to result in convictions. A key implication of this finding is that simply lowering required bail amounts will not ameliorate harms imposed by money bail. Our findings persist among a number of subgroups—nonwhite 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. This suggests that our effects are not merely driven by convictions for petty crimes.

We do not attempt to isolate the exact channel by which money bail results in convictions and recidivism. Money bail, as a source of pretrial detention, imposes significant costs on defendants. As the Supreme Court writes in *Gerstein v. Pugh* (420 U.S. 103, 114 [1975]), pretrial detention "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." Many defendants who are detained on money bail before trial may consequently choose to plead guilty to avoid or minimize further detention. Prosecutors commonly offer detained defendants a plea of time served, under which defendants receive credit for time already spent in detention and therefore are released immediately on conviction. Other potential channels include the difficulty detained defendants have communicating with their counsel and properly preparing 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 di-

4. Though bail bondsmen can offer bail amounts in exchange for a collateral value, which is typically 10 percent, even these relatively smaller collateral values may be out of reach for criminal defendants facing liquidity constraints. In Philadelphia, the court may accept 10 percent of the bail amount.

rectly influence recidivism through the harms of pretrial incarceration imposed on those unable to make bail, posttrial incarceration following conviction, or the stigma of conviction (see, for example, Baylor 2015; Appleman 2012; Phillips 2008).

Despite the multiplicity of possible channels, we emphasize that our results provide novel evidence of a causal role of money bail and pretrial detention 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 among 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 a large majority of arrestees in our sample qualified for representation by a public defender and therefore presumably are indigent.

The interactions between money bail and subsequent defendant outcomes pose substantive legal issues. From a liberty perspective, these relate to the incarceration of presumptively innocent people 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. Race is a further concern, and we find evidence consistent with racial discrimination in bail setting: nonwhite defendants are more likely to be assessed money bail yet less likely to be found guilty. However, this correlation is suggestive and may reflect unobserved factors that are correlated with race.

Our findings also raise institutional design questions regarding the American money bail system as a whole. The money bail system in Philadelphia and Pittsburgh has a lot in common with the money bail systems used in many cities around the country, such as New York and Baltimore. An arrestee sees a judicial officer who determines whether to release that person pending trial or impose money bail. Those people who are unable to pay their bails have the opportunity to plead guilty or remain in jail until trial. In systems similar to the ones in Philadelphia and Pittsburgh, our research suggests that money bail leads to convictions and recidivism.[5]

5. Of course, the impact may differ depending on the population. For instance, in certain places, defendants may be relatively well-off and have the general ability to pay money bail. In such a place, we would expect the causal impact of money bail to be lower than in Philadelphia, where many people are too poor to pay bail.

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 period covered by our analysis, judges varied widely in assessing bail amounts for similar defendants, which calls into question the ability of such tools to rein in judicial discretion.

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. As we are unable to explicitly observe defendants failing to appear, we construct two proxies based on the issuance of bench warrants. While these proxies are imperfect—both likely understate the true number of failures to appear—we find no evidence that money bail increases the probability of appearance. These results should be interpreted as preliminary, and a more nuanced study of court appearances using more complete data is necessary. Nevertheless it is notable that we are unable to find an obvious impact of money bail. Pretrial detention is expensive. Philadelphia spent an estimated $290 million on jailing in 2009, and 57 percent of the daily jailed population was detained awaiting trial (Philadelphia Research Initiative 2010). Rationalizing the costs imposed by money bail (via detention costs, convictions, and recidivism) requires substantial compensating public benefits, and we find no evidence that such benefits exist.

Our research has a close connection to the literature on pretrial justice (see the exceptionally detailed bibliography in Pretrial Justice Institute [2014]). There is a large body of evidence suggesting that pretrial custody status is associated with the ultimate outcomes of criminal cases, with detained defendants consistently faring worse than defendants at liberty (see American Bar Association 2007, p. 29). Past work has uncovered the correlation between money bail, pretrial detention, and conviction (for example, Phillips 2007, 2008) and examined other policy considerations regarding the design of pretrial detention systems (see Lowenkamp, VanNostrand, and Holsinger 2013a, 2013b; Bechtel et al. 2012; Phillips 2012).

In the economics literature, beyond Abrams and Rohlfs (2011) and Helland and Tabbarok (2004), our work is most closely related to papers utilizing random assignment of judges in the criminal justice system such as Kling (2006), Doyle (2007, 2008), Mueller-Smith (2016), and Aizer and Doyle (2015), as well as in other contexts, such as Chang and Schoar (2007) and Dobbie and Song (2015). Especially relevant is concurrent and complementary work such as Stevenson (2016), which uses a similar approach in Pennsylvania to examine the impacts of pretrial detention on case outcomes. Our work differs in that we also examine recidivism and establish a long-term negative outcome of incarcerations. We also differ in that our approach focuses on the decision of judges to set money bail, rather than the detention status of defendants.[6]

Our paper is structured as follows: Section 2 presents legal background on the money bail system in Philadelphia and Pittsburgh. Section 3 explains our data and empirical strategy. Section 4 contains estimation results. 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 (see *County of Riverside v. McLaughlin,* 500 U.S. 44, 56 [1991]; *Gerstein,* 420 U.S. 114). 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" (*Gerstein,* 420 U.S. 125). 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" (*Rothgery v. Gillespie Cnty., Tex.,* 554 U.S. 191, 199 [2008]).

At a bail hearing, judges have a number of options available to them: (1) release on recognizance, which requires the defendant to agree to appear at a later date; (2) nonmonetary conditions, which are restrictions such as pretrial supervision or a curfew; (3) an unsecured monetary condition, which is a written agreement to be liable for a fixed financial payment, akin to a promissory note; (4) a secured monetary condition under which the 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; and (5) no bail, so that the defendant

6. In principle, guilty pleas may be affected by bail setting even when bail is posted because of the financial cost of making bail. Table 5 examines the consequence of bail setting on the full interaction of outcomes of pretrial detention and case guilt.

is held 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 (see *Foucha v. Louisiana,* 504 U.S. 71, 80 [1992]; *United States v. Salerno,* 481 U.S. 739, 750 [1987]). "In our society liberty is the norm, and detention prior to trial or without trial is the carefully limited exception" (*Salerno,* 481 U.S. at 755). Therefore pretrial detention must be "narrowly focus[ed]" to the government's "compelling" interests in public safety and return to court (*Salerno,* 481 U.S. 750–51; see also *Stack v. Boyle,* 342 U.S. 1, 4 [1951]; American Bar Association 2007, p. 37). 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 (see *Stack,* 342 U.S. 5).

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" (*Bearden v. Georgia,* 461 U.S. 660, 671 [1983]). Persons may not be incarcerated solely because of their inability to make a payment (see *Bearden,* 461 U.S. 671; *Tate v. Short,* 401 U.S. 395 [1971]; *Williams v. Illinois,* 399 U.S. 235 [1970]; *Smith v. Bennett,* 365 U.S. 708, 709 [1961]). For this reason such payments must take into account a person's financial resources. These guarantees find a statutory parallel in the Pennsylvania Rule of Criminal Procedure 523, which explicitly requires magistrates to consider arrestees' financial resources when setting money bail.

2.2. Bail Hearings

In Pennsylvania, a magistrate presides over the initial appearance of an arrestee. In Philadelphia, a centralized bail court 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 closed-circuit television over satellite locations in the city where arrestees are held. The centralized location, large 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 preside over the initial appearance only; they do not preside over subsequent hearings or trials. As a result, magistrates impact the case only at the bail assessment and not at later stages.

In Pittsburgh, magistrates are elected to a 6-year term to serve in a district court, which administers a particular geographic section of Allegheny County. A single magistrate handles the majority of the arrests that occur in a jurisdiction, although many arrestees are seen by other magistrates during weekends, nights, and other periods when the presiding magistrate is not in service. As a result, defendants in Pittsburgh are assigned to judges in part on the basis of the location and time of the arrest.

At the pretrial detention hearing in both Pittsburgh and Philadelphia, a magistrate hears information from the defendant (or the defendant's counsel) and the prosecutor relevant to the defendant's 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 defendant's financial resources, family and community ties, criminal record, and prior failures to appear. These hearings typically last only a few minutes. In Philadelphia and Pittsburgh, magistrates also employ an empirical risk assessment tool meant to standardize decisions regarding pretrial detention.[7]

Should money bail be set, a detainee may secure a release only through the satisfaction of its financial terms. In Philadelphia, a detainee may post 10 percent of the money bail amount directly to the court. A detainee who cannot afford the financial condition of release remains incarcerated for months or even years awaiting trial. A detainee has the opportunity to move for a reduction in the money bail after the initial hearing. We focus on the initial assessment of money bail, as it is the product of a randomized judicial decision, and find that this decision is influential in determining the final amount the defendant is required to pay, regardless of later 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, which ensures that the magistrate's 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

We obtained comprehensive criminal data from the Administrative Office of the Pennsylvania Courts for 2010–15. These include records from the

7. We find that these tools do not eliminate the exercise of wide judicial discretion.

Table 1. Summary Statistics

	Philadelphia		Pittsburgh	
	Mean	SD	Mean	SD
Age	33.5	11.6	33.4	11.7
Nonwhite	.56		.42	
Race missing	.12		.027	
Male	.81		.77	
Prior cases	.42		.33	
Total offenses	3.42	2.95	4.68	3.48
Case guilty	.50		.77	
Total bail	24,083	74,891	12,964	28,697
Money bail	.62		.53	
Posted money bail	.31		.24	
Bench warrant	.019		.15	
Charged with future crime	.43		.33	
N	203,188		57,145	

Note. 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 from a merged data set of all bench warrants filed in association with a particular docket. Prior cases do not include crimes committed before 2010. Defendants are recorded as having posted money bail if money bail was initially set and their bail status was at some point listed as posted.

local magistrate courts and subsequent judicial and defendant decisions from the higher Court of Common Pleas. In Philadelphia, a separate municipal court system typically handles initial arraignments.

Table 1 summarizes the data for our focal region of Philadelphia, where we are best able to establish judicial randomization, and 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, bench warrant information, and final sentencing outcomes for individual defendants. Table A1 contains the 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 money bail would be to estimate this ordinary least squares regression:

Table 2. Regressions of Guilt on Assigned Bail

	Binary Indicator			Continuous Measure
	(1)	(2)	(3)	(4)
Any money bail	.014^{+}	.092**	.043**	
	(.008)	(.007)	(.006)	
Log(money bail)				.004**
				(.001)
Proportion guilty	.498	.498	.498	.498
Case controls	No	No	Yes	Yes
Offense fixed effects	No	Yes	Yes	Yes
N	200,643	200,643	200,617	200,617

Note. Data are from ordinary least squares regressions of a binary indicator of a case disposition of guilt on a binary indicator equal to one if money bail is initially assigned to the case or the continuous measure log(1 + money bail amount). Case controls include age, age^2, prior cases, number of offenses, and indicators for race, gender, and out of state. All regressions include month-of-arraignment fixed effects. Standard errors are clustered at the judge-year level.
$^{+}$ $p < .10$.
** $p < .01$.

$$\text{Guilt}_{it} = \alpha + \beta\text{Bail}_{it} + \varepsilon_{it},$$

where Bail_{it} is an indicator for whether individual *i* is assigned money bail in time *t*. Table 2 illustrates this strategy. Column 1 suggests that being assessed money bail results in a 1.4-percentage-point increase in the probability of pleading guilty. As shown in column 3, this goes up to 4.3 percentage points after adding a battery of additional controls. This relationship is confirmed in column 4, where we focus on the log of the bail amount. Figure 1 shows this correlation for possession of marijuana. Defendants charged with this offense are substantially more likely to be found guilty when assessed money bail.

While these estimates are consistent with a causal interpretation that higher bail amounts induce convictions, they are also consistent with a spurious correlation resulting from the endogenous bail assessment. Recall that bail assessments are not made randomly but are intended to be calibrated against the nature of the offense, the flight risk of the individual, and even the strength of the case. As these factors are also likely to be associated with the underlying guilt of the defendant, the results from Table 1 may not reflect a causal role of bail.

Concerns about the endogenous assignment of bail are heightened by

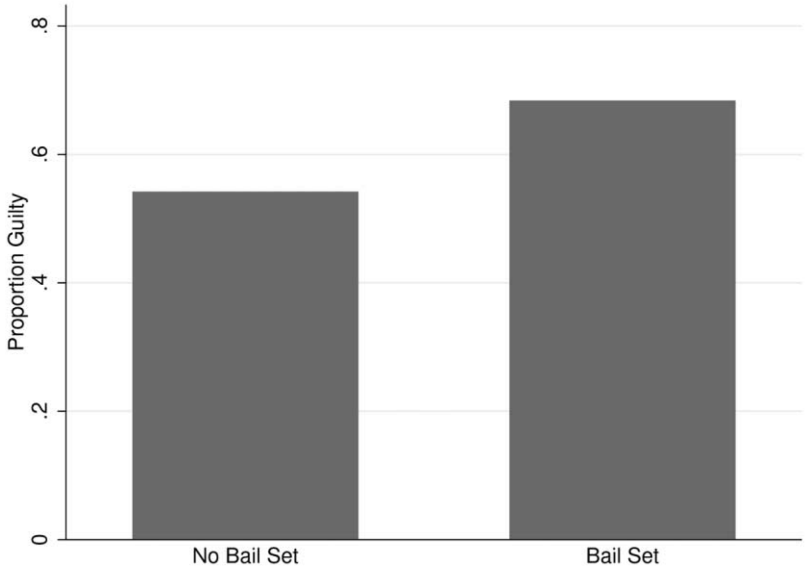

Figure 1. Guilt by bail status: possession of marijuana

the results shown in Figure 2. While there is a raw univariate correlation with guilt, the assessment of money bail is also associated with gender, race, and prior cases. The correlation of money bail with these covariates is indicative of the endogenous initial assignment of money bail.

The goal of our empirical strategy is to address this endogeneity concern using the effectively random assignment of defendants to judges. Bail judges differ widely in how they treat similarly situated defendants. Some judges are far more likely to impose money bail, and to impose money bail in greater amounts, than other judges. In other words, certain judges over time tend to set bail when other judges would not, all else being equal. We refer to each judge's propensity to set money bail as the judge's severity. Therefore, a defendant's chances of being assigned money bail depend on the severity of the bail judge, not just the characteristics of the case and the defendant. Because defendants are close to randomly assigned to bail judges, the judicial assignment serves as the treatment in a natural experiment. We isolate the effect of the severity of the bail judge in setting money bail to determine the role of money bail on defendant outcomes.

The coefficients plotted in Figure 3 reflect our attempt to isolate the impact of random judicial assignment on guilt. This graph shows the re-

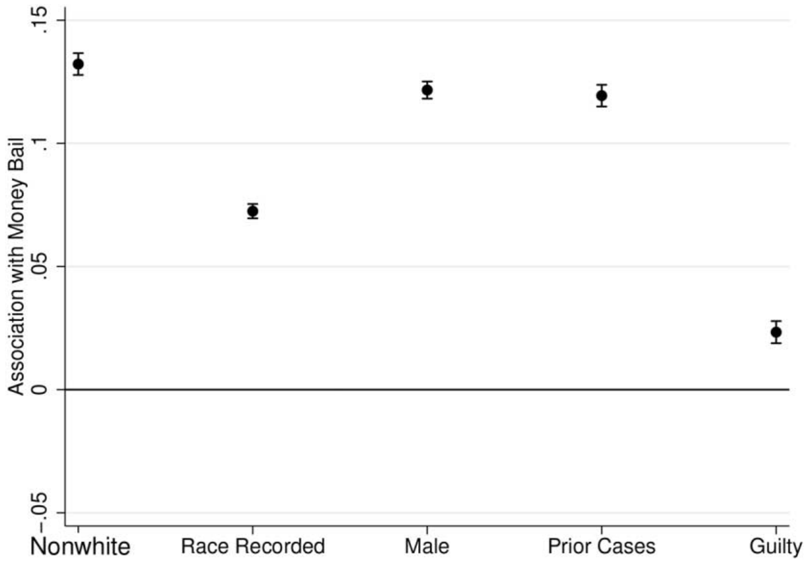

Figure 2. Randomization check: raw association with money bail

lationship between a battery of covariates and the component of money bail that is due only to judicial severity. The coefficients are created by regressing several covariates on the linear prediction of money bail on a judicial severity measure described below. None of the covariates appears to be related to the fraction of variation in money bail that is driven by judicial variation, which indicates random assignment. By contrast, our outcome variable, guilt, is associated with our instrument—which shows how the judicial assignment of bail can produce causal estimates of the impact of money bail.

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 use judge-specific fixed effects to instrument for whether a defendant is assigned bail. This would involve estimating a first stage, for individual i in court c with judge j, of

$$\text{Bail}_{icjt} = \alpha + \gamma_c + \delta_j + \upsilon_{it}$$

and estimating the effect of Bail_{icjt} on guilt in a second stage, where δ_j is a set of judge fixed effects. However, the assumptions required for instrumental variables (IV) estimation via two-stage least squares regression

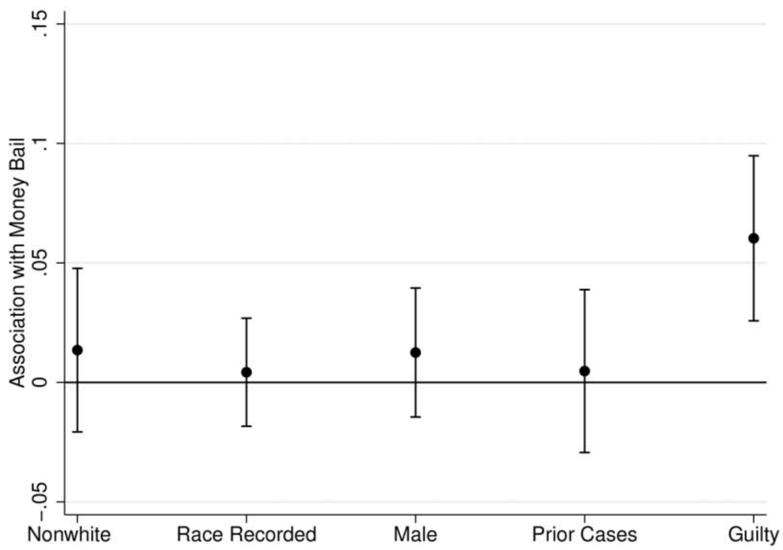

Figure 3. Randomization check: money bail as instrumented by judicial severity

may be violated in finite samples because of a mechanical correlation in the first stage. The estimated judge fixed effects are essentially an average across defendants, and with a small number of cases each defendant contributes significantly to the average. As discussed above, a defendant's own bail assessment is likely to be correlated with unobserved factors that are associated with guilt. If this is true, then averaging that bail assessment with a finite number of other defendants' assessments will not in general eliminate the correlation.

A solution to this problem in the literature (for example, Dobbie and Song 2015) involves estimating a leave-out mean for each defendant:

$$Z_{icjt} = \frac{1}{n_{cjt} - 1}\left[\sum_{k=1}^{n_{cjt}}(\text{Bail}_k) - \text{Bail}_i\right] - \frac{1}{n_{ct} - 1}\left[\sum_{k=1}^{n_{ct}}(\text{Bail}_k) - \text{Bail}_i\right],$$

which we refer to as judicial severity. The first term, Z_{icjt}, is simply the average of Bail_{kcjt} for all individuals faced by judge j except for i (all $k \neq i$). The second term subtracts the average of Bail_{kcjt} at court c, once again omitting individual i. Intuitively, Z_{icjt} is simply judge j's average relative to the court's average, computed using everyone but i. Because Z_{icjt} is computed without using individual i, there is no mechanical correla-

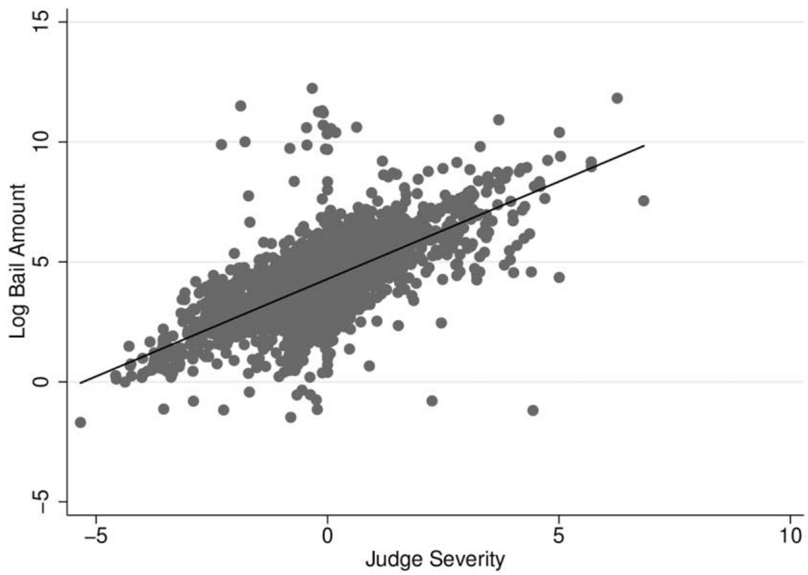

Figure 4. Average judicial severity by average log bail amounts: judge-year level

tion. This leave-out mean is then used as an instrument in place of judge fixed effects.

While the exposition above demonstrates a judge-level leave-out mean, our preferred instrument is slightly more granular. To account for possible nonrandom assignment by offense, we compute a leave-out mean at the offense-judge level, that is, the average for a judge for a given offense type, relative to the court average for that offense. For this instrument, we need only assume that individuals of the same offense category are randomly assigned to judges. Our primary specifications depend on a version of the instrument in which $Bail_{it}$ is defined as the binary decision of whether or not to assign bail. However, we also examine alternative continuous measures, including log(1 + bail amount).

Figure 4 shows that judge severity is highly predictive of bail amounts faced by criminal defendants.[8] Figure 5 shows that our judge severity

8. We measure judicial severity using a leave-out mean of log(1 + 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 residual.

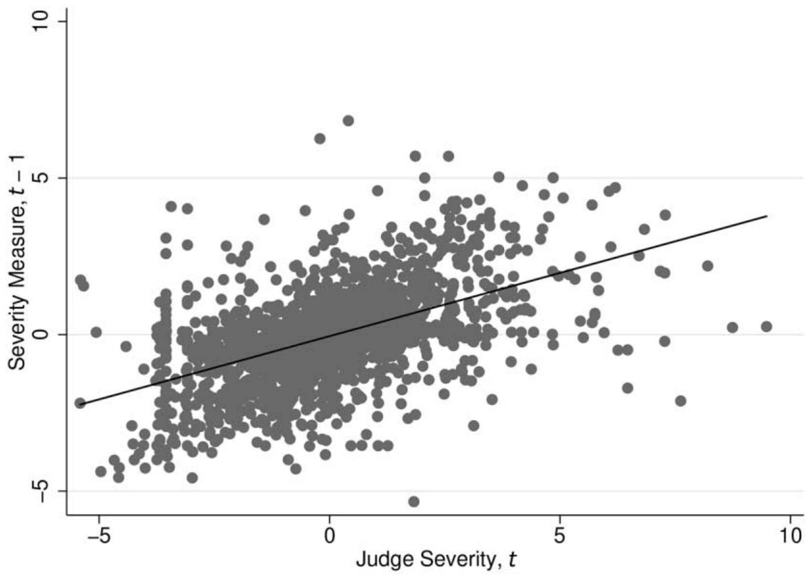

Figure 5. Average judicial severity by the judge's severity in the previous year

measure is consistent over time, which suggests that judge severity is driven by idiosyncratic personal factors rather than temporary shocks or case characteristics (judge severity is even consistent across different offices when judges move to serve in other jurisdictions).

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

$$\text{Guilt}_{icto} = \alpha + \beta\text{Bail}_{icto} + \boldsymbol{X}'_{icto}\delta + \eta_{cto} + \varepsilon_{ictjo}$$

and

$$\text{Bail}_{icto} = \alpha + \gamma Z_{ictjo} + \boldsymbol{X}'_{icto}\zeta + \rho_{cto} + \upsilon_{ictjo},$$

with errors clustered at the jurisdiction-judge-year level. Our identifying assumption, taken from judge randomization, is that

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

In Section 3.3, we provide supporting evidence for this assumption.

It is important to note that these results are created using an IV approach that focuses on criminal defendants induced to pay money bail as a result of judicial severity. In other words, we estimate a local average treatment effect identified on the basis of individuals for whom changes in bail assessment resulting from variation in judicial severity impact guilty pleas. These defendants are more likely to represent criminal cases for which there is more scope for judicial variation in bail setting. Nevertheless, we do find that our results persist in a number of important subcategories (including defendants facing felonies), and our results are quite comparable in both Philadelphia and Pittsburgh. These checks suggest that our results have external validity outside of the precise jurisdictions we examine.

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 examining the association between our leave-out mean estimator and a series of defendant covariates in Table 3. Across all specifications, we find strong evidence for random assignment: *F*-statistics of the joint significance of covariates we test against our instrument are .54 with only month fixed effects and .34 when including both month fixed effects and offense controls.

4. RESULTS

4.1. Instrumental Variables Results

Table 4 presents our main results from Philadelphia. Column 1 shows the first stage—a regression of our instrument of judicial severity against a binary indicator of whether the defendant was assessed money bail. While defendants are on average likely to be assessed money bail (62 percent), we find that judicial factors also play a large role. Our first stage suggests strong instrumental validity: being assigned to a more severe judge results in defendants facing a higher likelihood of being assessed money bail. Given the close-to-random assignment to judges and the lack of correlation between our instrument and observable defendant characteristics, we interpret this first stage as indicating that judicial severity provides effectively exogenous variation in money bail.

Column 2 presents the reduced form—a direct regression of our instrument of judge severity against the outcome of guilt. Although the relationship between judge severity and the outcome of guilt is attenuated—because not all people who receive a severe judge are given higher

Table 3. Randomization Tests

			Joint Regressions	
	Mean (1)	Pairwise (2)	No Controls (3)	Controls (4)
Nonwhite	.56	.00035	.00037	.00020
		(.000)	(.001)	(.001)
Race missing	.12	−.00026	−.000015	−.00014
		(.001)	(.001)	(.001)
Male	.81	.00053	.00043	−.000066
		(.001)	(.001)	(.001)
Age	33.5	−.0000010	−.00000041	.000016
		(.000)	(.000)	(.000)
Out of state	.031	.0018	.0019	.0026
		(.001)	(.001)	(.002)
Prior cases	.42	.00013	.00013	.00037
		(.000)	(.000)	(.001)
F-statistic			.54	.34
Offense fixed effects		No	Yes	Yes
Month fixed effects		No	No	Yes

Note. Data are from ordinary least squares 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 with offense fixed effects. Column 4 shows the coefficients from a regression identical to column 3 with month fixed effects. The tests of joint significance of all case characteristics are reported as *F*-statistics. For the joint regressions, $N = 200{,}617$.

bail amounts—the strong and significant relationship in the reduced form indicates a causal relationship between judge severity and conviction.

Column 3 scales the reduced form by the first stage to produce our instrumental variables estimate of the relationship between money bail and conviction. Our IV estimate suggests that defendants who are required to pay money bail as a result of being assigned to a severe judge are 6 percentage points more likely to be convicted. Given a baseline guilt level of 50 percent in our sample, our estimate suggests that the presence of money bail increases the likelihood that a defendant is found guilty by about 12 percent.

This estimate is large, is tightly identified through our measure of judicial severity, and suggests a powerful role for money bail in inducing convictions. Our data do not permit complete analysis of whether convictions result from plea bargains or trials. However, we have strong results when focusing on cases in which we can explicitly observe plea behavior,

Table 4. Instrumental Variables Regressions of Money Bail on Guilt

		Case Guilty	
	First Stage: Any Money Bail (1)	Reduced Form (2)	Instrumental Variables (3)
Severity	.587**	.036*	
	(.028)	(.017)	
Any money bail			.061*
			(.028)
Nonwhite	.014**	−.026**	−.027**
	(.003)	(.003)	(.003)
Male	.077**	.026**	.021**
	(.006)	(.003)	(.003)
Mean of dependent variable	.623	.498	.499
N	200,617	200,617	200,615

Note. Only the Philadelphia sample is included. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the judge-year level.

* $p < .05$.

** $p < .01$.

and cases proceeding to trial appear in our sample only rarely. We believe our estimates are primarily driven by defendant plea behavior.

Table 4 also provides suggestive estimates regarding the role of race in case outcomes. Nonwhite defendants are 1.4 percentage points more likely to be assessed money bail but less likely to be found guilty of crimes. While these results should not be interpreted causally, as they do not exploit judicial randomization and may reflect nonracial factors associated with race, they are consistent with racial bias in the criminal justice system. They are also consistent with other mechanisms of the legal process. For instance, prosecutors or judges may correct for an initial bias in arrest by dismissing or differentially pursuing cases involving nonwhite defendants. While our data do not permit a complete analysis of racial bias in bail setting, this remains an interesting avenue for future research.

We next detail the relationship between money bail, pretrial detention, and convictions. There are a number of paths a defendant may take following the initial bail assessment. We consider a categorization of four possible paths in a criminal case: defendants may be detained and found guilty, detained and found not guilty, released and found guilty, or released and found not guilty. Table 5 presents an analysis of the impact of

Table 5. Instrumental Variables Regressions of Money Bail on Guilt and Detention Status

	Guilty		Not Guilty	
	Detained (1)	Released (2)	Detained (3)	Released (4)
Any money bail	.161**	−.098+	.014	−.077
	(.059)	(.060)	(.050)	(.053)
Nonwhite	−.006*	−.021**	.029**	−.003
	(.002)	(.003)	(.003)	(.004)
Male	.029**	−.008	.028**	−.049**
	(.005)	(.005)	(.006)	(.006)
Mean of dependent variable	.226	.272	.178	.323

Note. Only the Philadelphia sample is included. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the judge-year level. $N = 200{,}615$.

+ $p < .10$.
* $p < .05$.
** $p < .01$.

money bail on the flow of defendants between these four categories. As the dependent variables across the four columns are mutually exclusive and exhaustive, one of the columns is redundant, in the sense that the coefficients in any row must sum to 0. However, examining all four columns provides a useful picture of the impact of money bail on the path that defendants take from bail assessment to their ultimate case outcomes.

Since defendants not assessed money bail are presumptively released, we can assume that the imposition of money bail is unlikely to increase the number who are released. For this reason, we observe that the judicial assignment of money bail reduces the outcome of release in both column 2 and column 4. Although we are not able to precisely estimate the effects in either column, Table 5 suggests that money bail decreases the probability that a defendant is released and ultimately found guilty by nearly 10 percentage points and decreases the probability that a defendant is released and found not guilty by nearly 8 percentage points.

The reduction in the released population must be matched by an increase in the detained population: we see a 16-percentage-point increase in the outcome of detention and guilt. In other words, money bail increases the probability of detention for those who would be counterfactually released, and the majority of the population that is detained as a result of money bail is ultimately convicted.

Table 6. Instrumental Variables Regressions of Guilt on Money Bail by Case Characteristics

	Felony	Public Defender	Male	Nonwhite
Any money bail	.081	.054^{+}	.060^{+}	.083*
	(.061)	(.029)	(.032)	(.034)
Nonwhite	−.045**	−.026**	−.026**	
	(.003)	(.003)	(.003)	
Male	.020**	.024**		.024**
	(.006)	(.004)		(.004)
Proportion guilty	.541	.492	.509	.515
N	94,658	126,757	162,691	112,280

Note. Only the Philadelphia sample is included. Felony refers to defendants who are charged with felony offenses; public defender refers to defendants represented by public defenders. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the judge-year level.

$^{+}$ $p < .10$.

* $p < .05$.

** $p < .01$.

4.2. Robustness

For robustness, we provide a number of additional checks. Table 6 explores our main IV specification for different subsamples. While none of these estimates are statistically different from our main estimates, it is noteworthy given our findings for race discussed above that our IV point estimate for nonwhites is higher, at 8.3 percentage points. Our finding for felonies, an 8.1-percentage-point increase, is not precisely estimated but is high in magnitude and suggestive that instances of guilt induced by higher bail are not for low-level crimes exclusively. Being convicted of a felony typically results in severe long-term impacts on defendant outcomes, including opportunities for future employment and voting status.[9]

Tables 7 and 8 explore alternative 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. Table 8 uses the log of the bail amount, conditional on being assigned money bail—that is, only the intensive margin. In Philadelphia, we find no evidence that the intensive margin matters; only the extensive margin of being assessed money bail does.

9. Convicted felons can vote in Pennsylvania.

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

		Case Guilty	
	First Stage: Log(Money Bail) (1)	Reduced Form (2)	Instrumental Variables (3)
Severity	.561**	.004+	
	(.027)	(.002)	
Log(money bail)			.006*
			(.003)
Nonwhite	.153**	−.026**	−.027**
	(.024)	(.003)	(.003)
Male	.829**	.026**	.021**
	(.058)	(.003)	(.004)
Mean of dependent variable	5.695	.498	.499
N	200,617	200,617	200,615

Note. Only the Philadelphia sample is included. Column 1 presents the first stage, an ordinary least squares regression of Log(Money Bail) on our judge severity measure. Column 2 presents the reduced form: a regression of Case Guilty on our judge severity measure. Column 3 presents the instrumental variables regression. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the judge-year level.

+ $p < .10$.
* $p < .05$.
** $p < .01$.

Next we turn to Pittsburgh. As discussed in 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. Instead of a central courtroom that handles all cases, individual magistrate judges are elected to districts in the city and are principally responsible for cases in each jurisdiction. Our judge measure therefore captures the variation arising from the difference between the principal judge and other judges, who account for 20–30 percent of cases in districts, typically because of the principal judge being absent on a weekend, night, or vacation or for some other reason. Our identifying assumption is that caseloads, conditional on observables, do not differ between the principal judge and other judges in a given district.

A randomization check in Table A2 suggests that there is nonrandom judicial assignment in Pittsburgh, with an *F*-statistic of 4.74 for various defendant characteristics regressed against a measure of judicial severity in Allegheny County. Nonetheless, to establish robustness of our primary

Table 8. Regressions of Guilt on Log(Money Bail): Intensive Margin

	First Stage: Log(Money Bail | Bail > 0) (1)	Case Guilty: Reduced Form (2)	Case Guilty: Instrumental Variables (3)
Severity	.489**	−.006	
	(.035)	(.008)	
Log(money bail)			−.013
			(.016)
Nonwhite	.047**	−.037**	−.036**
	(.007)	(.002)	(.002)
Male	.344**	.019**	.023**
	(.021)	(.004)	(.006)
Mean of dependent variable	9.143	.506	.499
N	124,352	124,352	124,338

Note. Only the Philadelphia sample is included, and defendants not assigned money bail are excluded. Column 1 presents the first stage, an ordinary least squares regression of log(money bail amount) on our judge severity measure. Column 2 presents the reduced form: a regression of Case Guilty on our judge severity measure. Column 3 presents the instrumental variables regression. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the judge-year level.

** $p < .01$.

finding outside Philadelphia, we attempt a version of our main specification for Pittsburgh in Table 9. Remarkably, given the extent of nonrandom assignment, we find estimates that are virtually identical for Pittsburgh—a 6.4-percentage-point increase in guilt as a result of money bail assessment. Because the Pittsburgh and Philadelphia samples are comparable, in subsequent analysis on recidivism we combine the two samples in order to maximize statistical power.

Table A3 examines how our results vary across the distribution of bail amounts. To avoid an endogenous assignment of bail amounts, we categorize offense categories by average bail amounts into quartiles. Interestingly, we find that our results appear to be largest in the first quartile, where bail amounts are lowest. This suggests that the imposition of money bail, even when bail amounts are low, is sufficient to lead to convictions.

Tables A4 and A5 focus on the five most common offenses for felonies and misdemeanors. Table A4 highlights the extensive margin, and our

Table 9. Regressions of Guilt on Log(Money Bail): Pittsburgh

		Case Guilty	
	First Stage: Any Money Bail (1)	Reduced Form (2)	Instrumental Variables (3)
Severity	.391**	.025+	
	(.026)	(.013)	
Any money bail			.064*
			(.031)
Nonwhite	.107**	−.004	−.011
	(.006)	(.006)	(.007)
Male	.084**	.053**	.047**
	(.006)	(.006)	(.006)
Mean of dependent variable	.495	.777	.766
N	38,149	38,149	38,141

Note. Column 1 presents the first stage, an ordinary least squares regression of Any Money Bail on our judge severity measure. Column 2 presents the reduced form: a regression of Case Guilty on our judge severity measure. Column 3 presents the instrumental variables regression. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the office-judge-year level.

$^+$ $p < .10$.

* $p < .05$.

** $p < .01$.

results appear among various categories of theft—retail theft, receiving stolen property, and retail theft (misdemeanor). Our estimates are somewhat lower and do not reach significance for drug offenses, incidents of driving under the influence, and gun possession charges. These results are comparable to those of Stevenson (2016), who also finds substantial results among those categories and lower effects on drug and other charges.

Table A5 examines the intensive margin—whether changes in the intensity of bail matter given that bail was set. Interestingly, we find effects here only among gun possession misdemeanors. It is possible that the effect could be driven by the relatively high average bail in this category (around $11,000). Though other offense categories (such as aggravated assault) also carry high bail amounts, they typically also carry greater consequences, which may deter defendants from pleading guilty. Future work will attempt to analyze why responses to bail setting appear to be particularly high for some offenses rather than others, which may assist in adjusting pretrial detention standards.

Table 10. Instrumental Variables Panel Regressions of Recidivism on Money Bail

	Philadelphia:	Combined Sample		
	All Charges	All Charges	Felony	Misdemeanor
Any money bail	.007	.007*	.002	.006*
	(.008)	(.004)	(.003)	(.003)
Nonwhite	−.012**	−.008**	.003**	−.012**
	(.001)	(.001)	(.001)	(.001)
Male	.026**	.017**	.018**	.002**
	(.001)	(.001)	(.001)	(.001)
Mean of dependent variable	.117	.0811	.0442	.0424

Note. All regressions include offense and month-of-arraignment fixed effects, controls for the calendar year, and case controls. Case controls are taken from the first case in our records only and include age, age^2, prior cases, number of offenses, and an indicator for out of state. Subsequent charges are included only as instances of recidivism. Episodes of recidivism in the first column may reflect future crimes committed anywhere else in the state. Standard errors are clustered at the defendant level. $N = 522{,}395$ for Philadelphia; $N = 862{,}163$ for the combined sample.

* $p < .05$.

** $p < .01$.

4.3. Other Outcomes

4.3.1. Recidivism. We next look at recidivism, which we explore in Table 10. Existing literature documents the role of incarceration on future criminal activity (see, for instance, Mueller-Smith 2016; Aizer and Doyle 2015). A variety of mechanisms appear to drive this relationship, including the negative impact of incarceration on labor market outcomes (encouraging illegal income seeking), family disruption, loss of human capital, and peer effects resulting from associations with other detainees.

We extend this literature by examining the role of money bail on future criminal activity. There are a number of channels through which money bail in particular might cause recidivism. As our results in Section 4.1 show, money bail leads to convictions, which in turn may entail incarceration and subsequent effects. Even without additional convictions, money bail may impact future criminal activity via job loss during pretrial detention, financial hardship caused by raising funds to make bail, or other factors. Our data do not permit, and we do not attempt, a complete separation of the various mechanisms linking bail assessment to future criminal activity.

We follow some of the prior literature in this area by restructuring our data into a yearly panel format. Our main specification follows the first criminal offense committed by defendants in our data:

$$\text{Recidivism}_{i,t+y} = \alpha + \boldsymbol{X}_{i,t} + \mu_y + \mu_t + \beta\text{Bail}_{i,t} + \varepsilon_{i,t+y},$$

where $\text{Recidivism}_{i,t+y}$ is a binary indicator equal to one if the defendant is charged with a crime in the yth calendar year after his or her initial charge (where the initial charge year is denoted t); $\boldsymbol{X}_{i,t}$ is the full list of defendant controls previously included (these are the age, race, and gender of the defendant and controls for the criminal charge), which are taken in the calendar year of criminal charge; m_y is a calendar year fixed effect; and m_t controls for the month of arraignment. The term $\text{Bail}_{i,t}$ is an indicator for whether the defendant was required to post money bail and is instrumented for using our judicial severity measure; β remains the key causal variable of interest, capturing the role of exogenous bail assessments on future recidivism. Our yearly defendant panel begins in the calendar year in which defendants enter our data as a result of an initial charge and ends in 2015 (the last year for which we have criminal charge data).

The base rate of yearly recidivism in our sample (around 12 percent in Philadelphia) along with the standard errors of our IV estimates result in some statistical imprecision in our estimate for Philadelphia. For the role of money bail assessment on the yearly probability of future criminal behavior, though the estimate of .007 is quite large economically (corresponding to a .7-percentage-point yearly increase in the probability of committing future crime, or a 6 percent increase), we are unable to statistically distinguish this result from 0. To gain statistical precision, we expand our sample to include data from both Pittsburgh and Philadelphia. Though the judicial assignment process is not as random in Pittsburgh as in Philadelphia, we find quite comparable results in both localities in most of our specifications—including recidivism. We estimate an identical effect of .007 in the combined sample (or an increase of 9 percent), an effect that is statistically significant at a 5 percent level.

When we separate future criminal charges into felonies and misdemeanors, we find that the bulk of our recidivism result is driven by money bail leading defendants to be charged with misdemeanors. This finding is consistent with prior literature and the intuition that incarceration spells should raise the chances of committing minor crimes more than severe ones.

Our effects can be compared with the literature examining the role of incarceration spells on future criminal activity. Our results are somewhat lower than those of Aizer and Doyle (2015), who find that juvenile incarceration increases adult incarceration by 23 percentage points, consistent with a larger role for incarceration spells on the future criminal behavior of younger defendants. Our finding is more comparable to that of Mueller-Smith (2016), who finds that each year of incarceration results in a 4–7-percentage-point quarterly increase in postrelease criminal activity. While these studies examine the role of incarceration spells on criminal behavior directly, we examine the role of money bail—which is unlikely to be a binding constraint for many defendants but leads to sizable financial costs or detention for some defendants. It is unsurprising that our results are somewhat smaller or attenuated but remain striking in that we find evidence that money bail leads to recidivism. Though we emphasize the statistical imprecision of our estimates, our results suggest that the assessment of money bail yields substantial negative externalities in terms of additional crime.

4.3.2. Failure to Appear. We finally analyze whether money bail impacts the probability that a defendant appears in court. While we do not explicitly observe failures to appear, we construct a series of proxies. The first, which we label explicit, is our most conservative. It reflects an explicit entry in the court calendar files of a warrant being issued as a result of the defendant failing to appear. While this surely captures instances in which the defendant failed to appear, the files lack a standard coding procedure, and so this measure may underreport the true number of failures to appear.[10] Our second measure indicates whether a warrant was issued at a scheduled court calendar event. This event is consistently coded when it occurs in the calendar files but may capture warrants issued for reasons other than failures to appear. This measure has a higher mean than our explicit measure, occurring in approximately one out of a hundred cases, but still may underreport the true number of failures to appear.

In Table 11 the coefficients on money bail are positive and insignificant in all specifications. While the imprecision of these estimates prevents us from drawing much from these results, we note that the goal of money bail is to ensure appearance at trial, that is, to have a substantial negative effect on failures to appear. Our results suggest that money bail has a negligible effect or, if anything, increases failures to appear.

Of course, a substantial caveat to these results is imposed by the lim-

10. The average of the binary indicator for this measure is extremely small, .001, which likely reflects this underreporting.

Table 11. Instrumental Variables Panel Regressions of Failure to Appear on Money Bail

	Philadelphia		Combined Sample	
	Explicit	Warrant	Explicit	Warrant
Any money bail	.003	.018	.002	.005
	(.003)	(.021)	(.002)	(.008)
Nonwhite	−.000	−.001	−.000	−.001
	(.000)	(.001)	(.000)	(.001)
Male	−.000	−.000	−.000	.000
	(.000)	(.002)	(.000)	(.001)
Mean of dependent variable	.001	.010	.001	.007
N	200,615	200,615	238,614	299,779

Note. All regressions include offense and month-of-arraignment fixed effects and case controls. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out of state. Standard errors are clustered at the judge-year level.

itations of our data, which rely on proxies to measure defendants' failures to appear. By contrast, prior research finds different estimates of appearance rates. For instance, Abrams and Rohlfs (2011) document that, in 2000, 22 percent of US defendants failed to appear, while 16 percent of those released on bail were rearrested. They also document in Philadelphia that defendants failed to appear around 10–13 percent of the time. Our analysis focuses on later periods and measures failures to appear among all defendants. Primarily, however, we use administrative court data to trace either when court appearances were shifted because of defendant nonappearance or when a warrant was issued. It is possible that a defendant who fails to appear may be warned prior to a warrant being issued, though we expect that the court appearance data will still count that event as a failure to appear.

We emphasize the incompleteness of our available data on failures to appear. It is possible in particular that our estimates may underestimate the role of nonappearances in the criminal justice system. We examine the role of money bail assessment on defendants' probability of appearing in court the best we can and find little evidence of a connection.

5. CONCLUSION

Our findings raise substantial questions about the nature of the money bail system. We find substantial variation among individual magistrates in setting money bail, which suggests that the imposition of money bail, and therefore pretrial detention, is a function of the judge one receives. We exploit the random assignment of defendants to judges to examine the causal implications of money bail. Defendants assessed money bail have a 6-percentage-point (12 percent) higher chance of conviction and a .7-percentage-point higher yearly probability of being charged with further crimes (or a 6–9 percent increase). Our results are robust to alternative specifications and examining different subgroups. Our results tend to be higher on the extensive margin—whether money bail was set at all—than the intensive margin of different bail amounts. Broadly, our results seem to be strongest for relatively minor offenses: those with low average bail amounts or offenses related to retail theft. However, we do find effects that are sizable, if not significant, among defendants charged with felonies.

These results have implications for both our understanding of criminal defendants' economic circumstances and the institutional design of the American money bail system. Existing research shows that a quarter of Americans report that they cannot come up with $2,000 in 30 days (Lusardi, Schneider, and Tufano 2011), and we demonstrate how these liquidity issues have real impacts on household outcomes. The demands of money bail are quite low for those with easy access to cash, so we expect that our findings are largely driven by those facing severe liquidity constraints.

We also document how money bail impacts the later outcome of recidivism, potentially through channels of pretrial detention, the financial imposition of paying bail, or the impact of postconviction incarceration spells. Our work complements other literature demonstrating how incarceration causally influences future criminal behavior (for instance, Mueller-Smith 2016; 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 trade-offs between harms to criminal defendants and the goals of punishment, our analysis indicates a much weaker trade-off regarding the imposition of money bail on criminal defendants. Money bail imposes many costs on society—including those stemming from pretrial detention, convictions, and recidivism—yet we find no evidence that money bail results in positive outcomes, such as an increase in defendants' rate of appearance at court. Reducing the number of arrestees held before trial may be a relatively low-cost way of decreasing the size of the incarcerated population.

The system of money bail also raises substantive issues related to equal protection. Past work notes the potential for racial discrimination in the bail system (for example, Ayres and Waldfogel 1994), and we find suggestive evidence consistent with this notion: nonwhite defendants are assessed bail more frequently, despite being convicted less often. However, our primary result highlights the importance of wealth in access to justice. Many defendants appear to be found guilty simply because of an inability to pay money bail, which indicates that there are two systems: one for the rich and one for the poor.

Table A1. Common Offenses

	N	Any Money Bail	Bail Amount ($)	Nonwhite	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
Driving under the influence: first offense	11,436	27	2,166	43	82
Retail theft	10,424	36	1,284	58	63
Simple assault	6,293	84	4,449	54	80
Possession of instrument of crime with 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 for Pittsburgh

	Mean	Pairwise	Joint Regressions: No Controls	Joint Regressions: Controls
Nonwhite	.42	.019**	.019**	.015**
		(.002)	(.002)	(.004)
Race missing	.027	.0050	.015*	−.013
		(.007)	(.007)	(.011)
Male	.77	.014**	.013**	.0093**
		(.003)	(.003)	(.003)
Age	33.4	−.00011	−.000042	.000053
		(.000)	(.000)	(.000)
Out of state	.029	.015*	.016*	.014+
		(.007)	(.007)	(.009)
Prior cases	.33	−.0063**	−.0060*	.0036
		(.002)	(.002)	(.003)
F-statistic			20.0	4.74
Offense fixed effects		No	Yes	Yes
Month fixed effects		No	No	Yes

Note. For the joint regressions, $N = 38{,}149$.
+ $p < .10$.
* $p < .05$.
** $p < .01$.

Table A3. Instrumental Variables Specification by Bail Amount

	First Quartile	Second Quartile	Third Quartile	Fourth Quartile
Any money bail	.097**	.013	.068	.040
	(3.05)	(.07)	(.57)	(.30)
Age	.014**	.0047	−.0078**	−.0097**
	(10.44)	(1.52)	(−4.06)	(−7.25)
Age^2	−.00016**	−.000039	.000084**	.00011**
	(−10.29)	(−1.12)	(3.37)	(6.03)
Nonwhite	−.040**	.011*	−.063**	−.055**
	(−5.82)	(2.11)	(−11.55)	(−8.32)
Race missing	−.22**	−.16**	−.20**	−.19**
	(−23.65)	(−8.70)	(−9.96)	(−13.71)
Male	−.021**	.036**	.11**	.033**
	(−3.95)	(3.08)	(6.86)	(4.21)
N	43,974	47,396	46,327	46,047
Average bail amount ($)	734.2	3,638.5	14,974.9	65,859.3

Note. Values are marginal effects; *t*-statistics are in parentheses. All regressions include offense and other controls and an interaction of office and month of arraignment.
* $p < .05$.
** $p < .01$.

Table A4. Instrumental Variables Specification by Bail Amounts: Extensive Margin

	Coefficient	N	Average Bail ($)
Felonies:			
Drug possession, with intent to distribute	.016	23,652	16,940.5
	(.27)		
Aggravated assault	−.33**	12,382	49,633.7
	(−2.36)		
Burglary	−.086	4,420	20,715.6
	(−.43)		
Retail theft	.24**	4,323	2,075.2
	(2.59)		
Receiving stolen property	.21**	3,644	20,077.5
	(2.38)		
Misdemeanors:			
Drug possession	.023	22,776	542.1
	(.62)		
Driving under the influence: first offense	.024	11,419	2,166.3
	(.56)		
Simple assault	.048	6,270	4,449.4
	(.71)		
Gun possession	.019	6,064	10,927.9
	(.20)		
Retail theft	.11*	6,059	719.6
	(2.03)		

Note. Values are marginal effects; *t*-statistics are in parentheses. All regressions include an interaction of office and month of arraignment.

* $p < .05$.

** $p < .01$.

Table A5. Instrumental Variables Specification by Bail Amounts: Intensive Margin

	Coefficient	*N*	Average Bail ($)
Felonies:			
Drug possession, with intent to distribute	−.016	20,336	16,940.5
	(−.98)		
Aggravated assault	−.020	12,033	49,633.7
	(−.62)		
Burglary	.046	4,185	20,715.6
	(1.27)		
Retail theft	.059	2,318	2,075.2
	(1.01)		
Receiving stolen property	−.39	2,078	20,077.5
	(−.83)		
Misdemeanors:			
Drug possession	−.100**	3,412	542.1
	(−2.90)		
Driving under the influence: first offense	−.021	3,090	2,166.3
	(−.55)		
Simple assault	−.022	5,239	4,449.4
	(−.67)		
Gun possession	.096**	5,147	10,927.9
	(2.84)		
Retail theft	−.027	1,442	719.6
	(−.68)		

Note. Values are marginal effects; *t*-statistics are in parentheses. All regressions include offense and other controls and an interaction of office and month of arraignment.
** $p < .01$.

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