

The Heavy Costs of High Bail: Evidence from Judge Randomization*

Arpit Gupta
COLUMBIA BUSINESS SCHOOL

Christopher Hansman
COLUMBIA UNIVERSITY

Ethan Frenchman
MARYLAND OFFICE OF THE PUBLIC DEFENDER

April 28, 2016

Abstract

As much as 20% of the incarcerated population comprises individuals held prior to trial, typically because bail has not been posted. Using a large sample of criminal cases in Philadelphia and Pittsburgh, we exploit the random assignment of bail judges and find that larger bail amounts lead to greater incarceration spell and a higher likelihood of guilty pleas. Our estimates suggest that being assigned a required money bail is associated with a 6 percentage point rise in the likelihood of pleading guilty, and a 4 percentage point rise in recidivism. We also find suggestive evidence of racial discrimination 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 pre-trial detention programs.

*We are grateful to workshop participants at Columbia Law School for helpful comments. We also thank Joel Mankoski and the Pennsylvania Office of the Administrative Courts for data access. We gratefully acknowledge financial assistance from the Center for Justice at Columbia University.

1 Introduction

On an average day 487,000 individuals are held in pretrial detention in the United States, typically because they are unable or unwilling to make bail. With deep backlogs in the criminal justice system, defendants sometimes wait in jail for months or years before reaching trial. Facing the choice between prohibitive bail and lengthy detention spells, defendants may take a third option and avoid trial altogether by accepting a plea deal.

This paper investigates the causal impact of monetary bail on guilty pleas and recidivism using comprehensive court data from two largest cities in Pennsylvania: Philadelphia and Pittsburgh. To do so, we exploit the quasi-random assignment of defendants to arraignment court magistrates. Because some magistrates tend to be more “strict” than others — assigning bail more frequently, and for higher amounts — a defendant’s magistrate assignment has a significant impact on whether or not he or she is assigned bail. The random nature of assignment is especially strong in Philadelphia, where all defendants are processed through a unique, centralized, 24 hour-a-day arraignment system.

In Philadelphia, we find that being assigned money bail significantly increases the probability of a guilty case outcome: criminal defendants who are assessed money bail as a result of their judicial assignment are 6 percentage points more likely to have a case disposition of guilty. These effects appear to be driven by defendants who fail to post bail, potentially due to liquidity or credit constraints, and persist in magnitude among defendants facing a felony conviction. We also document that the combination of harms faced by criminal defendants facing money bail, whether it be the financial strain of making bail, pre-trial incarceration for those unable to make bail, or post-trial incarceration after guilty outcomes, increases recidivism in our sample by 4 percentage points. As a robustness check, we also examine the assignment process in Pittsburgh, which is less random, and find similar results.

The relationship between bail and guilty outcomes that we document suggests a strong interaction between financial fragility and the criminal justice system. A large literature has examined the credit constraints facing American households which make even small bail amounts difficult to post. While its feasible that money bail could impact plea behavior amongst those with sufficient liquid assets, it is more likely that these effects come primarily

from the constrained. This possibility poses substantive legal issues. The equal protection clause of the 14th Amendment prohibits incarceration based on a defendant's ability to pay, and prohibitive bail to ensure pre-trial detention may only be set to ensure the states' compelling interests regarding defendants—the flight risk of defendants or public safety issues.

Our findings also raise institutional design questions regarding the bail system as a whole, given the large fraction of defendants detained prior to trial who might otherwise be released on recognizance or held with an ankle bracelet at far lower public cost. A criminal justice system which assigns guilt as a function of bail assessment or judicial factors is unlikely to be optimally functional.

Past work has noted the potential interaction between bail and guilty case outcomes (e.g. Phillips (2007), Phillips (2008)). However assessing the impacts of bail and the pre-trial detention process is fundamentally confounded by the endogenous nature of the bail assessment.¹ When judicial magistrates assign bail, they are frequently guided by risk assessment tools which already incorporate the probabilities of failing to appear and public safety. They additionally exercise discretion in incorporating criminal charges, prior case history, ties to the local community, and potentially even the strength of the case against the defendant as presented by a prosecuting attorney. These factors may well be related to underlying guilt, and render any raw correlations between bail assessments and guilty outcomes difficult to interpret. Our identification strategy, utilizing the random assignment of magistrates, allows us to circumvent this issue.

Our research relates to a large legal literature on bail and the pre-trial system,² as well as the substantial body of policy research that includes Lowenkamp, VanNostrand and Holsinger (2013), Bechtel et al. (2012) and Phillips (2012). Beyond Abrams and Rohlfs (2011), our work is most closely related to the growing set of papers in economics utilizing random assignment of administrators 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

¹A notable exception is Abrams and Rohlfs (2011) who exploit an experiment in Philadelphia in the 1980s, although they do not examine the impact of bail on guilt

²The Criminal Justice Institute has created an exceptionally detailed bibliography, available at: <http://www.pretrial.org/wpfb-file/pji-pretrial-bibliography-pdf/>

Schoar (2007), Aizer and Doyle (2015) and Dobbie and Song (2015). We differ primarily in presenting a novel exploration of the pre-trial sentencing process.

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

Constitutional protections guarantee defendants the right to a non-excessive bail hearing within 48 hours of arrest. The primary purpose of bail hearings is to set an appropriate bail to ensure defendants show up for a subsequent trial hearing, and do not pose a public safety threat (typically intimidating witnesses).

At a bail hearing, a 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. Release on Nonmonetary Conditions or Unsecured Bailbond – Allows some non-monetary restriction to be placed on the defendant, or a written agreement to be liable for a fixed bail amount
3. Release on Nominal Bail or a Monetary condition – Defendant must post a nominal fraction of the bail (with another individual or bail agency acting as surety) or the full amount

Bail bondsmen frequently assist defendants unable to provide cash bail, and typically demand 10% collateral, which is held after trial as interest. Jurisdictions differ substantially in their reliance on various bail options—for instance, in Washington D.C. there is no money bail; defendants are either held or released.

A variety of constitutional and legal protections constrain the practice of bail setting. As liberty is a fundamental right guaranteed by the 5th and 14th Amendments, pre-trial detention must be “narrowly tailored” to the states’ “compelling interests” regarding defendants (U.S. v. Salerno), which are typically interpreted as reflected either the flight risk or public safety of defendants. Other liberty concerns are raised under 8th Amendment protections against excessive bail, which have been interpreted to mean that a ‘Bail set at a figure higher than an amount reasonably calculated to fulfill [its] purpose is excessive;’ though what constitutes “excessive” has been strongly contested (Stack v. Boyle).

Bail also raises questions of 14th Amendment equal protection which prohibits incarceration for reasons of pure inability to pay (Tate v. Short). The law additionally prohibits differential access to justice depending on wealth (Griffin v. Illinois). Equal protection on bail assessment for other protected classes (ie, gender, race, etc.) is also guaranteed.

2.2 Bail Hearings

Bail Hearings are overseen by judicial magistrates, who do not necessarily need law degrees. In the majority of the state, excluding Philadelphia, these magistrates are elected on a six-year term to oversee cases in a particular district. A single magistrate handles the majority of the arrests that occur within their jurisdiction, although many cases are seen by other magistrates during weekends, nights, and other periods when the presiding magistrate is not in service.

Philadelphia, however, features a unique bail assessment process featuring a centralized municipal court. Defendants face bail hearings from one of a team of appointed arraignment court magistrates. Magistrates frequently preside over cases via teleconference to satellite offices in the city. The centralized process and high case load results in a judicial assignment process which is effectively random in practice (an assumption we test). Importantly for our purposes—bail magistrates set bail and do not impact any future case outcomes. In particular, they are not the same judge that manages the criminal sentencing.

When setting bail (typically a quick process averaging a few minutes); magistrates weigh the public safety and flight risk of letting defendants free to determine a required bail amount.

Aspects of the criminal charges faced by a defendant; background risk factors (i.e., prior criminal history, other outstanding charges, ties to the community); and the strength of the prosecution’s case against a defendant all play a part. Many jurisdictions have begun using standardized risk assessments to establish a baseline for all criminal defendants. We find that such tools do not eliminate the role for individual judicial discretion in weighing the appropriate role for bail in cases. Bail amounts can be adjusted subsequently following the initial hearing, possibly in response to the defendant’s financial condition. We focus on the first assessed 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 bail varies from state. In Pennsylvania, defendants first receive a bail hearing, then decide on their appropriate plea amount. This timeline ensures that bail is a factor in the defendant’s plea decision. A high bail amount ensures the defendant is required to produce a higher cash sum (for either the full amount or a collateral value for a loan) to post bail, or else faces a potentially arduous pre-trial detention spell (which can last several months in Philadelphia).

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 bail assessments on criminal defendants (though we focus on the bail amount resulting from the first hearing); disposition

information on the list of offenses that defendants are faced with (we account for the full case history, but typically focus on the most severe offense); bench warrant information issued regarding the docket; as well as final sentencing outcomes of individual defendants. Our first appendix table, Table 12, contains the top 10 most common offenses, and basic characteristics of the cases associated with those offenses.

3.2 Empirical Strategy

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

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

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.

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 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 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 strictness. They are created by regressing various covariates on the linear prediction of money bail on the judicial strictness measure generated 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.

More formally, we are attempting conceptually to isolate the impact of judge severity, 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. 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:

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

Our preferred measure computes judge deviations from the office-offense average to account for possible non-random assignment by offense.

Panel A of Figure 3 illustrates our estimate of judicial severity against the log bail amount, showing that judge Strictness 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 strictness is even consistent across different office).

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

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

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, particularly in Philadelphia, leads us to expect close to random assignment of cases across magistrates, 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 each covariate against our instrument in isolation with no additional controls and reports the coefficient. Column 3 regresses all covariates against our instrument along with 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 presents our first stage—a regression of our instrument against an 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. Being assigned to a hypothetical judge who exclusively assesses money bail for a particular offense when the office average is to never levy money bail for that offense would result in a 59% higher likelihood of receiving a money bail.

Our first stage suggests strong instrumental validity: being assigned to a stricter judge results in defendants face a higher likelihood of being forced to post money bail. As the preceding section suggests, we can view this assignment as causal based on an institutional understanding of the Philadelphia magistrate system and lack of correlation with covariates. Column two presents the reduced form—a direct regression of our instrument of judge Strictness 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.

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 case guilt outcomes. Our estimate suggests that defendants required to pay money bail for reasons 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, this 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 strictness, and suggests a powerful role for bail amounts in inducing guilty outcomes by defendants.

Table 4 also provides estimates for 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 stem from randomization and cannot be purely interpreted causally, they are certainly consistent with an interpretation of racial bias on the part of bail magistrates and prosecutors (in the sense of arresting many non-whites, who are subsequently not found guilty; and disproportionately assessing bail).

Table 5 shows our primary specification for outcomes for defendants that are split into four categories corresponding to the interaction of posting bail and a case disposition of guilty. Each of the four columns presents an IV regression with one of those category as the dependent variable. We see that there is a strong effect of money bail on the joint outcome of not posting bail and a case disposition of guilt, suggesting that the primary effect of bail on guilty pleas runs through those who are unable to make bail.

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, 8.1 percentage points is not significant but high in magnitude and suggestive that cases of guilt induced by higher bail are not purely for low-level crimes. Felonies on records frequently lead to difficulties in future employment and voting status.

Tables 7 and 8 explore alternate specifications of our judge strictness 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—only the intensive margin. In Philadelphia, we find no evidence that the intensive margin matters, only the extensive margin of being assessed money bail.

Table 9 examines results in Pittsburgh. As discussed in the legal background section, the nature of judicial assignment in Pittsburgh and the rest of the state is not as clean and does not permit as straightforward an analysis. 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.

A randomization check suggests that violations of randomization in Pittsburgh are minor quantitatively but significant statistically, with a F-Statistic of 4 for the county overall. Nonetheless, to establish robustness of our primary finding outside of the city of Philadelphia, we attempt a version of our main specification in Pittsburgh. Remarkably, given the extent of non-random assignment, we find estimates that are virtually identical in Pittsburgh—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.

4.3 Other Outcomes

Table 10 explores the outcome of recidivism. In order to avoid 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 (three months and one year windows provide similar results). While the first stage is identical in this specification, the reduced form examines the direct relation between our judicial instrument 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 extremely large and sizable in the context of a 6 percentage point increase in the likelihood of being assessed guilty. Our estimate on recidivism likely stems from a combination of factors: reflecting the role of pre-trial 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 sentencing spells on future criminal activity among those who were assessed money bail and were found guilty. Overall, these factors focus on the role of life-cycle transitions through the criminal justice system which may predispose individuals to commit more crime in the future.

Table 11 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 reason). Neither variable is statistically associated with money bail as instrumented for using our judge measure. Given the limitations of both variables, 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 bail setting in the pre-trial detention process. We exploit the judicial variation in bail setting in conjunction with randomized assignment 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, though do not have causally definitive estimates, and find little evidence that bail assessments improve defendant show up rates.

Our findings are particularly striking in light of the fact that Philadelphia, the site of our primary analysis, over this period employed a pre-trial risk assessment tool designed to appropriately target money bail to high-risk defendants. Our work suggests that judicial discretion persists in such settings suggesting that conditions setting bail are not “narrowly tailored to flight risk and public safety.

References

- Abrams, David S, and Chris Rohlfs.** 2011. “Optimal bail and the value of freedom: evidence from the Philadelphia bail experiment” *Economic Inquiry*, 49(3): 750–770.
- Aizer, Anna, and Joseph J. Doyle.** 2015. “Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges” *The Quarterly Journal of Economics*.
- Bechtel, Kristin, John Clark, Michael R Jones, and David J Levin.** 2012. “Dispelling the Myths: What Policy Makers Need to Know About Pretrial Research” *Pretrial Justice Institute*.
- Chang, Tom, and Antoinette Schoar.** 2007. “The effect of judicial bias in Chapter 11 reorganization” *Unpublished manuscript. Massachusetts Institute of Technology, Cambridge, Mass.*
- Dobbie, Will, and Jae Song.** 2015. “Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection” *American Economic Review*, 105(3): 1272–1311.
- Doyle Jr, Joseph J.** 2007. “Child protection and child outcomes: Measuring the effects of foster care” *The American Economic Review*, 97(5): 1583–1610.
- Doyle Jr, Joseph J.** 2008. “Child protection and adult crime: Using investigator assignment to estimate causal effects of foster care” *Journal of Political Economy*, 116(4): 746–770.
- Kling, Jeffrey R.** 2006. “Incarceration Length, Employment, and Earnings” *The American Economic Review*, 96(3): 863.
- Lowenkamp, Christopher T, Marie VanNostrand, and Alexander Holsinger.** 2013. “The hidden costs of pretrial detention” *Laura and John Arnold Foundation*.
- Mueller-Smith, Michael.** 2016. “The Criminal and Labor Market Impacts of Incarceration” *Unpublished manuscript, University of Michigan*.

Phillips, Mary T. 2007. “Bail, detention, and nonfelony case outcomes” *New York City Criminal Justice Agency, INC.*

Phillips, Mary T. 2008. “Bail, detention, and felony case outcomes” *New York City Criminal Justice Agency, INC.*

Phillips, Mary T. 2012. “A Decade of Bail Research in New York City” *New York City Criminal Justice Agency, INC.*

Figure 1
Guilt by Bail Status: Possession of Marijuana

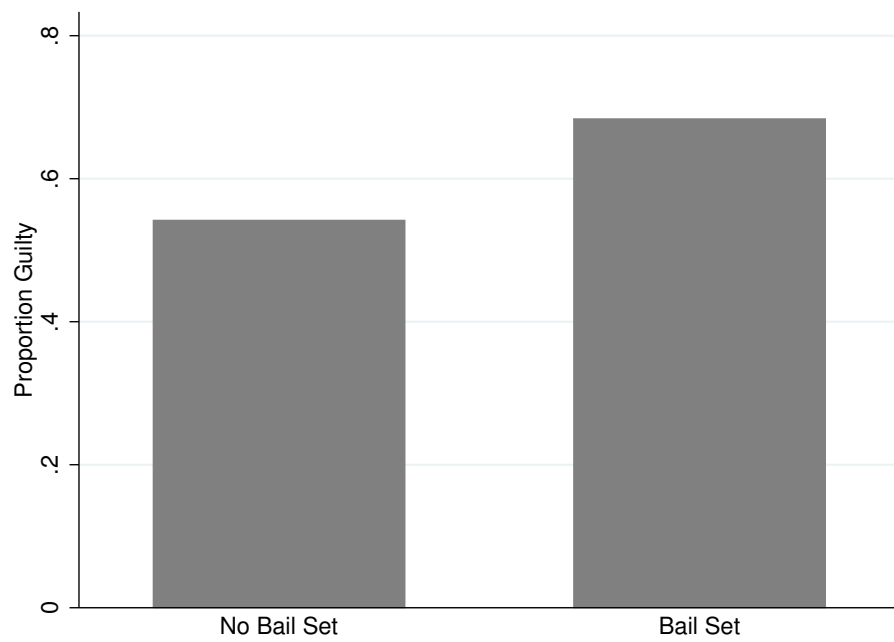


Figure 2
Covariates against Money Bail, Instrumented and Raw

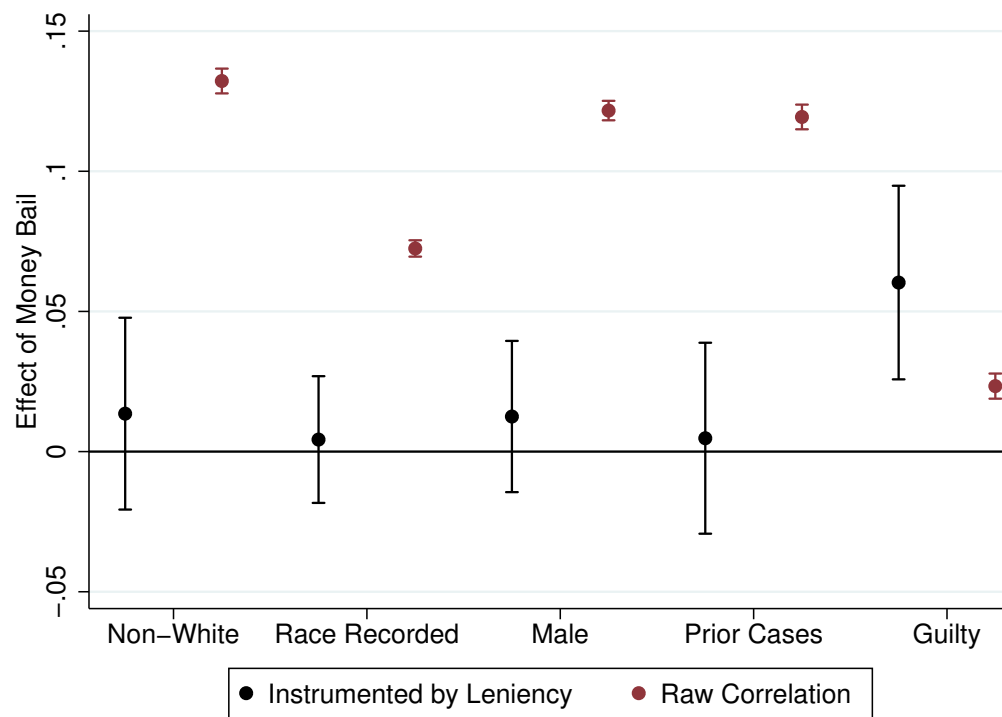
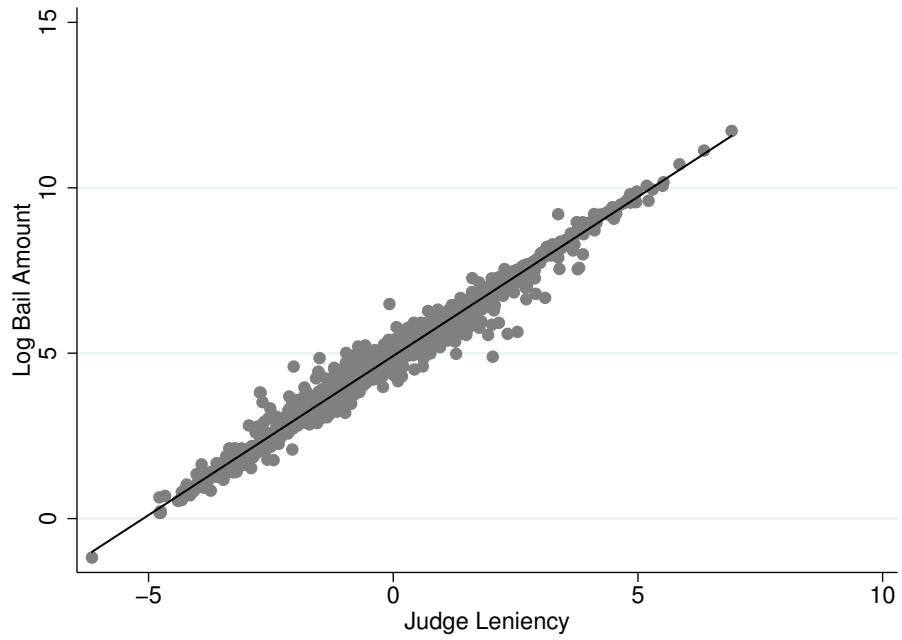


Figure 3
Judicial Severity vs. Log Bail Amount



Judicial Severity $t-1$ v. Severity in t

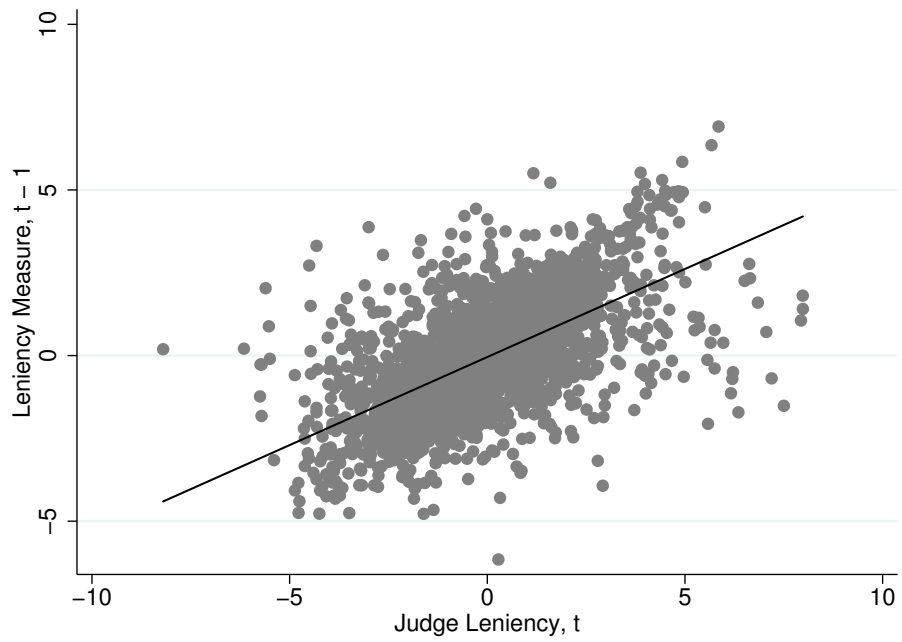


Table 1
Summary Statistics

	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	

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. 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 Strictness 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 Strictness measure on each case characteristic. Column 3 contains the coefficients from a single regression of the judge Strictness 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
Strictness	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 Strictness 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 Strictness measure. The second column presents the reduced form: a regression of Case Guilty on our judge Strictness 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. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
IV Regressions of Guilt on Money Bail by Bail Posting Status

	Guilty		Not Guilty	
	Bail Not Posted	Bail Posted	Bail Not Posted	Bail Posted
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 case dispositions on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge Strictness 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 posting bail and a case disposition of guilty. 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. 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 Strictness 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. 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
Strictness	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 Strictness 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 Strictness measure. The second column presents the reduced form: a regression of Case Guilty on our judge Strictness 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. 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
Strictness	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 Strictness 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 Strictness measure. The second column presents the reduced form: a regression of Case Guilty on our judge Strictness 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. 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
Strictness	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 Strictness 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 Strictness measure. The second column presents the reduced form: a regression of Case Guilty on our judge Strictness 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. Standard errors are clustered at the office-judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10
IV Regressions of Recidivism on Money Bail

	First Stage	Reduced Form	IV
	Any Money Bail	Recidivism	Recidivism
Strictness	0.588*** (0.028)	0.025** (0.012)	
Any Money Bail			0.043** (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*** (0.004)
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 recidivism on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge Strictness 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 Strictness measure. The second column presents the reduced form: a regression of recidivism on our judge Strictness 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. Standard errors are clustered at the judge-year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11
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 Strictness 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. Standard errors are clustered at the judge year level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12
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%