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# EXHIBIT A

# RACIAL BIAS IN BAIL DECISIONS*

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This article develops a new test for identifying racial bias in the context of bail decisions—a high-stakes setting with large disparities between white and black defendants. We motivate our analysis using Becker's model of racial bias, which predicts that rates of pretrial misconduct will be identical for marginal white and marginal black defendants if bail judges are racially unbiased. In contrast, marginal white defendants will have higher rates of misconduct than marginal black defendants if bail judges are racially biased, whether that bias is driven by racial animus, inaccurate racial stereotypes, or any other form of bias. To test the model, we use the release tendencies of quasi-randomly assigned bail judges to identify the relevant race-specific misconduct rates. Estimates from Miami and Philadelphia show that bail judges are racially biased against black defendants, with substantially more racial bias among both inexperienced and part-time judges. We find suggestive evidence that this racial bias is driven by bail judges relying on inaccurate stereotypes that exaggerate the relative danger of releasing black defendants. *JEL* Codes: C10, C26, J15, K14.

## I. Introduction

Racial disparities exist at every stage of the U.S. criminal justice system. Compared to observably similar whites, blacks are more likely to be searched for contraband (Antonovics and Knight 2009), more likely to experience police force (Fryer 2016), more likely to be charged with a serious offense (Rehavi and Starr 2014), more likely to be convicted (Anwar, Bayer, and Hjalmarson 2012), and more likely to be incarcerated (Abrams, Bertrand, and Mullainathan 2012). Racial disparities are particularly prominent in

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the setting of bail: in our data, black defendants are 3.6 percentage points more likely to be assigned monetary bail than white defendants and, conditional on being assigned monetary bail, receive bail amounts that are $9,923 greater.[1] One view is that these racial disparities are driven by statistical discrimination, or the use of observable group traits (such as race) to form accurate beliefs about the unobservable characteristics of defendants (e.g., Phelps 1972; Arrow 1973). A second view is that statistical discrimination alone cannot explain these disparities, leaving a role for various forms of racial bias, such as racial animus (e.g., Becker 1957) or inaccurate racial stereotypes (e.g., Bordalo et al. 2016). However, distinguishing between these contrasting explanations remains an empirical challenge.

To test whether racial bias is empirically relevant, Becker (1957, 1993) proposed an "outcome test" that compares the success or failure of decisions across groups at the margin. In our setting, the outcome test is based on the idea that rates of pretrial misconduct will be identical for marginal white and marginal black defendants if bail judges are racially unbiased and the disparities in bail setting are solely due to accurate statistical discrimination. In contrast, marginal white defendants will have higher rates of pretrial misconduct than marginal black defendants if these bail judges are racially biased against blacks, whether that racial bias is driven by racial animus, inaccurate racial stereotypes, or any other form of racial bias. The outcome test has been difficult to implement in practice, however, as comparisons based on average defendant outcomes are biased when whites and blacks have different risk distributions—the well-known inframarginality problem (e.g., Ayres 2002).

In recent years, two seminal publications have developed outcome tests of racial bias that partially circumvent this inframarginality problem. In the first paper, Knowles, Persico, and Todd (2001) show that if motorists respond to the race-specific probability of being searched, then all motorists of a given race will carry contraband with equal probability. As a result, the marginal and average success rates of police searches will be identical, and OLS estimates are not biased by inframarginality concerns.

1. Authors' calculation for Miami-Dade and Philadelphia using the data described in Section III. Racial disparities in bail setting are also observed in other jurisdictions. For example, black felony defendants in state courts are nine percentage points more likely to be detained pretrial compared to otherwise similar white defendants (McIntyre and Baradaran 2013).

Knowles, Persico, and Todd (2001) find no difference in the average success rate of police searches for white and black drivers, leading them to conclude that there is no racial bias in police searches. In a second important paper, Anwar and Fang (2006) develop a test of relative racial bias based on the idea that the ranking of search and success rates by white and black police officers should be unaffected by the race of the motorist even when there are inframarginality problems. Consistent with Knowles, Persico, and Todd (2001), Anwar and Fang (2006) find no evidence of relative racial bias in police searches, but note that their approach cannot be used to detect absolute racial bias.[2] However, the prior literature has been critiqued for its reliance on restrictive assumptions about the unobserved risk of blacks and whites (e.g., Brock et al. 2012).

In this article, we propose a new outcome test for identifying racial bias in the context of bail decisions. Bail is an ideal setting to test for racial bias for a number of reasons. First, the legal objective of bail judges is narrow, straightforward, and measurable: to set bail conditions that allow most defendants to be released while minimizing the risk of pretrial misconduct. In contrast, the objectives of judges at other stages of the criminal justice process, such as sentencing, are complicated by multiple hard-to-measure objectives, such as the balance between retribution and mercy. Second, mostly untrained bail judges must make on-the-spot judgments with limited information and little to no interaction with defendants. These institutional features make bail decisions particularly prone to the kind of inaccurate stereotypes or categorical heuristics that exacerbate racial bias (e.g., Fryer and Jackson 2008; Bordalo et al. 2016). Finally, bail decisions are extremely consequential for both white and black defendants, with prior work suggesting that detained defendants suffer about $30,000

2. We replicate the Knowles, Persico, and Todd (2001) and Anwar and Fang (2006) tests in our data, finding no evidence of racial bias in either case. The differences between our test and the Knowles, Persico, and Todd (2001) and Anwar and Fang (2006) tests are that (i) we identify treatment effects for marginal defendants rather than the average defendant, and (ii) we identify absolute rather than relative bias. See Section IV.C for additional details on why the Knowles, Persico, and Todd (2001) and Anwar and Fang (2006) tests yield different results than our test.

in lost earnings and government benefits alone (Dobbie, Goldin, and Yang 2018).[3]

We begin by formally developing two complementary estimators that use variation in the release tendencies of quasi-randomly assigned bail judges to identify the differences in pretrial misconduct rates at the margin of release required for the Becker outcome test. Our first estimator uses the standard instrumental variables (IVs) framework to identify differences in the local average treatment effects (LATEs) for white and black defendants near the margin of release. Though IV estimators are often criticized for the local nature of the estimates, we exploit the fact that the Becker test relies on (the differences between) exactly these kinds of local treatment effects to test for racial bias. In our context, the IV estimator measures the weighted average of racial bias across all bail judges with relatively few auxiliary assumptions, but at the potential cost that we cannot estimate judge-specific treatment effects, and the weighting scheme underlying the IV estimator is not always policy relevant. In contrast, our second estimator uses the marginal treatment effects (MTEs) framework developed by Heckman and Vytlacil (1999, 2005) to estimate judge-specific treatment effects for white and black defendants at the margin of release. Our MTE estimator therefore allows us to put equal weight on each judge in our sample, but with the estimation of the judge-specific estimates coming at the cost of additional auxiliary assumptions.

Next, we test for racial bias in bail setting using administrative court data from Miami and Philadelphia. We find evidence of significant racial bias against black defendants using both our IV and MTE estimators, ruling out statistical discrimination as the sole explanation for the racial disparities in bail. We find that marginally released white defendants are 22.2 to 23.1 percentage points more likely to be rearrested prior to disposition than marginally released black defendants using our IV and MTE estimators, respectively. Our estimates of racial bias are nearly identical if we account for other observable crime and defendant differences by race, suggesting that our results cannot be explained by black–white differences in certain types of crimes (e.g., the proportion of felonies versus misdemeanors) or

3. See Dobbie, Goldin, and Yang (2018), Gupta, Hansman, and Frenchman (2016), Leslie and Pope (2017), and Stevenson (2016) for evidence on the nonfinancial consequences of bail decisions.

black–white differences in defendant characteristics (e.g., the proportion with prior offenses versus no prior offenses). In sharp contrast to these results, naive OLS estimates indicate, if anything, racial bias against white defendants, highlighting the importance of accounting for both inframarginality and omitted variables when estimating racial bias in the criminal justice system.

Finally, we explore which form of racial bias is driving our findings. The first possibility is that, as originally modeled by Becker (1957, 1993), racial animus leads judges to discriminate against black defendants at the margin of release. This type of taste-based racial bias may be a particular concern in our setting due to the relatively low number of minority bail judges, the rapid-fire determination of bail decisions, and the lack of face-to-face contact between defendants and judges. A second possibility is that bail judges rely on incorrect inferences of risk based on defendant race due to antiblack stereotypes, leading to the relative overdetention of black defendants at the margin. These inaccurate antiblack stereotypes can arise if black defendants are overrepresented in the right tail of the risk distribution, even when the difference in the riskiness of the average black defendant and the average white defendant is very small (Bordalo et al. 2016). As with racial animus, these racially biased prediction errors in risk may be exacerbated by the fact that bail judges must make quick judgments on the basis of limited information, with virtually no training and, in many jurisdictions, little experience working in the bail system.

We find three sets of facts suggesting that our results are driven by bail judges relying on inaccurate stereotypes that exaggerate the relative danger of releasing black defendants versus white defendants at the margin. First, we find that both white and black bail judges exhibit racial bias against black defendants, a result that is inconsistent with most models of racial animus. Second, we find that our data are strikingly consistent with the theory of stereotyping developed by Bordalo et al. (2016). For example, we find that black defendants are sufficiently overrepresented in the right tail of the predicted risk distribution, particularly for violent crimes, to rationalize observed racial disparities in release rates under a stereotyping model. We also find that there is no racial bias against Hispanics, who, unlike blacks, are not significantly overrepresented in the right tail of the predicted risk distribution. Finally, we find substantially more racial bias when prediction errors of any kind are more likely to occur. For example, we find substantially less racial bias among both the full-time and more experienced part-time judges who are least likely to rely on simple race-based heuristics, and substantially more racial bias among the least experienced part-time judges who are most likely to rely on these heuristics.

Our findings are broadly consistent with parallel work by Kleinberg et al. (2018), who use machine learning techniques to show that bail judges make significant prediction errors for defendants of all races. Using a machine learning algorithm to predict risk using a variety of inputs such as prior and current criminal charges, but excluding defendant race, they find that the algorithm could reduce crime and jail populations while simultaneously reducing racial disparities. Their results also suggest that variables that are unobserved in the data, such as a judge's mood or a defendant's demeanor at the bail hearing, are the source of prediction errors, not private information that leads to more accurate risk predictions. Our results complement those of Kleinberg et al. (2018) by documenting one specific source of these prediction errors—racial bias among bail judges.

Our results also contribute to an important literature testing for racial bias in the criminal justice system. As discussed already, Knowles, Persico, and Todd (2001) and Anwar and Fang (2006) are seminal works in this area. Subsequent work has used outcome tests to examine racial bias in police search decisions (Antonovics and Knight 2009), capital sentencing (Alesina and La Ferrara 2014), and parole board release decisions (Anwar and Fang 2015; Mechoulan and Sahuguet 2015). Racial bias in bail setting has been studied using the prices charged by bail bond dealers (Ayres and Waldfogel 1994) and a parametric framework to account for unobserved heterogeneity across defendants (Bushway and Gelbach 2011). Our article is also related to work using LATEs provided by IV estimators to obtain effects at the margin of the instrument (e.g., Card 1999; Gruber, Levine, and Staiger 1999) and work using MTEs to extrapolate to other estimands of interest (e.g., Heckman and Vytlacil 2005; Heckman, Urzua, and Vytlacil 2006; Cornelissen et al. 2016; Brinch, Mogstad, and Wiswall 2017).

The remainder of the article is structured as follows. Section II provides an overview of the bail system, describes the theoretical model underlying our analysis, and develops our empirical test for racial bias. Section III describes our data and empirical methodology. Section IV presents the main results. Section V explores potential mechanisms, and Section VI concludes. The Online Appendix provides all additional results, theoretical proofs, and detailed information on our setting.

## II. An Empirical Test of Racial Bias

In this section, we motivate and develop our empirical test for racial bias in bail setting. Our theoretical framework closely follows the previous literature on the outcome test in the criminal justice system (e.g., Becker 1957, 1993; Knowles, Persico, and Todd 2001; Anwar and Fang 2006). Consistent with the prior literature, we show that we can test for racial bias by comparing treatment effects for the marginal black and marginal white defendants. We develop two complementary estimators to identify these race-specific treatment effects using the quasi-random assignment of cases to judges. Online Appendix B provides additional details and proofs.

### *II.A. Overview of the Bail System*

In the United States, bail judges are granted considerable discretion to determine which defendants should be released before trial. Bail judges are meant to balance two competing objectives when deciding whether to detain or release a defendant before trial. First, bail judges are directed to release all but the most dangerous defendants before trial to avoid undue punishment for defendants who have not yet been convicted of a crime. Second, bail judges are instructed to minimize the risk of pretrial misconduct by setting the appropriate conditions for release. In our setting, pretrial misconduct includes both the risk of new criminal activity and the risk of failure to appear for a required court appearance. Importantly, bail judges are not supposed to assess guilt or punishment at the bail hearing.

The conditions of release are set at a bail hearing typically held within 24 to 48 hours of a defendant's arrest. In most jurisdictions, bail hearings last only a few minutes and are held through a videoconference to the detention center such that judges can observe each defendant's demeanor. During the bail hearing, the assigned bail judge considers factors such as the nature of the alleged offense, the weight of the evidence against the defendant, the nature and probability of danger that the defendant's release poses to the community, the likelihood of flight based on factors such as the defendant's employment status and living situation, and any record of prior flight or bail violations, among other factors (Foote, Markle, and Woolley 1954). Because bail judges are granted considerable discretion in setting the appropriate bail conditions, there are substantial differences across judges in the same jurisdiction (e.g., Gupta, Hansman, and Frenchman 2016; Stevenson 2016; Leslie and Pope 2016; Dobbie, Goldin, and Yang 2018).

The assigned bail judge has a number of potential options when setting a defendant's bail conditions. For example, the bail judge can release low-risk defendants on a promise to return for all court appearances, known as release on recognizance (ROR). For defendants who pose a higher risk of flight or new crime, the bail judge can allow release but impose nonmonetary conditions, such as electronic monitoring or periodic reporting to pretrial services. The judge can also require defendants to post a monetary amount to secure release, typically 10% of the total bail amount. If the defendant fails to appear at the required court appearances or commits a new crime while out on bail, either he or the bail surety forfeits the 10% payment and is liable for the remaining 90% of the total bail amount. In practice, the median bail amount is $6,000 in our sample, and only 57% of defendants meet the required monetary conditions to secure release. Bail may be denied altogether for defendants who commit the most serious crimes, such as first- or second-degree murder.

One important difference between jurisdictions is the degree to which bail judges specialize in conducting bail hearings. In our setting, the bail judges we study in Philadelphia are full-time specialists who are tasked with setting bail seven days a week throughout the entire year. In contrast, the bail judges we study in Miami are part-time nonspecialists who assist the bail court by serving weekend shifts once or twice a year. These weekend bail judges spend their weekdays as trial judges. We explore the potential importance of these institutional features in Section V.

### *II.B. Model of Judge Behavior*

This section develops a stylized theoretical framework that allows us to define an outcome-based test of racial bias in bail setting. We begin with a model of taste-based racial bias or racial animus that closely follows Becker (1957, 1993). We then present an alternative model of racially biased prediction errors, which generates similar empirical predictions as the taste-based model.

*1. Taste-Based Discrimination.* Let $i$ denote a defendant and $\mathbf{V}_i$ denote all case and defendant characteristics considered by the bail judge, excluding defendant race $r_i$. The expected cost of release for defendant $i$ conditional on observable characteristics $\mathbf{V}_i$ and race $r_i$ is equal to the expected probability of pretrial misconduct $\mathbb{E}[\alpha_i|\mathbf{V}_i, r_i]$, which includes the likelihood of new crime and failure to appear, times the cost of misconduct $C$, which includes the social cost of any new crime or failures to appear. For simplicity, we normalize $C = 1$, so that the expected cost of release conditional on observable characteristics is equal to $\mathbb{E}[\alpha_i|\mathbf{V}_i, r_i]$. Moving forward, we simplify our notation by letting the expected cost of release conditional on observables be denoted by $\mathbb{E}[\alpha_i|r_i]$.

The perceived benefit of release for defendant $i$ assigned to judge $j$ is denoted by $t_r^j(\mathbf{V}_i)$, which is a function of observable case and defendant characteristics $\mathbf{V}_i$. The perceived benefit of release $t_r^j(\mathbf{V}_i)$ includes social cost savings from reduced jail time, private gains to defendants from an improved bargaining position with the prosecutor or increased labor force participation, and personal benefits to judge $j$ from any direct utility or disutility from being known as either a lenient or tough judge, respectively. Importantly, we allow the perceived benefit of release $t_r^j(\mathbf{V}_i)$ to vary by race $r \in W, B$ to allow for judge preferences to differ for white and black defendants.

DEFINITION 1. Following Becker (1957, 1993), we define judge $j$ as racially biased against black defendants if $t_W^j(\mathbf{V}_i) > t_B^j(\mathbf{V}_i)$. Thus, for racially biased judges, there is a higher perceived benefit of releasing white defendants than releasing observably identical black defendants.

For simplicity, we assume that bail judges are risk neutral and maximize the perceived net benefit of pretrial release. We also assume that the bail judge's sole task is to decide whether to release or detain a defendant given that this decision margin is the most important and consequential (Dobbie, Goldin, and Yang 2018; Kleinberg et al. 2018). In simplifying each judge's task to this single decision, we abstract away from the fact that bail judges may set different levels of monetary bail that take into account a defendant's ability to pay. We discuss possible extensions to the model that account for these features below.

Under these assumptions, the model implies that bail judge $j$ will release defendant $i$ if and only if the expected cost of pretrial release is less than the perceived benefit of release:

$$\mathbb{E}[\alpha_i | r_i = r] \leqslant t_r^j(\mathbf{V}_i). \tag{1}$$

Given this decision rule, the marginal defendant for judge $j$ and race $r$ is the defendant $i$ for whom the expected cost of release is exactly equal to the perceived benefit of release, that is, $\mathbb{E}[\alpha_i^j | r_i = r] = t_r^j(\mathbf{V}_i)$. We simplify our notation by letting this expected cost of release for the marginal defendant for judge $j$ and race $r$ be denoted by $\alpha_r^j$.

Based on the above framework and Definition 1, the model yields the familiar outcome-based test for racial bias from Becker (1957, 1993):

PROPOSITION 1. If judge $j$ is racially biased against black defendants, then $\alpha_W^j > \alpha_B^j$. Thus, for racially biased judges, the expected cost of release for the marginal white defendant is higher than the expected cost of release for the marginal black defendant.

Proposition 1 predicts that marginal white and marginal black defendants should have the same probability of pretrial misconduct if judge $j$ is racially unbiased, but marginal white defendants should have a higher probability of misconduct if judge $j$ is racially biased against black defendants.

*2. Racially Biased Prediction Errors in Risk.* In the taste-based model of discrimination outlined above, we assume that judges agree on the (true) expected cost of release, $\mathbb{E}[\alpha_i | r_i]$, but not the perceived benefit of release, $t_r^j(\mathbf{V}_i)$. An alternative approach is to assume that judges disagree on their (potentially inaccurate) predictions of the expected cost of release, as would be the case if judges systematically overestimate the probability of pretrial misconduct for black defendants relative to white defendants. We show that a model motivated by these kinds of racially biased prediction errors in risk can generate the same predictions as a model of taste-based discrimination.

Let $i$ again denote defendants and $\mathbf{V}_i$ denote all case and defendant characteristics considered by the bail judge, excluding defendant race $r_i$. The perceived benefit of releasing defendant $i$ assigned to judge $j$ is now defined as $t(\mathbf{V}_i)$, which does not vary by judge.

The perceived cost of release for defendant $i$ conditional on observable characteristics $\mathbf{V}_i$ is equal to the perceived probability of pretrial misconduct, $\mathbb{E}^j[\alpha_i|\mathbf{V}_i, r_i]$, which is now allowed to vary across judges. We can write the perceived cost of release as:

$$\mathbb{E}^j[\alpha_i|\mathbf{V}_i, r_i] = \mathbb{E}[\alpha_i|\mathbf{V}_i, r_i] + \tau_r^j(\mathbf{V}_i), \quad (2)$$

where $\tau_r^j(\mathbf{V}_i)$ is a prediction error that is allowed to vary by judge $j$ and defendant race $r_i$. To simplify our notation, we let the true expected probability of pretrial misconduct conditional on all variables observed by the judge be denoted by $\mathbb{E}[\alpha_i|r_i]$.

DEFINITION 2. We define judge $j$ as making racially biased prediction errors in risk against black defendants if $\tau_B^j(\mathbf{V}_i) > \tau_W^j(\mathbf{V}_i)$. Thus, judges making racially biased prediction errors systematically overestimate the true cost of release for black defendants relative to white defendants.

Following the taste-based model, bail judge $j$ will release defendant $i$ if and only if the benefit of pretrial release is greater than the perceived cost of release:

$$\mathbb{E}^j[\alpha_i|\mathbf{V}_i, r_i = r] = \mathbb{E}[\alpha_i|r_i = r] + \tau_r^j(\mathbf{V}_i) \leqslant t(\mathbf{V}_i). \quad (3)$$

Given the above setup, it is straightforward to show that the prediction error model can be reduced to the taste-based model of discrimination if we relabel $t(\mathbf{V}_i) - \tau_r^j(\mathbf{V}_i) = t_r^j(\mathbf{V}_i)$. As a result, we can generate identical empirical predictions using the prediction error and taste-based models.

Following this logic, our model of racially biased prediction errors in risk yields a similar outcome-based test for racial bias:

PROPOSITION 2. If judge $j$ systematically overestimates the true expected cost of release of black defendants relative to white defendants, then $\alpha_W^j > \alpha_B^j$. Thus, for judges who make racially biased prediction errors in risk, the true expected cost of release for the marginal white defendant is higher than the true expected cost of release for the marginal black defendant.

Parallel to Proposition 1, Proposition 2 predicts that marginal white and marginal black defendants should have the same probability of pretrial misconduct if judge $j$ does not systematically make prediction errors in risk that vary with race, but marginal white defendants should have a higher probability of misconduct if judge $j$ systematically overestimates the true expected cost of release of black defendants relative to white defendants.

Regardless of the underlying behavioral model that drives differences in judge behavior, the empirical predictions generated by these outcome-based tests are identical: if there is racial bias against black defendants, then marginal white defendants will have a higher probability of misconduct than marginal black defendants. In contrast, marginal white defendants will not have a higher probability of misconduct than marginal black defendants if observed racial disparities in bail setting are solely due to statistical discrimination.[4] Of course, finding higher misconduct rates for marginal white versus marginal black defendants does have a different interpretation depending on the underlying behavioral model. We return to this issue in Section V when we discuss more speculative evidence that allows us to differentiate between these two forms of racial bias.

### *II.C. Empirical Test of Racial Bias in Bail Setting*

The goal of our analysis is to empirically test for racial bias in bail setting using the rate of pretrial misconduct for white defendants and black defendants at the margin of release. Following the theory model, let the weighted average of pretrial misconduct rates for defendants of race $r$ at the margin for judge $j$, $\alpha_r^j$, for some weighting scheme, $w^j$, across all bail judges, $j = 1 \ldots J$, be

4. In contrast to the two models we consider in this section, models of (accurate) statistical discrimination suggest that blacks may be treated worse than observably identical whites if either (i) blacks are, on average, riskier given an identical signal of risk (e.g., Phelps 1972; Arrow 1973) or (ii) blacks have less precise signals of risk (e.g., Aigner and Cain 1977). In both types of (accurate) statistical discrimination models, however, judges use race to form accurate predictions of risk, both on average and at the margin of release. As a result, neither form of (accurate) statistical discrimination will lead to marginal white defendants having a higher probability of misconduct than marginal black defendants.

given by:

$$\alpha_r^{*,w} = \sum_{j=1}^{J} w^j \alpha_r^j$$

$$(4) \qquad = \sum_{j=1}^{J} w^j t_r^j,$$

where $w^j$ are non-negative weights that sum to 1 that will be discussed in further detail below. By definition, $\alpha_r^j = t_r^j$, where $t_r^j$ represents judge *j*'s threshold for release for defendants of race *r*. In our context, pretrial misconduct rates can be identified by the treatment effect of pretrial release on misconduct, as defendants detained before trial cannot, by definition, commit pretrial misconduct. Thus, $\alpha_r^{*,w}$ represents a weighted average of the treatment effects for defendants of race *r* at the margin of release across all judges.

Following this notation, the average level of racial bias among bail judges, $D^{*,w}$, for the weighting scheme $w^j$ is given by:

$$D^{*,w} = \sum_{j=1}^{J} w^j \left(t_W^j - t_B^j\right)$$

$$= \sum_{j=1}^{J} w^j t_W^j - \sum_{j=1}^{J} w^j t_B^j$$

$$(5) \qquad = \alpha_W^{*,w} - \alpha_B^{*,w}.$$

From equation (4), we can express $D^{*,w}$ as a weighted average across all judges of the difference in treatment effects for white and black defendants at the margin of release.

Standard OLS estimates will typically not recover unbiased estimates of the weighted average of racial bias, $D^{*,w}$, for two reasons. First, characteristics observable to the judge but not the econometrician may be correlated with pretrial release, resulting in omitted variable bias when estimating the treatment effects for black and white defendants. The second, and more important, reason OLS estimates will not recover unbiased estimates of racial bias is that the average treatment effect identified by OLS will not equal the treatment effect at the margin required by the outcome test unless one is willing to assume either identical risk distributions for black and white defendants or constant treatment effects across the entire distribution of both black and white defendants (e.g., Ayres 2002). Thus, even if the econometrician observes the full set of observables known to the bail judge, OLS estimates are still not sufficient to test for racial bias without restrictive assumptions.[5]

We therefore develop two complementary estimators for racial bias that use variation in the release tendencies of quasi-randomly assigned bail judges to identify differences in pretrial misconduct rates at the margin of release. Our first estimator uses the standard IV framework to identify the difference in LATEs for white and black defendants near the margin of release. Our IV estimator allows us to estimate a weighted average of racial bias across bail judges with relatively few auxiliary assumptions, but with the caveats that we cannot estimate judge-specific treatment effects and the weighting scheme underlying the IV estimator may not be policy relevant. In contrast, our second estimator uses the MTE framework developed by Heckman and Vytlacil (1999, 2005) to estimate judge-specific treatment effects for white and black defendants at the margin of release, allowing us to choose our own weighting scheme when calculating racial bias in our data. In practice, we choose to impose equal weights on each judge—a parameter with a clear economic interpretation—meaning that our MTE estimates can be interpreted as the average level of bias across judges.

*1. Setup.* We first briefly review the baseline assumptions that underlie our IV and MTE estimators. Online Appendix B provides empirical tests of each assumption.

Let $Z_i$ be a scalar measure of the assigned judge's propensity for pretrial release for defendant-case $i$ that takes on values ordered $\{z_0, ..., z_J\}$, where $J + 1$ is the number of total judges in the bail system. For example, a value of $z_j = 0.5$ indicates that judge $j$ releases 50% of all defendants. In practice, we construct $Z_i$ using a standard leave-out procedure that captures the pretrial release tendencies of judges. We calculate $Z_i$ separately for white and black defendants to relax the standard monotonicity

5. In Online Appendix C, we use a series of simple graphical examples to illustrate how a standard OLS estimator suffers from inframarginality bias whenever there are differences in the risk distributions of black and white defendants. We then use a simple two-judge example to illustrate how a judge IV estimator can alleviate the inframarginality bias.

assumption that the judge ordering produced by the scalar $Z_i$ is the same for both white and black defendants, implicitly allowing judges to exhibit different levels of racial bias.

Following Imbens and Angrist (1994), a race-specific estimator using $Z_i$ as an instrumental variable for pretrial release is valid and well defined under the following three assumptions:

ASSUMPTION 1. (EXISTENCE) $Cov(Released_i, Z_i) \neq 0.$,
ASSUMPTION 2. (EXCLUSION) $Cov(Z_i, \mathbf{v}_i) = 0$,
ASSUMPTION 3. (MONOTONICITY) $Released_i(z_j) - Released_i(z_{j-1}) \geqslant 0$,

where $\mathbf{v}_i = \mathbf{U}_i + \varepsilon_i$ consists of characteristics unobserved by the econometrician but observed by the judge, $\mathbf{U}_i$, and idiosyncratic variation unobserved by both the econometrician and judge, $\varepsilon_i$. Assumption 1 ensures that there is a first-stage relationship between our instrument $Z_i$ and the probability of pretrial release $Released_i$. Assumption 2 ensures that our instrument $Z_i$ is orthogonal to characteristics unobserved by the econometrician, $\mathbf{v}_i$. In other words, Assumption 2 assumes that the assigned judge only affects pretrial misconduct through the channel of pretrial release. Assumption 3 implies that for a given case, any defendant released by a strict judge would also be released by a more lenient judge, and any defendant detained by a lenient judge would also be detained by a more strict judge.

2. *IV Estimator for Racial Bias.* Given Assumptions 1–3, we formally define our IV estimator for racial bias, provide conditions for consistency, and discuss the interpretation of the IV weights.

*a. Defining our IV estimator:* Let the true IV-weighted level of racial bias, $D^{*,IV}$ be defined as:

$$D^{*,IV} = \sum_{j=1}^{J} w^j \left(t_W^j - t_B^j\right)$$
$$= \sum_{j=1}^{J} \lambda^j \left(t_W^j - t_B^j\right), \tag{6}$$

where $w^j = \lambda^j$, the standard IV weights defined in Imbens and Angrist (1994).

Let our IV estimator that uses judge leniency as an instrumental variable for pretrial release be defined as:

$$\begin{aligned} D^{IV} &= \alpha_W^{IV} - \alpha_B^{IV} \\ &= \sum_{j=1}^{J} \lambda_W^j \alpha_W^{j,j-1} - \sum_{j=1}^{J} \lambda_B^j \alpha_B^{j,j-1}, \end{aligned} \tag{7}$$

where $\lambda_r^j$ are again the standard IV weights and each pairwise treatment effect $\alpha_r^{j,j-1}$ captures the treatment effects of compliers within each $j, j-1$ pair. As we discuss in Online Appendix B, compliers for judge $j$ and $j-1$ are individuals such that $\alpha_r^{j,j-1} \in (t_r^{j-1}, t_r^j]$.

*b. Consistency of our IV estimator:* Our IV estimator $D^{IV}$ provides a consistent estimate of $D^{*,IV}$ under two conditions: (i) $Z_i$ is continuous and (ii) $\lambda_r^j$ is constant by race. See Online Appendix B for proofs of consistency. The first condition is that our judge leniency measure $Z_i$ is continuously distributed over some interval $[\underline{z}, \bar{z}]$. Intuitively, each defendant becomes marginal to a judge as the distance between any two judge leniency measures converges to zero, that is, the instrument becomes more continuous. Under this first condition, each race-specific IV estimate, $\alpha_r^{IV}$, approaches a weighted average of treatment effects for defendants at the margin of release. In Online Appendix B, we discuss the potential inframarginality bias that may result if our instrument is discrete, as is the case in our data. In practice, we find that the maximum inframarginality bias of our IV estimator $D^{IV}$ from $D^{*,IV}$ is 1.1 percentage points in our setting. The second condition for consistency is that the weights on the pairwise LATEs must be equal across race. This equal weights assumption ensures that the race-specific IV estimates from equation (7), $\alpha_W^{IV}$ and $\alpha_B^{IV}$, provide the same weighted averages of $\alpha_W^{j,j-1}$ and $\alpha_B^{j,j-1}$. In Online Appendix B, we empirically test whether the IV weights $\lambda_r^j$ are constant by race in our data, finding that the distributions of black and white IV weights are visually indistinguishable from each other and that the IV weights for each judge-by-year cell are highly correlated across race.

*c. Interpretation of the IV weights:* As discussed already, our IV estimator yields a weighted average of racial bias across bail judges, where the weights $\lambda^j$ are the standard IV weights defined in Imbens and Angrist (1994). To better understand the economic interpretation of an IV-weighted estimate of racial bias, Online Appendix B investigates the relationship between our IV weights and judge-by-year characteristics. We find that our IV weights are positively correlated with both the number of cases in a judge-by-year cell and judge-by-year specific estimates of racial bias, implying that the IV-weighted estimate of racial bias may be larger than an equal-weighted estimate of racial bias. We return to this later below when discussing the difference between our IV and MTE estimates.

*3. MTE Estimator for Racial Bias.* Finally, we formally define our MTE estimator of racial bias and provide conditions for consistency. Without loss of generality, we focus on an estimate of racial bias that places equal weight on each bail judge.

*a. Defining the MTE estimator:* Let the true equal-weighted MTE estimate of racial bias, $D^{*,MTE}$ be defined as:

$$D^{*,MTE} = \sum_{j=1}^{J} w^j \left(t_W^j - t_B^j\right)$$

$$(8) \qquad = \sum_{j=1}^{J} \frac{1}{J} \left(t_W^j - t_B^j\right),$$

where $w^j = \frac{1}{J}$, such that $D^{*,MTE}$ can be interpreted as the average level of racial bias across judges.

Let our equal-weighted MTE estimator of racial bias, $D^{MTE}$, be defined as:

$$(9) \qquad D^{MTE} = \sum_{j=1}^{J} \frac{1}{J} \left(MTE_W(p_W^j) - MTE_B(p_B^j)\right),$$

where $p_r^j$ is the probability that judge $j$ releases a defendant of race $r$ calculated using only the variation in pretrial release due to our judge leniency measure $Z_i$ (i.e., judge $j$'s race-specific propensity score). $MTE_r(p_r^j)$ is the estimated MTE at the propensity score for judge $j$ calculated separately for each defendant race $r$. In Online Appendix B, we show that $MTE_r(p_r^j) = \alpha_r^j$ when we map each judge $j$'s release decision under our theory model to the MTE framework developed by Heckman and Vytlacil (2005).

*b. Consistency of our MTE estimator:* Our MTE estimator $D^{MTE}$ provides a consistent estimate of $D^{*,MTE}$ if the race-specific

MTEs are identified over the entire support of the propensity score calculated using variation in $Z_i$. If $Z_i$ is continuous, the local instrumental variables (LIVs) estimand provides a consistent estimate of the MTE over the support of the propensity score with no additional assumptions (Heckman and Vytlacil 2005; Cornelissen et al. 2016). With a discrete instrument, however, our MTE estimator is only consistent under additional functional form restrictions that allow us to interpolate the MTEs between the values of the propensity score we observe in the data. Following Heckman and Vytlacil (2005) and Doyle (2007), we use a local polynomial function and information from the observed values of the propensity score to estimate the MTE curve over the full support of the propensity score. In Online Appendix B, we provide support for our functional form assumption by showing that we can recover each nonparametric LATE using the appropriately weighted MTE up to sampling error (Cornelissen et al. 2016).

### *II.D. Discussion and Extensions*

In this section, we discuss the interpretation of our test of racial bias under different assumptions and extensions.

*1. Racial Differences in Arrest Probability.* Our test for racial bias assumes that any measurement error in the outcome is uncorrelated with race. This assumption would be violated if, for example, judges minimize new crime, not just new arrests, and police are more likely to rearrest black defendants conditional on having committed a new crime (Fryer 2016; Goncalves and Mello 2018). In this scenario, we overestimate the probability of pretrial misconduct for black versus white defendants at the margin and, as a result, underestimate the true amount of racial bias in bail setting. It is therefore possible that our estimates reflect a lower bound on the true amount of racial bias among bail judges to the extent that judges minimize new crime.[6]

6. A related concern is that bail judges may be influenced by other court actors (e.g., prosecutors) when making decisions, such that racial bias stems from judges not overriding racially biased bail recommendations. However, we find substantial variation in pretrial release tendencies across judges, inconsistent with the idea that judges "rubber-stamp" bail recommendations. We also find that racial bias decreases with judge experience, inconsistent with other court actors driving the racial bias unless experience affects the probability of overriding biased recommendations.

*2. Omitted Objectives for Release.* We also assume that judges do not consider other objectives or outcomes, or what Kleinberg et al. (2018) refer to as "omitted payoff bias." We will have this kind of omitted payoff bias if, for example, bail judges consider how pretrial detention impacts a defendant's employment status and this outcome is correlated with race.

We explore the empirical relevance of omitted payoff bias in several ways. First, as will be discussed shortly, we find that our estimates are nearly identical if we measure pretrial misconduct using only rearrests versus using rearrests or failures to appear. These results are also consistent with Kleinberg et al. (2018), who find similar evidence of prediction errors using rearrests or failures to appear. Second, as will be discussed later, we also find similar estimates when we measure pretrial misconduct using crime-specific rearrest rates to address the concern that judges may be most concerned about reducing violent crimes. Third, we note that Dobbie, Goldin, and Yang (2018) find that white defendants at the margin of release are no more likely to be employed in the formal labor market up to four years after the bail hearing compared with black defendants at the margin of release. This goes against the idea that judges may be trading off minimizing pretrial misconduct with maximizing employment. Finally, we find that racial bias against black defendants is larger for part-time and inexperienced judges compared with full-time and experienced judges. There are few conceivable stories where omitted payoffs differ by judge experience. Taken together, we therefore believe that any omitted payoff bias is likely to be small in practice.

*3. Racial Differences in Ability to Pay Monetary Bail.* In our model, we abstract away from the fact that bail judges may set different levels of monetary bail that, by law, should take into account a defendant's ability to pay. Extending our model to incorporate these institutional details means that racial bias could also be driven by judges systematically overpredicting the relative ability of black defendants to pay monetary bail at the margin.

We explore the empirical relevance of racial differences in ability to pay monetary bail in two ways. First, we test whether the assignment of nonmonetary bail (i.e., either ROR or nonmonetary conditions) versus monetary bail has a larger impact on the probability of release for marginal black defendants,[7] which could occur if judges systematically overpredict black defendants' ability to pay monetary bail at the margin. To test this idea, Online Appendix Table A1, Panel A presents two-stage least squares estimates of the impact of nonmonetary versus monetary bail on pretrial release using a leave-out measure based on nonmonetary bail decisions as an instrumental variable. We find that the assignment of nonmonetary bail versus monetary bail has a nearly identical impact on the pretrial release rates for marginal black defendants and marginal white defendants. Second, we directly estimate racial bias in the setting of nonmonetary versus monetary bail to incorporate any additional bias stemming from this margin. We estimate these effects using a two-stage least squares regression of pretrial misconduct on nonmonetary bail, again using a leave-out measure based on nonmonetary bail decisions as an instrumental variable. Online Appendix Table A1, Panel B presents these estimates. We find similar estimates of racial bias when focusing on the nonmonetary versus monetary bail decision when we scale the estimated treatment effects by the "first-stage" effect of nonmonetary bail on pretrial release from Panel A.

*4. Judge Preferences for Nonrace Characteristics.* Another extension to our model concerns two distinct views about what constitutes racial bias. The first is that racial bias includes not only any bias due to phenotype, but also bias due to seemingly nonrace factors that are correlated with, if not driven by, race. For example, judges could be biased against defendants charged with drug offenses because blacks are more likely to be charged with these types of crimes. Our preferred estimates are consistent with this broader view of racial bias, measuring the disparate treatment of black and white defendants at the margin for <u>all</u> reasons unrelated to true risk of pretrial misconduct, including reasons related to seemingly nonrace characteristics such as crime type.

A second view is that racial bias is disparate treatment due to phenotype alone, not other correlated factors such as crime type. In Online Appendix B, we show that it is possible to test for this narrower form of racial bias using a reweighting procedure

7. Dobbie, Goldin, and Yang (2018) show that the assignment of ROR and nonmonetary conditions have a statistically identical impact on defendant outcomes, including pretrial misconduct. We therefore combine ROR and nonmonetary conditions into a single category in our analysis.

that weights the distribution of observables of blacks to match observables of whites in the spirit of DiNardo, Fortin, and Lemieux (1996) and Angrist and Fernández-Val (2013). This narrower test for racial bias relies on the assumption that judge preferences vary only by observable characteristics $\mathbf{X}_i$, that is, $t_r^j(\mathbf{V}_i) = t_r^j(\mathbf{X}_i)$. We find nearly identical estimates of racial bias using this reweighting procedure, suggesting that judge preferences over nonrace characteristics are a relatively unimportant contributor to our findings. We discuss these results in robustness checks below.

## III. Data and Instrument Construction

This section summarizes the most relevant information regarding our administrative court data from Philadelphia and Miami-Dade, describes the construction of our judge leniency measure, and provides support for the baseline assumptions required for our IV and MTE estimators of racial bias. Further details on the cleaning and coding of variables are contained in Online Appendix D.

### *III.A. Data Sources and Descriptive Statistics*

Philadelphia court records are available for all defendants arrested and charged between 2010 and 2014 and Miami-Dade court records are available for all defendants arrested and charged between 2006 and 2014. For both jurisdictions, the court data contain information on defendant's name, gender, race, date of birth, and zip code of residence. Because our ethnicity identifier does not distinguish between non-Hispanic white and Hispanic white, we match the surnames in our data set to census genealogical records of surnames. If the probability a given surname is Hispanic is greater than 70%, we label this individual as Hispanic. In our main analysis, we include all defendants and compare outcomes for marginal black and white (Hispanic and non-Hispanic) defendants. In robustness checks, we present results comparing marginal black and non-Hispanic white defendants.[8]

8. Online Appendix Table A2 presents results for marginal Hispanic white defendants compared to non-Hispanic white defendants. Perhaps in some part because of measurement error in our coding of Hispanic ethnicity, we find no evidence of racial bias against Hispanics.

The court data also include information on the original arrest charge, the filing charge, and the final disposition charge. We also have information on the severity of each charge based on state-specific offense grades, the outcome for each charge, and the punishment for each guilty disposition. Finally, the case-level data include information on attorney type, arrest date, and the date of and judge presiding over each court appearance from arraignment to sentencing. Importantly, the case-level data also include information on bail type, bail amount when monetary bail is set, and whether bail was met. Because the data contain defendant identifiers, we can measure whether a defendant was subsequently arrested for a new crime before case disposition. In Philadelphia, we also observe whether a defendant failed to appear for a required court appearance.

We make three restrictions to the court data to isolate cases that are quasi-randomly assigned to judges. First, we drop a small set of cases with missing bail judge information or missing race information. Second, we drop the 30% of defendants in Miami-Dade who never have a bail hearing because they post bail immediately following arrest; later we show that the characteristics of defendants who have a bail hearing are uncorrelated with our judge leniency measure. Third, we drop all weekday cases in Miami-Dade because, as explained in Online Appendix E, bail judges in Miami-Dade are assigned on a quasi-random basis only on the weekends. The final sample contains 162,836 cases from 93,914 unique defendants in Philadelphia and 93,417 cases from 65,944 unique defendants in Miami-Dade.

Table I reports summary statistics for our estimation sample separately by race and pretrial release status. On average, black defendants are 3.6 percentage points more likely to be assigned monetary bail compared with white defendants and receive bail amounts that are $7,281 greater than white defendants (including zeros). Conversely, black defendants are 2.0 percentage points and 1.6 percentage points less likely to be released on their own recognizance or to be assigned nonmonetary conditions compared to white defendants, respectively. As a result, black defendants are 2.4 percentage points more likely to be detained pretrial compared to white defendants.

Compared to white defendants, released black defendants are also 1.9 percentage points more likely to be rearrested for a new crime before case disposition, our preferred measure of pretrial misconduct. Released black defendants are also 0.9 percentage

TABLE I
DESCRIPTIVE STATISTICS

| | All defendants | | White | | Black | |
|---|---|---|---|---|---|---|
| | Released (1) | Detained (2) | Released (3) | Detained (4) | Released (5) | Detained (6) |
| Panel A: Bail type | | | | | | |
| Release on recognizance | 0.258 | 0.000 | 0.269 | 0.000 | 0.249 | 0.000 |
| Nonmonetary bail w/conditions | 0.195 | 0.030 | 0.203 | 0.033 | 0.189 | 0.028 |
| Monetary bail | 0.547 | 0.970 | 0.527 | 0.967 | 0.562 | 0.972 |
| Bail amount ($1,000s) | 13.235 | 35.286 | 11.957 | 24.782 | 14.180 | 42.227 |
| Panel B: Defendant characteristics | | | | | | |
| Male | 0.811 | 0.893 | 0.796 | 0.890 | 0.822 | 0.895 |
| Age at bail decision | 33.911 | 35.092 | 34.070 | 36.296 | 33.794 | 34.296 |
| Prior offense in past year | 0.287 | 0.466 | 0.272 | 0.464 | 0.299 | 0.466 |
| Arrested on bail in past year | 0.185 | 0.262 | 0.181 | 0.256 | 0.188 | 0.266 |
| Failed to appear in court in past year | 0.071 | 0.057 | 0.070 | 0.054 | 0.071 | 0.059 |
| Panel C: Charge characteristics | | | | | | |
| Number of offenses | 2.722 | 3.162 | 2.544 | 2.587 | 2.854 | 3.541 |
| Felony offense | 0.482 | 0.538 | 0.450 | 0.473 | 0.506 | 0.581 |
| Misdemeanor only | 0.518 | 0.462 | 0.550 | 0.527 | 0.494 | 0.419 |
| Any drug offense | 0.390 | 0.260 | 0.373 | 0.244 | 0.403 | 0.271 |
| Any DUI offense | 0.084 | 0.007 | 0.091 | 0.007 | 0.079 | 0.007 |
| Any violent offense | 0.310 | 0.331 | 0.288 | 0.241 | 0.326 | 0.390 |
| Any property offense | 0.238 | 0.387 | 0.237 | 0.406 | 0.239 | 0.376 |
| Panel D: Outcomes | | | | | | |
| Rearrest prior to disposition | 0.237 | 0.042 | 0.226 | 0.037 | 0.245 | 0.045 |
| Rearrest drug crime | 0.111 | 0.006 | 0.106 | 0.005 | 0.115 | 0.006 |
| Rearrest property crime | 0.086 | 0.022 | 0.082 | 0.022 | 0.089 | 0.022 |
| Rearrest violent crime | 0.078 | 0.021 | 0.061 | 0.013 | 0.091 | 0.026 |
| Failure to appear in court (Phl only) | 0.258 | 0.006 | 0.250 | 0.006 | 0.264 | 0.007 |
| Failure to appear in court or rearrest | 0.348 | 0.044 | 0.325 | 0.039 | 0.366 | 0.048 |
| Observations | 178,765 | 77,488 | 76,015 | 30,831 | 102,750 | 46,657 |

*Notes.* This table reports descriptive statistics for the sample of defendants from Philadelphia and Miami-Dade counties. The sample consists of bail hearings that were quasi-randomly assigned from Philadelphia between 2010 and 2014 and from Miami-Dade between 2006 and 2014, as described in the text. Information on race, gender, age, and criminal outcomes is derived from court records. Released is defined as being released at any point before trial. Detained is defined as never being released before trial. Bail amount (in $1,000s) includes zeros. Failure to appear in court is defined only in Philadelphia. See Online Appendix D for additional details on the sample and variable construction.

points, 0.7 percentage points, and 3.0 percentage points more likely to be rearrested for a drug, property, and violent crime, respectively. In Philadelphia, released black defendants are 1.4 percentage points more likely to fail to appear in court compared to white defendants. Defining pretrial misconduct as either failure to appear or rearrest in Philadelphia, and only rearrest in Miami, released black defendants are 4.1 percentage points more likely to commit any form of pretrial misconduct compared to white defendants. We also find that approximately 4% of detained defendants are rearrested for a new crime prior to case disposition—an outcome that should be impossible. We show that our results are unaffected by dropping these cases in robustness checks.[9]

### *III.B. Construction of the Instrumental Variable*

We estimate the causal impact of pretrial release for the marginal defendant using a measure of the tendency of a quasi-randomly assigned bail judge to release a defendant as an instrument for release. In both Philadelphia and Miami-Dade, there are multiple bail judges serving at each point in time, allowing us to use variation in bail setting across judges. Both jurisdictions assign cases to bail judges in a quasi-random fashion to balance caseloads: Philadelphia uses a rotation system where three judges work together in five-day shifts, with one judge working an eight-hour morning shift (7:30 AM–3:30 PM), another judge working the eight-hour afternoon shift (3:30 PM–11:30 PM), and the final judge working the eight-hour evening shift (11:30 PM–7:30 AM). Similarly, bail judges in Miami-Dade rotate through the weekend felony and misdemeanor bail hearings. See Online Appendix E for additional details.

Following Dobbie, Goldin, and Yang (2018), we construct our instrument using a residualized, leave-out judge leniency measure that accounts for the case assignment processes in Philadelphia and Miami-Dade. To construct this residualized judge leniency measure, we first regress pretrial release decisions on an exhaustive set of court-by-time fixed effects, the level at which defendants are quasi-randomly assigned to judges. In Miami, these court-by-time fixed effects include court-by-bail year-by-bail day of week fixed effects and court-by-bail month-by-bail day of week fixed effects. In Philadelphia, we add bail-day of week-by-bail shift fixed effects. We then use the residuals from this regression to calculate the leave-out mean judge release rate for each defendant. We calculate our instrument across all case types, but allow the instrument to vary across years and defendant race.[10]

9. To understand how these miscodings impact the interpretation of results, we follow Dahl, Kostøl, and Mogstad (2014) in calculating rearrest rates for marginally detained defendants. These estimates imply that the rearrest rate for marginally released defendants is approximately 2.0 to 3.0 percentage points higher than our estimated treatment effects.

10. Our leave-out procedure is essentially a reduced-form version of jackknife IV, with the leave-out leniency measure for judge $j$ being algebraically equivalent

Figure I presents the distribution of our residualized judge leniency measure for pretrial release at the judge-by-year level for all defendants, white defendants, and black defendants. Our sample includes 7 total bail judges in Philadelphia and 170 total bail judges in Miami-Dade. In Philadelphia, the average number of cases per judge is 23,262 during the sample period of 2010–2014, with the typical judge-by-year cell including 5,253 cases. In Miami-Dade, the average number of cases per judge is 550 during the sample period of 2006–2014, with the typical judge-by-year cell including 179 cases. Controlling for the exhaustive set of court-by-time fixed effects, the judge release measure ranges from −0.283 to 0.253 with a standard deviation of 0.040. In other words, moving from the least to most lenient judge increases the probability of pretrial release by 53.6 percentage points, a 76.8% change from the mean release rate of 69.8 percentage points.

### III.C. Instrument Validity

*1. Existence of First Stage.* To examine the first-stage relationship between judge leniency ($Z_{itj}$) and whether a defendant is released pretrial ($Released_{itj}$), we estimate the following equation for defendant-case $i$, assigned to judge $j$ at time $t$ using a linear probability model, estimated separately for white and black defendants:

$$Released_{itj} = \gamma_W Z_{itj} + \pi_W \mathbf{X}_{it} + v_{itj}, \tag{10}$$

$$Released_{itj} = \gamma_B Z_{itj} + \pi_B \mathbf{X}_{it} + v_{itj}, \tag{11}$$

where the vector $\mathbf{X}_{it}$ includes court-by-time fixed effects. The error term $v_{itj}$ is composed of characteristics unobserved by the econometrician but observed by the judge, as well as idiosyncratic variation unobserved to both the judge and econometrician. As described previously, $Z_{itj}$ are leave-out (jackknife) measures of judge leniency that are allowed to vary across years and defendant race. Robust standard errors are two-way clustered at the individual and judge-by-shift level.

to judge $j$'s fixed effect from a leave-out regression of residualized pretrial release on the full set of judge fixed effects and court-by-time fixed effects. In unreported results, jackknife IV and LIML estimates using the full set of judge fixed effects as instruments yield similar results.

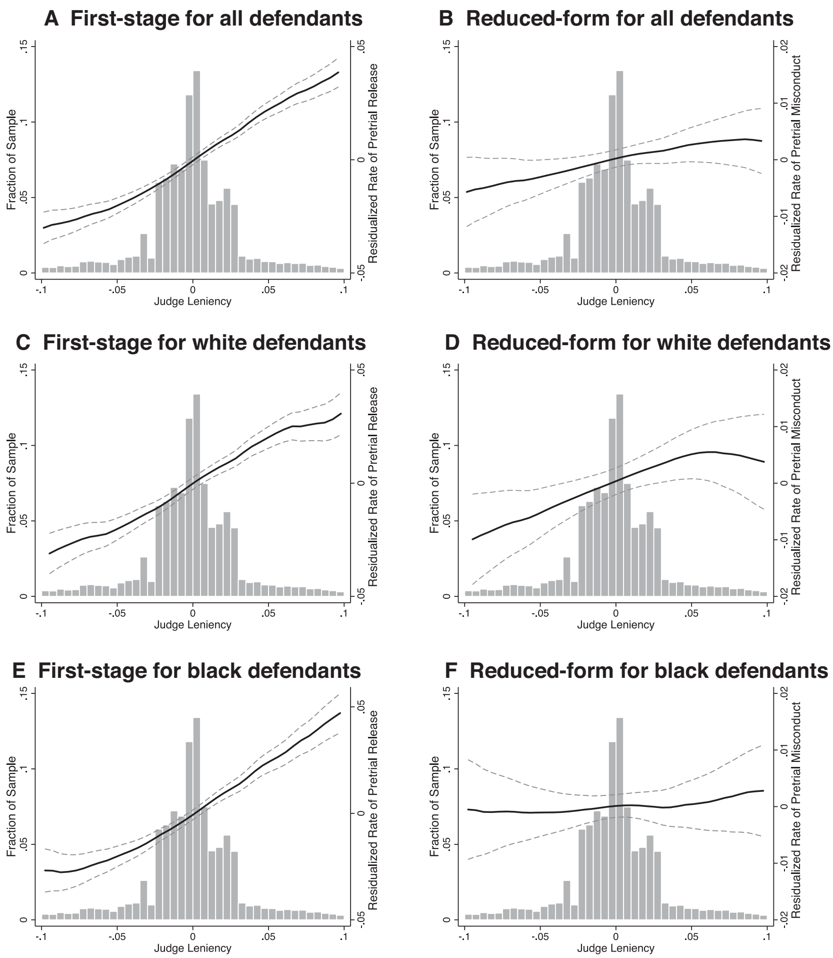


FIGURE I

First-Stage and Reduced-Form Results

These figures report the first-stage and reduced-form relationships between defendant outcomes and judge leniency. The regressions are estimated on the sample as described in the notes to Table I. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year, constructed separately by defendant race, following the procedure described in Section III.B. In the first-stage regressions, the solid line is a local linear regression of residualized pretrial release on judge leniency. In the reduced-form regressions, the solid line is a local linear regression of residualized pretrial misconduct on judge leniency. Pretrial release and pretrial misconduct are residualized using court-by-time fixed effects in the full sample. Standard errors are two-way clustered at the individual and judge-by-shift level.

TABLE II
FIRST-STAGE RESULTS

| | All defendants | | White | | Black | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pretrial Release | 0.405*** | 0.389*** | 0.373*** | 0.360*** | 0.434*** | 0.415*** |
| | (0.027) | (0.025) | (0.036) | (0.032) | (0.036) | (0.033) |
| | [0.698] | [0.698] | [0.711] | [0.711] | [0.688] | [0.688] |
| Court x year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline controls | No | Yes | No | Yes | No | Yes |
| Observations | 256,253 | 256,253 | 106,846 | 106,846 | 149,407 | 149,407 |

*Notes.* This table reports the first-stage relationship between pretrial release and judge leniency. The regressions are estimated on the sample as described in the notes to Table I. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year, constructed separately by defendant race, following the procedure described in Section III.B. All regressions include court-by-time fixed effects. Baseline controls include race, gender, age, whether the defendant had a prior offense in the past year, whether the defendant had a prior history of pretrial crime in the past year, whether the defendant had a prior history of failure to appear in the past year, the number of charged offenses, indicators for crime type (drug, DUI, property, violent, and other), crime severity (felony and misdemeanor), and indicators for any missing controls. The sample mean of the dependent variable is reported in brackets. Robust standard errors two-way clustered at the individual and judge-by-shift level are reported in parentheses. ***Significant at 1% level.

Figure I provides graphical representations of the first-stage relationship, for all defendants and separately by race, between our residualized measure of judge leniency and the residualized probability of pretrial release that accounts for our exhaustive set of court-by-time fixed effects. The graphs are a flexible analog to equations (10) and (11), where we plot a local linear regression of residualized pretrial release against judge leniency. The individual rate of residualized pretrial release is monotonically increasing in our leniency measure for both races.

Table II presents formal first-stage results from equations (10) and (11) for all defendants, white defendants, and black defendants. Columns (1), (3), and (5) begin by reporting results with only court-by-time fixed effects. Columns (2), (4), and (6) add our baseline crime and defendant controls: race, gender, age, whether the defendant had a prior offense in the past year, whether the defendant had a prior history of pretrial crime in the past year, whether the defendant had a prior history of failure to appear in the past year, the number of charged offenses, indicators for crime type (drug, DUI, property, violent, or other), crime severity (felony or misdemeanor), and indicators for any missing characteristics.

We find that our residualized judge instrument is highly predictive of whether a defendant is released pretrial. Our results show that a defendant assigned to a bail judge that is 10 percentage points more likely to release a defendant pretrial is 3.89 percentage points more likely to be released pretrial. Judge leniency is also highly predictive of pretrial release for both white and black defendants, with the first-stage coefficient being 0.360 and 0.415, respectively.[11]

*2. Exclusion Restriction.* Table III verifies that assignment of cases to bail judges is random after we condition on our court-by-time fixed effects. Columns (1), (3), and (5) of Table III use a linear probability model to test whether case and defendant characteristics are predictive of pretrial release. These estimates capture both differences in the bail conditions set by bail judges and differences in defendants' ability to meet the bail conditions. We control for court-by-time fixed effects and two-way cluster standard errors at the individual and judge-by-shift level. For example, we find that black male defendants are 10.4 percentage points less likely to be released pretrial compared to similar female defendants, whereas white male defendants are 8.6 percentage points less likely to be released pretrial compared to similar female defendants. White defendants with at least one prior offense in the past year are 16.8 percentage points less likely to be released compared with similar defendants with no prior offenses, and black defendants with at least one prior offense in the past year are 13.4 percentage points less likely to be released compared with similar defendants with no prior offenses. Columns (2), (4), and (6) assess whether these same case and defendant characteristics are predictive of our judge leniency measure using an identical specification. We find that judges with differing leniencies are assigned cases with very similar defendants.

Even with random assignment, the exclusion restriction could be violated if bail judge assignment affects the probability of pretrial misconduct through channels other than pretrial release. The

11. Consistent with prior work using judge leniency as an instrumental variable (e.g., Bhuller et al. 2016), the probability of being released pretrial does not increase one-for-one with our measure of judge leniency, probably because of attenuation bias due to sampling variation in the construction of our instrument. Consistent with this explanation, we find first-stage coefficients ranging from 0.6 to 0.7 in Monte Carlo simulations when judge tendencies are fixed over the course of the year, and 0.2 to 0.4 when judge tendencies are allowed to change within each year. It is important to note that attenuation bias due to sampling variation in our leniency measure does not bias our estimates because it affects both the first-stage and reduced-form proportionally.

TABLE III
TEST OF RANDOMIZATION

| | All | | White | | Black | |
|---|---|---|---|---|---|---|
| | Pretrial release (1) | Judge leniency (2) | Pretrial release (3) | Judge leniency (4) | Pretrial release (5) | Judge leniency (6) |
| Male | −0.09424*** | −0.00005 | −0.08593*** | 0.00004 | −0.10379*** | −0.00014 |
| | (0.00235) | (0.00024) | (0.00325) | (0.00038) | (0.00323) | (0.00031) |
| Age at bail decision | −0.01725*** | −0.00009 | −0.02250*** | −0.00015 | −0.01512*** | −0.00005 |
| | (0.00086) | (0.00009) | (0.00127) | (0.00016) | (0.00104) | (0.00010) |
| Prior offense in past year | −0.14922*** | −0.00017 | −0.16817*** | 0.00030 | −0.13411*** | −0.00044 |
| | (0.00287) | (0.00028) | (0.00445) | (0.00046) | (0.00362) | (0.00036) |
| Arrested on bail in past year | 0.01066*** | 0.00004 | 0.01967*** | −0.00166*** | 0.00495 | 0.00116*** |
| | (0.00355) | (0.00034) | (0.00552) | (0.00057) | (0.00439) | (0.00042) |
| Failed to appear in court in past year | 0.03318*** | 0.00012 | 0.03253*** | 0.00104** | 0.03245*** | −0.00047 |
| | (0.00413) | (0.00025) | (0.00631) | (0.00043) | (0.00529) | (0.00031) |
| Number of offenses | −0.02090*** | −0.00001 | −0.01829*** | −0.00002 | −0.02131*** | 0.00000 |
| | (0.00053) | (0.00003) | (0.00085) | (0.00006) | (0.00063) | (0.00004) |
| Felony offense | −0.17618*** | −0.00003 | −0.18817*** | −0.00014 | −0.16948*** | 0.00004 |
| | (0.00257) | (0.00012) | (0.00397) | (0.00020) | (0.00323) | (0.00014) |
| Any drug offense | 0.03514*** | −0.00038 | 0.02558*** | −0.00002 | 0.04069*** | −0.00063* |
| | (0.00258) | (0.00026) | (0.00357) | (0.00039) | (0.00332) | (0.00032) |
| Any property offense | −0.04272*** | −0.00013 | −0.05560*** | 0.00009 | −0.03188*** | −0.00029 |
| | (0.00285) | (0.00026) | (0.00388) | (0.00041) | (0.00354) | (0.00033) |
| Any violent offense | 0.01640*** | 0.00028 | 0.07515*** | 0.00033 | −0.02443*** | 0.00023 |
| | (0.00389) | (0.00025) | (0.00497) | (0.00045) | (0.00429) | (0.00029) |
| Joint $F$-test | [.00000] | [.60067] | [.00000] | [.21951] | [.00000] | [.08289] |
| Observations | 256,253 | 256,253 | 106,846 | 106,846 | 149,407 | 149,407 |

*Notes.* This table reports reduced-form results testing the random assignment of cases to bail judges. The regressions are estimated on the sample as described in the notes to Table I. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year, constructed separately by defendant race, following the procedure described in Section III.B. Columns (1), (3), and (5) report estimates from an OLS regression of pretrial release on the variables listed and court-by-time fixed effects. Columns (2), (4), and (6) report estimates from an OLS regression of judge leniency on the variables listed and court-by-time fixed effects. The $p$-value reported at the bottom of the columns is for an $F$-test of the joint significance of the variables listed in the rows. Robust standard errors two-way clustered at the individual and the judge-by-shift level are reported in parentheses. ***Significant at 1% level, **significant at 5% level, *significant at 10% level.

assumption that judges only systematically affect defendant outcomes through pretrial release is fundamentally untestable, and our estimates should be interpreted with this potential caveat in mind. However, we argue that the exclusion restriction assumption is reasonable in our setting. Bail judges exclusively handle one decision, limiting the potential channels through which they could affect defendants. In addition, we are specifically interested in short-term outcomes (pretrial misconduct) that occur prior to disposition, further limiting the role of alternative channels that could affect longer-term outcomes. Finally, Dobbie, Goldin, and Yang (2018) find that there are no independent effects of the money bail amount or nonmonetary bail conditions on defendant outcomes, and that bail judge assignment is uncorrelated with the assignment of public defenders and trial judges.

*3. Monotonicity.* The final condition needed for our IV and MTE estimators is that the impact of judge assignment on the probability of pretrial release is monotonic across defendants of the same race. In our setting, the monotonicity assumption requires that individuals released by a strict judge would also be released by a more lenient judge, and that individuals detained by a lenient judge would also be detained by a stricter judge. The monotonicity assumption is required to identify and interpret our IV estimator as a well-defined LATE and to estimate marginal treatment effects using the standard LIV approach. See Angrist, Imbens, and Rubin (1996) and Heckman and Vytlacil (2005) for additional details. Importantly, we allow our judge leniency measure to vary by defendant race to allow for the possibility that the degree of racial bias varies across judges. In practice, we observe that judge behavior is only imperfectly monotonic with respect to race (see Online Appendix Figure A1), with a regression of the ranking of each judge's leniency measure for whites on the ranking of each judge's leniency measure for blacks yielding a coefficient equal to 0.827 (std. err. = 0.010). The nonmonotonic behavior we observe with respect to race is driven by approximately 17.9% of judges who hear about 8.2% of all cases. Consistent with the monotonicity assumption within race, we find a strong first-stage relationship across various case and defendant types (see Online Appendix Table A3).[12]

12. One specific concern is that lenient judges may be better at using unobservable information to predict the risk of pretrial misconduct, as this would result in some high-risk defendants being released by only strict judges. Following

## IV. Results

In this section, we present our main results applying our empirical test for racial bias. We then show the robustness of our results to alternative specifications, before comparing the results from our empirical test with the alternative outcome-based tests developed by Knowles, Persico, and Todd (2001) and Anwar and Fang (2006).

### IV.A. Empirical Test for Racial Bias

*1. IV Estimates.* We begin by presenting IV estimates of racial bias that rely on relatively few auxiliary assumptions, but with the caveat that the weighting scheme underlying the estimator may not always be policy relevant. We estimate these IV results using the following two-stage least squares specifications for defendant-case $i$ assigned to judge $j$ at time $t$, estimated separately for white and black defendants:

$$Y_{itj} = \alpha_W^{IV} \, Released_{itj} + \beta_W \mathbf{X}_{it} + \mathbf{v}_{itj}, \tag{12}$$

$$Y_{itj} = \alpha_B^{IV} \, Released_{itj} + \beta_B \mathbf{X}_{it} + \mathbf{v}_{itj}, \tag{13}$$

where $Y_{itj}$ is the probability of pretrial misconduct, as measured by the probability of rearrest prior to case disposition. The vector $\mathbf{X}_{it}$ includes court-by-time fixed effects and baseline crime and defendant controls: race, gender, age, whether the defendant had a prior offense in the past year, whether the defendant had a prior history of pretrial crime in the past year, whether the defendant had a prior history of failure to appear in the past year, the number of charged offenses, indicators for crime type (drug, DUI, property, violent, or other), crime severity (felony or misdemeanor), and indicators for any missing characteristics. As described previously, the error term $\mathbf{v}_{itj} = \mathbf{U}_{itj} + \varepsilon_{itj}$ consists of characteristics unobserved by the econometrician but observed by the judge, $\mathbf{U}_{itj}$,

Kleinberg et al. (2018), we test for this possibility in Online Appendix Figure A2 by examining pretrial misconduct rates among observably identical defendants released by either lenient or strict judges. We find that predicted risk largely tracks true risk in all judge leniency quintiles, suggesting that lenient judges are neither more nor less skilled in predicting defendant risk. These results are broadly consistent with Kleinberg et al. (2018), who find that judges more or less agree on how to rank-order defendants based on their observable characteristics.

TABLE IV
PRETRIAL RELEASE AND CRIMINAL OUTCOMES

| | IV results | | | MTE results | | |
|---|---|---|---|---|---|---|
| | White (1) | Black (2) | $D^{IV}$ (3) | White (4) | Black (5) | $D^{MTE}$ (6) |
| Panel A: Rearrest for all crimes | | | | | | |
| Rearrest prior to disposition | 0.236*** | 0.014 | 0.222** | 0.249*** | 0.017 | 0.231** |
| | (0.073) | (0.070) | (0.101) | (0.084) | (0.080) | (0.117) |
| | [0.172] | [0.182] | – | [0.172] | [0.182] | – |
| Panel B: Rearrest by crime type | | | | | | |
| Rearrest for drug crime | 0.067 | 0.019 | 0.047 | 0.074 | −0.024 | 0.097 |
| | (0.043) | (0.043) | (0.060) | (0.048) | (0.054) | (0.074) |
| | [0.077] | [0.081] | – | [0.077] | [0.081] | – |
| Rearrest for property crime | 0.158*** | −0.005 | 0.163** | 0.149** | 0.043 | 0.106 |
| | (0.057) | (0.047) | (0.073) | (0.066) | (0.053) | (0.084) |
| | [0.065] | [0.068] | – | [0.065] | [0.068] | – |
| Rearrest for violent crime | 0.079** | −0.000 | 0.080 | 0.082* | −0.001 | 0.083 |
| | (0.039) | (0.042) | (0.058) | (0.044) | (0.050) | (0.068) |
| | [0.047] | [0.071] | – | [0.047] | [0.071] | – |
| Observations | 106,846 | 149,407 | – | 106,846 | 149,407 | – |

*Notes.* This table reports estimates of racial bias in pretrial release based on rearrest prior to case disposition. Columns (1) and (2) report two-stage least squares results of the impact of pretrial release on the probability of pretrial misconduct separately by race, and column (3) reports the difference between the white and black two-stage least squares coefficients, or $D^{IV}$ as described in the text. Columns (1)–(3) use instrumental variables weights for each specification and report robust standard errors two-way clustered at the individual and judge-by-shift level in parentheses. Columns (4) and (5) report the average marginal treatment effect of the impact of pretrial release on the probability of pretrial misconduct separately by race, and column (6) reports the difference between the white and black MTE coefficients, or $D^{MTE}$ as described in the text. Columns (4)–(6) use equal weights for each judge and report bootstrapped standard errors clustered at the judge-by-shift level in parentheses. All specifications control for court-by-time fixed effects and defendant race, gender, age, whether the defendant had a prior offense in the past year, whether the defendant had a prior history of pretrial crime in the past year, whether the defendant had a prior history of failure to appear in the past year, the number of charged offenses, indicators for crime type (drug, DUI, property, violent, or other), crime severity (felony or misdemeanor), and indicators for any missing characteristics. The sample means of the dependent variables are reported in brackets. ***Significant at 1% level, **significant at 5% level, *significant at 10% level.

and idiosyncratic variation unobserved by both the econometrician and judge, $\varepsilon_{itj}$. We instrument for pretrial release, $Released_{itj}$, with our measure of judge leniency, $Z_{itj}$, that is allowed to vary across years and defendant race. Robust standard errors are two-way clustered at the individual and judge-by-shift level.

Estimates from equations (12) and (13) are presented in columns (1) and (2) of Table IV. Column (3) reports our IV estimate of racial bias $D^{IV}$. Table IV, Panel A presents results for the probability of rearrest for any crime prior to case disposition, and Panel B presents results for rearrest rates for drug, property, and violent offenses separately. In total, 17.8% of defendants are rearrested for a new crime prior to disposition, with 7.9% of defendants rearrested for a crime that includes a drug offense, 6.7% of defendants rearrested for a crime that includes a property offense, and 6.1% of defendants rearrested for a crime that includes a violent offense.[13]

We find convincing evidence of racial bias against black defendants using our IV estimator. We find that marginally released white defendants are 23.6 percentage points more likely to be rearrested for any crime compared with marginally detained white defendants (column (1)). In contrast, the effect of pretrial release on rearrest rates for marginally released black defendants is a statistically insignificant 1.4 percentage points (column (2)). Loosely, these estimates imply that there is a 23.6% rate of rearrest for marginally released white defendants and a 1.4% rate of rearrest for marginally released black defendants, as detained defendants cannot be rearrested before trial.[14] Taken together, these IV estimates imply that marginally released white defendants are 22.2 percentage points more likely to be rearrested prior to disposition than marginally released black defendants (column (3)), consistent with racial bias against blacks ($p$ = .027). Importantly, we can reject the null hypothesis of no racial bias even assuming the maximum inframarginality bias in our IV estimator of 1.1 percentage points (see Online Appendix B).

In Panel B, we find suggestive evidence of racial bias against black defendants across all crime types, although the point estimates are too imprecise to make definitive conclusions. For

13. For completeness, Figure I provides a graphical representation of our reduced-form results separately by race. Following the first-stage results, we plot the reduced-form relationship between our judge leniency measure and the residualized rate of rearrest prior to case disposition, estimated on the full sample using local linear regression. Consistent with the first-stage estimates in Table II and IV estimates in Table IV, the reduced-form relationship between judge leniency and rearrest rates is much flatter for black defendants compared to white defendants.

14. Online Appendix Table A4 presents OLS results that measure the average level of risk among released white and black defendants conditional on observables. Along with our results in Table IV, these OLS estimates imply that the marginally released white defendant is riskier than the average released white defendant, while the marginally released black defendant is less risky than the average released black defendant. These results suggest that judges make substantial errors in predicting rearrest rates for black defendants, with all judges releasing relatively risky black defendants while disagreeing over relatively less risky black defendants. These findings are consistent with Kleinberg et al. (2018), who find that bail judges release many observably high-risk defendants while detaining many observably low-risk defendants.

example, we find that marginally released whites are about 8.0 percentage points more likely to be rearrested for a violent crime prior to disposition than marginally released blacks ($p$ = .173). Marginally released white defendants are also 4.7 percentage points more likely to be rearrested for a drug crime prior to case disposition than are marginally released black defendants ($p$ = .430) and 16.3 percentage points more likely to be rearrested for a property crime ($p$ = .025). These results suggest that judges are likely racially biased against black defendants even if they are most concerned about minimizing specific types of new crime, such as violent crimes.

*2. MTE Estimates.* Our second set of estimates comes from our MTE estimator, which allows us to put equal weight on each judge in our sample but at the cost of additional auxiliary assumptions. We estimate these MTE results using a two-step procedure. First, we estimate the entire distribution of MTEs using the derivative of residualized rearrest before case disposition, $\ddot{Y}_{itj}$, with respect to variation in the propensity score provided by our instrument, $p_r^j$, separately for white and black defendants:

$$MTE_W\left(p_W^j\right) = \frac{\partial}{\partial p_W^j}\mathbb{E}(\ddot{Y}_{itj}|p_W^j, W), \tag{14}$$

$$MTE_B\left(p_B^j\right) = \frac{\partial}{\partial p_B^j}\mathbb{E}(\ddot{Y}_{itj}|p_B^j, B), \tag{15}$$

where $p_r^j$ is the propensity score for release for judge $j$ and defendant race $r$ and $\ddot{Y}_{itj}$ is rearrest residualized using the full set of court-by-time fixed effects and baseline crime and defendant controls, $\mathbf{X}_{it}$. Following Heckman, Urzua, and Vytlacil (2006) and Doyle (2007), we also residualize $Z_{itj}$ and $Released_{itj}$ using $\mathbf{X}_{it}$. We then regress the residualized release variable on the residualized judge leniency measure to calculate $p_r^j$, a race-specific propensity score. Next, we compute the numerical derivative of a local quadratic estimator relating $\ddot{Y}_{itj}$ to $p_r^j$ to estimate race-specific MTEs. See Figure II for estimates of the full distribution of MTEs by defendant race.

Second, we use the race-specific MTEs to calculate the level of racial bias for each judge $j$. We calculate the average level of bias across all bail judges using a simple average of these judge-specific

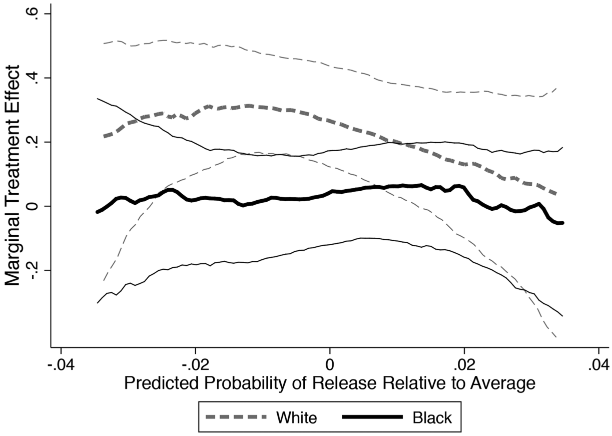


FIGURE II

Marginal Treatment Effects

This figure reports the marginal treatment effects (MTEs) of pretrial release on pretrial rearrest separately by race. To estimate each MTE, we first estimate the predicted probability of release using only judge leniency. We then estimate the relationship between the predicted probability of release and rearrest prior to disposition using a local quadratic estimator (bandwidth = 0.030). Finally, we use the numerical derivative of the local quadratic estimator to calculate the MTE at each point in the distribution. Standard errors are computed using 500 bootstrap replications clustered at the judge-by-shift level. The thin black lines are 90 percent confidence intervals for the black MTE and the thin dashed gray lines are 90 percent confidence intervals for the white MTE. See the text for additional details.

estimates:

$$\sum_{j=1}^{J} \frac{1}{J} \left( MTE_W \left( p_W^j \right) - MTE_B \left( p_B^j \right) \right). \tag{16}$$

We calculate standard errors by bootstrapping this two-step procedure at the judge-by-shift level. See Online Appendix B for additional details.

Estimates from equations (14) and (15) are presented in columns (4) and (5) of Table IV, with column (6) reporting our MTE equal-weighted estimate of racial bias $D^{MTE}$ from equation (16). Consistent with our IV estimates, we find that marginally released white defendants are 24.9 percentage points more likely to be rearrested for any crime compared to marginally detained white defendants (column (4)), while the effect of pretrial release on rearrest rates for marginally released black defendants is a statistically insignificant 1.7 percentage points (column (5)). Our MTE estimates therefore imply that marginally released white defendants are 23.1 percentage points more likely to be rearrested prior to disposition than are marginally released black defendants (column (6)), consistent with racial bias against black defendants ($p = .048$).

In addition, Figure II shows that the MTEs for white defendants lie strictly above the MTEs for black defendants, implying that marginally released white defendants are riskier than marginally released black defendants at all points in the judge leniency distribution. In other words, the results from Figure II show that there is racial bias against black defendants at every part of the judge leniency distribution. These results, along with the fact that IV and MTE approaches yield qualitatively similar estimates of racial bias, suggest that both the choice of IV weights and the additional parametric assumptions required to estimate the race-specific MTEs do not greatly affect our estimates of racial bias.

### *IV.B. Robustness*

Online Appendix Tables A5 and A6 explore whether our main findings are subject to omitted payoff bias. We find that our estimates are qualitatively similar when we use a measure of pretrial misconduct defined as failure to appear in Philadelphia, the only city where we observe this information (columns (1) and (2) of Online Appendix Table A5), or when we define pretrial misconduct as either failure to appear or rearrest in Philadelphia and only rearrest in Miami (columns (5) and (6) of Online Appendix Table A5). We also find that marginally released white defendants generate larger social costs than do marginally released black defendants when we estimate results separately for a subset of more serious crimes and weight each individual estimate by the corresponding social cost (Online Appendix Table A6).

Online Appendix Table A7 explores the sensitivity of our main results to a number of different specifications. Columns (1) and (6) drop a small number of defendants who the data indicate were rearrested prior to disposition despite never being released. Column (2) presents reweighted estimates with the weights chosen to match the distribution of observable characteristics by race (see Section II.D and Online Appendix B for details). Columns

(3) and (7) present results comparing outcomes for marginal non-Hispanic white defendants and black defendants. Columns (4) and (8) present results clustering more conservatively at the individual and judge level. Column (5) assesses whether monetary bail amounts have an independent effect on the probability of pretrial misconduct—a potential violation of the exclusion restriction—by controlling for monetary bail amount as an additional regressor in both our first- and second-stage regressions.[15] Under these alternative specifications, we continue to find evidence of racial bias against black defendants.

### *IV.C. Comparison to Other Outcome Tests*

Online Appendix Tables A4 and A8–A9 replicate the outcome tests from Knowles, Persico, and Todd (2001) and Anwar and Fang (2006). The Knowles, Persico, and Todd (2001) test relies on the prediction that under the null hypothesis of no racial bias, the average pretrial misconduct rate given by standard OLS estimates will not vary by defendant race. In contrast to our IV and MTE tests, however, standard OLS estimates suggest racial bias against white defendants. The Anwar and Fang (2006) test instead relies on the prediction that under the null hypothesis of no relative racial bias, the treatment of black and white defendants will not depend on judge race. However, this test also fails to find racial bias in our setting because both white and black judges are racially biased against black defendants. We also find that the IV and MTE estimates of racial bias are similar among white and black judges, although the confidence intervals for these estimates are large. Taken together, these results highlight the importance of accounting for inframarginality and omitted variables, as well as the importance of developing empirical tests that can detect absolute racial bias in the criminal justice system. See Arnold, Dobbie, and Yang (2017) for additional details on these results.

## V. Potential Mechanisms

In this section, we attempt to differentiate between two alternative forms of racial bias that could explain our findings: (i) racial

15. In these specifications, the coefficient on monetary bail amount is −0.002 ($p = .500$) for white defendants and −0.001 ($p = .184$) for black defendants, suggesting that monetary bail amount has no significant independent effect on pretrial misconduct, consistent with findings reported in Dobbie, Goldin, and Yang (2018).

animus (e.g., Becker 1957, 1993) and (ii) racially biased prediction errors in risk (e.g., Bordalo et al. 2016).

*V.A. Racial Animus*

The first potential explanation for our results is that judges either knowingly or unknowingly discriminate against black defendants at the margin of release as originally modeled by Becker (1957, 1993). Bail judges could, for example, harbor explicit animus against black defendants that leads them to value the freedom of black defendants less than the freedom of observably similar white defendants. Bail judges could also harbor implicit biases against black defendants, leading to the relative overdetention of blacks despite the lack of any explicit animus. Racial animus may be a particular concern in bail setting due to the relatively low number of minority bail judges, the rapid-fire determination of bail decisions, and the lack of face-to-face contact between defendants and judges. Prior work has shown that it is exactly these types of settings where racial prejudice is most likely to translate into the disparate treatment of minorities (e.g., Greenwald et al. 2009).

One suggestive piece of evidence against this hypothesis is provided by the Anwar and Fang (2006) test of relative racial bias, which indicates that bail judges are monolithic in their treatment of white and black defendants. Consistent with these results, we also find that IV and MTE estimates of racial bias are similar among white and black judges. These estimates suggest that either racial animus is not driving our results or that black and white bail judges harbor equal levels of racial animus toward black defendants.

*V.B. Racially Biased Prediction Errors in Risk*

A second explanation for our results is that bail judges are making racially biased prediction errors in risk, potentially due to inaccurate antiblack stereotypes. Bordalo et al. (2016) show, for example, that representativeness heuristics—probability judgments based on the most distinctive differences between groups—can exaggerate perceived differences between groups. In our setting, these kinds of race-based heuristics or antiblack stereotypes could lead bail judges to exaggerate the relative danger of releasing black defendants versus white defendants at the margin of release. These race-based prediction errors could also be

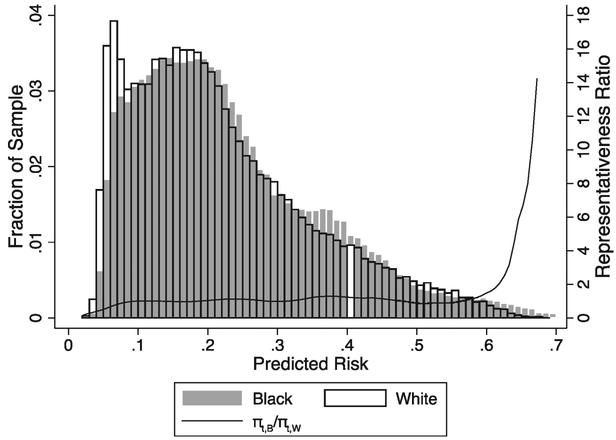


FIGURE III

Predicted Risk Distribution by Defendant Race

This figure reports the predicted distribution of pretrial misconduct risk separately by race. Pretrial misconduct risk is estimated using the machine learning algorithm described in Online Appendix F. The solid line represents the representativeness ratio for black versus white defendants as described in the text, or the estimated misconduct risk for blacks divided by the estimated misconduct risk for whites. See the text for additional details.

exacerbated by the fact that bail judges must make quick judgments on the basis of limited information and with virtually no training.

*1. Representativeness of Black and White Defendants.* We first explore whether our data are consistent with the formation of antiblack stereotypes that could lead to racially biased prediction errors. Extending Bordalo et al. (2016) to our setting, these antiblack stereotypes should only be present if blacks are overrepresented among the right tail of the predicted risk distribution relative to whites (both Hispanic and non-Hispanic). To test this idea, Figure III presents the distribution of the predicted risk of rearrest prior to case disposition calculated using the full set of crime and defendant characteristics, as well as the likelihood ratios, $\frac{\mathbb{E}(x|Black)}{\mathbb{E}(x|White)}$, throughout the risk distribution.[16]

16. Our measures of representativeness and predicted risk may be biased if judges base their decisions on variables that are not observed by the econometrician (e.g., demeanor at the bail hearing). Following Kleinberg et al. (2018), we can

Results for each individual characteristic in our predicted risk measure are also presented in Online Appendix Table A10. Consistent with the potential formation of antiblack stereotypes, we find that black defendants are significantly underrepresented in the left tail and overrepresented in the right tail of the predicted risk distribution. For example, black defendants are 1.2 times less likely than whites to be represented among the bottom 25% of the predicted risk distribution, but 1.1 times more likely to be represented among the top 25% and 1.2 times more likely to be represented among the top 5% of the predicted risk distribution.

In Online Appendix F, we show that these black–white differences in the predicted risk distribution are large enough to rationalize the black–white differences in pretrial release rates under the Bordalo et al. (2016) stereotypes model. First, as a benchmark for the stereotypes model, we compute the fraction of black defendants that would be released if judges applied the same release threshold for whites to blacks. We rank-order both black and white defendants using our predicted risk measure, finding that 70.8% of black defendants would be released pretrial if judges use the white release threshold for both black and white defendants. By comparison, only 68.8% of black defendants are actually released pretrial. Thus, to rationalize the black–white difference in release rates, the stereotypes model will require that judges believe that black defendants are riskier than they actually are.

In the stereotypes model, judges form beliefs about the distribution of risk through a representativeness-based discounting model, where the weight attached to a given risk type $t$ is increasing in the representativeness of $t$. Formally, let $\pi_{t,r}$ be the probability that a defendant of race $r$ is in risk category $t$. The stereotyped beliefs for black defendants, $\pi^{st}_{t,B}$, is given by:

$$\pi^{st}_{t,B} = \pi_{t,B} \frac{\left(\dfrac{\pi_{t,B}}{\pi_{t,W}}\right)^{\theta}}{\sum_{s \in T} \pi_{s,B}\left(\dfrac{\pi_{s,B}}{\pi_{s,W}}\right)^{\theta}}, \tag{17}$$

test for the importance of unobservables in bail decisions by splitting our sample into a training set to generate the risk predictions and a test set to test those predictions. We find that our measure of predicted risk from the training set is a strong predictor of true risk in the test set, indicating that our measure of predicted risk is not systematically biased by unobservables (see Online Appendix Figure A3).

where $\theta$ captures the extent to which representativeness distorts beliefs and the representativeness ratio, $\frac{\pi_{t,B}}{\pi_{t,W}}$, is equal to the probability a defendant is black given risk category $t$ divided by the probability a defendant is white given risk category $t$.

Using the definition of $\pi^{st}_{t,B}$ from equation (17), we can calculate the full stereotyped risk distribution for black defendants under different values of $\theta$. For each value of $\theta$, we can then calculate the implied release rate for black defendants under the above assumption that judges use the white release threshold for both black and white defendants. By iterating over different values of $\theta$, we can find the level of $\theta$ that equates the implied and true release rates for black defendants. Using this approach, we find that $\theta = 1.9$ can rationalize the true average release rate for blacks. To understand how far these beliefs are from the true distribution of risk, we plot the stereotyped distribution for blacks with $\theta = 1.9$ alongside the true distribution of risk for blacks in Online Appendix Figure A4. The mean predicted risk is 0.235 under the true distribution of risk for blacks, compared with 0.288 under the stereotyped distribution for blacks with $\theta = 1.9$.[17] These results indicate that a relatively modest shift in the true risk distribution for black defendants is sufficient to explain the large racial disparities we observe in our setting. See Online Appendix F for additional details on the stereotypes model and these calculations.

Further evidence in support of antiblack stereotypes comes from a comparison of the crime-specific distributions of risk. Black defendants are most overrepresented in the right tail of the predicted risk distribution for new violent crimes (see Online Appendix Figure A5), where we also tend to find strong evidence of racial bias.

A final piece of evidence in support of stereotyping comes from a comparison of the Hispanic and black distributions of risk relative to the non-Hispanic white distribution. Recall that we find no evidence of racial bias against Hispanic defendants (see Online Appendix Table A2). Consistent with the stereotyping model, we also find that the risk distributions of Hispanic and white defendants overlap considerably. In contrast, the risk distribution for blacks is shifted to the right relative to both the Hispanic and white distributions (see Online Appendix Figure A6). Thus,

17. Our estimate of $\theta$ is quantitatively similar to the magnitude of stereotypes in explaining investor overreaction to stock market news and the formation of credit cycles (Bordalo et al. 2017; Bordalo, Gennaioli, and Shleifer 2018).

all of our results are broadly consistent with bail judges making race-based prediction errors due to antiblack stereotypes and representativeness-based thinking, which in turn leads to the overdetention of black defendants at the margin of release.

*2. Racial Bias and Prediction Errors in Risk.* We can also test for race-based prediction errors by examining situations where prediction errors of any kind are more likely to occur. One such test for race-based prediction errors uses a comparison of experienced and inexperienced judges. When a defendant violates the conditions of release, such as by committing a new crime, he or she is taken into custody and brought to court for a hearing, during which a bail judge decides whether to revoke bail. As a result, judges may be less likely to rely on inaccurate racial stereotypes as they acquire greater on-the-job experience, at least in settings with limited information and contact. Consistent with this idea, we find that more experienced bail judges are more likely to release defendants, but not make misclassification errors (see Online Appendix Figure A7). In contrast, although it appears plausible that race-based prediction errors will decrease with experience, there is no reason to believe that racial animus will change with experience.

To test this idea, columns (1)–(4) of Table V present our estimates of racial bias, $D^{IV}$ and $D^{MTE}$, separately by court. Although we caution that there are likely many differences in the criminal justice systems of the two cities in our sample, one distinction is the degree to which bail judges specialize in conducting bail hearings. In Philadelphia, bail judges are full-time judges who specialize in setting bail 24 hours a day, seven days a week, hearing an average of 5,253 cases each year. Conversely, the Miami bail judges in our sample are part-time generalists who work as trial court judges on weekdays and assist the bail court on weekend, hearing an average of only 179 bail cases each year. Consistent with racially biased prediction errors being more common among inexperienced judges, we find that racial bias is higher in Miami than Philadelphia ($p = .325$ for IV, $p = .442$ for MTE). In Miami, we find that marginally released white defendants are 25.1 percentage points more likely to be rearrested using our IV estimator ($p = .027$) and 24.9 percentage points more likely to be rearrested using our MTE estimator ($p = .040$), compared with marginally released black defendants. In Philadelphia, we find no statistically significant evidence of racial bias under either our IV or

TABLE V

RACIAL BIAS IN PRETRIAL RELEASE BY JUDGE EXPERIENCE

| | Judge specialization | | | | Judge experience | | | |
|---|---|---|---|---|---|---|---|---|
| | Miami $D^{IV}$ (1) | Miami $D^{MTE}$ (2) | Phl $D^{IV}$ (3) | Phl $D^{MTE}$ (4) | Miami low Exp $D^{IV}$ (5) | Miami low Exp $D^{MTE}$ (6) | Miami high Exp $D^{IV}$ (7) | Miami high Exp $D^{MTE}$ (8) |
| Panel A: Rearrest for all crimes | | | | | | | | |
| Rearrest prior to disposition | 0.251** | 0.249** | 0.040 | 0.078 | 0.487** | 0.510** | 0.144 | 0.036 |
| | (0.114) | (0.121) | (0.184) | (0.195) | (0.237) | (0.233) | (0.178) | (0.164) |
| | [0.149] | [0.149] | [0.194] | [0.194] | [0.148] | [0.148] | [0.152] | [0.152] |
| Panel B: Rearrest by crime type | | | | | | | | |
| Rearrest for drug crime | 0.053 | 0.103 | 0.008 | 0.015 | 0.141 | 0.185 | −0.013 | 0.006 |
| | (0.066) | (0.077) | (0.138) | (0.150) | (0.119) | (0.138) | (0.101) | (0.110) |
| | [0.057] | [0.057] | [0.092] | [0.092] | [0.057] | [0.057] | [0.057] | [0.057] |
| Rearrest for property crime | 0.196** | 0.127 | −0.031 | −0.014 | 0.296** | 0.293* | 0.146 | 0.035 |
| | (0.084) | (0.096) | (0.110) | (0.199) | (0.140) | (0.163) | (0.111) | (0.124) |
| | [0.078] | [0.078] | [0.060] | [0.060] | [0.078] | [0.078] | [0.079] | [0.079] |
| Rearrest for violent crime | 0.082 | 0.079 | 0.065 | 0.066 | 0.204 | 0.218* | 0.032 | −0.036 |
| | (0.065) | (0.075) | (0.115) | (0.119) | (0.134) | (0.119) | (0.100) | (0.099) |
| | [0.050] | [0.050] | [0.067] | [0.067] | [0.048] | [0.048] | [0.051] | [0.051] |
| Observations | 93,417 | 93,417 | 162,836 | 162,836 | 47,692 | 47,692 | 45,725 | 45,725 |

*Notes.* This table reports estimates of racial bias for different subgroups of judges. $D^{IV}$ is the difference between the white and black two-stage least squares coefficient estimates of pretrial release on pretrial misconduct using instrumental variables weights. $D^{MTE}$ is the difference between the average white and black MTE estimates of pretrial release on pretrial misconduct using equal weights by judge. Columns (1) and (2) report estimates for nonspecialist bail judges in Miami-Dade. Columns (3) and (4) report estimates for specialist bail judges in Philadelphia. Columns (5)–(8) report estimates for nonspecialist bail judges in Miami with below- and above-median years of experience. The sample is described in the notes to Table 1. The dependent variable is listed in each row. All specifications control for court-by-time fixed effects and defendant race, gender, age, whether the defendant had a prior offense in the past year, whether the defendant had a prior history of pretrial crime in the past year, whether the defendant had a prior history of failure to appear in the past year, the number of charged offenses, indicators for crime type (drug, DUI, property, violent, or other), crime severity (felony or misdemeanor), and indicators for any missing characteristics. The sample means of the dependent variables are reported in brackets. For IV specifications, robust standard errors two-way clustered at the individual and judge-by-shift level are reported in parentheses. For MTE specifications, bootstrapped standard errors clustered at the judge-by-shift level are reported in parentheses. **significant at 5% level, *significant at 10% level.

MTE estimates, suggesting the possible importance of experience in alleviating any prediction errors.[18]

Columns (5)–(8) of Table V provide additional evidence on this issue by exploiting the substantial variation in the experience profiles of the Miami bail judges in our sample. Splitting by the median number of years hearing bail cases, the average experienced Miami judge has 9.5 years of experience working in the bail system, while the average inexperienced Miami judge has only 2.5 years of experience. Consistent with our across-court findings, we find suggestive evidence that inexperienced judges are more racially biased than experienced judges ($p$ = .193 for IV, $p$ = .095 for MTE). Among inexperienced judges, we find that marginally released white defendants are 48.7 percentage points more likely to be rearrested using our IV estimator ($p$ = .040) and 51.0 percentage points more likely to be rearrested using our MTE estimator ($p$ = .029), compared with marginally released black defendants. Among experienced judges, we find no statistically significant evidence of racial bias under either our IV or MTE estimates.

Taken together, our results suggest that bail judges make racially biased prediction errors in risk. In contrast, we find limited evidence in support of the hypothesis that bail judges harbor racial animus toward black defendants. These results are broadly consistent with recent work by Kleinberg et al. (2018) showing that bail judges make significant prediction errors in risk for all defendants, perhaps due to overweighting the most salient case and defendant characteristics such as race and the nature of the charged offense. Our results also provide additional support for the stereotyping model developed by Bordalo et al. (2016), which suggests that probability judgments based on the most distinctive differences between groups—such as the significant overrepresentation of blacks relative to whites in the right tail of the risk distribution—can lead to antiblack stereotypes and, as a result, racial bias against black defendants.

18. Our IV estimate of racial bias in Philadelphia should be interpreted with some caution given that we only observe seven judges for this city in our data. The maximum inframarginality bias of our IV estimator in Philadelphia is 16.4 percentage points, compared with only 1.6 percentage points in Miami-Dade. We note, however, that there is no inframarginality bias of our MTE estimator for either city if we have correctly specified the shape of the MTE function.

## VI. Conclusion

In this article, we test for racial bias in bail setting using the quasi-random assignment of bail judges to identify pretrial misconduct rates for marginal white and marginal black defendants. We find evidence that there is substantial bias against black defendants, ruling out statistical discrimination as the sole explanation for the racial disparities in bail. Our estimates are nearly identical if we account for observable crime and defendant differences by race, indicating that our results cannot be explained by black–white differences in the probability of being arrested for certain types of crimes (e.g., the proportion of felonies versus misdemeanors) or black–white differences in defendant characteristics (e.g., the proportion of defendants with prior offenses versus no prior offenses).

We find several pieces of evidence consistent with our results being driven by racially biased prediction errors in risk, as opposed to racial animus among bail judges. First, we find that both white and black bail judges are racially biased against black defendants, a finding that is inconsistent with most models of racial animus. Second, we find that black defendants are sufficiently overrepresented in the right tail of the predicted risk distribution to rationalize observed racial disparities in release rates under a theory of stereotyping. Finally, racial bias is significantly higher among both part-time and inexperienced judges, and descriptive evidence suggests that experienced judges can better predict misconduct risk for all defendants. Taken together, these results are most consistent with a model of bail judges relying on inaccurate stereotypes that exaggerate the relative danger of releasing black defendants versus white defendants at the margin.

The findings from this article have a number of important implications. If racially biased prediction errors among inexperienced judges are an important driver of black–white disparities in pretrial detention, providing judges with increased opportunities for training or on-the-job feedback could play an important role in decreasing racial disparities in the criminal justice system. Consistent with recent work by Kleinberg et al. (2018), our findings also suggest that providing judges with data-based risk assessments may also help decrease unwarranted racial disparities.

The empirical test developed in this article can be used to test for bias in other settings. Our test for bias is appropriate whenever there is the quasi-random assignment of decision makers and the objective of these decision makers is both known and well measured. Our test can therefore be used to explore bias in settings as varied as parole board decisions, disability insurance applications, bankruptcy filings, and hospital care decisions.

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## SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online. Data and code replicating tables and figures in this article can be found in Arnold, Dobbie, and Yang (2018), in the Harvard Dataverse, doi:10.7910/DVN/REUOXC.

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# EXHIBIT B

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# The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges†

*By* Will Dobbie, Jacob Goldin, and Crystal S. Yang*

*Over 20 percent of prison and jail inmates in the United States are currently awaiting trial, but little is known about the impact of pretrial detention on defendants. This paper uses the detention tendencies of quasi-randomly assigned bail judges to estimate the causal effects of pretrial detention on subsequent defendant outcomes. Using data from administrative court and tax records, we find that pretrial detention significantly increases the probability of conviction, primarily through an increase in guilty pleas. Pretrial detention has no net effect on future crime, but decreases formal sector employment and the receipt of employment- and tax-related government benefits. These results are consistent with* (*i*) *pretrial detention weakening defendants' bargaining positions during plea negotiations and* (*ii*) *a criminal conviction lowering defendants' prospects in the formal labor market.* (*JEL* J23, J31, J65, K41, K42)

Each year, more than 11 million individuals around the world are imprisoned prior to conviction. The United States leads all other countries with approximately half a million individuals detained before trial on any given day, nearly double the next highest country, China (Walmsley 2013). The high rate of pretrial detention in the United States is due to both the widespread use of monetary bail and the limited financial resources of most defendants. Nationwide, less than 25 percent of felony defendants are released without financial conditions, and the typical felony defendant is assigned a bail amount of more than $55,000 (Reaves 2013). Furthermore, we find in our data that the typical defendant earned less than $7,000 in the year

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prior to arrest, likely explaining why less than 50 percent of defendants are able to post bail even when it is set at $5,000 or less.

The high rate of pretrial detention, particularly for poor and minority defendants, has contributed to an ongoing debate on the effectiveness of the current bail system. Critics argue that excessive bail conditions and pretrial detention can disrupt defendants' lives, putting jobs at risk and increasing the pressure to accept unfavorable plea bargains.[1] There are also concerns that pretrial detention is determined by a defendant's wealth, not risk to the community, leading the Department of Justice to conclude that the bail systems in many jurisdictions "are not only unconstitutional, but they also constitute bad public policy" (Department of Justice 2016, p. 13). Others claim that the bail system is operating as designed, and that releasing more defendants would increase pretrial flight and crime rates. This debate is currently playing out across the country, with a number of jurisdictions exploring alternatives to pretrial detention such as electronic or in-person monitoring for low-risk defendants.[2] To date, however, there is little systematic evidence on the causal effects of detaining an individual before trial.

Estimating the causal impact of pretrial detention on defendants has been complicated by two important issues. First, there are few datasets that include information on both bail hearings and long-term outcomes for a large number of defendants.[3] Second, defendants who are detained before trial are likely unobservably different from defendants who are not detained, biasing cross-sectional comparisons. For example, defendants detained pretrial may be more likely to be guilty or more likely to commit another crime in the future, biasing ordinary least squares estimates upward.[4]

In this paper, we use new data linking over 420,000 criminal defendants from two large, urban counties to administrative court and tax records to estimate the impact of pretrial detention on criminal case outcomes, pretrial flight, future crime, foregone

[1] As one lawyer told the *New York Times*, "[m]ost of our clients are people who have crawled their way up from poverty or are in the throes of poverty. …Our clients work in service-level positions where if you're gone for a day, you lose your job. …People who live in shelters, where if they miss their curfews, they lose their housing. …So when our clients have bail set, they suffer on the inside, they worry about what's happening on the outside, and when they get out, they come back to a world that's more difficult than the already difficult situation that they were in before." See Nick Pinto, "The Bail Trap," *New York Times*, August 13, 2015, http://www.nytimes.com/2015/08/16/magazine/the-bail-trap.html.

[2] For example, New York City has earmarked substantial funds to supervise low-risk defendants instead of requiring them to post bail or face pretrial detention, and Illinois lawmakers passed a bill in May 2015 requiring that a nonviolent defendant be released pretrial without bond if his or her case has not been resolved within 30 days. Other cities are considering the use of risk-based assessment tools to more accurately predict each defendant's flight risk, and some communities have created charitable bail organizations such as the Bronx Freedom Fund and the Brooklyn Community Bail Fund, which posts bail for individuals held on misdemeanor charges when bail is set at $2,000 or less.

[3] Data tracking defendants often contain some information on pretrial detention and outcomes from the criminal justice process (i.e., arrest, charging, trial, and sentencing), but do not contain unique identifiers that allow defendants to be linked to longer-term outcomes. For example, the Bureau of Justice Statistics' State Court Processing Statistics (SCPS) program periodically tracks a sample of felony cases for about 110,000 defendants from a representative sample of 40 of the nation's 75 most populous counties, but does not include the identifiers necessary to link to other datasets.

[4] Prior work based on cross-sectional comparisons has yielded mixed results, with some papers suggesting little impact of pretrial detention on conviction rates (Goldkamp 1980), and others finding a significant relationship between pretrial detention and the probability of conviction (Ares, Rankin, and Sturz 1963; Cohen and Reaves 2007; Phillips 2008) and incarceration (Foote 1954; Williams 2003; Oleson et al. 2014). There is also mixed evidence on whether bail amounts are correlated with the probability of jumping bail (Landes 1973; Clarke, Freeman, and Koch 1976; Myers 1981).

earnings, and social benefits. Our empirical strategy exploits plausibly exogenous variation in pretrial release after the first bail hearing from the quasi-random assignment of cases to bail judges who vary in the leniency of their bail decisions. This empirical design recovers the causal effects of pretrial release after the first bail hearing for individuals at the margin of release; i.e., cases on which bail judges disagree on the appropriate bail conditions. We measure bail judge leniency using a leave-out, residualized measure based on all other cases that a bail judge has handled during the year. The leave-out leniency measure is highly predictive of detention decisions, but uncorrelated with case and defendant characteristics. Importantly, bail judges in our sample are different from trial and sentencing judges, who are assigned through a different process, allowing us to separately identify the effects of being assigned to a lenient bail judge as opposed to a lenient judge in all phases of the case. This instrumental variables (IV) research strategy is similar to that used by Kling (2006), Aizer and Doyle (2015), and Mueller-Smith (2015) to estimate the impact of incarceration in the United States; Bhuller et al. (2016) to estimate the impact of incarceration in Norway; and Di Tella and Schargrodsky (2013) to estimate the impact of electronic monitoring in Argentina.[5]

We begin by estimating the impact of initial pretrial release on case outcomes. We find that initial pretrial release decreases the probability of being found guilty by 14.0 percentage points, a 24.2 percent change from the mean for detained defendants, with larger effects for defendants with no prior offenses in the past year. The decrease in conviction is largely driven by a reduction in the probability of pleading guilty, which decreases by 10.8 percentage points, a 24.5 percent change. Conversely, initial pretrial release has a small and statistically insignificant effect on post-trial incarceration, likely because detained defendants plead to time served and because most charged offenses in our sample carry minimal prison time. These results suggest that initial pretrial release affects case outcomes primarily through a strengthening of defendants' bargaining positions before trial, particularly for defendants charged with less serious crimes and with no prior offenses.

Next, we explore the impact of initial pretrial release on pretrial flight and new crime, two frequently cited costs of release. We find that initial pretrial release increases the probability of failing to appear in court by 15.6 percentage points, a 128.9 percent increase, with smaller effects for defendants with no prior offenses. In contrast, we find no detectable effect of initial pretrial release on new crime up to two years after the bail hearing. This null result is driven by offsetting incapacitation and criminogenic effects. While initial pretrial release increases the likelihood of rearrest prior to case disposition by 18.9 percentage points, a 121.9 percent change, it also decreases the likelihood of rearrest following case disposition by 12.1 percentage points, a 35.3 percent change. These short-run incapacitation and medium-run criminogenic effects nearly exactly offset each other for the marginal defendant, at least over the time horizons we observe in the data. These results also

[5] Outside of the criminal justice setting, Chang and Schoar (2008), Dobbie and Song (2015) and Dobbie, Goldsmith-Pinkham, and Yang (forthcoming) use bankruptcy judge propensities to grant bankruptcy protection; Maestas, Mullen, and Strand (2013), French and Song (2014), Dahl, Kostol, and Mogstad (2014), and Autor et al. (2017) use disability examiner propensities to approve disability claims; and Doyle (2007, 2008) uses case worker propensities to place children in foster care.

suggest that the most empirically relevant cost of pretrial release is increased flight, not new crime.

Finally, we examine the effects of initial pretrial release on formal sector employment and social benefits receipt. We find evidence that pretrial release increases both formal sector employment and the receipt of employment- and tax-related government benefits, with larger effects among individuals with no prior offenses in the past year. Initial pretrial release increases the probability of employment in the formal labor market three to four years after the bail hearing by 9.4 percentage points, a 24.9 percent increase from the detained defendant mean. Pretrial release also increases the amount of Unemployment Insurance (UI) benefits received over the same time period by \$293, a 119.6 percent increase, and the amount of Earned Income Tax Credit (EITC) benefits received by \$209, a 58.5 percent increase. The probability of having any formal sector income over this time period increases by 10.7 percentage points, a 23.2 percent increase, and the probability of filing a tax return increases by 5.1 percentage points, a 16.7 percent increase.

To examine the potential mechanisms driving our labor market results, we explore whether those who are more likely to be employed are also those who do not have a criminal conviction. We find that in the first two years after the bail hearing, our employment results are primarily driven by an increase in the joint probability of not having a criminal conviction and being employed in the formal labor market. By the third to fourth years after the bail hearing, our employment estimates are entirely driven by the joint probability of having no criminal conviction and being employed. These results are consistent with the stigma of a criminal conviction lowering defendants' prospects in the formal labor market (e.g., Pager 2003, Agan and Starr 2016), which in turn limits defendants' eligibility for employment-related benefits like UI and EITC. In contrast, we find no evidence that our labor market results can be explained by changes in job stability or by any incapacitation effects.

We conclude by using our new estimates to conduct a partial cost-benefit analysis that accounts for administrative jail expenses, costs of apprehending defendants, costs of future crime, and economic impacts on defendants. We estimate that the net benefit of pretrial release at the margin is between \$55,143 and \$99,124 per defendant. The large net benefit of pretrial release is driven by both the significant collateral consequences of having a criminal conviction on labor market outcomes and the relatively low costs of apprehending defendants who fail to appear in court. The results from this exercise suggest that unless there are large general deterrence effects of detaining individuals before trial, releasing more defendants will likely increase social welfare.

Our findings are related to an important literature estimating the effects of incarceration and sentence length on defendants. Kling (2006) finds no impact of sentence length on labor market outcomes using prison records from Florida and California. However, Mueller-Smith (2015) finds that post-conviction incarceration reduces employment and increases future crime using data on defendants from Harris County, Texas, and Aizer and Doyle (2015) find that juvenile incarceration reduces high school completion and increases adult incarceration using data on juveniles from Chicago. Consistent with Mueller-Smith (2015) and Aizer and Doyle (2015), we find that pretrial detention reduces employment and increases future crime through a criminogenic effect (although unlike those papers, we find that this criminogenic effect is offset by an incapacitation effect). Importantly, however, our paper is the first to shed light on the effects of a criminal conviction and the effects of pre-conviction detention, as opposed to incarceration per se.[6]

Our paper is also related to a number of recent papers conducted in parallel to our study that estimate the effects of bail decisions on case decisions (e.g., Gupta, Hansman, and Frenchman 2016; Leslie and Pope 2016; Stevenson 2016; Didwania 2017). Where our outcomes overlap, we find similar results: Stevenson (2016) finds that pretrial detention leads to a 6.6 percentage point increase in the likelihood of being convicted in Philadelphia, with larger effects for first or second time arrestees. Gupta, Hansman, and Frenchman (2016) similarly find that the assignment of monetary bail in Philadelphia leads to a 6 percentage point increase in the likelihood of being convicted, with some evidence of higher recidivism following the initial case decision, while Leslie and Pope (2016) show that pretrial detention increases the probability of conviction by 7 to 13 percentage points in New York City. Finally, in the federal system, Didwania (2017) finds that pretrial detention increases a defendant's sentence length and the probability of receiving at least a mandatory minimum sentence.

We make four contributions relative to this parallel work. First, and most importantly, our data allow us to estimate effects on a wide-range of long-term outcomes such as labor market outcomes and take-up of public assistance. These estimates allow us to, for the first time, conduct a partial welfare analysis that incorporates causal estimates of both costs and benefits of pretrial detention. Second, we are able to provide some of the first evidence on why pretrial detention impacts defendants, with our results suggesting that the stigma of a criminal conviction in the formal labor market is an important mechanism linking detention to long-term outcomes. Third, we estimate results separately for pre- and post-trial crime, showing that there are offsetting incapacitation and criminogenic effects. Finally, we present new evidence that the exclusion restriction implicit in the judge IV strategy—that judge assignment only affects defendants' outcomes through the channel of pretrial release—is likely to hold in our setting. This evidence is critical for correctly interpreting the IV estimates and using our findings to evaluate recent bail reforms.

The remainder of the paper is structured as follows. Section I provides a brief overview of the bail system and judge assignment in our context. Section II describes our data and provides summary statistics. Section III describes our empirical strategy. Section IV presents the results, Section V offers interpretation, and Section VI concludes. An online Appendix provides additional results and detailed information on the outcomes used in our analysis.

[6] Our results are also related to a broad literature documenting the presence of racial disparities at various stages of the criminal justice process (e.g., Ayres and Waldfogel 1994, Bushway and Gelbach 2011, McIntyre and Baradaran 2013, Rehavi and Starr 2014, Anwar, Bayer, and Hjalmarsson 2012, Abrams, Bertrand, and Mullainathan 2012, Alesina and La Ferrara 2014), and suggest that the costs of pretrial detention are disproportionately borne by black defendants. See Arnold, Dobbie, and Yang (2017) for additional evidence on racial bias in bail setting.

## I. The Bail System in the United States

### A. *Overview*

In the United States, the bail system is meant to allow all but the most dangerous criminal suspects to be released from custody while ensuring their appearance at required court proceedings and the public's safety. The federal right to non-excessive bail is guaranteed by the Eighth Amendment to the US Constitution, with almost all state constitutions granting similar rights to defendants.[7]

In most jurisdictions, bail conditions are determined by a bail judge within 24 to 48 hours of a defendant's arrest. The assigned bail judge has a number of potential options when setting bail. First, defendants who show a minimal risk of flight may be released on their promise to return for all court proceedings, known broadly as release on recognizance (ROR). Second, defendants may be released subject to some nonmonetary conditions, such as monitoring or drug treatment, when the court finds that these measures are required to prevent flight or harm to the public. Third, defendants may be required to post a bail payment to secure release if they pose an appreciable risk of flight or threat of harm to the public. Defendants are typically required to pay 10 percent of the bail amount to secure release, with most of the bail money refunded after the case is concluded if there were no failures to appear in court or other release violations. Those who do not have the 10 percent deposit in cash can borrow this amount from a commercial bail bondsman, who will accept cars, houses, jewelry, and other forms of collateral. Bail bondsman also charge a non-refundable fee for their services, generally 10 percent of the total bail amount.[8] If the defendant fails to appear, either the defendant or the bail surety is theoretically liable for the full value of the bail amount and forfeits any amount already paid. Finally, for more serious crimes, the bail judge may also require that the defendant is detained pending trial by denying bail altogether. Bail denial is often mandatory in first- or second-degree murder cases, but can be imposed for other crimes when the bail judge finds that no set of conditions for release will guarantee appearance or protect the community from the threat of harm posed by the suspect.

The bail judge will usually consider factors such as the nature of the alleged offense, the weight of the evidence against the defendant, any record of prior flight or bail violations, and the financial ability of the defendant to pay bail (Foote 1954). Because each defendant poses a different set of risks, bail judges are granted considerable discretion in evaluating each defendant's circumstances when making decisions about release. In addition, because bail hearings occur very shortly after

[7] For instance, the Eighth Amendment to the US Constitution states that "[e]xcessive bail shall not be required." In our setting, Article I, §14 of the Pennsylvania Constitution states that "[a]ll prisoners shall be bailable by sufficient sureties, unless for capital offenses or for offenses for which the maximum sentence is life imprisonment or unless no condition or combination of conditions other than imprisonment will reasonably assure the safety of any person and the community…," and Article I, §14 of the Florida Constitution states that "[u]nless charged with a capital offense or an offense punishable by life imprisonment…every person charged with a crime…shall be entitled to pretrial release on reasonable conditions."

[8] A bail bondsman is any person or corporation that acts as a surety by pledging money or property as bail for the appearance of persons accused in court. If the defendant misses a court appearance, the bail agency will often hire someone to locate the missing defendant and have him taken back into custody. The bail bondsman may also choose to sue the defendant or whoever helped to guarantee the bond to recoup the bail amount. Repayment may come in the form of cash, but it can also be made by seizure of the assets used to secure the bail bond.

arrest and last only a few minutes, judges generally have limited information on which to base their decisions (Goldkamp and Gottfredson 1988). This discretion, coupled with limited information, results in substantial differences in bail decisions across bail judges. Defendants generally have the opportunity to appeal the initial bail decision in later proceedings, which can lead to modifications of the initial bail conditions.

Following the bail hearing, a defendant usually attends a preliminary arraignment, where the court determines whether there is probable cause for the case and the defendant formally enters a plea of guilty or not guilty. If the case is not dismissed and the defendant does not plead guilty, the case proceeds to trial by judge (bench trial) or jury (jury trial). Plea bargaining usually begins around the time of arraignment and can continue throughout the criminal proceedings. If a defendant pleads guilty or is found guilty at trial, he or she is sentenced at a later hearing. Online Appendix Figure A1 provides the general timeline of the criminal justice process in a typical jurisdiction, although the precise timing of the process differs across jurisdictions.

### B. *Our Setting: Philadelphia County and Miami-Dade County*

*Philadelphia County.*—Immediately following arrest in Philadelphia County, defendants are brought to one of six police stations around the city where they are interviewed by the city's Pretrial Services Bail Unit. The Bail Unit operates 24 hours a day, 7 days a week, and interviews all adults charged with offenses in Philadelphia through videoconference, collecting information on the arrested individual's charge severity, personal and financial history, family or community ties, and criminal history. The Bail Unit then uses this information to calculate a release recommendation based on a four-by-ten grid of bail guidelines (see online Appendix Figure A2) that is presented to the bail judge. However, these bail guidelines are only followed by the bail judge about half of the time, with judges often imposing monetary bail instead of the recommended nonmonetary options (Shubik-Richards and Stemen 2010).

After the Pretrial Services interview is completed and the charges are approved by the Philadelphia District Attorney's Office, the defendant is brought in for a bail hearing. Since the mid-1990s, bail hearings have been conducted through videoconference by the bail judge on duty, with representatives from the district attorney and local public defender's offices (or private defense counsel) also present. However, while a defense lawyer is present at the bail hearing, there is no real opportunity for defendants to speak with the attorney prior to the hearing. At the hearing itself, the bail judge reads the charges against the defendant, informs the defendant of his right to counsel, sets bail after hearing from representatives from the prosecutor's office and the defendant's counsel, and schedules the next court date. After the bail hearing, the defendant has an opportunity to post bail, secure counsel, and notify others of the arrest. If the defendant is unable to post bail, he is detained but has the opportunity to petition for bail modification in subsequent court proceedings.

*Miami-Dade County.*—The Miami-Dade bail system follows a similar procedure, with one important exception. As opposed to Philadelphia where all defendants are required to have a bail hearing, most defendants in Miami-Dade can avoid a bail hearing and be immediately released following arrest and booking by posting an amount designated by a standard bail schedule. The bail schedule ranks offenses according to their seriousness and assigns an amount of bond that must be posted to permit a defendant's release. Critics have argued that this kind of standardized bail schedule discriminates against poor defendants by setting a fixed price for release according to the charged offense rather than taking into account a defendant's ability to pay, or propensity to flee or commit a new crime. Approximately 30 percent of all defendants in Miami-Dade are released prior to a bail hearing, with the other 70 percent attending a bail hearing (Goldkamp and Gottfredson 1988). Thus, our estimates from Miami-Dade should be interpreted as the causal effect of pretrial release among defendants who cannot pay the standard bail amount.[9]

If a defendant is unable to post bail immediately in Miami-Dade, there is a bail hearing within 24 hours of arrest where defendants can argue for a reduced bail amount. Miami-Dade conducts separate daily hearings for felony and misdemeanor cases through videoconference by the bail judge on duty. At the bail hearing, the court will determine whether or not there is sufficient probable cause to detain the arrestee and, if so, the appropriate bail conditions. The bail amount may be lowered, raised, or remain the same as the scheduled bail amount depending on the case situation and the arguments made by the defense counsel and prosecutor. While monetary bail amounts at this stage often follow the standard bail schedule, the choice between monetary versus nonmonetary bail conditions varies widely across judges in Miami-Dade (Goldkamp and Gottfredson 1988).

*Mapping to Empirical Design.*—Our empirical strategy exploits variation in the pretrial release tendencies of the assigned bail judge. There are four features of the Philadelphia and Miami-Dade bail systems that make them an appropriate setting for our research design. First, there are multiple bail judges serving simultaneously, allowing us to measure variation in bail decisions across judges. At any point in time, Philadelphia has six bail judges that only make bail decisions. In Miami-Dade, weekday cases are handled by a single bail judge, but weekend cases are handled by approximately 60 different judges on a rotating basis. These weekend bail judges are trial court judges from the misdemeanor and felony courts in Miami-Dade that assist the bail court with weekend cases.

Second, the assignment of judges is based on rotation systems, providing quasi-random variation in which bail judge a defendant is assigned to. In Philadelphia, the six bail judges serve rotating eight-hour shifts in order to balance caseloads. Three judges serve together every five days, with one bail judge serving the morning shift (7:30 AM–3:30 PM), another serving the afternoon shift (3:30 PM–11:30 PM), and the final judge serving the night shift (11:30 PM–7:30 AM). While it may be endogenous whether a defendant is arrested in the morning or at night or on a specific day of

[9] Specifically, the estimates from Miami-Dade will differ from estimates in a court without a pre-hearing release schedule if two conditions are met: (i) there are heterogeneous treatment effects across defendants who can and cannot pay the standard bail amount and (ii) there are a nontrivial number of defendants who can pay the standard bail amount that, in the absence of such a system, would have been affected by judge assignment (i.e., that are "compliers" in the framework outlined in Section III).

the week, the fact that these six bail judges rotate through all shifts and all days of the week allows us to isolate the independent effect of the judge from day-of-week and time-of-day effects. In Miami-Dade, the weekend bail judges rotate through the felony and misdemeanor bail hearings each weekend to ensure balanced caseloads during the year. Every Saturday and Sunday beginning at 9:00 AM, one judge works the misdemeanor shift and another judge works the felony shift. Because of the large number of judges in Miami-Dade, any given judge works a bail shift approximately once or twice a year.[10]

Third, there is very limited scope for influencing which bail judge will hear the case, as most individuals are brought for a bail hearing shortly following the arrest. In Philadelphia, all adults arrested and charged with a felony or misdemeanor appear before a bail judge for a formal bail hearing, which is usually scheduled within 24 hours of arrest. A defendant is automatically assigned to the bail judge on duty. There is also limited room for influencing which bail judge will hear the case in Miami-Dade, as arrested felony and misdemeanor defendants are brought in for their hearing within 24 hours following arrest to the bail judge on duty. However, given that defendants can post bail immediately following arrest in Miami-Dade without having a bail hearing, there is the possibility that defendants may selectively post bail depending on the identity of the assigned bail judge. It is also theoretically possible that a defendant may self-surrender to the police in order to strategically time their bail hearing to a particular bail judge. As a partial check on this important assumption of random assignment, we test the relationship between observable characteristics and bail judge assignment.

Fourth, in both the Philadelphia and Miami-Dade systems, the bail judge is different from trial and sentencing judges, and these subsequent judges are assigned through a different process, allowing us to separately identify the effects of being assigned to a lenient bail judge as opposed to a lenient bail, trial, and sentencing judge. In Philadelphia, cases are randomly assigned to a completely separate pool of trial judges following the bail hearing. In Miami-Dade, cases are also randomly assigned to trial judges following the bail hearing, although this pool of trial judges is the same set of judges that rotate through weekend bail shifts. In both jurisdictions, the rotation schedules of the bail judges also do not align with the schedule of any other actors in the criminal justice system. For example, in both Philadelphia and Miami-Dade, different prosecutors and public defenders handle matters at each stage of criminal proceedings and are not assigned to particular bail judges.

[10] There are two potential complications with the judge rotation systems used in our setting. First, most defendants in our sample have the opportunity to appeal the initial bail decision in later proceedings, which can lead to modifications of the initial bail conditions. In our sample, approximately 20 percent of defendants petition for some modification of the initial bail decision. These subsequent bail decisions will often be made by a different judge than the initial bail judge. We therefore calculate our judge instrument using the first assigned bail judge. While this may lead to a weaker first-stage relationship between pretrial release and bail judge assignment, it has the advantage of not capturing any (potential) nonrandom assignment to subsequent bail judges. The second complication is that bail judges in our sample occasionally exchange scheduled shifts to work around conflicts when one judge cannot appear in court that day. This practice leads to some modest differences in the probability that particular judges are assigned to a specific day-of-the-week or specific shift time. We therefore account for both time and shift fixed effects when calculating judge leniency.

## II. Data

### A. *Data Sources and Sample Construction*

Our empirical analysis uses court data from Philadelphia and Miami-Dade merged to tax data from the Internal Revenue Service (IRS). Online Appendix B contains relevant information on the cleaning and coding of the variables used in our analysis. This section summarizes the most relevant information from the online Appendix.

In Philadelphia, court records are available for the Pennsylvania Court of Common Pleas and the Philadelphia Municipal Court for all defendants arrested and charged between 2007–2014. In Miami-Dade, court records are available for the Miami-Dade County Criminal Court and Circuit Criminal Court for all defendants arrested between 2006–2014. For both jurisdictions, the raw court data have information at the charge-, case-, and defendant-level. The charge-level data include information on the original arrest charge, the filing charge, and the final disposition charge. We also have information on the severity of each charge based on state-specific offense grades, the outcome for each charge, and the punishment for each guilty charge.

The case-level data include information on attorney type, arrest date, and the date of and judge presiding over each court appearance from bail to sentencing. Importantly, the case-level data also include information on bail type, bail amount when monetary bail was set, and whether bail was met. Case-level data from Philadelphia also allow us to measure whether a defendant received a subsequent bail modification, failed to appear in court for a required proceeding (as proxied by the issuance of a bench warrant or the holding of a bench warrant hearing), or absconded from the jurisdiction. Finally, the defendant-level data include information on each defendant's name, gender, ethnicity, date of birth, and zip code of residence. The presence of unique defendant identifiers allows us to measure both the number of prior offenses and any recidivism in the same county during our sample period.[11]

We make three sample restrictions to the court data. First, we drop the handful of cases with missing bail judge information as we cannot measure judge leniency for these individuals. Second, we drop the 30 percent of defendants in Miami-Dade who never have a bail hearing because they post bail immediately following arrest and booking. Third, we drop all weekday cases in Miami-Dade. Recall that in Miami-Dade, bail judges are assigned on a rotating basis only on the weekends. In contrast, bail judges are assigned on a rotating basis on all days in Philadelphia. The analysis sample contains 328,492 cases from 172,407 unique defendants in Philadelphia and 93,358 cases from 65,820 unique defendants in Miami-Dade.

To explore the impact of pretrial release on subsequent formal sector employment, tax filing behavior, and the receipt of social insurance, we match these court records to administrative tax records at the IRS. The IRS data include every individual who has ever acquired a social security number (SSN), including those who are institutionalized. Information on formal sector earnings and employment comes either from annual W-2s issued by employers and/or from tax returns filed by individual taxpayers. Individuals with no W-2s or self-reported income in any particular

[11] In our main results, we include all cases for each defendant. In robustness checks, we show that our results are larger and more precisely estimated if we restrict the sample to each defendant's first observed case.

year are assumed to have had no earnings in that year. Individuals with zero earnings are included in all regressions throughout the paper to capture any effects of pretrial release on the extensive margin. We define an individual as being employed in the formal labor sector if W-2 earnings are greater than zero in a given year. We focus on the W-2 measure because it provides a consistent measure of individual wage earnings for both filers and non-filers.

To measure total household earnings, we use adjusted gross income (AGI) based on income from all sources (wages, interest, self-employment, UI benefits, etc.) as reported on the individual's tax return. For individuals who did not file a tax return in a given year, we impute AGI to equal the individual's W-2 earnings plus UI income reported by the state UI agency following Chetty, Friedman, and Rockoff (2014). We define an individual as having any income if AGI is greater than zero in a given year. All dollar amounts are in terms of year 2013 dollars and reported in thousands of dollars. We top- and bottom-code earnings in each year at the ninety-ninth and first percentiles, respectively, to reduce the influence of outliers. To increase precision, we typically use the average (inflation indexed) annual individual and household income from the first two full years after the bail hearing, and average from the third and fourth years after the bail hearing, as outcome measures.

The IRS data also include information on Unemployment Insurance (UI) from information returns filed with the IRS by state UI agencies, and information on the Earned Income Tax Credit (EITC) claimed by the taxpayer on his or her return. Following the earnings measure, we use the average (inflation indexed) receipt of UI and EITC earnings from the first two full years, and average from the third and fourth years after the bail hearing, as outcome measures.

We match the court data to administrative tax data from the IRS using first and last name, date of birth, gender, zip code, and state of residence. Online Appendix B provides details on the match procedure used. In brief, defendants were matched to Social Security records on the basis of their date of birth, gender, and the first four letters of their last name. Duplicate matches were iteratively pruned based on first name, state of residence, and zip code, and any remaining duplicates were dropped from the sample. An individual who never files a tax return and for whom an information return is never filed will generally be excluded from our sample for the analyses that rely on the IRS data. Because the filing of tax and information returns may be related to pretrial release, we restrict the matching process to tax information submitted before the year of the defendant's arrest.

Our match rate in Philadelphia is 81 percent and our match rate in Miami-Dade is 73 percent. Our match rates are higher than match rates in most prior studies linking criminal court records to administrative UI records using name, date of birth, and social security number, which typically range around 60 to 70 percent (Travis, Western, and Redburn 2014). Importantly, the probability of being matched to the IRS data is not significantly related to judge leniency (see Table 3). For outcomes contained in the IRS data, we limit our estimation sample to these matched cases.

## B. *Descriptive Statistics*

Table 1 reports summary statistics for our estimation sample. We present summary statistics for those who are initially detained pretrial and those who are initially

Table 1—Descriptive Statistics

| | Initial bail decision | |
|---|---|---|
| | Detained (1) | Released (2) |
| *Panel A. Bail type* | | |
| Release on recognizance | 0.018 | 0.367 |
| Nonmonetary bail | 0.038 | 0.218 |
| Monetary bail | 0.944 | 0.414 |
| Bail amount ($ thousands) | 48.061 | 12.447 |
| *Panel B. Subsequent bail outcomes* | | |
| Bail modification petition | 0.434 | 0.071 |
| Released in 14 days | 0.099 | 1.000 |
| Released before trial | 0.411 | 1.000 |
| *Panel C. Defendant characteristics* | | |
| Male | 0.877 | 0.785 |
| White | 0.383 | 0.424 |
| Black | 0.607 | 0.556 |
| Age at bail decision | 33.926 | 33.469 |
| Prior offense in past year | 0.355 | 0.200 |
| Baseline earnings | 4.524 | 7.223 |
| Baseline employed | 0.320 | 0.423 |
| Baseline any income | 0.772 | 0.814 |
| *Panel D. Charge characteristics* | | |
| Number of offenses | 3.715 | 2.497 |
| Felony offense | 0.625 | 0.326 |
| Misdemeanor only | 0.375 | 0.674 |
| Any drug offense | 0.283 | 0.420 |
| Any DUI offense | 0.025 | 0.116 |
| Any violent offense | 0.292 | 0.191 |
| Any property offense | 0.343 | 0.185 |
| *Panel E. Outcomes* | | |
| Any guilty offense | 0.578 | 0.486 |
| Guilty plea | 0.441 | 0.207 |
| Any incarceration | 0.300 | 0.145 |
| Failure to appear in court | 0.121 | 0.179 |
| Rearrest in 0–2 years | 0.462 | 0.398 |
| Earnings ($ thousands) in 1–2 years | 5.224 | 7.911 |
| Employed in 1–2 years | 0.378 | 0.509 |
| Any income in 1–2 years | 0.458 | 0.522 |
| Earnings ($ thousands) in 3–4 years | 5.887 | 8.381 |
| Employed in 3–4 years | 0.378 | 0.483 |
| Any income in 3–4 years | 0.461 | 0.508 |
| Observations | 186,938 | 234,127 |

*Notes:* This table reports descriptive statistics for the sample of defendants from Philadelphia and Miami-Dade counties. Data from Philadelphia are from 2007–2014 and data from Miami-Dade are from 2006–2014. Information on ethnicity, gender, age, and criminal outcomes is derived from court records. Information on earnings, employment, and income is derived from the IRS data and is only available for the 77 percent of the criminal records matched to these data. See the online data Appendix for additional details on the sample and variable construction.

released pretrial. We measure initial pretrial release based on whether a defendant is released within the first three days of the bail hearing for two reasons. First, policy advocates have argued that the adverse effects of pretrial detention start as early as three days and, as a result, recent policy initiatives have focused on this time period.[12] Second, three days is the margin over which the initial bail judge is most likely to affect pretrial detention. Following the initial bail hearing, defendants have the opportunity to petition for a bail modification that could result in a different bail judge making a different detention decision. In Section IVF, we explore the robustness of our results to alternative measures of pretrial release, including a measure of ever being released before trial.

Panel A of Table 1 provides summary statistics on bail decisions in our setting. Among defendants who are released pretrial within the first three days, 36.7 percent are released ROR, 21.8 percent are released on nonmonetary bail, and 41.4 percent are released on monetary bail with an average bail amount of $12,447 and median bail amount of $5,000. In contrast, among those who are detained for at least 3 days, 94.4 percent are detained on monetary bail with an average bail amount of $48,061 and median bail amount of $7,500.

Panel B presents subsequent bail outcomes by three-day detention status. Among defendants who are detained for at least 3 days after the bail hearing, 43.4 percent petition for bail modification, 9.9 percent are released within 14 days, and 41.1 percent are released at some point prior to case disposition. In contrast, among defendants released within three days of the bail hearing, 7.1 percent petition for bail modification.

Panel C presents demographic characteristics of defendants in our sample. In our sample, 38.3 percent of initially detained defendants are white and 60.7 percent are black. Among initially released defendants, 42.4 percent are white and 55.6 percent are black. Initially detained defendants are more likely to be male than female, and more likely to have a prior offense in the past year. On average, both initially detained and initially released defendants are approximately 34 years of age at the time of bail. Panel C also presents selected baseline labor market outcomes by three-day detention status. Among defendants detained for at least three days, 32.0 percent are employed in the year prior to arrest, 77.2 percent have any income, and the average annual income is $4,524. Among defendants released within 3 days, 42.3 percent are employed in the year prior to arrest, 81.4 percent have any income, and the average annual income is $7,223.

Panel D presents offense characteristics of defendants in our sample. Initially detained defendants are arrested and charged with more offenses and are more likely to be charged with violent or property offenses. Specifically, the average detained defendant is charged with 3.7 offenses compared to 2.5 offenses for released defendants. Among initially detained defendants, 29.2 percent are charged with a violent offense and 34.3 percent are charged with a property offense. In contrast, only 19.1 percent of initially released defendants are charged with a violent offense and 18.5 percent are charged with a property offense. In general, initially released defendants are substantially less likely to be charged with felonies compared to initially detained defendants.

Finally, panel E presents case outcomes, future crime, and labor market outcomes by three-day detention status. In our sample, 57.8 percent of initially detained

[12] See, for example, the 3DaysCount project at the Pretrial Justice Institute (http://projects.pretrial.org/3dayscount/).

defendants are found guilty of at least one charge compared to 48.6 percent of initially released defendants. Forty-four percent of initially detained defendants plead guilty compared to just 20.7 percent of initially released defendants.[13] Initially detained defendants are also 15.5 percentage points more likely to be incarcerated compared to initially released defendants.

Defendants released within three days are more likely to fail to appear in court, with 17.9 percent of initially released defendants failing to appear compared to 12.1 percent of initially detained defendants. In terms of future crime, among defendants who we observe for two full years post-arrest, defendants detained for at least three days are more likely to be rearrested compared to defendants released within three days, with 46.2 percent of initially detained defendants rearrested compared to 39.8 percent of initially released defendants.

In terms of labor market outcomes, initially released defendants earn substantially more in the two years after the bail hearing compared to initially detained defendants and are more likely to be employed. In our sample, 37.8 percent of initially detained defendants are employed compared to 50.9 percent of initially released defendants. Given these low rates of employment, annual wage earnings of all defendants are also low, with initially detained defendants making \$5,224 in reported earnings compared to \$7,911 for initially released defendants. Initially released defendants are also more likely to receive any income in the first two years after the bail hearing compared to initially detained defendants. Differences in earnings outcomes of initially released and detained defendants also persist three to four years after the bail hearing. During this time period, 37.8 percent of initially detained defendants are employed in the formal labor market compared to 48.3 percent of initially released defendants, with initially detained defendants making annual reported earnings of \$5,887 compared to \$8,381 for initially released defendants.

Additional summary statistics by mutually exclusive bail types and defendant and case characteristics are presented in online Appendix Tables A1–A4. We find that defendants with a prior offense, black defendants, defendants who are nonemployed, and defendants from zip codes with below-median incomes are substantially more likely to be initially detained before trial than their respective counterparts. These more disadvantaged defendants also have worse case and labor market outcomes following the bail hearing.

## III. Research Design

*Overview.*—For individual $i$ and case $c$, consider a model that relates outcomes such as future crime to an indicator for whether the individual was released within the first three days, $Released_{ic}$:

$$Y_{ict} = \beta_0 + \beta_1 Released_{ic} + \beta_2 \mathbf{X}_{ict} + \varepsilon_{ict}, \tag{1}$$

[13] In a representative sample of adjudicated felony defendants in the 75 largest counties in 2009, 66 percent were found guilty, 64 percent pled guilty, and 34 percent were not convicted (Reaves 2013). In our sample of both felony and misdemeanor defendants, among adjudicated cases, 56 percent were found guilty, 33 percent pled guilty, and 44 percent were not convicted. Our sample has lower conviction and plea rates than the representative sample likely because we include misdemeanor defendants and because Philadelphia has one of the nation's lowest rates of convictions and guilty pleas given its wide use of bench trials.

where $Y_{ict}$ is the outcome of interest for individual $i$ in case $c$ in year $t$, $\mathbf{X}_{ict}$ is a vector of case- and defendant-level control variables, and $\varepsilon_{ict}$ is an error term. The key problem for inference is that OLS estimates of equation (1) are likely to be biased by the correlation between pretrial release and unobserved defendant characteristics that are correlated with the outcomes. For example, bail judges may be more likely to detain defendants who have the highest risk of committing a new crime in the future. In this scenario, OLS estimates will be biased toward a finding that pretrial release lowers future crime.

To address this issue, we estimate the causal impact of pretrial release using a measure of the tendency of a quasi-randomly-assigned bail judge to release a defendant pretrial as an instrument for release. In this specification, we interpret any difference in the outcomes for defendants assigned to more or less lenient bail judges as the causal effect of the change in the probability of pretrial release associated with judge assignment. This empirical design identifies the local average treatment effect (LATE), i.e., the causal effect of bail decisions for individuals on the margin of being released before trial.

*Instrumental Variable Calculation.*—We construct our instrument using a residualized, leave-out judge leniency measure that accounts for case selection following Dahl et al. (2014). We use this residualized measure of judge leniency for two main reasons. First, because the judge assignment procedures in Philadelphia and Miami-Dade are not truly random as in other settings, selection may impact our estimates if we used a simple leave-out mean to measure judge leniency following the previous literature (e.g., Kling 2006, Aizer and Doyle 2015). For example, bail hearings following DUI arrests disproportionately occur in the evenings and on particular days of the week, leading to case selection. If certain bail judges are more likely to work evening or weekend shifts due to shift substitutions, the simple leave-out mean will be biased. The use of a residualized measure of judge leniency accounts for this kind of potential case selection.

Second, this approach controls for differences across courts (Miami and Philadelphia) in both defendant characteristics and leniency of bail judges. In robustness checks, we also present results using a non-residualized version of our judge leniency measure controlling for court-by-time fixed effects and find very similar results.[14]

Specifically, given the rotation systems in both counties, we account for court-by-bail year-by-bail day of week fixed effects and court-by-bail month-by-bail day of week fixed effects. In Philadelphia, we add additional bail-day of week-by-bail shift fixed effects. Including these exhaustive court-by-time effects effectively limits the comparison to defendants at risk of being assigned to the same set of judges. With the inclusion of these controls, we can interpret the within-cell variation in the instrument as variation in the propensity of a quasi-randomly assigned bail judge to

[14] Online Appendix Table A5 presents randomization checks using this non-residualized judge leniency measure (still controlling for court-by-time fixed effects). The estimates suggest that this non-residualized measure is also orthogonal to defendant and case characteristics. In practice, our two-stage least squares results are nearly identical using both our residualized and non-residualized measures of judge leniency due to the fact that both measures are constructed using the same sample of cases.

release a defendant relative to the other cases seen in the same shift and/or same day of the week.

Let the residual pretrial release decision after removing the effect of these court-by-time fixed effects be denoted by

$$(2) \qquad Released^*_{ict} = Released_{ic} - \gamma \mathbf{X}_{ict} = Z_{ctj} + \varepsilon_{ict},$$

where $\mathbf{X}_{ict}$ includes the respective court-by-time fixed effects. The residual release decision, $Released^*_{ict}$, includes our measure of judge leniency $Z_{ctj}$, as well as idiosyncratic defendant level variation $\varepsilon_{ict}$.

For each case, we then use these residual bail release decisions to construct the leave-out mean decision of the assigned judge within a bail year:

$$(3) \qquad Z_{ctj} = \left(\frac{1}{n_{tj} - n_{itj}}\right)\left(\sum_{k=0}^{n_{tj}} (Released^*_{ikt}) - \sum_{c=0}^{n_{itj}} (Released^*_{ict})\right),$$

where $n_{tj}$ is the number of cases seen by judge $j$ in year $t$ and $n_{itj}$ is the number of cases of defendant $i$ seen by judge $j$ in year $t$. Effectively, we remove the residualized bail release decisions of all of a defendant's cases seen by judge $j$ in each year.

The leave-out judge measure given by equation (3) is the release rate for the first assigned judge after accounting for the court-by-time fixed effects. This leave-out measure is important for our analysis because regressing outcomes for defendant $i$ on our judge leniency measure without leaving out the data from defendant $i$ would introduce the same estimation errors on both the left- and right-hand side of the regression and produce biased estimates of the causal impact of being released pretrial. In our two-stage least-squares results, we use our predicted judge leniency measure, $Z_{ctj}$, as an instrumental variable for whether the defendant is released pretrial.[15]

In our main results, we calculate the instrument across all case types (i.e., both felonies and misdemeanors), but allow the instrument to vary across years in order to capture the fact that judge release decisions evolve over time. Not surprisingly, our residualized judge leniency measure is correlated across years, but the correlation between any two years falls as the distance between the two years increases (see online Appendix Table A6). In practice, judge leniency in the current year is the best predictor of bail decisions in that year. In online Appendix Table A7, we find that while future and past decisions still contain some predictive value, judge leniency calculated in the current year is by far the most predictive of pretrial release decisions in that year. In robustness checks, we present results that use a measure of judge leniency that pools case decisions from all years and results that allow judge tendencies to vary by case severity and by crime type.

[15] Algebraically, the leave-out mean measure is equivalent to a judge fixed effect estimated in a leave-out regression estimated in each year. Our leave-one-out procedure is essentially a reduced-form version of jackknife IV, which is recommended when the number of instruments (the judge fixed effects) is likely to increase with sample size (Stock, Wright, and Yogo 2002, Kolesár et al. 2015). Results using a full set of judge fixed effects as instruments are presented in robustness checks.

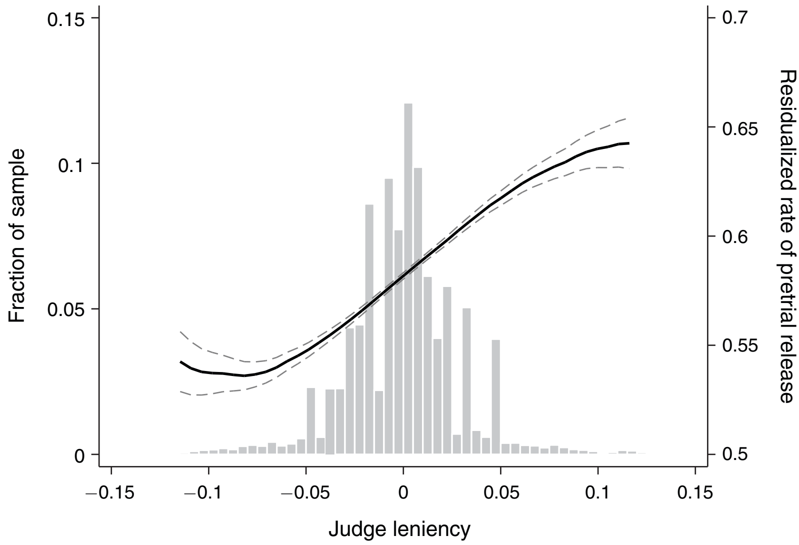


FIGURE 1. DISTRIBUTION OF JUDGE LENIENCY MEASURE AND FIRST STAGE

*Note:* This figure reports the distribution of the judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III.

*Judge Variation.*—Figure 1 presents the distribution of our residualized judge leniency measure for pretrial release at the judge-by-year level. Our sample includes 9 total bail judges in Philadelphia and 170 total bail judges in Miami-Dade. In any given year, there are 6 bail judges serving in Philadelphia and approximately 60 bail judges serving in Miami-Dade. In Philadelphia, the median number of cases per judge is 35,128 during the sample period of 2007–2014, with the median judge-by-year cell including 6,748 cases. All judge-by-year cells in Philadelphia also have more than 600 cases. In Miami-Dade, the median number of cases per judge is 507 during the sample period of 2006–2014, with the median judge-by-year cell including 181 cases. Over 95 percent of judge-by-year cells in Miami-Dade also have more than 50 cases.

Controlling for our vector of court-by-time effects, the judge release measure ranges from −0.156 to 0.175 with a standard deviation of 0.030. In other words, moving from the least to most lenient judge increases the probability of pretrial release by 33.1 percentage points, a 59.1 percent change from the mean three-day release rate of 56.0 percentage points.

The variation in our judge leniency measure comes from several potential sources. In practice, a judge determines whether a defendant is released pretrial through a combination of different bail decisions (see panel A of Table 1). Some judges may release defendants through ROR. Others may release defendants through conditional nonmonetary release. Finally, some judges may impose monetary bail that a defendant is able to post to secure his or her release. Online Appendix Figure A3 presents the distribution of residualized judge leniency for these other bail margins and shows substantial variation across judges in the use of each bail type. In our preferred specification, we collapse these various bail decisions into a binary decision of whether the defendant is released within three days of the bail hearing because it captures a margin of particular policy relevance. Section IVF explores the impact of other margins such as being assigned monetary bail.[16]

We use the variation in judge leniency described above to instrument for pretrial detention to identify the local average treatment effect of pretrial detention for defendants whose initial detention outcomes are altered by judge assignment. The conditions necessary to interpret these two-stage least squares estimates as the causal impact of pretrial detention are: (i) that judge assignment is associated with pretrial detention, (ii) that judge assignment only impacts defendant outcomes through the probability of being detained, and (iii) that defendants released by a strict judge would also be released by a lenient one. We now consider whether each of these conditions holds in our data.

*First Stage.*—To examine the first-stage relationship between bail judge leniency and whether a defendant is initially released pretrial (*Released*), we estimate the following equation for individual $i$ and case $c$, assigned to judge $j$ at time $t$ using a linear probability model:

$$Released_{ictj} = \alpha_0 + \alpha_1 Z_{ctj} + \alpha_2 \mathbf{X}_{ict} + \varepsilon_{ict}, \tag{4}$$

where the vector $\mathbf{X}_{ict}$ includes court-by-time fixed effects. As described previously, $Z_{ctj}$ are leave-out (jackknife) measures of judge leniency that are allowed to vary across years. We obtain similar results using a probit model, which is unsurprising given that the mean three-day pretrial release rate is 0.556 and far from zero or one. Robust standard errors are two-way clustered at the individual and judge level.

Figure 1 provides a graphical representation of the first-stage relationship between our residualized measure of judge leniency and the probability of pretrial release controlling for our exhaustive set of court-by-time fixed effects, overlaid over the distribution of judge leniency. The graph is a flexible analog to equation (4), where we plot a local linear regression of actual individual pretrial release against judge leniency. The individual rate of pretrial release is monotonically, and approximately linearly, increasing in our leniency measure. A 10 percentage point increase in the residualized judge's release rate in other cases is associated with an approximately 7 percentage point increase in the probability that an individual is released before trial.

Panel A of Table 2 presents formal first-stage results from equation (4). Column 1 of Table 2 presents the mean three-day pretrial release rate. Column 2 begins by reporting results only with court-by-time fixed effects. Column 3 adds our baseline crime and defendant controls: race, gender, age, whether the defendant had a prior

[16] To determine which bail decisions are most predictive of whether a defendant is released pretrial, we regress pretrial release on each residualized judge leniency measure separately calculated for ROR, nonmonetary bail, monetary bail, and bail amount (including zeros). See online Appendix Table A8. We find that defendants assigned to judges who are more likely to use conditional nonmonetary bail are more likely to be released before trial. Conversely, defendants assigned to judges who are more likely to use monetary bail and assign higher monetary bail amounts are less likely to be released pretrial. In contrast, we find no significant relationship between our residualized judge leniency measure for ROR and the probability of pretrial release. In combination, these results suggest that defendants on the margin of pretrial release are those for whom judges disagree about the appropriateness of conditional nonmonetary bail versus monetary bail.

TABLE 2—JUDGE LENIENCY AND PRETRIAL RELEASE

| | Sample mean | Judge leniency | |
|---|---|---|---|
| | (1) | (2) | (3) |
| *Panel A. Initial release* | | | |
| Released in 3 days | 0.556 | 0.639 | 0.641 |
| | (0.497) | (0.063) | (0.062) |
| *Panel B. Subsequent bail outcomes* | | | |
| Bail modification petition | 0.208 | −0.407 | −0.407 |
| | (0.406) | (0.058) | (0.052) |
| Released in 14 days | 0.600 | 0.629 | 0.632 |
| | (0.490) | (0.053) | (0.052) |
| Released before trial | 0.738 | 0.496 | 0.496 |
| | (0.440) | (0.032) | (0.029) |
| Court × time fixed effects | — | Yes | Yes |
| Baseline controls | — | No | Yes |
| Observations | 421,065 | 421,065 | 421,065 |

*Notes:* This table reports first-stage results. The regressions are estimated on the sample as described in the notes to Table 1. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. Column 1 reports the mean and standard deviation of the dependent variable. Column 2 reports results controlling for our full set of court-by-time fixed effects. Column 3 adds baseline controls: defendant race, defendant gender, defendant age, whether the defendant had a prior offense within the past year, number of offenses, indicators for whether the defendant is arrested for a drug, DUI, violent, or property offense, whether the most serious offense is a felony, whether the defendant was matched to the IRS data, baseline individual wages, baseline household wages, baseline UI, baseline EITC, baseline tax filing status, baseline employment, baseline any UI, baseline any EITC, baseline any income, and indicators for missing characteristics. Robust standard errors two-way clustered at the individual and judge level are reported in parentheses in columns 2 and 3.

offense in the past year, the number of charged offenses, indicators for crime type (drug, DUI, property, violent, other) and crime severity (felony or misdemeanor), and indicators for missing characteristics. Column 3 also adds our baseline IRS controls for the year prior to bail: tax filing status, the amount of reported W-2 earnings, household income, UI, and EITC, as well as indicators for any W-2 earnings, household income, UI, and EITC, and indicators for missing IRS data.

Consistent with Figure 1, we find that our residualized judge instrument is highly predictive of whether a defendant is released pretrial. Including controls in column 3 does not change the magnitude of the estimated first-stage effect, consistent with the quasi-randomness of bail judge assignment. With all controls (column 3), our results show that a defendant assigned to a bail judge that is 10 percentage points more likely to release a defendant pretrial is 6.4 percentage points more likely to be released within three days.

The probability of pretrial release does not increase one-for-one with our measure of judge leniency, likely because of measurement error that attenuates the effect toward zero. For instance, judge leniency may drift over the course of the year or fluctuate with case characteristics, reducing the accuracy of our leave-one-out measure. Nevertheless, the results from Figure 1 and Table 2 confirm that judge leniency is highly predictive of release outcomes in our setting.

Panel B of Table 2 presents additional first-stage results on subsequent bail outcomes. We find that a defendant assigned to a bail judge that is 10 percentage points more likely to release a defendant pretrial is 4.1 percentage points less likely to petition for bail modification, 6.3 percentage points more likely to be released within 14 days of the bail hearing, and 5.0 percentage points more likely to ever be released before trial. These results indicate that the bail decision made by the first assigned bail judge is extremely persistent.

*Instrument Validity.*—Two additional conditions must hold to interpret our two-stage least squares estimates as the local average treatment effect (LATE) of initial pretrial release: (i) bail judge assignment only impacts defendant outcomes through the probability of pretrial release and (ii) the impact of judge assignment on the probability of pretrial release is monotonic across defendants.

Table 3 verifies that assignment of cases to bail judges is random after we condition on our court-by-time fixed effects. The first column of Table 3 uses a linear probability model to test whether case and defendant characteristics are predictive of pretrial release. These estimates capture both differences in the bail conditions set by the bail judges and differences in these defendants' ability to meet the bail conditions. We control for court-by-time fixed effects and two-way cluster standard errors at the individual and judge level. We find that male defendants are 11.8 percentage points less likely to be released pretrial compared to similar female defendants, a 21.1 percent decrease from the mean pretrial release rate of 56.0 percent. Black defendants are 4.0 percentage points less likely to be released compared to white defendants, a 7.1 percent decrease from the mean. Defendants with a prior offense in the past year are 15.5 percentage points less likely to be released compared to defendants with no prior offense, a 27.7 percent decrease. Additionally, defendants arrested for felonies are 25.6 percentage points less likely to be released than those arrested for misdemeanors, a 45.7 percent decrease. Finally, individuals who are matched to IRS records, and defendants with higher baseline earnings, UI benefits, EITC benefits, and baseline employment status are more likely to be released pretrial. Column 2 assesses whether these same case and defendant characteristics are predictive of our judge leniency measure using an identical specification. We find evidence that bail judges of differing tendencies are assigned very similar defendants (joint $p$-value $= 0.78$).

Nevertheless, the exclusion restriction could also be violated if bail judge assignment impacts future outcomes through channels other than pretrial release. For example, it is possible that there are independent effects of the conditions imposed by bail judges. If judge leniency impacts future outcomes through any other channels, then the resulting LATE would incorporate any additional impacts associated with judge assignment. The assumption that judges only systematically affect defendant outcomes through pretrial release is fundamentally untestable, and our estimates should be interpreted with this potential caveat in mind. However, we argue that the exclusion restriction assumption is reasonable in our setting. Recall that in both Philadelphia and Miami-Dade, a separate judge, assigned through a different process, takes over the subsequent trial and sentencing stages. All other court actors such as the prosecutor and public defender are also assigned through a different process. These institutional characteristics make it unlikely that the

TABLE 3—TEST OF RANDOMIZATION

| | Pretrial release (1) | Judge leniency (2) |
|---|---|---|
| Male | −0.11781 | 0.00007 |
| | (0.00716) | (0.00015) |
| Black | −0.03941 | 0.00003 |
| | (0.00362) | (0.00017) |
| Age at bail decision | −0.01287 | −0.00005 |
| | (0.00236) | (0.00006) |
| Prior offense in past year | −0.15492 | 0.00019 |
| | (0.00739) | (0.00012) |
| Number of offenses | −0.02409 | 0.00000 |
| | (0.00120) | (0.00002) |
| Felony offense | −0.25575 | 0.00005 |
| | (0.01821) | (0.00010) |
| Any drug offense | 0.12528 | 0.00013 |
| | (0.00909) | (0.00019) |
| Any DUI offense | 0.10966 | 0.00019 |
| | (0.01679) | (0.00024) |
| Any violent offense | −0.01740 | 0.00003 |
| | (0.01838) | (0.00017) |
| Any property offense | 0.01097 | −0.00011 |
| | (0.01688) | (0.00016) |
| Matched to IRS data | 0.00868 | −0.00002 |
| | (0.00194) | (0.00012) |
| Baseline earnings | 0.00113 | −0.00001 |
| | (0.00009) | (0.00000) |
| Baseline UI | 0.00279 | −0.00001 |
| | (0.00048) | (0.00002) |
| Baseline EITC | 0.01233 | 0.00002 |
| | (0.00087) | (0.00008) |
| Baseline filed return | 0.05136 | −0.00018 |
| | (0.00387) | (0.00017) |
| Baseline employed | 0.02523 | 0.00019 |
| | (0.00272) | (0.00015) |
| Baseline any EITC | −0.01856 | −0.00003 |
| | (0.00418) | (0.00021) |
| Baseline any income | 0.00000 | 0.00000 |
| | (0.00000) | (0.00000) |
| Baseline any UI | 0.02431 | 0.00026 |
| | (0.00363) | (0.00029) |
| Joint *F*-test | [0.00000] | [0.78320] |
| Observations | 421,065 | 421,065 |

*Notes:* This table reports reduced form results testing the random assignment of cases to bail judges. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. Column 1 reports estimates from an OLS regression of pretrial release on the variables listed and court-by-time fixed effects. Column 2 reports estimates from an OLS regression of judge leniency on the variables listed and court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge level are reported in parentheses. The *p*-value reported at the bottom of columns 1 and 2 is for an *F*-test of the joint significance of the variables listed in the rows with the standard errors two-way clustered at the individual and judge-by-year level. See the online data Appendix for additional details on the sample and variable construction.

assignment of a bail judge is correlated with the assignment of other criminal justice actors, who may independently affect defendant outcomes.[17] Finally, unlike sentencing judges who impose multiple treatments such as incarceration, probation, and fines (Mueller-Smith 2015), bail judges exclusively handle one decision, limiting the potential channels through which they could affect defendants. In robustness checks, we partially explore potential threats to the exclusion restriction, finding no evidence that this identifying assumption is violated.

To the extent that the exclusion restriction is violated, our reduced form estimates can be interpreted as the causal impact of being assigned to a more or less lenient bail judge. These reduced form results are available in online Appendix Table A9. Our reduced form estimates are very similar to the two-stage least estimates throughout, consistent with the strong first-stage relationship between the propensity of the assigned judge to release a defendant pretrial and one's own detention outcome.

The second condition needed to interpret our estimates as the LATE of initial pretrial release is that the impact of judge assignment on the probability of pretrial release is monotonic across defendants. In our setting, the monotonicity assumption requires that individuals released by a strict judge would also be released by a more lenient judge, and that individuals detained by a lenient judge would also be detained by a stricter judge. If the monotonicity assumption is violated, our two-stage least squares estimates would still be a weighted average of marginal treatment effects, but the weights would not sum to one (Angrist, Imbens, and Rubin 1996, Heckman and Vytlacil 2005). The monotonicity assumption is therefore necessary to interpret our estimates as a well-defined LATE. The bias away from this LATE is an increasing function of the number of individuals for whom the monotonicity assumption does not hold and the difference in the marginal treatment effects for those individuals for whom the monotonicity assumption does and does not hold. The amount of bias is also a decreasing function of the first-stage relationship described by equation (4) (Angrist, Imbens, and Rubin 1996).

An implication of the monotonicity assumption is that the first-stage estimates should be non-negative for all subsamples. Online Appendix Table A10 and online Appendix Table A11 present these first-stage results separately by crime severity, crime type, prior criminal history, race, baseline employment, and above and below median zip code income using the full sample of cases to calculate our measure of judge leniency. In panel A, we find that our residualized measure of judge leniency is consistently positive and sizable in all subsamples, in line with the monotonicity assumption. In panel B, we also find that our additional first-stage results are consistently same-signed and sizable across all subsamples.

Online Appendix Figure A4 further explores how judges treat cases of observably different defendants by plotting our residualized judge leniency measures calculated separately by race, offense type, offense severity, prior criminal history, employment

[17] For example, our exclusion restriction could be violated if the inability to post monetary bail is considered during the appointment of a public defender. Generally, eligibility for a public defender is determined based solely on income, although it is possible that the amount of bail paid may be a factor in determining eligibility for appointment of a public defender in Florida. See Fl. Stat. §27.52. However, in unreported results, we find that our judge leniency measure is uncorrelated with having a public defender. In addition, we find in unreported results that our judge leniency measure is uncorrelated with the next assigned courtroom (49 total), suggesting that bail judge assignment is uncorrelated with the assignment of subsequent judges.

status, and zip code income. Each plot reports the coefficient and standard error from an OLS regression relating each measure of judge leniency. Consistent with our monotonicity assumption, we find that the slopes relating the relationship between judge leniency in one group and judge leniency in another group are non-negative, suggesting that judge tendencies are similar across observably different defendants and cases. In robustness checks, we also relax the monotonicity assumption by letting our leave-out measure of judge leniency differ across case characteristics following Mueller-Smith (2015).[18]

*Understanding Our LATE.*—Our two-stage least squares estimates represent the LATE for defendants who would have received a different bail decision had their case been assigned to a different judge. To better understand this LATE, we characterize the number of compliers and their characteristics following the approach developed by Abadie (2003) and extended by Dahl et al. (2014). See online Appendix C for a more detailed description of these calculations.

We find that approximately 13 percent of defendants in our sample are "compliers," meaning that they would have received a different initial bail outcome had their case been assigned to the most lenient judge instead of the most strict judge. In comparison, 36 percent of our sample are "never takers," meaning that they would be initially detained by all judges, and 51 percent are "always takers," meaning that they would be initially released pretrial regardless of the judge assigned to the case. Compliers in our sample are 14 percentage points more likely to be charged with a misdemeanor, 16 percentage points more likely to be charged with nonviolent offenses, and 4 percentage points more likely to have a prior offense in the past year compared to the average defendant. Compliers are not systematically different from the average defendant by race or baseline employment status, however.

## IV. Results

In this section, we examine the effects of initial pretrial release using the judge IV strategy described above. We first analyze the effects of initial pretrial release on case outcomes, before turning to its effects on pretrial flight, future crime, and labor market outcomes.

[18] In a related paper using bail data from Philadelphia, Stevenson (2016) argues that there are economically important violations of the monotonicity assumption in our setting. In contrast, we do not find systematic evidence of violations of monotonicity, nor do we believe any potential bias is large given our strong first stage. Moreover, there are a number of results within Stevenson (2016) that suggest any bias from violations of the monotonicity assumption is likely to be small. For example, Stevenson (2016) finds similar LATEs across various subsamples, indicating that LATEs may not be different between compliers and defiers (and thus there would be no bias from a violation of monotonicity). In addition, results using judge fixed effects with and without interactions with crime and defendant characteristics are similar and same-signed in Stevenson (2016), again indicating that any potential monotonicity violations would lead to very little bias in practice. In contrast, Mueller-Smith (2015) finds economically significant biases from the violation of the monotonicity assumption at the sentencing stage, as indicated by the IV results using judge fixed effects without interactions yielding an opposite-signed result from IV results using judge instruments interacted with crime type.

Table 4—Pretrial Release and Criminal Outcomes

| | Detained mean | OLS results | | | 2SLS results | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. Case outcomes* | | | | | | |
| Any guilty offense | 0.578 | −0.072 | −0.057 | −0.046 | −0.123 | −0.140 |
| | (0.494) | (0.014) | (0.009) | (0.007) | (0.047) | (0.042) |
| Guilty plea | 0.441 | −0.188 | −0.099 | −0.082 | −0.095 | −0.108 |
| | (0.497) | (0.008) | (0.010) | (0.007) | (0.056) | (0.052) |
| Any incarceration | 0.300 | −0.161 | −0.104 | −0.110 | 0.006 | −0.012 |
| | (0.458) | (0.012) | (0.006) | (0.007) | (0.029) | (0.030) |
| *Panel B. Court process outcomes* | | | | | | |
| Failure to appear in court | 0.121 | 0.063 | 0.010 | 0.021 | 0.158 | 0.156 |
| | (0.326) | (0.004) | (0.008) | (0.007) | (0.046) | (0.046) |
| Absconded | 0.002 | 0.005 | 0.002 | 0.002 | 0.005 | 0.005 |
| | (0.045) | (0.000) | (0.000) | (0.000) | (0.004) | (0.004) |
| *Panel C. Future crime* | | | | | | |
| Rearrest in 0–2 years | 0.462 | −0.050 | −0.015 | 0.016 | 0.024 | 0.015 |
| | (0.499) | (0.011) | (0.006) | (0.005) | (0.061) | (0.063) |
| Rearrest prior to disposition | 0.155 | 0.051 | 0.066 | 0.100 | 0.192 | 0.189 |
| | (0.362) | (0.008) | (0.007) | (0.007) | (0.038) | (0.042) |
| Rearrest after disposition | 0.343 | −0.075 | −0.049 | −0.041 | −0.114 | −0.121 |
| | (0.475) | (0.006) | (0.002) | (0.003) | (0.057) | (0.055) |
| Court × time fixed effects | — | Yes | Yes | Yes | Yes | Yes |
| Baseline controls | — | No | Yes | Yes | No | Yes |
| Complier weights | — | No | No | Yes | No | No |
| Observations | 186,938 | 421,065 | 421,065 | 421,065 | 421,065 | 421,065 |

*Notes* This table reports OLS and two-stage least squares results of the impact of pre-trial release. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Two-stage least squares models instrument for pretrial detention using a judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge level are reported in parentheses in columns 2–6.

### A. *Case Outcomes*

Panel A of Table 4 presents OLS and two-stage least squares estimates of the impact of being released from jail within three days of the bail hearing on various case outcomes. Column 1 reports the dependent variable mean for defendants who are detained for at least three days pretrial. Columns 2 and 3 report OLS estimates where each column further controls for potential omitted variables to learn about the source(s) and size of any bias. Column 2 begins by reporting results only with court-by-time fixed effects. Column 3 adds our baseline crime, defendant, and IRS controls, as described previously. Column 4 reports OLS estimates reweighted so that the proportion of compliers matches the share of the estimation sample following the procedure developed by Bhuller et al. (2016).[19] Finally, columns 5 and 6

[19] Specifically, we split our estimation sample into eight mutually exclusive and collectively exhaustive subgroups based on prior criminal history and the predicted probability of incarceration, two important sources of heterogeneity as documented below. We then calculate the share of compliers in each subgroup using the procedure

report two-stage least squares results where we instrument for pretrial release within three days using the leave-out measure of judge leniency described in Section III, with and without baseline controls. Robust standard errors two-way clustered at the individual and judge level are reported throughout.

The OLS estimates show that initially released defendants have significantly better case outcomes than initially detained defendants. In all specifications, initially released defendants are significantly less likely to be found guilty of an offense, to plead guilty to a charge, and to be incarcerated following case disposition. However, the magnitudes of these OLS estimates are extremely sensitive to the addition of baseline crime controls. For example, in our OLS results with only our court-by-time fixed effects (column 2), we find that a defendant who is initially released pretrial is 18.8 percentage points less likely to plead guilty, a 42.6 percent decrease from the mean for initially detained defendants. When we add baseline controls (column 3), the magnitude of the estimate is approximately halved, dropping to 9.9 percentage points. Reweighting our estimation sample to match the sample of compliers (column 4) further decreases the size of the estimate to 8.2 percentage points. These results suggest that, at least for case outcomes, baseline controls are important for addressing potential omitted variable bias. The similarity in OLS results with and without reweighting further suggest that any differences between OLS and two-stage least squares estimates, as discussed next, are unlikely accounted for by heterogeneity in effects, at least due to observables.

The two-stage least squares estimates in columns 5 and 6 improve upon our OLS estimates by exploiting plausibly exogenous variation in initial pretrial release from the quasi-random assignment of cases to bail judges. These two-stage least squares results confirm that defendants initially released before trial have significantly better case outcomes than otherwise similar defendants who are initially detained before trial. With the full set of controls (column 6), we find that the marginal released defendant is 14.0 percentage points less likely to be found guilty, a 24.2 percent decrease from the mean, and 10.8 percentage points less likely to plead guilty, a 24.5 percent decrease from the mean. These results are consistent with the theory that pretrial release strengthens a defendant's bargaining position in plea negotiations. In online Appendix Table A12, we find that marginal released defendants are also convicted of fewer offenses, more likely to be convicted of a lesser charge, and less likely to plead guilty to time served.

We also find that the marginal released defendant is 1.2 percentage points less likely to be incarcerated after case disposition, a 4.0 percent decrease from the mean, although the estimate is not statistically significant. Large standard errors mean that the difference between the OLS and two-stage least squares estimates for incarceration is not statistically significant, however. Our small and insignificant effect on post-trial incarceration is likely because detained defendants largely plead guilty to time served and because many offenses in our sample are associated with minimal prison time. In online Appendix Table A13, we also find that pretrial release significantly reduces the number of days detained prior to disposition by

outlined in online Appendix C. The weights are calculated as the share of compliers relative to the share of the estimation sample in each subgroup.

14.1 days but has no significant effect on the number of days incarcerated after disposition. These findings suggest that pretrial release primarily reduces time spent in jail at the pretrial stage.

### B. *Failures to Appear and Future Crime*

The results described above suggest that there are significant costs of pretrial detention for defendants. However, it is also possible that pretrial detention benefits society by increasing court appearances or by reducing future crime.

Panel B of Table 4 examines the impact of initial pretrial release on flight in our Philadelphia sample, as we do not observe these measures in our Miami-Dade data. We find that initial pretrial release leads to substantial increases in failing to appear for required court appearances. Controlling for our full set of controls (column 6), we find that the marginal released defendant is 15.6 percentage points more likely to fail to appear in court, a 128.9 percent increase from the mean. The probability of fleeing from the jurisdiction also increases by 0.5 percentage points, a 250 percent increase from the initially detained defendant mean, but the estimate is not statistically significant due to the relative infrequency of this outcome. These findings indicate that initial pretrial detention reduces missed court appearances and flight, presumably through an incapacitation effect.[20]

Panel C of Table 4 presents estimates of the impact of initial pretrial release on the probability of future criminal behavior. For our future crime results, our sample is limited to the 302,862 defendants who we observe for two years following the bail hearing. We measure future crime using the probability of rearrest, but the results follow a similar pattern if we use new convictions instead. In unreported results, we find similar estimates when looking up to four years following the bail hearing although our sample size is reduced. Both with and without baseline controls, our two-stage least squares results suggest no detectable net effect on future crime up to two years after the bail hearing, although large standard errors make definitive conclusions difficult.

To better understand this null effect, we estimate the impact of initial pretrial release on crime committed before and after case disposition. Results are similar splitting pre- and post-disposition periods using the median time from arrest to disposition rather than the actual time to disposition. With all baseline controls (column 6), we find that the marginal released defendant is 18.9 percentage points more likely to be rearrested for a new crime prior to disposition, a 121.9 percent increase from the mean, but 12.1 percentage points less likely to be arrested after case disposition, a 35.3 percent decrease from the mean. In panel B of online Appendix Table A12, we find similar but less precise results on the intensive margin of recidivism—a margin that may be more relevant to some policymakers—using the number of new counts. The marginal released defendant is arrested for 1.09 more counts prior to disposition, but 0.73 fewer counts after case disposition. The net effect of

[20] In online Appendix Table A12, we also find that the marginal released defendant waits for an extra 40.9 days between bail and case disposition, a 20.8 percent increase from the mean. Increases in case disposition length may be due to speedy trial rules in both Pennsylvania and Florida, which effectively place limits on how long a defendant can be detained pretrial, and the fact that marginal released defendants may wait longer between bail and case disposition because they are less likely to plead guilty.

pretrial detention on new counts over the first two years is a statistically insignificant 0.35, although we note that the 95 percent confidence intervals include relatively large effects due to the large standard errors.

Taken together, we interpret these results as suggesting that pretrial detention has two main opposing effects on future crime. First, pretrial detention prevents new criminal activity prior to case disposition through a short-run incapacitation effect. Second, pretrial detention increases new crime after case disposition through a medium-run criminogenic effect. These latter results are consistent with Aizer and Doyle (2015), who find that juvenile incarceration increases adult incarceration, and Mueller-Smith (2015), who finds that post-conviction incarceration increases future crime.

### C. *Labor Market and Tax Administration Outcomes*

We next present estimates of the impact of initial pretrial release on formal sector earnings and engagement. Participation in the formal labor market is important for social welfare given its correlation with future criminal activity (e.g., Grogger 1998; Raphael and Winter-Ebmer 2001; Gould, Weinberg, and Mustard 2002), and because it partially proxies for consumption. Apart from direct employment effects, pretrial release may also impact defendant welfare by affecting the take-up of social safety net programs. In particular, being released before trial may strengthen defendants' ties to the formal employment sector or affect their attitudes toward the government, which may change the likelihood that they file a tax return. Because certain social benefit programs such as the EITC are only available through the tax code, changes in tax filing behavior may affect take-up of such programs. Similarly, pretrial release may affect participation in social welfare programs such as UI, which are also tied to formal sector employment.

Table 5 presents estimates of the impact of initial pretrial release on individual-level formal sector earnings and employment. For outcomes measured across the first two years after the bail hearing, our sample is limited to the 299,312 cases matched to IRS data with cases before 2014, and for outcomes measured over the third to fourth years after the bail hearing, our sample is limited to the 221,616 cases matched to IRS data with cases before 2012.

The OLS estimates in Table 5 show that initially released defendants have significantly higher formal sector earnings and employment following the bail hearing. The two-stage least squares estimates are broadly similar to the OLS estimates with baseline controls, but less precisely estimated. With our full set of baseline controls (column 6), we find that marginal released defendants are 11.3 percentage points more likely to have any income two years after bail, a 24.7 percent increase from the mean. Estimates on other outcomes in the first two years after the bail hearing are smaller and not statistically different from zero. By three to four years after the bail hearing, initially released defendants are 9.4 percentage points more likely to be employed in the formal labor sector, a 24.9 percent increase from the mean. Formal sector earnings are \$948 higher per year over the same time period, a 16.1 percent increase from the mean, and the probability of having any income is 10.7 percentage points higher, a 23.2 percent increase from the mean, broadly consistent with the more precise OLS estimates.

TABLE 5—PRETRIAL RELEASE AND LABOR MARKET OUTCOMES

| | Detained mean | OLS results | | | 2SLS results | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. Years 1–2* | | | | | | |
| Earnings ($ thousands) | 5.224 | 2.689 | 0.389 | 0.162 | 0.030 | −0.524 |
| | (15.196) | (0.073) | (0.033) | (0.056) | (1.404) | (0.966) |
| Household income ($ thousands) | 10.179 | 2.703 | 0.232 | −0.042 | 1.809 | −0.015 |
| | (22.844) | (0.162) | (0.090) | (0.083) | (1.939) | (1.439) |
| Employed | 0.378 | 0.134 | 0.050 | 0.040 | 0.065 | 0.036 |
| | (0.485) | (0.003) | (0.002) | (0.003) | (0.049) | (0.042) |
| Any income | 0.458 | 0.104 | 0.036 | 0.020 | 0.135 | 0.113 |
| | (0.498) | (0.003) | (0.002) | (0.003) | (0.073) | (0.064) |
| *Panel B. Years 3–4* | | | | | | |
| Earnings ($ thousands) | 5.887 | 2.426 | 0.199 | −0.039 | −0.005 | 0.948 |
| | (15.897) | (0.093) | (0.055) | (0.087) | (1.441) | (1.128) |
| Household income ($ thousands) | 10.922 | 2.456 | 0.020 | −0.107 | 1.090 | 0.181 |
| | (23.974) | (0.171) | (0.111) | (0.104) | (2.109) | (1.883) |
| Employed | 0.378 | 0.104 | 0.031 | 0.021 | 0.099 | 0.094 |
| | (0.485) | (0.003) | (0.002) | (0.003) | (0.053) | (0.057) |
| Any income | 0.461 | 0.090 | 0.030 | 0.023 | 0.125 | 0.107 |
| | (0.498) | (0.003) | (0.002) | (0.003) | (0.055) | (0.056) |
| Court × time fixed effects | — | Yes | Yes | Yes | Yes | Yes |
| Baseline controls | — | No | Yes | Yes | No | Yes |
| Complier weights | — | No | No | Yes | No | No |
| Observations | 144,290 | 334,943 | 334,943 | 334,943 | 334,943 | 334,943 |

*Notes:* This table reports OLS and two-stage least squares results of the impact of pretrial release. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Two-stage least squares models instrument for pretrial detention using a judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge level are reported in parentheses in columns 2–6.

A valid question is why we find a significant impact of initial pretrial release on the extensive margin of employment, but insignificant effects on the intensive margin. In online Appendix Figure A5, we plot two-stage least squares estimates and corresponding 95 percent confidence intervals of the impact of initial pretrial release on the probability of individual earnings and household income falling above various thresholds. Initially released defendants are more likely to have individual earnings above $5,000, but no more likely to have individual earnings above higher thresholds. Initially released defendants are also more likely to have household income above $10,000, but again no more likely to have household income above higher thresholds. These results suggest that pretrial release primarily affects earnings at the extreme low-end of the income distribution, with little discernible effects at other points of the distribution.[21] The results also suggest that our intensive margin

[21] It is not clear why pretrial release affects the extensive margin, but not the intensive margin, of employment. One possible explanation is that a criminal conviction can qualify defendants for specific job training and reentry services that help on the intensive margin of employment, but are relatively unhelpful on the extensive margin of employment.

Table 6—Pretrial Release and Social Benefits Take-Up

| | Detained mean | OLS results | | | 2SLS results | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. Years 1–2* | | | | | | |
| Filed return | 0.421 | 0.092 | 0.032 | 0.018 | 0.126 | 0.102 |
| | (0.494) | (0.003) | (0.002) | (0.003) | (0.054) | (0.049) |
| UI ($ thousands) | 0.283 | 0.419 | 0.211 | 0.210 | 0.142 | 0.061 |
| | (1.541) | (0.027) | (0.021) | (0.025) | (0.138) | (0.155) |
| EITC ($ thousands) | 0.331 | 0.190 | 0.094 | 0.074 | 0.208 | 0.179 |
| | (0.948) | (0.010) | (0.004) | (0.004) | (0.124) | (0.107) |
| Any UI | 0.066 | 0.068 | 0.030 | 0.028 | 0.054 | 0.037 |
| | (0.249) | (0.002) | (0.001) | (0.002) | (0.026) | (0.025) |
| Any EITC | 0.219 | 0.070 | 0.033 | 0.023 | 0.105 | 0.097 |
| | (0.413) | (0.003) | (0.002) | (0.002) | (0.062) | (0.059) |
| *Panel B. Years 3–4* | | | | | | |
| Filed return | 0.306 | 0.057 | 0.019 | 0.017 | 0.068 | 0.051 |
| | (0.461) | (0.003) | (0.001) | (0.002) | (0.032) | (0.032) |
| UI ($ thousands) | 0.245 | 0.280 | 0.158 | 0.130 | 0.279 | 0.293 |
| | (1.335) | (0.021) | (0.018) | (0.018) | (0.193) | (0.193) |
| EITC ($ thousands) | 0.357 | 0.179 | 0.091 | 0.071 | 0.281 | 0.209 |
| | (0.998) | (0.008) | (0.005) | (0.006) | (0.144) | (0.127) |
| Any UI | 0.064 | 0.055 | 0.030 | 0.024 | 0.016 | 0.013 |
| | (0.246) | (0.002) | (0.002) | (0.002) | (0.033) | (0.033) |
| Any EITC | 0.233 | 0.057 | 0.025 | 0.020 | 0.123 | 0.105 |
| | (0.423) | (0.003) | (0.002) | (0.003) | (0.050) | (0.049) |
| Court × time fixed effects | — | Yes | Yes | Yes | Yes | Yes |
| Baseline controls | — | No | Yes | Yes | No | Yes |
| Complier weights | — | No | No | Yes | No | No |
| Observations | 144,290 | 334,943 | 334,943 | 334,943 | 334,943 | 334,943 |

*Notes:* This table reports OLS and two-stage least squares results of the impact of pretrial release. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Two-stage least squares models instrument for pre-trial detention using a judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge level are reported in parentheses in columns 2–6.

estimates may be particularly noisy due to the right-skewness of the income distribution among defendants in our sample. Consistent with this explanation, we find in unreported results that total household income is significantly higher among marginal released defendants when we top-code earnings at the seventy-fifth percentile of the earnings distribution in our sample.

Table 6 presents estimates for tax filing, UI receipt, and EITC receipt: measures of formal sector engagement that are particularly welfare-relevant in our low-income population. In our two-stage least squares results with the full set of controls (column 6), we find that released defendants are 10.2 percentage points more likely to file a tax return one to two years after the bail hearing, a 24.2 percent increase from the mean. Pretrial release also increases the receipt of EITC benefits by $179 per year over the same time period, a 54.1 percent increase. Three to four years after the bail hearing, released defendants are 5.1 percentage points more likely to file a tax return, a 16.7 percent increase from the mean, and receive an additional $293 in

UI benefits and \$209 in EITC benefits per year, 119.6 and 58.5 percent increases from the mean, respectively. These results suggest that pretrial release allows individuals to remain connected to the formal sector, potentially increasing consumption, both through employment in the formal labor market and the increased take-up of social benefits that are tied to formal sector employment.

### D. *Additional IRS Outcomes*

Online Appendix Table A14 presents estimates of the impact of initial pretrial release on marriage and mobility as measured in individual tax returns. We define marriage as having a tax return that reports being married at any point in the indicated post-bail hearing years. A move is defined as having a mismatch between the zip code in the administrative court data and the zip code reported on a tax return in the indicated years. We use aggregate IRS data to code these moves as being to a higher- or lower-income zip code. Importantly, our marriage and mobility measures are missing for individuals who do not file a tax return in the relevant post-bail hearing years. As a result, our estimates for these outcomes may be biased by the 5.1 to 10.2 percentage point difference in the probability of filing a tax return for marginal released defendants (see Table 6). To explore the importance of this selection bias, we also estimate results with imputed outcomes for non-filers.

In our two-stage least squares estimates with the full set of controls (column 6), we find that initial pretrial release has no statistically significant effect on the probability of marriage at either one to two or three to four years after the bail hearing. In unreported results, we also find statistically insignificant effects if we assume that all non-filers are unmarried or assume that all non-filers are married. These results are consistent with Lopoo and Western (2005) who find that the observed negative relationship between incarceration and marriage is largely driven by a short-run incapacitation effect and that those at risk of imprisonment are extremely unlikely to marry, even in the absence of incarceration.

We find that initially released defendants are 13.3 percentage points less likely to move in the two years after the bail hearing, a 17.3 percent decrease from the mean, largely due to a decrease in moves to higher-income zip codes. This mobility estimate falls to a statistically insignificant 0.8 percentage points if we assume that all non-filers do not move, and falls to a statistically significant 5.2 percentage points if we assume that all non-filers move. Results are qualitatively similar, but not as precisely estimated, in the third to fourth years following the bail hearing.

### E. *Subsample Results*

Table 7 presents two-stage least squares subsample results by prior criminal history, an important margin given that it measures an individual's ties to the criminal sector. We find that the impacts of pretrial release are generally largest for those without a prior offense in the past year. For individuals without a recent prior offense, released defendants are 18.8 percentage points less likely to be found guilty, 14.2 percentage points less likely to plead guilty, and 15.9 percentage points more likely to have any income three to four years after the bail hearing. In contrast, almost all results for individuals with a recent prior offense are small and

TABLE 7—RESULTS BY PRIOR CRIMINAL HISTORY

| | No priors (1) | Priors (2) | $p$-value (3) |
|---|---|---|---|
| Any guilty offense | −0.188 | −0.050 | 0.136 |
| | (0.050) | (0.055) | |
| | [0.495] | [0.614] | |
| Guilty plea | −0.142 | −0.052 | 0.358 |
| | (0.057) | (0.064) | |
| | [0.280] | [0.393] | |
| Any incarceration | 0.015 | −0.054 | 0.222 |
| | (0.034) | (0.052) | |
| | [0.189] | [0.282] | |
| Failure to appear in court | 0.141 | 0.189 | 0.014 |
| | (0.052) | (0.046) | |
| | [0.149] | [0.180] | |
| Rearrest in 0–2 years | −0.016 | 0.087 | 0.383 |
| | (0.006) | (0.097) | |
| | [0.360] | [0.615] | |
| Rearrest prior to disposition | 0.178 | 0.233 | 0.805 |
| | (0.006) | (0.070) | |
| | [0.171] | [0.255] | |
| Rearrest after disposition | −0.155 | −0.053 | 0.269 |
| | (0.006) | (0.095) | |
| | [0.233] | [0.443] | |
| Employed in 1–2 years | 0.075 | −0.054 | 0.174 |
| | (0.051) | (0.076) | |
| | [0.487] | [0.360] | |
| Any income in 1–2 years | 0.173 | −0.005 | 0.075 |
| | (0.077) | (0.074) | |
| | [0.508] | [0.459] | |
| Employed in 3–4 years | 0.111 | 0.040 | 0.507 |
| | (0.063) | (0.085) | |
| | [0.465] | [0.365] | |
| Any income in 3–4 years | 0.159 | −0.003 | 0.161 |
| | (0.063) | (0.093) | |
| | [0.509] | [0.428] | |
| Court × time fixed effects | Yes | Yes | — |
| Baseline controls | Yes | Yes | — |
| Observations | 307,840 | 113,225 | — |

*Notes:* This table reports two-stage least squares results of the impact of pretrial release by defendant prior criminal history. The regressions are estimated on the judge sample as described in the notes to Table 1. The dependent variable is listed in each row. Two-stage least squares models instrument for pretrial detention using a judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. Column 3 presents $p$-values on the difference between the coefficients. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge level are reported in parentheses and the mean of the dependent variable is reported in brackets in all specifications.

imprecisely estimated. The one exception is that released defendants with a recent prior offense are significantly more likely to fail to appear in court than released defendants with no recent prior offenses.

In online Appendix Tables A15 and A16, we present additional two-stage least squares subsample results by crime severity, highest crime type, and defendant characteristics. While we caution against the strong interpretation of these subsample results given concerns about multiple hypothesis testing, there is some evidence that the results are larger for defendants charged with misdemeanor and drug offenses, although large standard errors mean that none of the differences are statistically significant. Our labor market results are also somewhat larger for individuals who were employed prior to the bail hearing, but not meaningfully different for defendants from high- and low-income zip codes. Overall, these results suggest that the social costs imposed by pretrial detention may be larger for those with more limited ties to the criminal justice system and stronger ties to the formal labor sector.

### F. *Robustness Checks*

*Threats to Exclusion Restriction.*—As discussed previously, interpreting our two-stage least squares estimates as the causal impact of pretrial release requires our judge instrument to affect defendants' outcomes only through the channel of release, rather than through an alternative channel such as the conditions of release. To further explore this issue, we estimate results that differentiate between three mutually exclusive release types: release without any conditions (ROR), release with nonmonetary conditions, and release with monetary conditions. By separately estimating these three decision margins relative to pretrial detention, we can test whether our results are driven solely by a defendant being released before trial, or by some combination of pretrial release and release conditions imposed by the bail judge. Unfortunately, our data do not allow us to identify the specific conditions of release ranging from minimal requirements, like reporting to a Pretrial Services officer, to more intensive conditions, like electronic monitoring or home confinement. In online Appendix Table A17, we first document a strong first-stage relationship between a defendant's pretrial release conditions and the assigned judge's propensity for release ROR, release with nonmonetary conditions, and release with monetary conditions, with judges independently varying across these three margins.

In online Appendix Table A18, we present OLS and two-stage least squares estimates of the impact of being released from jail within three days of the bail hearing with no conditions, with nonmonetary conditions, and with monetary conditions. Our two-stage least squares estimates show no statistically significant differences in the effect of pretrial release on any of our main outcomes across these three release types, although the magnitudes of estimates are generally larger for those released with monetary conditions. These findings indicate that it is pretrial release itself that most likely affects case outcomes, suggesting that the exclusion restriction is unlikely to be violated by release conditions, either nonmonetary or monetary, having an independent effect on outcomes. Thus, our findings indicate that previous papers estimating the impact of monetary bail on case outcomes (e.g., Gupta et al. 2016) are identifying the effect of pre-detention due to the assignment of monetary bail, not the effect of monetary bail per se.

Another potential violation of the exclusion restriction is if judges affect not only the pretrial release decision, but also the length of stay in pretrial detention. Following Aizer and Doyle (2015), we explore this concern in two ways. First, we test whether our judge leniency measure is predictive of the number of days detained conditional on being detained at all before trial. Second, we test whether a separate leave-out measure based on length of stay has any additional predictive value for the number of days detained (including zero length of stays) beyond our preferred leave-out instrument. These results are reported in online Appendix Table A19, Consistent with the exclusion restriction, we find that our preferred leave-out instrument is not predictive of the number of days detained conditional on being detained at all before trial. We also find that there is no additional explanatory value of the separate length of stay leave-out measure. In unreported results, we also find similar but imprecise results if we estimate our preferred specification separately for short versus long stays in detention, and below we show similar results for different definitions of pretrial release. Taken together, these results suggest that our main results are driven by judge variation in initial pretrial release, not judge variation in length of stay.

*Alternative Specifications.*—Online Appendix Table A20 explores the sensitivity of our main results to alternative specifications. Column 1 uses a leave-out measure of judge leniency that is allowed to differ for misdemeanors and felonies, thereby relaxing the monotonicity assumption. Column 2 uses a leave-out measure that is allowed to differ for the five mutually exclusive and collectively exhaustive crime types (drug, violent, DUI, property, and other) again relaxing the monotonicity assumption. These results are very similar to our preferred specification, indicating that the potential bias from any monotonicity violations is likely to be small in our setting. Column 3 estimates results on whether the defendant is released within 14 days of the bail hearing, and column 4 estimates results on whether the defendant is ever released pretrial. Column 5 estimates results on whether the defendant is not assigned monetary bail (i.e., is released ROR or with nonmonetary conditions). Results across all specifications are similar to our preferred specification.

Online Appendix Table A21 presents a second set of robustness checks. Column 1 uses a leave-out measure of judge leniency that is not residualized by court-by-time fixed effects. Column 2 uses a leave-out measure of judge leniency that pools cases across all years. Column 3 presents bootstrap-clustered standard errors that correct for any estimation error in both our judge leniency measure and outcome measures.[22] Column 4 uses a randomly selected subset of 25 percent of cases to calculate a leave-out measure of judge leniency that is used as an instrument in the mutually exclusive subset of cases. Column 5 calculates judge leniency based on the scheduled bail judge, which differs from the assigned bail judge approximately 30 percent of the time, and column 6 presents results using a full set of judge fixed effects as instruments (first-stage $F$-statistic $= 506.5$). Results are generally similar to our preferred specification across all alternative specifications, although some of our estimates lose statistical significance. In particular, our point estimates on rearrest post-disposition are more sensitive to alternative specifications, although large standard errors mean that the results are not statistically different across specifications.

Finally, online Appendix Table A22 presents our main results for each defendant's first observed case (column 1), the sample matched to the IRS (column 2),

[22] Specifically, we cluster bootstrap our specifications following Cameron, Gelbach, and Miller (2008). This procedure involves sampling at the judge level, with replacement, and then generating the judge leniency and outcome measures within this sampled data. We then run our two-stage least squares regressions within the sample data to calculate our standard errors. We report results from this bootstrap procedure with 500 simulations for our main results. In unreported results, we find that our first-stage results continue to be statistically significant at the 1 percent level when the standard errors are calculated using this bootstrap procedure.

Philadelphia only (column 3), and Miami-Dade only (column 4). Consistent with our subsample results from Table 7, we find larger and more precisely estimated effects in the first case sample, particularly for our labor market outcomes. Results across the other three specifications are similar to our preferred specification, although there is considerably more noise in the court-specific subsamples. None of the estimates suggest that our preferred estimates are invalid.

## V. Discussion

In this section, we tentatively explore the potential mechanisms that might explain our findings on case outcomes, future crime, and labor market outcomes.

*Case Outcomes.*—Pretrial release could affect case outcomes through at least three main channels. First, pretrial release may strengthen a defendant's bargaining position during plea negotiations. For example, it is possible that pretrial release decreases a defendant's incentive to plead guilty to obtain a faster release from jail. Along the same lines, it is also possible that pretrial release affects a defendant's ability to prepare an adequate defense or negotiate a settlement with prosecutors. For example, a defendant may have a harder time gathering exculpatory evidence if he is detained. Second, pretrial release may increase the ability of both prosecutors and defendants to strategically delay the resolution of a case, such that it could strengthen the bargaining position of both parties. The third way that pretrial release could impact conviction rates is that seeing detained defendants in jail uniforms and shackles may bias judges or jurors at trial. For example, jurors may assume that only guilty defendants are detained before trial.

While there is no conclusive evidence on this issue, two pieces of evidence suggest that our results are likely driven by changes in a defendant's bargaining position. First, as discussed previously, we find that released defendants are substantially less likely to be convicted of any offense due to a reduction in guilty pleas, not changes in conviction rates at trial where jury bias may come into play. Second, we find that those who are released pretrial receive more favorable plea deals than those who are detained. For example, we find that released defendants are substantially more likely to be convicted of a lesser charge and are convicted of fewer total offenses (online Appendix Table A11). The fact that so many of our results are driven by changes in the plea bargaining phase, and not the trial phase, suggests that pretrial release affects case outcomes primarily through changes in bargaining power. While we cannot rule out that pretrial release may affect case outcomes by increasing strategic, and potentially socially costly, delays by both parties, the fact that we find pretrial release yields case outcomes that are more favorable from the perspective of the defendant (and less favorable from the perspective of the prosecutor) suggests that our results are at least in part driven by an improvement in defendants' bargaining power.

*Future Crime.*—Pretrial release may decrease future crime following case disposition through two main channels. First, pretrial release may decrease crime if pretrial detention is criminogenic because of harsh prison conditions and negative peer effects (e.g., Chen and Shapiro 2007, Bayer, Hjalmarsson, and Pozen 2009).

Second, pretrial release can reduce future crime through an increased likelihood of employment, which subsequently discourages further criminal activity. To assess whether pretrial release reduces future crime through the channel of increased employment, we explore whether those who are more likely to be employed in the formal labor market are also those less likely to commit future crime.

In online Appendix Table A23, we present estimates of the joint probability of future crime and employment in the several years after the bail hearing. These joint estimates provide partial evidence on whether reductions in future crime are driven by defendants who are employed or whether the decline in future crime occurs independently of employment. If the decrease in future crime occurs independently of employment, we would expect to see similar reductions in future crime among those who are employed and those not employed. We find suggestive evidence that in the first two years after the bail hearing, pretrial release increases the joint probability of not being rearrested and of being employed, although our estimates are not precisely estimated. Similarly, we find an increase in the joint probability of not being rearrested and being employed in the third to fourth years after the bail hearing. These results indicate that decreases in future crime may be driven by the same defendants who are employed, suggesting that pretrial release may decrease future crime through the channel of increased labor market attachment.

*Labor Market Outcomes.*—Pretrial release could improve labor market outcomes through at least three main channels. First, pretrial release might increase labor market attachment through an incapacitation effect since defendants cannot work in the formal sector while detained pretrial or incarcerated post-conviction. Defendants who are imprisoned are also ineligible to claim UI benefits and EITC benefits for wages earned while incarcerated. Second, pretrial release might affect outcomes because detention is highly disruptive to defendants' lives, potentially leading to job loss which makes it harder for defendants to find new employment. Finally, pretrial detention could independently lower future employment prospects through the stigma of a criminal conviction (e.g., Pager 2003, Agan and Starr 2016), which could in turn limit defendants' eligibility for employment-related benefits like UI and EITC.

We view our results as being inconsistent with the incapacitation channel. In online Appendix Figure A6, we graphically present two-stage least squares estimates of the impact of pretrial release on the probability of being incarcerated either pre- or post-disposition at different points in time after the bail hearing. We find that early on, pretrial release significantly reduces the probability of being incarcerated but that by approximately 250 days or 0.7 years after the bail hearing, the effect of pretrial release on incarceration becomes statistically insignificant from zero. Given that we find evidence that pretrial release increases formal labor market employment up to three to four years after the bail hearing, we conclude that incapacitation is unlikely to fully explain our labor market results.

We also view our results as being inconsistent with the disruption channel. In unreported results, we find no evidence that pretrial release decreases job disruption as measured by the probability of being employed with the same employer at baseline, likely because job turnover is very high in our sample. Only 16 percent of individuals employed at baseline stay with the same employer in the year after arrest.

To partially test whether pretrial release affects labor market outcomes through the criminal conviction channel, we explore whether those who are more likely to be employed in the labor market are also those who do not have a criminal conviction. In online Appendix Table A24, we present estimates of the joint probability of conviction in the initial case and employment in the several years after the bail hearing to explore the plausible interdependence between these two outcomes. Again, if the criminal conviction channel explains our labor market outcomes, we would expect to see an increase in employment among those who do not have a criminal conviction. If, on the other hand, pretrial release affects employment independently, we would see similar increases in employment among those with and without a criminal conviction.

We find that in the first two years after the bail hearing, our main employment results are primarily driven by an increase in the joint probability of not having a criminal conviction and being employed in the formal labor market. Conversely, we find a decrease in the joint probability of having a criminal conviction and being employed. By the third to fourth years after the bail hearing, our employment estimates are entirely driven by the joint probability of having no criminal conviction and being employed. These results suggest that the increase in employment among those released pretrial is concentrated among defendants who do not have a criminal conviction in the initial case. We conclude from these results that pretrial release primarily affects future labor market outcomes through the channel of a criminal conviction.[23]

## VI. Conclusion

This paper estimates the impact of being released before trial on criminal case outcomes, future crime, formal sector employment, and the receipt of government benefits. We find that pretrial release significantly decreases the probability of conviction, primarily through a decrease in guilty pleas. Pretrial release increases pretrial crime and failures to appear in court, but reduces crime following case disposition, leading to no detectable net effect on future crime. Finally, we find that pretrial release increases formal sector attachment both through an increase in formal sector employment and the receipt of tax- and employment-related government benefits. Many of the estimated effects are larger for defendants with no prior offenses in the past year.

We argue that these results are consistent with (i) pretrial release strengthening defendants' bargaining positions during plea negotiations, and (ii) a criminal conviction lowering defendants' attachment to the formal labor market. Our results suggest that adverse labor market outcomes and criminogenic effects begin at the pretrial stage prior to any finding of guilt, highlighting the long-term costs of weakening a defendant's negotiating position before trial and the importance of bail in the criminal justice process.

[23] These results are also consistent with our subsample results (e.g., prior versus no prior offense in the past year) where we generally find that subsamples with the largest effect of pretrial release on pleading guilty also have the largest effect on employment outcomes.

An important open question is whether the benefits of pretrial release documented in our analysis are, on net, larger than the costs of apprehending individuals who fail to appear in court and the costs of future criminality. While a comprehensive cost-benefit analysis is beyond the scope of this paper, we consider a partial back-of-the-envelope calculation that takes into account the administrative costs of jail, the costs of apprehending individuals who fail to appear, the costs of future criminality, and the economic impact on defendants.[24] See online Appendix D for a description of this exercise. Based on these tentative calculations, we estimate that the total net benefit of pretrial release for the marginal defendant is anywhere between \$55,143 and \$99,124. Intuitively, pretrial release on the margin increases social welfare because of the significant long-term costs associated with having a criminal conviction, the criminogenic effect of detention which offsets the incapacitation benefit, and the relatively low costs associated with apprehending defendants who miss court appearances.[25] These calculations suggest that unless there is a large general deterrence effect of pretrial detention (which we are unable to measure in our paper), detaining more individuals on the margin is unlikely to be welfare-improving. However, we caution that this partial cost-benefit analysis is speculative for at least two reasons. First, rearrests may be an imperfect proxy for true criminal behavior if there is substantial underreporting of new crime and/or if the probability of detection is affected by conviction.[26] Second, many of our estimates are imprecise and, as a result, the confidence interval surrounding our cost-benefit calculation is large.

Nevertheless, our results suggest that it may be welfare enhancing to use alternatives to pretrial detention, at least on the margin. For example, to the extent that recidivism rates are not appreciably higher than under pretrial detention, electronic monitoring may provide many of the same benefits of detention without the substantial costs to defendants documented in our analysis.

There are three important caveats to our analysis. First, we are unable to estimate the deterrent effects of a more or less strict bail system. If a more strict bail system has a large deterrent effect, our analysis will understate the benefits of pretrial detention. Second, we are unable to measure the impacts of pretrial detention on informal sector earnings or consumption. If lost formal sector earnings are largely replaced by informal earnings, the case against pretrial detention is perhaps weaker. Finally, given these concerns, we are unable to draw any sharp welfare conclusions about the

[24] For example, our cost-benefit analysis does not include the direct disutility to defendants of having to spend time in jail. However, if there are additional unmeasured costs of pretrial detention not currently included in our analysis, our partial cost-benefit exercise would suggest larger net benefits to pretrial release. We also note that the welfare implications of an increase in guilty pleas is unclear and, as a result, difficult to quantify in our cost-benefit framework. On the one hand, if a defendant would have been found guilty at trial and pretrial detention simply speeds up the process, an increase in plea rates might be welfare-enhancing by saving limited court resources. On the other hand, if an innocent defendant pleads guilty as a result of pretrial detention, social welfare is decreased, with damages from wrongful conviction estimated at approximately \$50,000 per year in most states (see http://www.cnn.com/interactive/2012/03/us/table.wrongful.convictions/).

[25] Recall that the benefits of pretrial release are relatively larger and the costs of release relatively smaller for defendants with no recent priors (Table 7), suggesting that the net benefit of pretrial release is even larger for this subsample.

[26] While there are few existing estimates measuring the effect of pretrial release on the probability of detection, under the assumption of no real change in true criminal behavior, one would need to believe that that the probability of detection is over 13 percentage points higher for marginal detained defendants relative to marginal released defendants in order to explain our results on post-disposition new crime.

optimality of the current bail system using our research design. While beyond the scope of this paper, developing a framework to assess the precise welfare effects of the bail system is an important area of future work.

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# EXHIBIT C

08.04.2015
NEWS

# Why Jails Have More Suicides than Prisons

A new report and a growing phenomenon.

By MAURICE CHAMMAH and TOM MEAGHER
Graphics by TOM MEAGHER

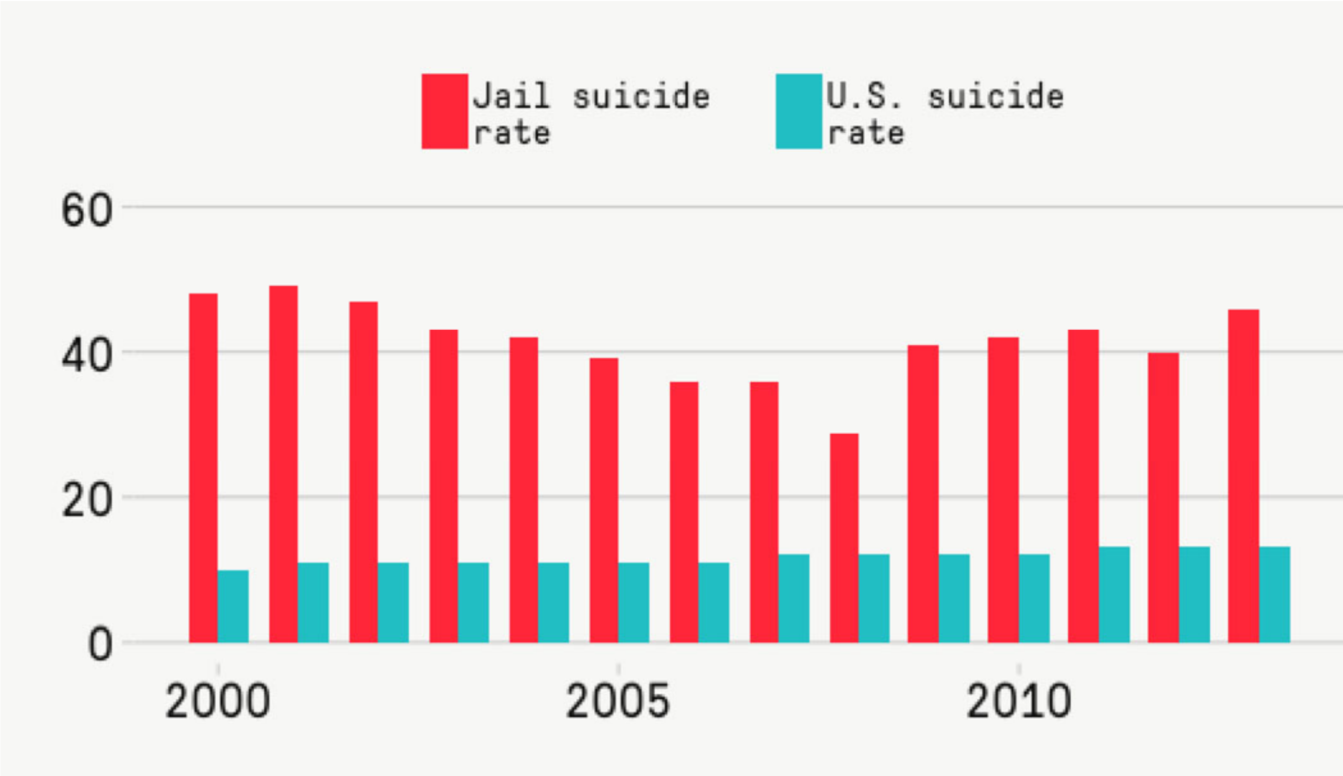
A report released today by the federal Bureau of Justice Statistics shows that among the causes of death behind bars, suicide in county jails — a leading cause of death in such facilities — is on the rise. These statistics, collected between 2000 and 2013, come in the wake of Sandra Bland's death at the Waller County jail in east Texas, which received national attention and is currently being investigated by the FBI and a panel of lawyers for evidence of wrongdoing.

One reason why jails have a higher suicide rate (46 per 100,000 in 2013) than prisons ( 15 PER 100,000[1] ) is that people who enter a jail often face a first-time "shock of confinement"; they are stripped of their job, housing, and basic sense of normalcy. Many commit suicide before they have been convicted at all. According to the BJS report, those rates are seven times higher than for convicted inmates.

15 PER 100,000[1]
The suicide rate among the general U.S. population is 13 per 100,000.

Jails also have less information to work with when they try to assess new inmates for suicide risks. "Prisons know who they're getting," said Michele Deitch, a professor at the University of Texas's LBJ School of Public Affairs who testified last week at a Texas Legislature hearing over the death of Sandra Bland. By the time someone arrives in prison, mental health issues — including suicidal tendencies — have had time to surface.

Rates of suicide for local jail inmates and U.S. residents, per 100,000




SOURCE: BUREAU OF JUSTICE STATISTICS, DEATHS IN CUSTODY REPORTING PROGRAM AND CENTERS FOR DISEASE CONTROL AND PREVENTION

In addition to suicide, a primary cause of jail deaths, and one that can be linked to failures in the intake process, are deaths related to drugs and alcohol. BJS found that jails have had increased rates of "drug or alcohol intoxication deaths" IN RECENT YEARS[2] .

IN RECENT YEARS[2]
In 2012, there were 57 drug- and alcohol-related deaths. In 2013, there were 70.

Although jails have improved their intake protocols over the past several decades, these protocols remain far less robust than in state prisons, which, Deitch said, "are shaped by years of litigation and the demands of the state legislature." Prisons are more likely to have their policies scrutinized by an accreditor like the American Correctional Association. When a scandal — a suicide, or a wrongful death at the hands of guards — hits a state prison system, the entire state may change the way inmates are handled.

In 1980, Judge William Wayne Justice ruled in the massive prisoner rights lawsuit Ruiz v. Estelle and found that the Texas prison system, which now includes roughly 150,000 prisoners, had "no program whatever for the identification, treatment, or supervision of inmates with suicidal tendencies." After that ruling, the agency was forced to screen incoming inmates for their risk of suicide.

Often small facilities never get sued at all, even in the wake of a suicide caused by negligence. "Cases tend to focus on big facilities because that's where we can help the most people," says Amy Fettig, an attorney with the American Civil Liberties Union's National Prison Project. "But to do all these little jails, which would take the same amount of resources, it's just impossible, and so nothing gets done." According to several prior BJS studies, the nation's smallest jails have a suicide rate more than six times as high as the nation's largest jails.

Many advocates point to a lack of statewide organizations that could force jails to improve their procedures before they get sued. Texas has a Commission on Jail Standards, with four inspectors for the state's roughly 250 jails. New York has a Commission of Corrections, with 15 inspectors for more than 500 jail and prison facilities, and California's Division of Facilities Standards and Operations has nine inspectors for more than 600 facilities. The missions of these government bodies vary, but as Deitch wrote in a 50-state survey several years ago, "formal and comprehensive external oversight — in the form of inspections and routine monitoring of conditions that affect the rights of prisoners — is truly rare in this country."

# EXHIBIT D

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ARTICLE

# The Downstream Consequences of Misdemeanor Pretrial Detention

Paul Heaton, Sandra Mayson & Megan Stevenson*

**Abstract.** In misdemeanor cases, pretrial detention poses a particular problem because it may induce innocent defendants to plead guilty in order to exit jail, potentially creating widespread error in case adjudication. While practitioners have long recognized this possibility, empirical evidence on the downstream impacts of pretrial detention on misdemeanor defendants and their cases remains limited. This Article uses detailed data on hundreds of thousands of misdemeanor cases resolved in Harris County, Texas—the third-largest county in the United States—to measure the effects of pretrial detention on case outcomes and future crime. We find that detained defendants are 25% more likely than similarly situated releasees to plead guilty, are 43% more likely to be sentenced to jail, and receive jail sentences that are more than twice as long on average. Furthermore, those detained pretrial are more likely to commit future crimes, which suggests that detention may have a criminogenic effect. These differences persist even after fully controlling for the initial bail amount, offense, demographic information, and criminal history characteristics. Use of more limited sets of controls, as in prior research, overstates the adverse impacts of detention. A quasi-experimental analysis based on case timing confirms that these differences likely reflect the causal effect of detention. These results raise important constitutional questions and suggest that Harris County could save millions of dollars per year, increase public safety, and reduce wrongful convictions with better pretrial release policy.

* Paul Heaton is a Senior Fellow at the University of Pennsylvania Law School and the Academic Director of the Quattrone Center for the Fair Administration of Justice. Sandra Mayson and Megan Stevenson are Quattrone Fellows at the University of Pennsylvania Law School. The Authors contributed equally to the work and are listed alphabetically. They are deeply grateful for thoughtful input from Alex Bunin; Jonah Gelbach; John Hollway; Seth Kreimer; David Rudovsky; and participants in the April 2016 Quattrone Center Advisory Board meeting, the University of Pennsylvania Faculty Ad Hoc Workshop, and the 2016 Conference on Empirical Legal Studies. They would also like to thank the editors of the *Stanford Law Review* for their superb editorial assistance. The contents of this Article are solely the responsibility of the Authors.

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### Introduction

The United States likely detains millions of people each year for inability to post modest bail. There are approximately eleven million annual admissions into local jails.[1] Many of those admitted remain jailed pending trial. At midyear 2014, there were an estimated 467,500 people awaiting trial in local jails, up from 349,800 at the same point in 2000 and 298,100 in 1996.[2] Available evidence suggests that the large majority of pretrial detainees are detained because they cannot afford their bail, which is often a few thousand dollars or less.[3]

This expansive system of pretrial detention has profound consequences both within and beyond the criminal justice system. A person detained for even a few days may lose her job, housing, or custody of her children.[4] There is

1. *See* Todd D. Minton & Zhen Zeng, Bureau of Justice Statistics, U.S. Dep't of Justice, Jail Inmates at Midyear 2014, at 1 (2015), https://www.bjs.gov/content/pub/pdf/jim14.pdf.
2. Darrell K. Gilliard & Allen J. Beck, Bureau of Justice Statistics, U.S. Dep't of Justice, Prison and Jail Inmates at Midyear 1996, at 7 (1997), https://www.bjs.gov/content/pub/pdf/pjim97.pdf; Minton & Zeng, *supra* note 1, at 3 tbl.2. Jail incarceration rates rose steadily between 1983 and 2007. *See* Ram Subramanian et al., Vera Inst. of Justice, Incarceration's Front Door: The Misuse of Jails in America 7-10 (2015), https://www.vera.org/publications/incarcerations-front-door-the-misuse-of-jails-in-america (to locate, select the "Full Report" hyperlink). This trend accompanied a shift away from release on recognizance and toward reliance on cash bail. Whereas between the years 1990 and 1994, 41% of pretrial releases were on recognizance and 24% were by cash bail, between 2002 and 2004 the relation was reversed: 23% of releases were on recognizance, and 42% were by cash bail. Thomas H. Cohen & Brian A. Reaves, Bureau of Justice Statistics, U.S. Dep't of Justice, State Court Processing Statistics, 1990-2004: Pretrial Release of Felony Defendants in State Courts 2 (2007), https://www.bjs.gov/content/pub/pdf/prfdsc.pdf. In 2009, 61% of releases in felony cases in the seventy-five largest urban jurisdictions included financial conditions of release. Brian A. Reaves, Bureau of Justice Statistics, U.S. Dep't of Justice, Felony Defendants in Large Urban Counties, 2009—Statistical Tables 1, 15 (2013).
3. *See* N.Y.C. CRIMINAL JUSTICE AGENCY, ANNUAL REPORT 2013, at 22, 30 & exh. 18 (2014), http://www.nycja.org/lwdcms/doc-view.php?module=reports&module_id=1410&doc_name=doc (documenting bail of $500 or less in 33% of nonfelony cases and 3% of felony cases in New York City and reporting that 30% of felony defendants and 46% of nonfelony defendants whose bail was $500 or less were detained until the disposition of their case); Cohen & Reaves, *supra* note 2, at 1 (reporting that five in six felony defendants detained until disposition had bail set and that approximately 30% of felony defendants with bail set at $5000 or less were detained); Reaves, *supra* note 2, at 15 (reporting that nine in ten felony defendants detained until disposition had bail set). What is unclear is how many of the defendants detained despite bail are there for inability to pay and how many elect not to post bail for reasons other than financial inability (for instance, because they have a probation detainer or plan to plead guilty and expect a custodial sentence). *See infra* Table 1 and accompanying text (discussing rates of misdemeanor pretrial detention in Harris County).
4. *See, e.g.*, Curry v. Yachera, No. 15-1692, 2016 WL 4547188, at *3 (3d Cir. Sept. 1, 2016) ("While imprisoned [pretrial on a bail he could not afford], [Curry] missed the birth of his only child, lost his job, and feared losing his home and vehicle."); OPEN SOC'Y JUSTICE

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also substantial reason to believe that detention affects case outcomes. A detained defendant "is hindered in his ability to gather evidence, contact witnesses, or otherwise prepare his defense."[5] This is thought to increase the likelihood of conviction, either by trial or by plea, and may also increase the severity of any sanctions imposed.[6] More directly, a detained person may plead guilty—even if innocent—simply to get out of jail.[7] Not least importantly, a money bail system that selectively detains the poor threatens the constitutional principles of due process and equal protection.[8]

To date, however, empirical evidence of the downstream effects of pretrial detention has been limited. There is ample documentation that those detained pretrial are convicted more frequently, receive longer sentences, and commit more future crimes than those who are not (on average).[9] But this is precisely what one would expect if the system detained those who pose the greatest flight or public safety risk. One key question for pretrial law and policy is whether detention actually *causes* the adverse outcomes with which it is linked, independently of other factors. On this question, past empirical work is inconclusive.[10]

This Article presents original evidence that pretrial detention causally affects case outcomes and the commission of future crimes. Using detailed data on hundreds of thousands of misdemeanor cases resolved in Harris County, Texas (the third-largest county in the United States[11]), this Article deploys two

INITIATIVE, THE SOCIOECONOMIC IMPACT OF PRETRIAL DETENTION: A GLOBAL CAMPAIGN FOR PRETRIAL JUSTICE REPORT 13 (2011), http://www.unicef.org/ceecis/Socioeconomic_impact_pretrial_detention.pdf (attempting to "catalogue the socioeconomic impact of excessive pretrial detention around the world"); Nick Pinto, *The Bail Trap*, N.Y. TIMES MAG. (Aug. 13, 2015), http://nyti.ms/1INtghe (chronicling the story of a woman who, "five months after her arrest, . . . was still fighting in family court to regain custody of her daughter").

5. Barker v. Wingo, 407 U.S. 514, 533 (1972).
6. *See infra* Part I.C (describing prior research finding evidence of these effects).
7. *See infra* note 16 and accompanying text.
8. *See infra* Part IV.B.
9. *See infra* Part I.C.
10. The literature has produced suggestive evidence of the causal effects of detention, but prior studies were limited by the data available and the number of variables for which they were able to control. *See infra* Part I.C. Only one study, a report published by the New York City Criminal Justice Agency, has focused on misdemeanor cases specifically. MARY T. PHILLIPS, N.Y.C. CRIMINAL JUSTICE AGENCY, PRETRIAL DETENTION AND CASE OUTCOMES, PART 1: NONFELONY CASES (2007) [hereinafter PHILLIPS, NONFELONY CASES], http://www.nycja.org/lwdcms/doc-view.php?module=reports&module_id=669&doc_name=doc. That study found that misdemeanor pretrial detention is correlated with unfavorable case outcomes. *Id.* at 25-43, 55-56. Because of the limited set of controls, however, it is unclear whether the relationship is causal.
11. *Largest Counties in the U.S. 2015*, STATISTA, https://www.statista.com/statistics/241702/largest-counties-in-the-us (last visited Mar. 3, 2017).

quantitative methods to estimate the causal effect of detention: (1) a regression analysis that controls for a significantly wider range of confounding variables than past studies, and (2) a quasi-experimental analysis related to case timing. The results provide compelling evidence that pretrial detention causally increases the likelihood of conviction, the likelihood of receiving a carceral sentence, the length of a carceral sentence, and the likelihood of future arrest for new crimes.

This Article intentionally focuses on misdemeanor cases. "Misdemeanor" may sound synonymous with "trivial," but that connotation is misleading. Misdemeanors matter. Misdemeanor convictions can result in jail time, heavy fines, invasive probation requirements, and collateral consequences that include deportation, loss of child custody, ineligibility for public services, and barriers to finding employment and housing.[12] Beyond the consequences of misdemeanor convictions for individuals, the misdemeanor system has a profound impact because it is enormous: while national data on misdemeanors are lacking, a 2010 analysis found that misdemeanors represented more than three-quarters of the criminal caseload in state courts where data were available.[13]

For misdemeanor defendants who are detained pretrial, the worst punishment may come before conviction.[14] Conviction generally means getting out of jail; people detained on misdemeanor charges are routinely offered sentences for "time served" or probation in exchange for tendering a guilty plea.[15] And

12. Alexandra Natapoff, *Misdemeanors*, 85 S. CAL. L. REV. 1313, 1316-17 (2012) (reporting that a misdemeanor conviction can limit a person's access to "employment, as well as educational and social opportunities"; can limit eligibility for "professional licenses, child custody, food stamps, student loans, health care," or public housing; can "lead to deportation"; and "heightens the chances of subsequent arrest, and can ensure a longer felony sentence later on"); Jenny Roberts, *Crashing the Misdemeanor System*, 70 WASH. & LEE L. REV. 1089, 1090 (2013) (noting that misdemeanor convictions "can affect future employment, housing, and many other basic facets of daily life").

13. ROBERT C. LAFOUNTAIN ET AL., NAT'L CTR. FOR STATE COURTS, EXAMINING THE WORK OF STATE COURTS: AN ANALYSIS OF 2008 STATE COURT CASELOADS 47 (2010), http://www.courtstatistics.org/~/media/Microsites/Files/CSP/EWSC-2008-Online .ashx; *see also* Natapoff, *supra* note 12, at 1315 ("Most U.S. convictions are misdemeanors, and they are generated in ways that baldly contradict the standard due process model of criminal adjudication.").

14. *See* MALCOLM M. FEELEY, THE PROCESS IS THE PUNISHMENT: HANDLING CASES IN A LOWER CRIMINAL COURT 9-10 (1979) (reporting that in a sample of more than 1600 cases, "twice as many people were sent to jail prior to trial than after trial"). This practice stands in sharp contrast to the traditional right to pretrial release on bail for noncapital defendants. *Cf.* Stack v. Boyle, 342 U.S. 1, 4 (1951) ("[The] traditional right to freedom before conviction . . . serves to prevent the infliction of punishment prior to conviction.").

15. Jenny Roberts, *Why Misdemeanors Matter: Defining Effective Advocacy in the Lower Criminal Courts*, 45 U.C. DAVIS L. REV. 277, 308 (2011) ("In such cases, defendants must

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their incentives to take the deal are overwhelming. For defendants with a job or apartment on the line, the chance to get out of jail may be impossible to pass up. Misdemeanor pretrial detention therefore seems especially likely to induce guilty pleas, including wrongful ones.[16] This is also, perversely, the realm where the utility of cash bail or pretrial detention is most attenuated. These defendants' incentives to abscond should be relatively weak, and the public safety benefit of detention is dubious.[17]

Despite these structural problems, money bail practices that result in systemic misdemeanor pretrial detention have persisted nationwide. In Harris County, Texas—the site of this study—more than half of all misdemeanor defendants are detained.[18] Other jurisdictions also detain people accused of misdemeanors at surprising rates.[19] There are several possible reasons for this. A money bail system may be easier to operate than a system of broad release with effective pretrial services. The bail bondsman lobby is a potent political force.[20] The individual judges or magistrates who make pretrial custody

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generally choose between remaining in jail to fight the case or taking an early plea with a sentence of time served or probation.").

16. *See, e.g.*, Samuel R. Gross & Barbara O'Brien, *Frequency and Predictors of False Conviction: Why We Know So Little, and New Data on Capital Cases*, 5 J. EMPIRICAL LEGAL STUD. 927, 930-31 (2008) (noting that "it is entirely possible that most wrongful convictions . . . are based on negotiated guilty pleas to comparatively light charges" to avoid "prolonged pretrial detention"); Natapoff, *supra* note 12, at 1316 ("[E]very year the criminal system punishes thousands of petty offenders who are not guilty."); *id.* at 1343-47 (cataloging the pressures that lead innocent misdemeanor defendants to plead guilty); Alexandra Natapoff, *Negotiating Accuracy: DNA in the Age of Plea Bargaining*, *in* WRONGFUL CONVICTIONS AND THE DNA REVOLUTION: TWENTY-FIVE YEARS OF FREEING THE INNOCENT (Daniel Medwed ed., forthcoming 2017), http://papers.ssrn.com/sol3/papers.cfm? abstract_id=2693218 ("Because most of those arrested [for public-order offenses pursuant to aggressive broken windows policing in New York City] pled out to avoid pre-trial detention, that police policy resulted in numerous wrongful convictions.").

17. That is both because people accused of misdemeanors are likely to pose much less of a threat than people charged with more serious offenses and because detention for the life of a misdemeanor case constitutes only very short-term incapacitation—which may be outweighed by criminogenic effects. *See infra* Part III.C.

18. *Infra* Table 1.

19. *See, e.g.*, Charlie Gerstein, Note, *Plea Bargaining and the Right to Counsel at Bail Hearings*, 111 MICH. L. REV. 1513, 1525 n.81 (2013) ("In New York . . . 25 percent of nonfelony defendants are held on bail. In Baltimore, that number is closer to 50 percent."). In Philadelphia and New York City, around 25% of misdemeanor defendants are detained pretrial. Statistics for Philadelphia are based on the Authors' calculations using Philadelphia court records; for statistics for New York City, see PHILLIPS, NONFELONY CASES, *supra* note 10, at 13.

20. *See* Press Release, Am. Bail Coal., Former U.S. Solicitor General Paul D. Clement Files *Amicus* Brief in Defense of the Eighth Amendment Constitutional Right to Bail on Behalf of the American Bail Coalition, the Georgia Association of Professional Bondsmen, and the Georgia Sheriffs' Association (June 21, 2016),
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decisions suffer political blowback if they release people (either directly or via affordable bail) who subsequently commit violent crimes, but they suffer few consequences, if any, for setting unaffordable bail that keeps misdemeanor defendants detained. In short, institutional actors in the misdemeanor system have strong incentives to rely on money bail practices that result in systemic pretrial detention.[21]

Given the inertia, misdemeanor bail policy is unlikely to shift in the absence of compelling empirical evidence that the status quo does more harm than good. This Article provides such evidence through the use of two types of quantitative analysis. The first is a regression analysis that controls for a wide range of confounding factors: defendant demographics, extensive criminal history variables, wealth measures (zip code and claims of indigence), judge effects, and 121 different categories of charged offense. Importantly, the analysis also controls for the precise amount of bail set at the initial hearing, meaning that the effects of bail are assessed by comparing defendants presumably viewed by the court as representing equal risk but who nonetheless differ in whether they are ultimately detained. In addition, this Article undertakes a quasi-experimental analysis that, akin to a randomized controlled trial that would be used to determine the effect of a treatment in an experimental setting, measures the effects of detention by leveraging random variation in the access defendants have to bail money based on the timing of arrest. These quasi-experimental results are very similar to those produced through regression analysis with detailed controls.

This Article finds that defendants who are detained on a misdemeanor charge are much more likely than similarly situated releasees to plead guilty and serve jail time. Compared to similarly situated releasees, detained defendants are 25% more likely to be convicted and 43% more likely to be sentenced to jail. On average, their incarceration sentences are nine days longer, more than double that of similar releasees. Furthermore, we find that

http://www.americanbailcoalition.org/in-the-news/former-u-s-solicitor-general-paul-d-clement-files-amicus-brief-defense-constitutional-right-bail (showing that the bail bond industry is represented in federal court by a prominent Supreme Court advocate and former Solicitor General); Nat'l Ass'n of Pretrial Serv. Agencies, The Truth About Commercial Bail Bonding in America 4-5 (2009), https://www.pretrial.org/download/pji-reports/Facts%20and%20Positions%201.pdf (describing legislative efforts by the American Legislative Exchange Council on behalf of the bail bond industry); *About the American Bail Coalition*, AM. BAIL COALITION, http://www.americanbailcoalition.org/about-us (last visited Mar. 3, 2017) ("The American Bail Coalition is a trade association made up of national bail insurance companies . . . .").

21. However, that may be changing in some places thanks to recent reform efforts. *See, e.g.*, *Ending the American Money Bail System*, EQUAL JUST. UNDER L., http://equaljusticeunderlaw.org/wp/current-cases/ending-the-american-money-bail-system (last visited Mar. 3, 2017) (describing the organization's litigation campaign against money bail systems).

pretrial detainees are more likely than similarly situated releasees to commit future crimes. Although detention reduces defendants' criminal activity in the short term through incapacitation, by eighteen months post-hearing, detention is associated with a 30% increase in new felony charges and a 20% increase in new misdemeanor charges, a finding consistent with other research suggesting that even short-term detention has criminogenic effects. These results raise important constitutional questions and suggest that with modest changes to misdemeanor pretrial policy, Harris County could save millions of dollars per year, increase public safety, and reduce wrongful convictions.

Interest in pretrial policy is now surging. In the months prior to publication of this Article, several other studies have been released that also use both a natural experiment and complex multivariate regression to estimate the effects of pretrial detention.[22] Those studies are set in Philadelphia, New York City, Pittsburgh, and Miami, and they too find that pretrial detention has a causal adverse effect on case outcomes.[23] As a whole, this body of research presents compelling evidence that detention effects exist across case types and jurisdictions. This Article offers a unique contribution by focusing on misdemeanor cases, setting its analysis in Harris County, and putting its empirical findings in constitutional context.

This Article proceeds in four parts. Part I provides background on pretrial detention and surveys the existing empirical literature assessing its effects. Part II outlines the pretrial process in Harris County, which has much in common with the process in other large jurisdictions, and describes the dataset. Part II also reports the results of an empirical analysis on the relationship between wealth and detention rates. Part III presents the results from a series of empirical analyses designed to measure the effect of pretrial detention on case and crime outcomes. Part IV, finally, explores the implications of the results for ongoing constitutional and policy debates.

## I. The Pretrial Process and Prior Empirical Literature

### A. On Bail and Pretrial Detention

The pretrial process begins with arrest and ends with the disposition of the criminal case. Since the Founding, the United States has relied heavily on a money bail system adapted from the English model to ensure the appearance of the accused at trial.[24] Bail is deposited with the court and serves as security. If

22. *See infra* notes 63-73 and accompanying text.

23. *See infra* notes 63-73 and accompanying text.

24. *See* Hermine Herta Meyer, *Constitutionality of Pretrial Detention*, 60 GEO. L.J. 1139, 1146 (1972) (chronicling the history of the bail system in Anglo-Saxon law); Timothy R. Schnacke, Nat'l Inst. of Corr., U.S. Dep't of Justice, Fundamentals of Bail: A Resource

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the accused appears in court when ordered to do so, his bail is returned at the conclusion of the case; if not, it is forfeited.[25] But whereas in eighteenth-century England many offenses were "unbailable," the American colonies guaranteed a broad right to bail with a narrow exception for capital cases.[26] In 1951, the Supreme Court held that the Excessive Bail Clause prohibits bail "set at a figure higher than an amount reasonably calculated" to ensure the appearance of the accused.[27] The Court ruminated that "[u]nless this right to bail before trial is preserved, the presumption of innocence, secured only after centuries of struggle, would lose its meaning."[28]

The second half of the twentieth century brought major changes to America's pretrial system. In the 1960s, the realization that many people were detained pretrial for their inability to post bail led to a national reform movement.[29] Reform efforts sought to limit the use of money bail in favor of simple release on recognizance (ROR), where the defendant is released solely on his promise to return to court.[30] In the 1970s and 1980s, concerns about

Guide for Pretrial Practitioners and a Framework for American Pretrial Reform 21-44 (2014), http://www.clebp.org/images/2014-11-05_final_bail_fundamentals_september_8,_2014.pdf.

25. *See, e.g.*, ARIZ. R. CRIM. P. 7.6(c)(2) ("If at the [bail forfeiture] hearing, the [bail] violation is not explained or excused, the court may enter an appropriate order of judgment forfeiting all or part of the amount of the bond, which shall be enforceable by the state as any civil judgment."); PA. R. CRIM. P. 535(D) ("[W]ithin 20 days of the full and final disposition of the case, the [bail] deposit shall be returned to the depositor . . . ."); *id.* 536(A)(2)(a) ("When . . . the defendant has violated a condition of the bail bond, the bail authority may order the cash or other security forfeited and shall state in writing or on the record the reasons for so doing."); TEX. CODE CRIM. PROC. ANN. art. 17.02 (West 2015) ("Any cash funds deposited under this article shall . . . , on order of the court, be refunded . . . after the defendant complies with the conditions of the defendant's bond . . . ."); *id.* art. 22.01 ("When a defendant . . . fails to appear in any court in which such case may be pending and at any time when his personal appearance is required under this Code, . . . a forfeiture of his bail and a judicial declaration of such forfeiture shall be taken . . . .").

26. *See* Judiciary Act of 1789, ch. 20, § 33, 1 Stat. 73, 91-92 (guaranteeing a right to bail in noncapital cases); JOHN S. GOLDKAMP, TWO CLASSES OF ACCUSED: A STUDY OF BAIL AND DETENTION IN AMERICAN JUSTICE 55-60 (1979) (explaining the "classic" state constitutional bail clause and statutory definition of the right to bail); Schnacke, *supra* note 24, at 29-33 (describing the "bail/no bail" dichotomy in early America).

27. Stack v. Boyle, 342 U.S. 1, 5 (1951).

28. *Id.* at 4.

29. *See* GOLDKAMP, *supra* note 26, at 23-25, 84.

30. *See, e.g.*, Bail Reform Act of 1966, Pub. L. No. 89-465, § 2, 80 Stat. 214, 214 ("The purpose of this Act is to revise the practices relating to bail to assure that all persons, regardless of their financial status, shall not needlessly be detained pending their appearance . . . ."); NAT'L CONFERENCE ON BAIL & CRIMINAL JUSTICE, PROCEEDINGS AND INTERIM REPORT OF THE NATIONAL CONFERENCE ON BAIL AND CRIMINAL JUSTICE, at xiii-xxxii (1965) (describing the proceedings of a high-level policy committee convened to address inequalities in the bail system and shift toward release on recognizance); Robert F.

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rising rates of pretrial crime led to a second wave of reform, this time directed at identifying and managing defendants who posed a threat to public safety.[31] The federal government and many states enacted pretrial preventive detention statutes, and almost every jurisdiction in the country amended its pretrial laws to direct courts to consider "public safety" when setting bail or conditions of release.[32]

As of this writing, most U.S. jurisdictions rely heavily on money bail as the central mechanism of the pretrial system.[33] The Supreme Court has held that "the fixing of bail for any individual defendant must be based upon standards relevant to the purpose of assuring the presence of *that* defendant," including "the nature and circumstances of the offense charged, the weight of the evidence against him, the financial ability of the defendant to give bail and the character of the defendant."[34] Many jurisdictions, however, do not adhere to this mandate. Bail hearings are typically just a few minutes long, often conducted over videoconference and without defense representation.[35] Some jurisdictions employ bail "schedules" with predetermined bail amounts for each offense, which do not consider individual circumstances relevant to flight risk

Kennedy, U.S. Att'y Gen., Address to the Criminal Law Section of the American Bar Association 4 (Aug. 10, 1964), https://www.justice.gov/sites/default/files/ag/legacy/2011/01/20/08-10-1964.pdf ("We have been deeply concerned anout [sic] the effect of bail on the poor man. The Allen Committee . . . recommended that release on recognizance be increased wherever possible at the Federal level and we have followed that recommendation.").

31. *See* John S. Goldkamp, *Danger and Detention: A Second Generation of Bail Reform*, 76 J. CRIM. L. & CRIMINOLOGY 1, 5-6, 15 (1985).

32. *See id.* at 15.

33. *See supra* note 2 and accompanying text.

34. Stack v. Boyle, 342 U.S. 1, 5 & n.3 (1951) (emphasis added) (quoting FED. R. CRIM. P. 46(c) (amended 1966)).

35. *See* Pretrial Justice Inst., 2009 Survey of Pretrial Services Programs 44-45 (2009), http://www.pretrial.org/download/pji-reports/new-PJI%202009%20Survey%20of%20Pretrial%20Services%20Programs.pdf. While there is no systematic survey of bail hearing lengths, many jurisdictions report bail hearings of just a few minutes. For example, bail hearings are three minutes long on average in North Dakota, *Length of a Bail Hearing in North Dakota: 3 Minutes*, NAT'L CTR. FOR ACCESS TO JUST. (Jan. 25, 2013), http://ncforaj.org/2013/01/25/length-of-a-bail-hearing-in-north-dakota-3-minutes, and they are often less than two minutes long in Illinois's Cook County, Injustice Watch Staff, *Change Difficult as Bail System's Powerful Hold Continues Punishing the Poor*, INJUSTICE WATCH (Oct. 14, 2016), http://injusticewatch.org/interactives/bent-on-bail. Harris County bail hearings, the length of which is evidenced by the time stamp on the court records, are usually only a couple of minutes long, as is true in Philadelphia. *See* Megan Stevenson, Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes 6 (Jan. 12, 2017) (unpublished manuscript), https://papers.ssrn.com/abstract=2777615.

or ability to pay.[36] In many jurisdictions, judges set higher bail for defendants they perceive as dangerous, either as directed by statute or on their own initiative, despite the fact that money bail is a dubious mechanism for managing future crime risk.[37]

Once bail is set, detention status depends on a defendant's ability and willingness to pay bail. Those who post bail are released. Often a bail bondsman serves as a middleman, posting the refundable bail deposit in exchange for a nonrefundable fee (usually about 10% of the total).[38] Those who do not post bail are detained pending trial. The length of pretrial detention varies tremendously by jurisdiction and the particulars of a given case. In most places, the state must institute formal charges and arraign the defendant within a few days of arrest, and misdemeanor cases may be resolved within a few weeks.[39] In other places the timeline is longer, and a misdemeanor defendant may be detained for weeks or months before she is even arraigned.[40]

It has long been conventional wisdom that pretrial detention has an adverse effect on case outcomes (from the perspective of the accused).[41] If this is

36. *See* Pretrial Justice Inst., Pretrial Justice in America: A Survey of County Pretrial Release Policies, Practices and Outcomes 2, 7 (2010), https://www.pretrial.org/download/research/PJI%20Pretrial%20Justice%20in%20America%20-%20Scan%20of%20Practices%202009.pdf (indicating that 64% of the U.S. counties surveyed reported using a bail schedule). *But cf.* ABA STANDARDS FOR CRIMINAL JUSTICE: PRETRIAL RELEASE § 10-5.3(e) (3d ed. 2007) [hereinafter ABA STANDARDS: PRETRIAL RELEASE] ("Financial conditions . . . should never be set by reference to a predetermined schedule of amounts fixed according to the nature of the charge.").

37. *Cf.* ABA STANDARDS: PRETRIAL RELEASE, *supra* note 36, § 10-5.3(b) ("Financial conditions of release should not be set to prevent future criminal conduct during the pretrial period or to protect the safety of the community or any person.").

38. *See* Justice Policy Inst., For Better or for Profit: How the Bail Bonding Industry Stands in the Way of Fair and Effective Pretrial Justice 6 (2012), http://www.justicepolicy.org/uploads/justicepolicy/documents/_for_better_or_for_profit_.pdf.

39. In the Harris County data, the median time to disposition for detained misdemeanor defendants is three days, and 80% of detained defendants had their cases resolved within eighteen days.

40. In Louisiana, people may be detained on misdemeanor arrest charges for up to seventy-five days without being arraigned. *See* LA. CODE CRIM. PROC. ANN. art. 701(B)(1)(a) (2016) (requiring that formal charges be instituted within forty-five days of arrest if the misdemeanor defendant is detained); *id.* art. 701(C) (requiring arraignment within thirty days of the filing of formal charges).

41. *See* Esmond Harmsworth, *Bail and Detention: An Assessment and Critique of the Federal and Massachusetts Systems*, 22 NEW ENG. J. CRIM. & CIV. CONFINEMENT 213, 217 (1996) ("The idea that detention correlates with, and causes, increased conviction rates goes back to Wayne Morse and R. Beattie's study of Multnomah County, Oregon in the 1920s and Caleb Foote's Philadelphia studies in the 1950s." (footnote omitted)); Patricia Wald, *Pretrial Detention and Ultimate Freedom: A Statistical Study*, 39 N.Y.U. L. REV. 631, 632 (1964) ("[W]e can no longer disregard the impact of prior detention . . . on the sentencing process."); *see also infra* Part I.C.

true, there are at least six possible mechanisms. Detained defendants might experience worse outcomes because they (1) have increased incentives to plead guilty, including potentially overwhelming incentives; (2) cannot effectively prepare a defense; (3) have reduced financial resources for their defense; (4) cannot demonstrate positive behavior; (5) cannot obstruct the prosecution; and (6) lack the advantage of long delay.

Most obviously, detention alters the incentives for fighting a charge. A detained defendant generally has less to lose by pleading guilty; detention may have already caused major disruption to her life. And whereas for a released defendant the prospect of a criminal sentence—custodial or otherwise—represents a serious loss of liberty, for a detainee it is, at worst, an extension of the status quo. A second possible mechanism is that detention may limit the ability of the accused to develop a defense by working with his attorney or collecting relevant evidence. Relatedly, detention might limit the financial resources a person has to dedicate to her defense (if, for instance, detention results in loss of wages). Fourth, detention prevents an accused person from engaging in commendable behavior that might mitigate her sentence or increase the likelihood of acquittal, dismissal, or diversion. Such foreclosed conduct includes paying restitution, seeking drug or mental health treatment, and demonstrating commitment to educational or professional advancement.[42] Fifth, detention might prevent the accused from engaging in reprehensible behaviors that have similar effects on the case outcome, like intimidating witnesses, destroying evidence, or engaging in bad-faith delay tactics. Finally, even if released defendants do not actively seek to delay adjudication, it may be the case that they have better outcomes simply because their cases move more slowly,[43] which entails some inevitable degradation of evidence.

42. *See, e.g.*, IND. CODE § 35-38-1-7.1(b) (2016) (listing mitigating factors for sentencing, including a showing that the defendant "is likely to respond affirmatively to probation" or "has made or will make restitution"); N.C. GEN. STAT. § 15A-1340.16(e) (2016) (listing mitigating factors, including a showing of "substantial or full restitution to the victim"; "good character or . . . a good reputation in the community"; that the defendant "is currently involved in or has successfully completed a drug treatment program or an alcohol treatment program subsequent to arrest and prior to trial"; and that the defendant "supports the defendant's family," "has a support system in the community," "is gainfully employed," or "has a good treatment prognosis, and a workable treatment plan is available").

43. *See, e.g.*, Cohen & Reaves, *supra* note 2, at 7 ("Released [felony] defendants waited a median of 127 days from time of arrest until adjudication, nearly 3 times as long as those who were detained (45 days)."). Many states have contracted speedy trial limits for those detained pretrial. *See, e.g.*, LA. CODE CRIM. PROC. ANN. art. 701(D)(1)(b) ("The trial of a defendant charged with a misdemeanor shall commence within thirty days if he is continued in custody and within sixty days if he is not continued in custody."); *infra* note 136 (noting that in the Harris County dataset, the median time to judgment is 3 days for detained defendants and 125 days for released defendants and that there are large disparities in case resolution time between detained and released defendants even
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B. Challenges for Empirical Study

As a practical matter, testing whether detention has a causal impact on case outcomes is complicated by the fact that those detained are systematically different from those released. Because those who are detained pretrial are likely to have committed more serious crimes, have a longer criminal history, or have less wealth, one might expect to observe differences in case outcomes between detainees and releasees even absent any causal effect of pretrial custody status. To take a simple example, if crime is correlated over time such that more frequent offenders in one period are more likely to offend in future periods and a bail process detains defendants with more past convictions, then one would expect the future recidivism of those detained (who are already high-frequency offenders) to be greater than that of those who are released even when pretrial release does not affect behavior at all. Thus, estimates of the causal effect of bail must properly account for any sorting effect of bail that occurs in the real world.

This sorting effect is further complicated by the fact that defendants themselves usually know whether they are guilty or innocent, but, in general, courts or researchers do not. A defendant who is factually guilty and plans to plead guilty may wish to forgo bail simply to get the punishment over with, anticipating that she will receive credit for time served. On the other hand, a defendant who believes she has a strong case for innocence may have greater incentive to post bail to avoid being detained when innocent.

A final difficulty for measuring the effect of pretrial detention is that data on those factors known to be relevant for determining outcomes tend to be limited. While court records usually contain basic demographic information, current charges, and criminal history, they generally do not contain information on strength of evidence, wealth, or lawyer quality. Because case-level factors such as the quality of evidence cannot generally be observed, empirical studies on the impacts of pretrial detention are subject to the potential for bias in measuring causal effects. The degree of bias depends not only on how significantly the unobserved factors affect the outcome of interest but also on how closely correlated they are with pretrial detention.

after controlling for the charged offense). And simply as a matter of policy, criminal justice system actors tend to prioritize detained defendants' cases in order to minimize the length of pretrial detention. *Cf.* ABA STANDARDS FOR CRIMINAL JUSTICE: SPEEDY TRIAL AND TIMELY RESOLUTION OF CRIMINAL CASES § 12-1.3(b) (3d ed. 2006) ("The time limits concerning speedy trial for detained defendants should ordinarily be shorter than the limits applicable to defendants on pretrial release.").

C. Prior Empirical Literature

Notwithstanding these challenges, there is a body of prior empirical work dedicated to assessing the effects of pretrial detention on criminal justice outcomes. To varying degrees, prior studies have attempted to control for underlying differences between detainees and releasees in order to estimate the true causal effect of detention. Earlier studies, which preceded the advent of computers and digitized data systems, could only control for a few variables at a time. More recent studies have been able to control for a wider variety of variables, moving closer to an accurate causal estimate.

The first major empirical study addressing the causal effect of detention was an innovative study known as the Manhattan Bail Project conducted by the Vera Foundation starting in 1961.[44] The researchers conducted pretrial interviews and verifications designed to assess flight risk on the basis of community ties.[45] They then recommended ROR to the judge in a randomly selected subset of cases that met certain criteria for low flight-risk status.[46] For the rest of the cases that met the same criteria, they offered no recommendation to the judge.[47] To a modern researcher, this experimental approach is an ideal way of determining the causal impact of pretrial detention: those for whom the ROR recommendation was communicated should be statistically identical to those for whom it was not, with the only difference being a higher pretrial release rate among the former. If the two groups also had differing case outcomes, one could infer that the difference was due to pretrial detention.

Disappointingly, the researchers did not report overall outcomes for these two groups. They only compared case outcomes among those in the reporting group who were released versus those in the nonreporting group who were detained.[48] They found that those detained were dramatically more likely to be found guilty and sentenced to prison than those who were not.[49]

The Manhattan Bail Project made a profound contribution but was limited by its design. Because the two groups actually compared were subject to the additional filter of a release decision, they cannot be considered statistically identical. Comparing their outcomes might therefore provide a biased view of the causal impact of pretrial detention.[50]

44. Charles E. Ares et al., *The Manhattan Bail Project: An Interim Report on the Use of Pre-Trial Parole*, 38 N.Y.U. L. REV. 67, 71 (1963).

45. *Id.* at 72-73.

46. *Id.* at 74.

47. *Id.*

48. *Id.* at 87 & tbl.12.

49. *Id.*

50. A follow-up study using data on seven hundred of the Manhattan Bail Project cases used some basic cross-tabulations to find that the correlation between detention and

*footnote continued on next page*

Another important early study came to different conclusions. John Goldkamp in 1980 examined whether pretrial detention affected case outcomes at three separate stages in a criminal proceeding: dismissal at the outset, entry into a diversion program, and verdict.[51] Focusing on roughly eight thousand Philadelphia criminal cases, Goldkamp found that after controlling for six variables—charge seriousness, existence of detainers/warrants, number of prior arrests, being under supervision, existence of open cases, and number of charges—pretrial detention had no discernible impact on any of these phases.[52] The only outcome for which Goldkamp found some support for a causal channel of influence was on the likelihood of being sentenced to incarceration.[53] His method, however, divided the impact pretrial detention has on case outcomes into three steps: the impact on dismissal, diversion, and adjudication. The impact on any of these steps would necessarily be less than the overall impact on case outcomes.

Empirical scholarship evaluating pretrial detention waned in the 1980s and 1990s, but the new millennium brought new research. Since 2000, a number of correlational studies have been published that have provided evidence on this subject, although not always as a primary focus of the paper.[54] Although most of these studies have evaluated relatively small samples, they have taken advantage of improvements in data to control for a wider variety of underlying differences in defendant characteristics. Most of these studies have found that pretrial detention is correlated with unfavorable case outcomes for defendants.[55]

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unfavorable case outcomes is not explained away by prior record, bail amount, type of counsel, family integration, or employment stability. Anne Rankin, *The Effect of Pretrial Detention*, 39 N.Y.U. L. REV. 641, 655 (1964).

51. John S. Goldkamp, *The Effects of Detention on Judicial Decisions: A Closer Look*, 5 JUST. SYS. J. 234, 237 (1980).

52. *Id.* at 240-44.

53. *Id.* at 249.

54. *See* sources cited *infra* note 55.

55. *E.g.*, Gail Kellough & Scot Wortley, *Remand for Plea: Bail Decisions and Plea Bargaining as Commensurate Decisions*, 42 BRIT. J. CRIMINOLOGY 186, 187 (2002) (finding that a negative personality assessment by police increases the likelihood of detention in Canada and that those detained are more likely to plead guilty); Michael J. Leiber & Kristan C. Fox, *Race and the Impact of Detention on Juvenile Justice Decision Making*, 51 CRIME & DELINQ. 470, 471 (2005) (assessing how the interaction between race and detention status affects juvenile delinquency case outcomes); J.C. Oleson et al., *The Effect of Pretrial Detention on Sentencing in Two Federal Districts*, 33 JUST. Q. 1103, 1117-19 (2014) (showing that pretrial detention is associated with an increased prison sentence in federal courts); Christine Tartaro & Christopher M. Sedelmaier, *A Tale of Two Counties: The Impact of Pretrial Release, Race, and Ethnicity upon Sentencing Decisions*, 22 CRIM. JUST. STUD. 203, 203-04, 208, 218 (2009) (examining heterogeneity in the effects of pretrial detention on sentences of incarceration for minority defendants in different Florida counties); Marian R. Williams, *The Effect of Pretrial Detention on Imprisonment Decisions*,
*footnote continued on next page*

The new millennium also brought the publication of several important research studies funded by nonprofit organizations. Although not published in peer-reviewed or academic journals, these papers represented an advance because of their large sample sizes. In 2007 and 2008, the New York Criminal Justice Agency (CJA) published two reports that assessed the impact of pretrial detention on case outcomes for nonfelony and felony cases, respectively.[56] Several years later, the Laura and John Arnold Foundation funded a pair of studies that assessed the impact of pretrial detention on case outcomes and on future crime.[57]

With sample sizes in the tens to hundreds of thousands, the CJA and Arnold Foundation studies controlled for offense type (within eight to ten main classifications) along with gender, ethnicity, age, and criminal history.[58] They still found substantial correlations between pretrial detention and conviction rates, sentences of incarceration, and postdisposition crime.[59] The Arnold Foundation study in particular found large effects: low risk defendants detained throughout the pretrial period were 5.41 times more likely to be sentenced to jail and 3.76 times more likely to be sentenced to prison than similarly situated defendants who were released at some point before trial.[60]

These large effects, however, are unlikely to represent the true causal effect of pretrial detention. The researchers in these studies did not control for the particular offense charged, using only broad offense categories such as "violent" offenses.[61] Without controlling for the exact offense, the researchers are unlikely to be comparing apples to apples. There is substantial variation within each broad offense class. Those released on a violent offense are more

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28 CRIM. JUST. REV. 299, 306, 313 (2003) (showing that pretrial detention is correlated with increased incarceration sentences using a small sample of Florida felony cases).

56. PHILLIPS, NONFELONY CASES, *supra* note 10; MARY T. PHILLIPS, N.Y.C. CRIMINAL JUSTICE AGENCY, PRETRIAL DETENTION AND CASE OUTCOMES, PART 2: FELONY CASES (2008) [hereinafter PHILLIPS, FELONY CASES].

57. Christopher T. Lowenkamp et al., The Hidden Costs of Pretrial Detention (2013) [hereinafter Lowenkamp et al., Hidden Costs], http://www.arnoldfoundation.org/wp-content/uploads/2014/02/LJAF_Report_hidden-costs_FNL.pdf; Christopher T. Lowenkamp et al., Investigating the Impact of Pretrial Detention on Sentencing Outcomes (2013) [hereinafter Lowenkamp et al., Investigating the Impact], http://www.arnoldfoundation.org/wp-content/uploads/2014/02/LJAF_Report_state-sentencing_FNL.pdf.

58. PHILLIPS, NONFELONY CASES, *supra* note 10, at 11, 23 tbl.7; Lowenkamp et al., Investigating the Impact, *supra* note 57, at 12 & tbl.2.

59. PHILLIPS, NONFELONY CASES, *supra* note 10, at 55-56; PHILLIPS, FELONY CASES, *supra* note 56, at 57-59; Lowenkamp et al., Hidden Costs, *supra* note 57, at 4; Lowenkamp et al., Investigating the Impact, *supra* note 57, at 4.

60. Lowenkamp et al., Investigating the Impact, *supra* note 57, at 11.

61. PHILLIPS, NONFELONY CASES, *supra* note 10, at 76 tbl.B; Lowenkamp et al., Investigating the Impact, *supra* note 57, at 12 tbl.2.

likely to be facing minor charges like simple assault, and those detained on a violent offense are more likely to be facing serious charges like murder or rape. Given that likely variation, the study does not necessarily compare outcomes across similarly situated individuals, and differences in outcomes would be expected even in the absence of a causal effect.

In general, then, despite major improvements in data and analysis, prior research on the downstream effects of pretrial detention has controlled for only a limited set of confounding variables, making it difficult to distinguish the effect of detention from the effects of underlying differences between detainees and releasees. Previous studies have typically controlled only for limited measures of prior criminal involvement and grouped cases into a limited number of offense categories. They have also tended to lack controls for defendants' wealth, which clearly affects pretrial release in cash bail systems and is also likely to affect defendants' access to high-quality defense counsel and services—such as counseling or drug treatment—that might encourage courts to impose more lenient sentences. It is difficult, in other words, to exclude the possibility of "omitted variable bias" from the outcomes this past research has yielded.[62]

The newest empirical work on pretrial detention effects seeks to avoid the problem of omitted variable bias by deploying quasi-experimental design. A working paper by Megan Stevenson, one of this Article's Authors, uses a natural experiment in Philadelphia to estimate the causal effect of pretrial detention on case outcomes.[63] Stevenson exploits the fact that defendants have their bail set by different bail magistrates with broad discretion. Some magistrates tend to set bail at unaffordable rates, while others set bail more leniently.[64] The group of defendants randomly assigned to a high-bail magistrate are detained pretrial at higher rates than the group assigned to the more lenient magistrate.[65] In all other respects, however, the two groups should be similar.[66]

62. *See generally* CAROL S. ANESHENSEL, THEORY-BASED DATA ANALYSIS FOR THE SOCIAL SCIENCES 90 (2d ed. 2013) (defining "omitted variable bias" as "the result of leaving out of the model a variable" that both "is a cause of the dependent variable" and "is associated with one or more of the independent variables in the model" and noting that this results in the model "incorporating the effect of the omitted variable in the error term").

63. Stevenson, *supra* note 35.

64. *See id.* at 41 fig.2 (showing average detention rates per magistrate for defendants facing different types of charges).

65. *See id.* at 17.

66. *Id.* at 14-15.

Stevenson ultimately finds that defendants who receive the strict magistrate are more likely to plead guilty and receive harsher sentences.[67] Since this quasi-experimental method eliminates the bias that results from comparing individuals with different underlying characteristics, it produces a causal estimate of the effect of pretrial detention. Stevenson also performs a standard regression analysis (controlling for a detailed set of variables) that yields similar results, suggesting that with enough controls, researchers can produce reasonable estimates of the causal effects of pretrial detention even in the absence of a natural experiment.[68]

Several other very recent working papers developed in 2016 have used a research design similar to Stevenson's to assess the impact of pretrial detention in New York, Miami, Philadelphia, and Pittsburgh. Gupta, Hansman, and Frenchman find that money bail in Philadelphia leads to a 12% increase in the likelihood of conviction, and money bail in Philadelphia and Pittsburgh leads to a 6-9% increase in the yearly probability of receiving a new charge.[69] Dobbie, Goldin, and Yang find that pretrial detention leads to a 27.3% increase in the likelihood of conviction but prevents a 37.6% increase in the likelihood of rearrest pretrial.[70] They find no discernible postdisposition crime effects of pretrial detention, but they do find suggestive evidence that pretrial detention decreases ties to the formal employment sector.[71] Their research is set in both Philadelphia and Miami.[72] Leslie and Pope look at the impacts of pretrial detention in New York City and estimate that detention leads to a 13 percentage point increase in the likelihood of conviction among felony defendants and a 7.4 percentage point increase in the likelihood of conviction among misdemeanor defendants.[73]

This Article offers several contributions to the empirical literature on pretrial detention. First, like the other 2016 studies, this Article offers both a quasi-experimental analysis and a regression analysis with a large set of highly detailed controls. Second, it focuses on misdemeanor defendants and assesses the effect of pretrial detention on both case outcomes and future crime. Third,

67. *Id.* at 17-18.

68. *Id.* at 18.

69. Arpit Gupta et al., *The Heavy Costs of High Bail: Evidence from Judge Randomization*, 45 J. LEGAL STUD. 471, 472-73 (2016).

70. Will Dobbie et al., *The Effects of Pre-Trial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges* 3 (Nat'l Bureau of Econ. Research, Working Paper No. 22511, 2016), http://www.nber.org/papers/w22511.pdf.

71. *See id.* at 3-4.

72. *Id.* at 1.

73. Emily Leslie & Nolan G. Pope, The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from NYC Arraignments 15-16 (Nov. 9, 2016) (unpublished manuscript), http://home.uchicago.edu/~npope/pretrial_paper.pdf.

it offers the first large-scale empirical study of misdemeanor pretrial detention in Harris County—which, because its pretrial process is representative of many jurisdictions[74] and because of the sheer number of people it affects, presents a particularly illuminating location of study.

## II. Misdemeanor Pretrial Detention in Harris County

### A. The Misdemeanor Pretrial Process

The present analysis focuses on Harris County, Texas, the third-largest county in the United States. Countywide, around 70,000 misdemeanors are processed each year, and these cases are adjudicated by the Harris County Criminal Courts at Law.[75] Indigent defense in the county was historically provided though an appointed private counsel system, but a public defender office was established in 2010 and has gradually expanded.[76] Still today, though, the public defender office handles only a small subset of misdemeanor cases, with the remainder of cases assigned to appointed private counsel.[77]

After arrest and booking, alleged misdemeanants are held at the county jail in downtown Houston until a bail hearing occurs.[78] Bail hearings are held continuously every day during the year and nearly always occur within twenty-four hours of the initial booking.[79] To manage the large volume of new defendants who arrive each day, the county has developed a videoconferencing

74. *See infra* Part II.B.

75. We report this total misdemeanor count on the basis of data on file with the Authors.

76. Tony Fabelo et al., Council of State Gov'ts Justice Ctr., Improving Indigent Defense: Evaluation of the Harris County Public Defender 12, 14 & fig.3 (2013), http://harriscountypublicdefender.org/wp-content/uploads/2013/10/JCHCPDFinal Report.pdf.

77. The Harris County Public Defender office represents only those misdemeanor defendants who are severely mentally ill, as identified by a computer algorithm on the basis of three criteria: (1) they have taken prescribed psychoactive drugs in the last ninety days; (2) they have a diagnosis of schizophrenia, bipolar disorder, or major depression; or (3) they are assigned to the jail's specialty mental health housing. This totals approximately 2500 persons annually. E-mail from Alex Bunin, Harris Cty. Pub. Def., to Paul Heaton, Senior Fellow, Univ. of Pa. Law Sch., (June 16, 2016, 11:41 AM) (on file with authors).

78. For details on some of the processes described here, see First Amended Class Action Complaint, ODonnell v. Harris County, No. 16-cv-01414 (S.D. Tex. Sept. 1, 2016); and Harris Cty. Criminal Courts at Law, Rules of Court (as amended through Dec. 1, 2016), http://www.ccl.hctx.net/attorneys/rules/rules.pdf. Others are reported as described in e-mail and telephone correspondence with Alex Bunin of the Harris County Public Defender office. *See, e.g.*, E-mail from Alex Bunin, Harris Cty. Pub. Def., to Megan Stevenson, Quattrone Fellow, Univ. of Pa. Law Sch. (Oct. 20, 2015, 2:57 PM) (on file with authors).

79. First Amended Class Action Complaint, *supra* note 78, ¶¶ 54, 56.

process for bail hearings. Defendants are taken to a conferencing facility within the jail and participate in the hearing by speaking to a split video screen that shows a prosecutor and the magistrate handling the hearing.[80] Bail hearings are typically handled in an assembly-line fashion, with some hearings lasting approximately a minute.[81] Unless they have somehow managed to retain counsel, which is very rare, defendants are not represented at the bail hearings.[82]

Magistrates making bail determinations have access to information from a pretrial services report that includes the defendant's prior criminal record and can also ask the defendant questions during the bail hearing.[83] Texas statutory law defines bail as "the security given by the accused that he will appear and answer before the proper court the accusation brought against him."[84] Notwithstanding this unitary focus on ensuring appearance, the law also directs the officer who sets bail to consider public safety in determining the bail amount.[85]

In Harris County, bail is typically set according to a bail schedule promulgated by the county courts. The schedule proposes bail of $500 for a low-level misdemeanor by a defendant with no prior criminal record and escalates bail in $500 increments according to the seriousness of the charged offense and the number of prior felony and misdemeanor convictions, up to a maximum of $5000.[86] Although release without bail—referred to as a "personal bond" in Harris County—is allowed, it is not included on the schedule and occurs infrequently.[87] Prosecutors have an opportunity during the bail hearing to argue for departures from the schedule.

80. *See id.* ¶¶ 56, 65-66.

81. *Id.* ¶ 67.

82. *Id.* ¶ 61.

83. This practice was described in e-mail and telephone correspondence with Alex Bunin. *See* E-mail from Alex Bunin to Megan Stevenson, *supra* note 78.

84. TEX. CODE CRIM. PROC. ANN. art. 17.01 (West 2015).

85. *Id.* art. 17.15(5).

86. Harris Cty. Criminal Courts at Law, *supra* note 78, § 9.1, at 15. A nonprofit advocacy organization, Equal Justice Under Law, recently filed a civil rights lawsuit against Harris County on behalf of misdemeanor pretrial detainees, alleging that reliance on the bail schedule violates the Due Process and Equal Protection Clauses. *See* First Amended Class Action Complaint, *supra* note 78, at 2; *see also* Lise Olsen, *Harris County's Pretrial Detention Practices Challenged as Unlawful in Federal Court*, HOUS. CHRON. (May 19, 2016), http://www.houstonchronicle.com/news/houston-texas/houston/article/Harris-County-s-pretrial-detention-practices-7759726.php.

87. *See* TEX. CODE CRIM. PROC. ANN. art. 17.03 (defining "personal bond" and judicial authority to order it); HARRIS CTY. PRETRIAL SERVS., 2015 ANNUAL REPORT (n.d.) (noting that only 8.5% of misdemeanor defendants in Harris County posted personal bond in 2015).

Nearly all misdemeanor defendants in Harris County are theoretically eligible for appointed counsel in the event of indigence.[88] To apply for appointed counsel, defendants must complete a form that asks about their income and other assets.[89] Judges may also direct questions regarding defendants' financial circumstances from the bench either during the bail hearing or in later proceedings.[90] When it would facilitate a more orderly transaction of court business, particularly when defendants appear pro se (without a lawyer), the judge may appoint indigent counsel without a formal request.[91] Although Texas law and the county's written policy prohibit judges from considering whether a defendant made bail in deciding whether she qualifies for appointed counsel (except to the extent it reflects her financial circumstances),[92] there is considerable anecdotal evidence suggesting that this rule is violated in practice.[93] Thus under the current system, one potential impact of posting bail may be to alter one's chances of receiving an appointed attorney.

### B. Representativeness of Harris County's Misdemeanor Pretrial System

The study is set in a populous urban area with criminal justice structures comparable to those in many large cities in the United States. Harris County is the third-largest county in the United States and is home to Houston, the nation's fourth-largest city.[94] Harris County boasts a diverse population of about 4.5 million residents, 19.6% of whom are African American, 42% Hispanic/Latino, 25.3% foreign-born, and 17.3% living below the federal

88. *See* Harris Cty. Dist. Courts, Standards and Procedures: Appointment of Counsel for Indigent Defendants § 1.0 (2009), https://www.justex.net/JustexDocuments/0/FDAMS/standards.pdf. The analysis that follows controls for public defender representation on the theory that these cases may be systematically different from other cases.

89. *See id.* § 1.1.

90. *See id.* § 1.4.2.

91. This is apparent on the basis of the data, which sometimes show counsel appointed without a motion (often on the day of final adjudication) and were confirmed in a personal conversation with Alex Bunin, Harris County Public Defender, on July 27, 2016.

92. TEX. CODE CRIM. PROC. ANN. art. 26.04(*l*)-(m); Harris Cty. Dist. Courts, *supra* note 88, § 1.2.

93. *See, e.g.*, Emily DePrang, *Poor Judgment*, TEX. OBSERVER (Oct. 12, 2015, 8:56 AM CST), https://www.texasobserver.org/poor-judgment; Paul B. Kennedy, *Who Is Indigent in Harris County?*, DEF. RESTS (Jan. 25, 2010, 3:28 PM), http://kennedy-law.blogspot.com/2010/01/who-is-indigent-in-harris-county.html.

94. *See Largest Counties in the U.S. 2015*, *supra* note 11; *see also* Population Div., U.S. Census Bureau, The 15 Most Populous Cities: July 1, 2014 (2015), https://www.census.gov/content/dam/Census/newsroom/releases/2015/cb15-89_table3.xlsx.

poverty line.[95] In Houston, which houses nearly half the county's population, the 2014 Federal Bureau of Investigation (FBI) index crime rate was 1 per 100 residents for violent crime and 5.7 per 100 residents overall, placing Houston thirtieth among the 111 U.S. cities with populations above 200,000.[96]

While the Bureau of Justice Statistics has collected extensive information about more serious crimes,[97] there are no nationally representative data available on the numbers of misdemeanor arrests and convictions, let alone data about pretrial detention rates, bail, or sentencing. Nonetheless, other empirical studies on the effects of pretrial detention provide some insight into misdemeanor pretrial practices in other large urban areas and suggest that Harris County is not an outlier. In New York City, about 35% of misdemeanor defendants spend more than a week detained pretrial[98] and 14% of misdemeanor defendants remain in jail during the entire pretrial period.[99] Sixty-seven percent of misdemeanor defendants in New York City are convicted, and the vast majority of these convictions are guilty pleas.[100] Ten percent of misdemeanor defendants in New York City receive a sentence of incarceration.[101]

In Philadelphia, 25% of misdemeanor defendants remain in jail for more than three days after the bail hearing, and 50% are found guilty of at least one charge.[102] Philadelphia, however, differs from many other jurisdictions in its broad use of bench trials (trials in front of a judge instead of a jury), which are the default for misdemeanor cases.[103] As a result, the plea rate is much lower: only half of misdemeanor convictions in Philadelphia are achieved through plea negotiation. Sixteen percent of misdemeanor defendants receive a

95. *QuickFacts: Harris County, Texas*, U.S. CENSUS BUREAU, https://www.census.gov/quickfacts/table/PST045215/48201 (last visited Mar. 3, 2017).

96. The Authors' calculations are based on data compiled from the FBI. *See Table 8: Offenses Known to Law Enforcement*, FED. BUREAU INVESTIGATION, https://ucr.fbi.gov/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-8/Table_8_Offenses_Known_to_Law_Enforcement_by_State_by_City_2014.xls (last visited Mar. 3, 2017).

97. *Data Collection: National Judicial Reporting Program (NJRP)*, BUREAU JUST. STAT., http://www.bjs.gov/index.cfm?ty=dcdetail&iid=241 (last visited Mar. 3, 2017).

98. For the data underlying the Authors' calculations, see PHILLIPS, NONFELONY CASES, *supra* note 10, at 53 fig.11.

99. *See id.* at 14 & tbl.2.

100. For the data underlying the Authors' calculations, see Leslie & Pope, *supra* note 73, at 30 tbl.2.

101. *Id.*

102. The Philadelphia statistics in this paragraph are from the Authors' calculations using data from Philadelphia court records. The underlying data can be requested from the Pennsylvania court system. *See Public Record Policies*, UNIFIED JUD. SYS. PA., http://www.pacourts.us/public-record-policies (last visited Mar. 3, 2017).

103. Stephen J. Schulhofer, *Is Plea Bargaining Inevitable?*, 97 HARV. L. REV. 1037, 1051 (1984).

sentence of incarceration, including those who receive a sentence of time served.

The statistics in Harris County differ somewhat, but not dramatically, from those in New York City and Philadelphia.[104] The detention rate is a bit higher: about 53% of misdemeanor defendants in Harris County are detained for more than seven days. The conviction rate is similar (68%), and, as in New York City, most convictions come about through guilty pleas (65%). The misdemeanor incarceration rate in Harris County is much higher than in the other two cities; 58% of those convicted receive a jail sentence, including time served. The average jail sentence, however, is relatively short at less than a month.

Other pretrial practices in Harris County are regularly observed in other jurisdictions. For example, the use of a schedule specifying bail amounts based on the charge and prior convictions is not uncommon.[105] A 2009 survey of pretrial services around the country indicates that 57% of jurisdictions use videoconferencing for bail hearings,[106] as Harris County does. This same survey also indicates that about half of U.S. jurisdictions, like Harris County, do not provide representation at bail hearings. The use of commercial bail bondsmen is also fairly widespread. Four states—Illinois, Kentucky, Oregon, and Wisconsin—have banned the commercial bail bond industry, but bail bondsmen remain a common source for bail funds in most other states.[107] Thus, although Harris County has unique features, it is similar to many other jurisdictions in detaining substantial numbers of misdemeanor defendants pretrial; in its reliance on a cash bail schedule; in holding short, videoconfer-

104. All Harris County statistics in this paragraph are from the Authors' calculations using the dataset described in Part II.C below.

105. The nonprofit advocacy organization Equal Justice Under Law has filed ten class action challenges in eight states to money bail practices that do not take ability to pay into account, including the use of bail schedules. The jurisdictions where lawsuits have been filed include both large urban areas, like San Francisco and Harris County, and smaller cities like Clanton, Alabama. *See Ending the American Money Bail System*, *supra* note 21 (describing the organization's litigation campaign); *see also, e.g.*, Walker v. City of Calhoun, No. 4:15-CV-0170-HLM, 2016 WL 361612, at *14 (N.D. Ga. Jan. 28, 2016) (granting the plaintiff's motion for a preliminary injunction and holding that the city "may not continue to keep arrestees in its custody for any amount of time solely because the arrestees cannot afford a secured monetary bond"); Order, Pierce v. City of Velda City, No. 4:15-cv-570-HEA (E.D. Mo. June 3, 2015) (granting declaratory judgment for the plaintiffs, including new standards for pretrial release); First Amended Class Action Complaint, *supra* note 78.

106. Pretrial Justice Inst., *supra* note 35, at 45 tbl.35.

107. *See* Cohen & Reaves, *supra* note 2, at 2, 4 (showing that 48% of all pretrial releases studied were based on financial conditions, most of which—33% of all releases—were on surety bond).

ence bail hearings without court-appointed representation for the accused; and in the prominent role of a commercial bail bond industry.

### C. Data Description

Study data are derived from the court docket sheets maintained by the Harris County District Clerk.[108] These docket sheets include the universe of unsealed criminal cases adjudicated in the county and document considerable detail regarding each case. This Article focuses on 380,689 misdemeanor cases filed between 2008 and 2013. For each case, the docket data include the defendant's name, address, and demographic information; prior criminal history; and most serious charge. To obtain information about the neighborhood environment for each defendant, the court data were linked by the defendant's zip code of residence—which was available for 85% of defendants—to zip code-level demographic data from the 2008-2012 American Community Survey.[109] The docket data also report the time of the bail hearing; the bail amount; whether and when bail was posted, the judge and courtroom assignment; motions and other metrics of procedural progress; and the final case outcome, including whether the case was resolved through a plea.

The discussion and analysis below focus on the bail amount set at the initial hearing, which is likely to have a disproportionate impact on detention both because it is the operative bail during the early period when most defendants who post bail do so and because it serves as a reference point for any further negotiations over bail. However, in Harris County, as in other jurisdictions, judges can exercise discretion to adjust bail as additional facts about a particular defendant or case come to light.

The court data have a few important limitations. Only a single, most serious charge is recorded in each misdemeanor case. This makes it impossible to clearly differentiate defendants with large numbers of charges. Although court personnel have access to criminal history information from across the state, these data only include criminal history data covering offenses within Harris County, not other jurisdictions. A further limitation is that the data do not always provide clear indications of failure to appear, an obvious outcome of interest in a comprehensive evaluation of bail since one of the main purposes of bail is to ensure appearance in court. The attorney information is also incomplete—although the data indicate the identity of court-appointed

108. For these data, see *Search Our Records and Documents*, HARRIS COUNTY DISTRICT CLERK'S OFF., http://www.hcdistrictclerk.com/edocs/public/search.aspx (last visited Mar. 3, 2017).

109. *American FactFinder—American Community Survey*, U.S. CENSUS BUREAU, https://factfinder.census.gov/faces/nav/jsf/pages/programs.xhtml?program=acs (last visited Mar. 3, 2017).

counsel and the fact that they are court-appointed, the identity of counsel is not observed when privately retained. The data do not clearly distinguish those who proceed pro se and those who hire a private attorney. Race and citizenship data are not carefully verified, so they may not be fully reliable.[110] Additionally, although these data represent the near universe of criminal cases in the county, a small fraction of criminal court records are sealed or otherwise unavailable on the online court docket database. And finally, arrestees who successfully complete diversion programs through which they avoid having charges filed are not included in the data.[111]

110. This was observed in e-mail and telephone correspondence with Alex Bunin. *See, e.g.*, E-mail from Alex Bunin to Paul Heaton, *supra* note 77.

111. An example of one such program operating in Harris County is the First Chance Intervention Program, which diverts first-time, low-level marijuana offenders. *See 1st Chance Intervention Program*, HARRIS COUNTY OFF. DISTRICT ATT'Y, https://app.dao.hctx.net/OurOffice/FirstChanceIntervention.aspx (last visited Mar. 3, 2017).

**Table 1**
Characteristics of Defendants by Pretrial Release Status

| | **Overall** | **Detained** | **Released** |
|---|---|---|---|
| Convicted | 68.3% | 79.4% | 55.7% |
| Guilty plea | 65.6% | 76.8% | 52.8% |
| Any jail sentence | 58.7% | 75.0% | 40.2% |
| Jail sentence days | 17.0 | 25.4 | 7.4 |
| Any probation sentence | 14.0% | 6.2% | 22.9% |
| Probation sentence days | 49.4 | 22.5 | 79.9 |
| Requested appointed counsel | 53.2% | 71.3% | 32.6% |
| Amount of bail | $2225 | $2786 | $1624 |
| Class A misdemeanor | 30.7% | 33.5% | 27.4% |
| Male | 76.8% | 79.8% | 73.5% |
| Age (years) | 30.8 | 31.6 | 30.0 |
| Black | 38.9% | 45.6% | 31.3% |
| Citizen | 74.1% | 71.5% | 77.0% |
| Prior misdemeanors | 1.51 | 2.08 | 0.85 |
| Prior felonies | 0.74 | 1.11 | 0.31 |
| *Sample size* | *380,689* | *202,386* | *178,303* |

Table 1 above presents summary statistics describing the sample of misdemeanor defendants examined in the study. Any individual who did not post bond within the first seven days following the bail hearing is categorized as detained. The data reveal stark differences in plea rates, conviction rates, and jail sentences for detainees as compared to those who are able to make bail. However, detainees are also different from releasees across a number of preexisting characteristics that seem likely to be related to case outcomes. For example, detainees are much more likely to request appointed counsel due to indigence (71% versus 33%), are disproportionately charged with more serious Class A misdemeanors (34% versus 27%), and have more extensive prior criminal records. Thus, it remains unclear to what extent the differences in case outcomes reflect the effect of detention versus other preexisting differences across the two groups.

D. Pretrial Detention and Wealth

Not listed in Table 1 because it is unobserved in the data—though probably the most obvious characteristic that would differentiate the detained and released—is wealth. A clear concern with a predominantly cash-based bail system as exists in Harris County is that individuals with money or other liquid assets will be most able to make bail, skewing the system in favor of the wealthy. Although the individual wealth of each defendant is unobserved, one can proxy for defendant wealth based upon median income in each defendant's zip code of residence. To illustrate the prominent role of wealth in the pretrial system, Figure 1 below calculates the pretrial detention rate for defendants residing in each of the 217 zip codes observed in the data that contain at least fifty defendants and plots this against the median household income in the zip code.

The pattern is striking. Those who come from poorer zip codes are substantially more likely to be detained than those from wealthier zip codes. Only about 30% of defendants from the wealthiest zip codes are detained pretrial, versus around 60-70% of defendants from the poorest zip codes.

Although Figure 1 suggests that wealth may be an important determinant of pretrial release, it is possible that the patterns in Figure 1 reflect differential offending by defendants from lower-income zip codes. If, for example, lower-income misdemeanor defendants commit more serious offenses or tend to have more extensive criminal histories, one might expect them to be assigned higher bail amounts and be more likely to be detained for legally appropriate reasons. Figure 2 below, however, demonstrates that the strongly negative wealth/detention relationship persists when focusing on the pool of defendants with no prior charges in Harris County. Moreover, Figure 3 below, which shows the average seriousness of the charge, demonstrates that there is no relationship between wealth and offense seriousness.[112] Thus, the wealth gradient does not seem to be explainable simply as a matter of more extensive or more serious offending by low-income defendants.

Nor does the lower detention rate of wealthier defendants appear to be caused solely by differences in evidence or other factors related to public safety. This can be demonstrated by constructing an expected probability of detention for each defendant from the actual detention rates of all other defendants in the sample who were assigned identical bail amounts at the initial hearing. This measure captures the average custody outcome for all defendants who were considered by the court as representing the same degree of risk, at least as expressed through the bail amount. For defendants falling within each decile of the zip code income distribution, one can then compare this expected detention measure to the true rates of detention.

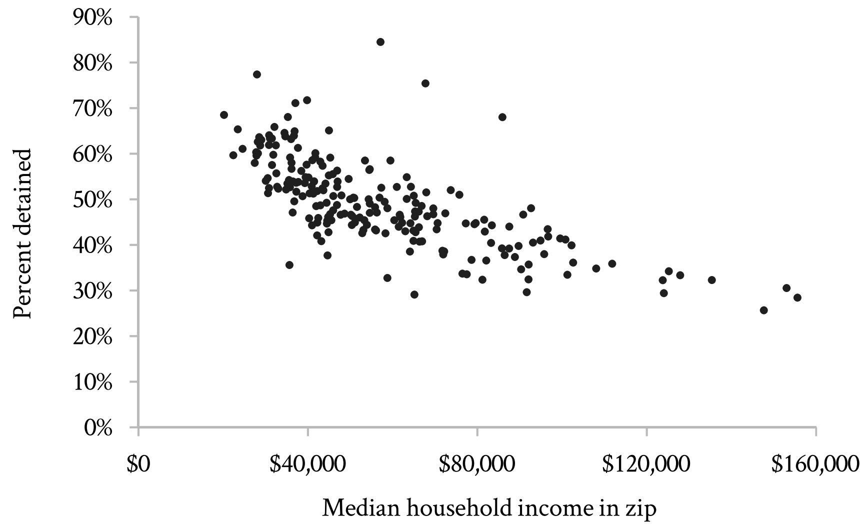
Figure 4 below reveals a striking pattern in which the actual detention rates for the poorest defendants are substantially above those that would be predicted based upon their assigned bail and the reverse is true for the

112. In a zip code-level regression of average seriousness on median household income, the estimated coefficient on income is practically small and not statistically significant.

wealthiest defendants. Defendants in the lowest-income decile are about 15% (8 percentage points) more likely to be detained than would be expected based on their court-assigned bail. Those in the top decile are 19% (9 percentage points) less likely to be detained. Because these comparisons already account for the bail amount, the differences cannot be plausibly attributed to anything in the court record that might implicate worthiness for bail. It thus appears that wealthier defendants are advantaged in their ability to obtain pretrial release beyond what would be expected simply based on the merits of their case.

**Figure 1**

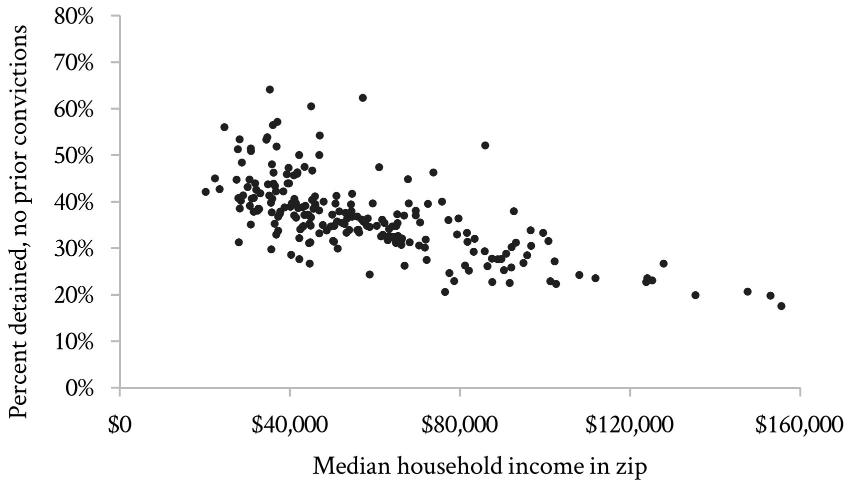
Relationship Between Wealth and Detention Rates Among Misdemeanor Defendants in Harris County, Texas




This figure reports detention rates versus median income by zip code. Each dot in the chart represents defendants residing within a particular zip code.

**Figure 2**

Relationship Between Wealth and Detention Rates Among Misdemeanor Defendants with No Prior Criminal Record in Harris County, Texas



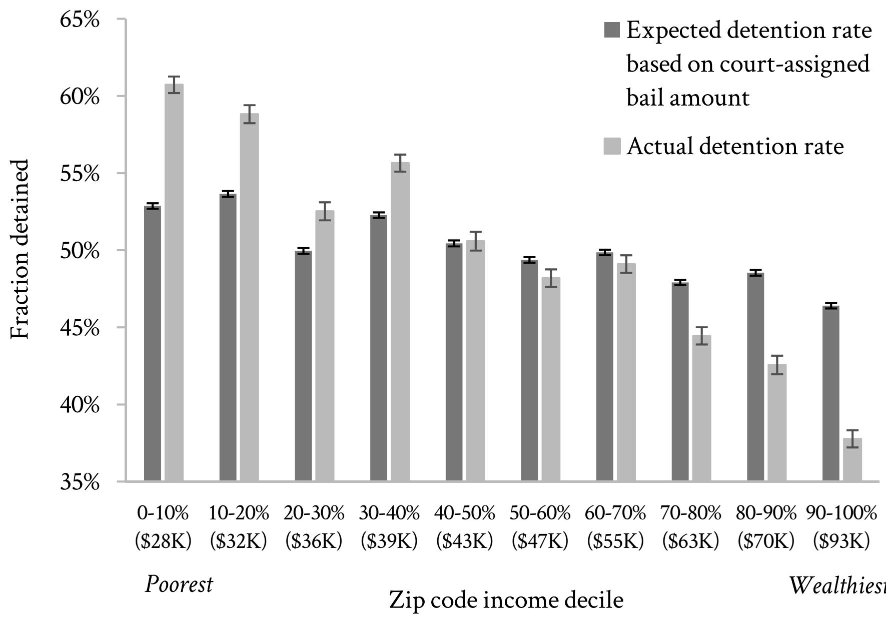

This figure reports detention rates versus median income by zip code. Each dot in the chart represents defendants residing within a particular zip code.

**Figure 3**
Relationship Between Wealth and Offense Seriousness Among Misdemeanor Defendants in Harris County, Texas


This figure reports the fraction of defendants charged with a Class A misdemeanor versus median income by zip code. Each dot in the chart represents defendants residing within a particular zip code.

**Figure 4**

Expected Detention Rates Versus Actual Detention Rates by Income Decile




Expected detention rates are calculated by comparing defendants to all other defendants with equal bail amounts. Whiskers represent 95% confidence intervals.

## III. Analysis of the Effects of Pretrial Detention

This Part reports results from both a regression analysis and a quasi-experimental analysis of the effects of pretrial detention. The regression analysis yields more generalizable and more precise estimates, but it at least potentially suffers from bias due to failure to fully control for all relevant factors affecting outcomes. The quasi-experiment should address such omitted variable problems. However, both approaches ultimately yield similar conclusions, suggesting the regression analysis may work well when sufficient controls are available.

A. Regression Analysis

To begin to assess the impacts of bail, a series of regression models were estimated where the unit of observation is a case, the outcome is whether the case resulted in conviction, and the primary explanatory variable is a 0/1 indicator for whether a particular defendant was detained pretrial. The models progressively introduce richer and richer sets of control variables to assess the extent to which the measured "effects" of detention might simply be attributable to uncontrolled factors other than detention.[113] As additional controls are added, the estimates may get closer to the true causal estimate. But these estimates are all subject to the limitation that there may be uncontrolled, unobserved factors—such as defendant wealth or quality of evidence—that bias these as estimates of the causal effects of detention.

Table 2 below reports the regression estimates. The first specification reports a coefficient from a bivariate regression with no controls. The baseline conviction rate for those not detained is 56%, so detainees are 23.6 percentage points (or 42%) more likely to be convicted. Specification 2 adds controls for the charged offense along with the age, race, gender, and citizenship status of the defendant. In contrast to prior research, which tends to group crimes into a small number of general categories (for example, "sex offense" or "minor public order offense"), this regression controls for 121 different offense categories representing a wide range of different types and severities of offenses. The only prior study to focus entirely on misdemeanors controlled for 10 offense categories.[114] These additional controls do not dramatically alter the measured relationship between detention and conviction.

Specification 3 adds controls for defendant build, skin color, and nativity and also includes a full set of fixed effects for the zip code of residence. One clear drawback of attempting to measure the effects of pretrial detention through regression modeling is that wealth and socioeconomic status (SES) are strong predictors of case outcomes and seem likely to be correlated with pretrial detention, but they are rarely observed in court data. By including zip code controls, these regression models in essence compare two individuals who come from the same neighborhood but differ in pretrial detention status. While wealth and SES can vary within a zip code, the high degree of

113. We do not seek, by this methodology, to measure the effect of any of the variables we progressively introduce. For that purpose, this methodology would be flawed. *See generally* Jonah B. Gelbach, *When Do Covariates Matter?: And Which Ones, and How Much?*, 34 J. LAB. ECON. 509, 509-10 (2016) (discussing sequence sensitivity when adding covariates). We simply seek to assess the impact of detention under various specifications of increasing complexity.

114. *See* PHILLIPS, NONFELONY CASES, *supra* note 10, at 76 tbl.B.

socioeconomic segregation that exists in Harris County[115] (as in many urban areas in the United States) suggests that zip codes can be a reasonable proxy for SES.[116] Once again, the additional controls do not dramatically alter the results.

Specification 4 includes indicators for the number of prior misdemeanor and felony charges and convictions as additional controls. Controlling for prior criminal history is important because prior offenses enter directly into the bail schedule and thus have a direct influence on detention. Prior criminal history may also factor into the outcome of the current case, particularly with regard to sentencing. As noted above, the criminal history data only capture criminal justice contacts within Harris County.[117] After conditioning on factors such as citizenship status, nativity, and residence location, however, it seems less likely that patterns of out-of-county offending would differ systematically between those who are detained and those who are released. This suggests that the available controls may be adequate for capturing prior criminal activity. Somewhat surprisingly, controlling for prior criminal activity only modestly reduces the estimated relationship between detention and conviction.

Although individual wealth is not directly observed, one can further proxy for wealth with whether a particular defendant requested appointed counsel, thereby claiming indigence. Specification 5 adds an indigence indicator to the set of control variables. Controlling for this proxy for wealth appreciably reduces the coefficient estimate on detention, but the estimate remains statistically significant and practically large.

Specification 6 adds a full set of indicators for the actual bail amount set. This specification compares individuals who have the same bail set at their hearing—and who are also equivalent across all variables enumerated in prior specifications—but differ in their detention status. Since the amount of cash bail is, at least in theory, supposed to adjust to reflect the risk of flight and threat to public safety, conditioning precisely to the bail amount is akin to comparing individuals only to others whom the court has deemed equally risky. On a conceptual level, comparing individuals with similar court-determined risk is attractive because it means that any subsequent difference in outcomes cannot result from the sorting function of the bail process; the controls completely account for the instrumentality of sorting, which is the

115. *See, e.g.*, Harris Cty. Cmty. Servs. Dep't, Program Year 2013-2017: Consolidated Plan 3-25, 3-37 (2013), http://www.csd.hctx.net/PYConsolidatedPlan.aspx (showing substantial amounts of segregation by income and educational attainment in Harris County).

116. Using average zip code wealth as a proxy for individual wealth is a common method when individual-level data are not available. For a more detailed discussion, see Arline T. Geronimus et al., *On the Validity of Using Census Geocode Characteristics to Proxy Individual Socioeconomic Characteristics*, 91 J. AM. STAT. ASS'N 529 (1996).

117. *See supra* Part II.C.

bail amount. In this, the preferred specification, pretrial detention is associated with a 14 percentage point (or 25%) increase in the likelihood of conviction.

**Table 2**

Regression Estimates of the Effect of Pretrial Detention on Conviction

| **Specification** | | |
|---|---|---|
| 1. | No controls | 0.236** |
| | | (0.001) |
| 2. | Add controls for offense and basic demographics | 0.266** |
| | | (0.002) |
| 3. | Add controls for zip code of residence and other characteristics | 0.255** |
| | | (0.002) |
| 4. | Add controls for prior criminal history | 0.220** |
| | | (0.002) |
| 5. | Add control for a claim of indigence | 0.151** |
| | | (0.002) |
| 6. | Add control for bail amount | 0.140** |
| | | (0.002) |

This table reports coefficient estimates from linear probability regressions estimating the relationship between pretrial detention and whether a misdemeanor defendant is convicted. The unit of observation is a case, and the sample size is 380,689. The dependent variable is an indicator for whether a particular defendant in a case was convicted, and the primary explanatory variable of interest is an indicator for whether the defendant in the case was released pretrial. Each table entry reports a coefficient from a separate regression; coefficients on other control variables are unreported. The mean conviction probability among those not detained was 0.557. Specification 1 is a simple bivariate regression. Specification 2 adds controls for defendant age (85 categories), gender, race (6 categories), citizenship status (3 categories), charged offense (121 categories), and week of case filing (289 categories). Specification 3 adds controls for the defendant's skin tone (14 categories), build (5 categories), whether the defendant was born in Texas, and zip code of residence (223 categories). Specification 4 adds controls for the number of prior misdemeanor and felony charges (10 misdemeanor and 10 felony categories) and convictions (10 misdemeanor and 10 felony categories). Specification 5 adds an indicator for whether a defendant requested appointed counsel due to indigence. Specification 6 adds a full set of initial bail amount fixed effects (315 categories) as additional controls. Because the public defender handles a nonrandom subset of misdemeanors, all regressions with controls include an indicator for cases handled by the public defender. Robust standard errors are reported in parentheses. ** indicates an estimate that is statistically significant at the 0.01 level.

One variable not included in these specifications that might be important is the type of defense representation actually provided (for instance, hired private counsel, public defender, appointed private counsel, or no counsel). It is not included for two reasons: First, it cannot fully control for representation type because the data do not distinguish between those who hire a private attorney and those who choose to represent themselves.[118] While one can control for whether the defendant receives a court-appointed attorney, this specification is difficult to interpret, as it essentially places those with a hired attorney and those representing themselves in the same category. Second, it might not be optimal to control for counsel type even if the data were available. The type of counsel may itself be an outcome of whether the defendant is detained pretrial; to control for it is thus to ignore one important effect of detention. Changes to detention policy would likely also alter the type of representation received by defendants.[119]

Finally, controlling for counsel type might actually introduce a new source of bias. In general, statistical practice cautions against controlling for variables that are not predetermined (for instance, variables that are influenced by the main variable of interest). The evidence suggests that judges are more likely to approve a request for counsel if the defendant is detained.[120] This suggests that releasees who receive court-appointed attorneys may be poorer and have more challenging cases than detainees with appointed counsel. Thus, controlling for attorney status would tend to bias the results toward zero; instead of comparing similarly situated individuals, one would be comparing relatively wealthy detainees with relatively poor releasees.

Nonetheless, for the sake of completeness, a specification that controls for whether the defendant received a court-appointed attorney was also estimated. The estimated coefficient was 0.042 with a $p$-value $< 0.01$—a smaller bail/conviction relationship, but one that remains statistically significant and relevant for policy purposes. This is not the preferred specification, however, due to both the data limitations and the difficulties of interpreting the results of a regression that controls for one of the outcomes of pretrial detention.

The basic message from the analysis of conviction is that accounting for preexisting differences in detainees and releasees is important, but even after controlling for a fairly wide range of relevant characteristics, pretrial detention remains a sizeable predictor of outcomes.

118. In Harris County, judges will as a rule not proceed in misdemeanor cases without eventually assigning counsel. But in rare cases, defendants insist on representing themselves. *See* E-mail from Alex Bunin to Paul Heaton, *supra* note 77.

119. Detention may also affect attorney type through other channels. Those who have lost their job as a result of detention, for instance, may be less able to afford a private attorney.

120. There is some evidence that judges see the posting of bail as an indication that a defendant is not indigent enough to merit public defense. *See* DePrang, *supra* note 93. In Harris County, 90% of detainee requests for counsel are granted but only 44% of releasee requests, according to the Authors' calculations. This could be because the act of paying bail is interpreted as evidence that the defendant has funds or because detainees are unable to work while detained.

Table 3 below extends the analysis to consider a range of additional case outcomes. The first row of the table replicates the previously reported results for conviction. The columns of the table report results from regressions with no controls, with a limited set of controls (basic offense and demographics, similar to much of the past research measuring the effects of detention), and from the preferred specification that controls for a rich set of defendant and case characteristics and the bail amount (equivalent to Specification 6 in Table 2). Although detention has a sizable impact on all outcomes, estimated effects become smaller as one controls for a richer set of defendant and case characteristics. This indicates that prior research, which controlled for a limited set of variables, may indeed have overestimated the causal effect of detention.

The table demonstrates that nearly all of the difference in convictions can be explained by higher plea rates among those who are detained, with detainees pleading at a 25% (13 percentage points) higher rate than similarly situated releasees. We also find that those detained are more likely to receive jail sentences instead of probation. In our preferred specification, those detained are 43% (17 percentage points) more likely to receive a jail sentence and receive jail sentences that are nine days longer than (or more than double that of) nondetainees. This estimate of the impact of pretrial detention includes in the sample those without a jail sentence, so it incorporates both the extensive effect on jail time (those detainees who, but for detention, would not have received a jail sentence at all) and the intensive effect on jail time (those who would have received a jail sentence regardless but whose sentence may be longer as a result of detention). Those detained are both less likely to receive sentences of probation and receive fewer days of probation (including, once again, both the extensive and intensive margin).

These results do not provide definitive evidence on which of the various potential mechanisms linking detention to case outcomes operate in Harris County. However, the overall patterns in Table 3 are consistent with an environment in which released defendants are able to engage in prophylactic measures—maintaining a clean record, engaging in substance abuse treatment or anger management, or providing restitution—that lead to charges being dismissed and encourage more lenient treatment. Detained defendants, in contrast, have essentially accumulated credits toward a final sentence of jail as a result of their detention and are therefore more likely to accede to and receive sentences of imprisonment.

One can also construct estimates of the effects of detention analogous to those presented in Tables 2 and 3 but limiting the sample to various subsets of the defendant population. Comparing the estimated impact of detention across different subgroups offers a means of assessing whether certain types of defendants are more or less disadvantaged by detention. For example, if one mechanism through which detention induces guilty pleas is causing some defendants to "pre-serve" their expected sentences so that contesting guilt has little ultimate effect on the amount of punishment, one might expect to see larger effects of detention for offenses where the expected punishment is low.

**Table 3**

Regression Estimates of the Effect of Pretrial Detention on Other Case Outcomes

| | | Estimated Effect of Pretrial Detention | | |
|---|---|---|---|---|
| **Outcome** | **Average for Those Released** | **No Controls** | **Limited Controls** | **Preferred Specification** |
| Conviction | 0.557 | 0.236** (0.001) | 0.266** (0.002) | 0.140** (0.002) |
| Guilty plea | 0.528 | 0.240** (0.002) | 0.264** (0.002) | 0.133** (0.002) |
| Received jail sentence | 0.402 | 0.348** (0.002) | 0.317** (0.002) | 0.172** (0.002) |
| Jail sentence days | 7.38 | 18.0** (0.10) | 15.85** (0.10) | 8.67** (0.12) |
| Received probation | 0.229 | -0.167** (0.001) | -0.125** (0.001) | -0.076** (0.001) |
| Probation days | 79.9 | -57.5** (0.45) | -41.2** (0.46) | -25.3** (0.55) |

This table reports coefficient estimates from linear regressions estimating the relationship between case outcomes and whether a defendant was detained pretrial. Each entry represents results from a unique regression. The "limited controls" column reports regressions with controls as in Specification 2 of Table 2, and the "preferred specification" column reports regressions with controls as in Specification 6 of Table 2. The "jail sentence days" and "probation days" outcomes include defendants assigned no jail or probation. ** indicates an estimate that is statistically significant at the 0.01 level. Standard errors are reported in parentheses.

Table 4 below reports such a subgroup analysis. It first considers differences by prior criminal history, comparing defendants with no prior charges in Harris County to those with prior charges. Categorizing by charges rather than convictions accounts for the possibility that some individuals who are charged but later acquitted may have nonetheless accumulated experience with pretrial detention. Several mechanisms suggest that there may be different effects of detention for someone who has never been previously detained. First, those with prior experience in detention may experience less psychological or emotional discomfort because they have a clearer idea of what detention entails, a sort of acclimation effect.[121] Second, these defendants may experience fewer collateral consequences of detention, either because they have already been labeled as offenders or because they have accumulated experience in dealing with collateral consequences. Finally, those with no prior record may be more likely to receive plea offers that involve low sanctions, increasing their incentives to accept the plea even if innocent.

Table 4 reveals that defendants without prior records are disproportionately affected by detention. Detention has more than twice the effect on conviction for first-time offenders and appreciably increases their likelihood of being given a custodial sentence. Although other explanations are possible, this pattern is consistent with a scenario in which defendants detained for the first time are particularly eager to cut a deal to escape custody as quickly as possible; more experienced defendants, who perhaps have become acclimated to the jail environment or who face more serious consequences of conviction, are less influenced by their detention status. It appears that one consequence of pretrial detention, at least as practiced in Harris County, is that it causes large numbers of first-time alleged misdemeanants to be convicted and sentenced to jail time, rather than receiving intermediate sanctions or avoiding a criminal conviction altogether.

Table 4 demonstrates few differences in outcomes between "whites" and "nonwhites" or between U.S. citizens and noncitizens.[122] Incentives to post bail may be different for noncitizens with immigration detainers, who are often held in custody for immigration purposes even after posting bail.[123] However, the fact that results are similar for citizens and noncitizens suggests that detainers may not be an important omitted variable here.

There is some important heterogeneity in the effects of custody by the primary offense of record. For driving while intoxicated (DWI), for example, detention has little effect on adjudication of guilt—presumably because there is sufficient evidence from alcohol tests in most cases to convict.[124] But there is

121. *Cf.* Craig Haney, *The Psychological Impact of Incarceration: Implications for Post-Prison Adjustment*, *in* PRISONERS ONCE REMOVED: THE IMPACT OF INCARCERATION AND REENTRY ON CHILDREN, FAMILIES, AND COMMUNITIES 33, 37-38 (Jeremy Travis & Michelle Waul eds., 2003) (discussing acclimation to incarceration).

122. As noted above, *see supra* Part II.C, the race and citizenship designations in the Harris County data may not be wholly reliable.

123. *See* Lena Graber & Amy Schnitzer, Nat'l Immigration Project of the Nat'l Lawyers Guild, The Bail Reform Act and Release from Criminal and Immigration Custody for Federal Criminal Defendants 1 (2013), https://nationalimmigrationproject.org/PDFs/practitioners/practice_advisories/crim/2013_Jun_federal-bail.pdf.

124. *See* TEX. OFFICE OF COURT ADMIN., ANNUAL STATISTICAL REPORT FOR THE TEXAS JUDICIARY: FISCAL YEAR 2014, at 72 (2015), http://www.txcourts.gov/media/885306/Annual-Statistical-Report-FY-2014.pdf (showing that DWI has the highest rate of

*footnote continued on next page*

evidence that those who are not detained are much more readily able to substitute probation for a custodial sentence. The largest effects on conviction accrue for assault and trespassing, two crimes for which physical evidence may be lacking and the ability to obtain statements from witnesses in court may play an important role.[125]

Consistent with the evidence for defendants of varying criminal history, when examining subsets of the defendant population based on assigned bail, the most substantial effects are observed for those with low bail, at least for conviction and type of sentence. Effects on sentence length are largest in absolute terms for those with higher bail amounts, but this is perhaps unsurprising because these defendants face more serious sentences overall. Detention has a greater *relative* effect on sentence length for people with low bail, given their shorter average sentence lengths. One implication of these patterns is that Harris County could potentially achieve much of the benefit of liberalizing access to pretrial release by focusing on those with the lowest bail amounts, which may make reform more politically feasible. This may also be true in other jurisdictions similar to Harris County.

Finally, the effects of bail by zip code wealth quartile were analyzed to determine whether those detained from wealthier zip codes fare as badly in their case outcomes as those from poorer zip codes. Although Table 4 shows that those from the poorest areas of the county are much more likely to be detained, the effects of detention itself are fairly uniform across the wealth distribution. Thus, those who cannot post bond suffer higher conviction rates and a lowered likelihood of probation versus jail even when they come from more affluent parts of the county.

guilty pleas and the highest conviction rate across all categories of misdemeanors in the state).

125. Stevenson observes similar patterns in her Philadelphia data. *See* Stevenson, *supra* note 35, at 21-22, 43 fig.4a (showing that effect sizes are largest among case types where evidence tends to be weaker).

**Table 4**
Estimated Effects of Pretrial Detention for Population Subgroups

| | Group | Group Detention Date | Convic-tion | Sentenced to Jail? | Jail Sentence (Days) | Sentenced to Probation? | Probation Sentence (Days) |
|---|---|---|---|---|---|---|---|
| Criminal history | No prior charges | 0.384 | 0.195** (0.003) | 0.213** (0.003) | 7.07** (0.126) | -0.084** (0.003) | -23.6** (0.909) |
| | Prior charges | 0.634 | 0.092** (0.002) | 0.128** (0.002) | 9.44** (0.177) | -0.057** (0.001) | -23.0** (0.677) |
| Citizen-ship | U.S. citizen | 0.514 | 0.145** (0.002) | 0.163** (0.002) | 8.24** (0.137) | -0.064** (0.002) | -19.9** (0.630) |
| | Noncitizen | 0.586 | 0.114** (0.004) | 0.178** (0.004) | 9.50** (0.219) | -0.099** (0.003) | -36.4** (1.12) |
| Race | White | 0.481 | 0.143** (0.002) | 0.184** (0.002) | 9.63** (0.156) | -0.085** (0.002) | -29.6** (0.784) |
| | Nonwhite | 0.603 | 0.132** (0.003) | 0.148** (0.003) | 7.12** (0.173) | -0.058** (0.002) | -16.5** (0.728) |
| Offense | Drug | 0.464 | 0.150** (0.004) | 0.143** (0.004) | 5.31** (0.142) | -0.033** (0.003) | -7.34** (0.868) |
| | DWI | 0.309 | 0.034** (0.004) | 0.224** (0.005) | 13.22** (0.331) | -0.190** (0.005) | -82.8** (2.35) |
| | Assault | 0.597 | 0.215** (0.007) | 0.210** (0.007) | 15.51** (0.528) | -0.046** (0.005) | -12.3** (2.11) |
| | Theft | 0.592 | 0.151** (0.005) | 0.132** (0.005) | 5.26** (0.245) | -0.094** (0.004) | -23.1** (1.48) |
| | Trespassing | 0.809 | 0.196** (0.008) | 0.229** (0.008) | 8.04** (0.409) | -0.047** (0.004) | -12.5** (1.30) |
| Bond amount | $0-$500 | 0.353 | 0.179** (0.003) | 0.198** (0.003) | 5.75** (0.109) | -0.082** (0.003) | -2.88** (1.02) |
| | $501-$2500 | 0.464 | 0.146** (0.003) | 0.173** (0.003) | 8.42** (0.180) | -0.075** (0.002) | -24.2** (0.975) |
| | $2501+ | 0.704 | 0.085** (0.003) | 0.128** (0.003) | 10.92** (0.265) | -0.053** (0.002) | -25.3** (0.855) |
| Zip code income quartile | 1st quartile | 0.597 | 0.131** (0.004) | 0.175** (0.004) | 9.13** (0.267) | -0.087** (0.003) | -29.6** (1.07) |
| | 2d quartile | 0.550 | 0.127** (0.004) | 0.166** (0.004) | 8.61** (0.261) | -0.084** (0.003) | -27.8** (1.14) |
| | 3d quartile | 0.495 | 0.148** (0.004) | 0.170** (0.004) | 8.25** (0.230) | -0.069** (0.003) | -21.9** (1.17) |
| | 4th quartile (highest) | 0.423 | 0.158** (0.004) | 0.168** (0.004) | 8.32** (0.238) | -0.053** (0.003) | -16.9** (1.37) |

This table reports coefficient estimates from linear regressions estimating the relationship between case outcomes and whether a defendant was detained pretrial for subgroups of the defendant population. Each entry represents results from a unique regression. Controls are as in Specification 6 of Table 2. ** indicates that the estimate is statistically significant at the 0.01 level. Standard errors are reported in parentheses.

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B. Natural Experiment

The preceding analysis indicates that even after controlling for a wide range of defendant and case characteristics, including bail amount (which should capture the information observed by the court when making bail decisions), there remains a large gap in case outcomes between those who are detained and observationally similar defendants who make bail. Nevertheless, it remains possible that some of the differences in outcomes revealed thus far reflect unobserved factors other than pretrial detention that were not controlled for in the regression analysis.

From a purely research perspective, the ideal approach to estimating the causal effect of pretrial detention would be to randomly select a subset of defendants, detain them, and then compare their downstream outcomes with those who were not detained. Random assignment to detention status would help ensure that the two groups were otherwise comparable on other factors that might influence outcomes, including culpability. As a practical matter, however, implementing such an experiment would be unethical.

Absent the ability to run a true experiment, one might seek to identify a naturally occurring "experiment" or some situation that causes pretrial detention to vary across different defendants for reasons unrelated to their underlying characteristics or culpability. Comparing outcomes among those more likely to be detained for such idiosyncratic reasons to those less likely to be detained could offer another measure of the effects of detention.

The next analysis compares defendants with bail hearings earlier in the week to those with hearings later in the week as a sort of natural experiment, under the theory that those with bail set later in the week are more likely to actually make bail. This analysis is limited to bail hearings that occur Tuesday through Thursday so as to focus on a set of days with fairly uniform crime patterns and avoid comparisons between crime occurring on the weekends—which tends to involve different types of actors and activities—and crime occurring on weekdays.[126]

Table 5 below helps illustrate the logic behind this natural experiment, reporting the time elapsed between the bail hearing and posting of bond for

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126. The similarity of cases between Tuesday and Thursday can be seen in Table 6, and the differences between midweek and weekend crimes are from the Authors' calculations using Harris County data.

those who successfully make bail. The first forty-eight hours following the bail hearing appear to be a fairly critical period for making bail, as 77% of all those who eventually make bail do so during this period. Put differently, at the time of the bail hearing, a representative defendant has a 44% chance of being detained until judgment, but after two days have elapsed without yet making bail, the chances of never making bail have risen to 75%.

**Table 5**
Time Elapsed Between Bail Bond Hearing and Release for Misdemeanor Defendants Posting Bond in Harris County, Texas

| | **Number of Defendants** | **Percent of Defendants** |
|---|---|---|
| Same day | 107,327 | 50.30% |
| 1 day later | 50,191 | 23.52% |
| 2 days later | 7598 | 3.56% |
| 3 days later | 3794 | 1.78% |
| 4 days later | 2867 | 1.34% |
| 5 days later | 2493 | 1.17% |
| 6 days later | 2103 | 0.99% |
| 7 days later | 1930 | 0.90% |
| > 7 days later | 35,088 | 16.44% |

Typically, defendants rely on friends or family members to either post cash bail at a predetermined facility[127] or visit a bail bonding company, which then posts a surety bond. The premise behind the natural experiment is that it is easier to get ahold of someone who is willing to show up to post bail on the weekend than during the week. As an example, consider a defendant with a Tuesday bail hearing who then must get in contact with someone to post bail. Family members or friends may be reluctant to disrupt school or work schedules to come to the bail facility and post bond, and they may be more difficult to contact if they are at work or otherwise away from home. A similarly situated defendant with a bail hearing on a Thursday, in contrast, may have an easier time getting ahold of someone who is willing to appear to post bail since the acquaintance could more easily do so on a Saturday.

An additional factor that may contribute to a defendant's ability to make bail is liquidity. Because bail must be paid in cash or cash equivalents (cashier's

127. In Harris County, this is the correctional office complex located at 49 San Jacinto Street in Houston. *See Inmate Bonding Process*, HARRIS COUNTY SHERIFF'S OFF., http://www.sheriff.hctx.net/JailInfo/inmate_info_inmate_bondingprocess.aspx (last visited Mar. 3, 2017).

check or money order) in Harris County,[128] to the extent access to cash varies over the course of the week, this is likely to affect access to pretrial release. Many workers are paid on Friday, and so workers may have more ready access to cash on weekends immediately after being paid than at other times during the week.[129] Thus, this liquidity channel might also explain why those with bail hearings closer to the weekend could be more likely to make bail.

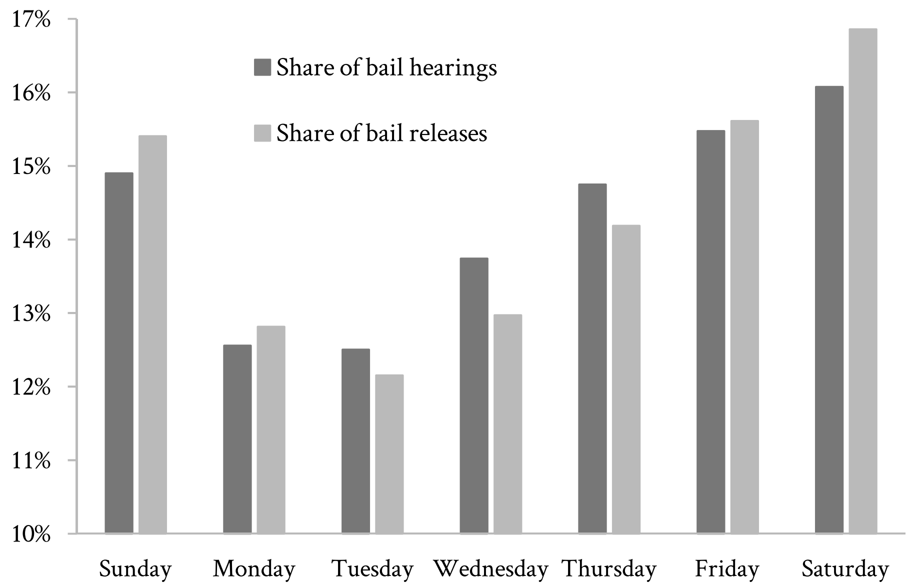
Figure 5 below provides evidence that weekend availability may indeed be a constraint affecting pretrial release by comparing the distribution of bail hearing dates over the course of the week with the dates on which defendants actually post bond. If it were equally easy to get a friend to post bond on any day of the week, one might expect the distribution of release days to closely mirror the distribution of bail hearings. In actuality, however, the figure reveals that releases are disproportionately more likely on Saturdays and Sundays and less likely in the middle of the week. While other factors certainly influence the patterns shown in Figure 5, this simple comparison suggests that it may be easier to obtain release if the critical forty-eight-hour period when pretrial releases most often occur overlaps with a weekend.

128. *See id.*

129. Figure A.1 in the Appendix below provides direct evidence for this point by plotting Google search volume for the terms "payday," "check cashing," and "payday loans" by day of the week. Search volume for "payday" peaks on Friday, and demand for check-cashing services is highest on Friday, Saturday, and Sunday. Searches for "payday loans" show a reverse pattern, with the lowest search traffic observed on Saturdays and Sundays. Payday loans are typically provided by outlets similar to those offering check-cashing services.

**Figure 5**

Comparison of Timing of Bail Hearings Versus Timing of Release by Day of Hearing




The basic premise underlying this natural experiment is that defendants with bail hearings on Thursdays should be largely similar to those with bail hearings on Tuesdays and Wednesdays, including in underlying culpability, but Thursday defendants may be more likely to make bail simply because there is an upcoming weekend when family or a friend can more easily appear with the cash necessary to post bail. Table 6 below explores this possibility by comparing the average characteristics of defendants with bail hearings held on Tuesday, Wednesday, and Thursday and reports results from tests designed to assess whether there is a statistically significant difference across the three groups of defendants in the listed characteristics. Because there is abundant evidence that the composition of offenses varies by day of the week[130] and differences in the charged offense could legitimately affect pretrial detention, the comparisons in Table 6 control for the underlying offense, which is

130. *See, e.g.*, Gerhard J. Falk, *The Influence of the Seasons on the Crime Rate*, 43 J. CRIM. L. & CRIMINOLOGY 199, 212 (1952); Marcus Felson & Erika Poulsen, *Simple Indicators of Crime by Time of Day*, 19 INT'L J. FORECASTING 595, 596 (2003); Chief Justice Earl Warren Inst. on Law & Soc. Policy, When and Where Does Crime Occur in Oakland?: A Temporal and Spatial Analysis (January 2008-July 2013), at 5 (2014), https://www.law.berkeley .edu/files/When_and_Where_Does_Crime_Occur_in_Oakland.pdf.

conceptually equivalent to comparing defendants charged with the same offense who appear at bail hearings on different days.

**Table 6**

Average Characteristics of Defendants by Day of Bail Hearing

| | **Tuesday** | **Wednesday** | **Thursday** | ***p*-Value** |
|---|---|---|---|---|
| Amount of bail | $2297 | $2300 | $2297 | 0.945 |
| Pretrial release | 40.6% | 41.8% | 44.2% | 0.000 |
| Class A misdemeanor | 31.1% | 31.1% | 31.1% | 0.916 |
| Male | 75.3% | 74.9% | 75.2% | 0.159 |
| Age (years) | 30.7 | 30.7 | 30.7 | 0.809 |
| Black | 43.1% | 44.0% | 44.3% | 0.000 |
| Citizen | 76.2% | 76.0% | 76.1% | 0.822 |
| Height (in.) | 67.8 | 67.8 | 67.8 | 0.576 |
| Weight (lbs.) | 164.8 | 164.7 | 164.9 | 0.573 |
| Born in Texas | 46.0% | 46.0% | 46.3% | 0.495 |
| Dark complexion | 20.7% | 20.8% | 21.2% | 0.212 |
| Prior misdemeanor charges | 1.90 | 1.91 | 1.90 | 0.476 |
| Prior misdemeanor convictions | 1.63 | 1.65 | 1.63 | 0.407 |
| Prior felony charges | 1.05 | 1.06 | 1.04 | 0.272 |
| Prior felony convictions | 0.83 | 0.84 | 0.82 | 0.109 |
| Requested appointed counsel | 55.2% | 54.6% | 53.6% | 0.000 |

Reported *p*-values are *p*-values from statistical tests of the null hypothesis that the characteristics listed in each row do not vary on average across all three days of the week. Averages are calculated controlling for underlying offense, so Class A misdemeanor rates are equal by design.

Table 6 suggests a remarkable degree of similarity among defendants with bail hearings on Tuesdays, Wednesdays, and Thursdays across a broad range of case and offender characteristics. While for a few characteristics (including race and appointed counsel request) there are statistically significant differences due to the large sample, the sizes of these differences are quite small. Importantly, as demonstrated in the first row of the table, the actual bail amounts set for these different groups are statistically and practically the same on average, and, as shown in Appendix Figure A.2, the entire distribution of bail amounts is in fact virtually unvarying across day of bail hearing. These patterns provide strong evidence that courts view these three sets of defendants as identical in terms of their worthiness for pretrial release. However, the second row of the table demonstrates that, despite being assessed the same bail amounts, defendants with hearings on Thursday are about 3.6 percentage points (or 9%) more likely to make bail than those with hearings on Tuesday. This difference seems likely attributable to ease in producing cash for bail, which may be greater on weekends for the reasons described above. Because the convenience of paying bail is likely unrelated to a defendant's underlying culpability, the weekend effect shown in Figure 5 offers a plausible source of variation in pretrial detention that might be used to measure its causal effect.[131]

The main results from the analysis based on the natural experiment are presented in Table 7 below. For reference in gauging the magnitude of pretrial detention's impacts, the first column reports the average outcome among defendants released pretrial. The second column reports coefficient estimates from ordinary regressions similar to those presented previously, where the offense, defendant demographics, zip code, prior criminal history, indigence status, and bail amount have been controlled. These estimates differ from those presented in the third column of Table 3 only because the sample for this analysis is restricted to the subset of defendants with bail hearings on Tuesday, Wednesday, or Thursday. The final column reports effects as measured by the natural experiment, which are estimated using two-stage least squares in an instrumental variables (IV) framework.[132]

Several patterns in the table are notable. The natural experiment/IV estimates are large, almost all statistically significant, and, consonant with the regression results, indicate that pretrial detention greatly influences case outcomes. As a general matter, the IV point estimates indicate larger effects of pretrial detention than the regression estimates, suggesting that the estimates presented earlier, to the extent they imperfectly capture the causal effect of pretrial detention due to inability to control for all relevant factors, may in fact

131. One might wonder why defendants arrested on Tuesday do not simply wait until the weekend to post bail and get out. There are several possible explanations. It may be that for those who lose jobs or suffer other major life disruptions as the result of pretrial detention, the damage is done within the first few days and spending money on bail thus offers diminishing returns (especially if the money will go to a bail bondsman). Moreover, for a crime with an expected punishment of a few days' imprisonment, a quick guilty plea may become relatively more attractive after a few days than posting bail.

132. Two-stage least squares is a regression-based approach for measuring the effect of an explanatory variable (here, detention) on an outcome, controlling for other factors, that relies on an "instrument" (here, day of week of bail hearing) that shifts the explanatory variable but is thought to be otherwise unrelated to the outcome. By only exploiting variation in the explanatory variable that arises due to the instrument—which may be less prone to incorporating the influences of unobserved, confounding factors—this approach is designed to deliver better causal estimates. *See* JOSHUA D. ANGRIST & JÖRN-STEFFEN PISCHKE, MOSTLY HARMLESS ECONOMETRICS: AN EMPIRICIST'S COMPANION 113-216 (2009).

understate its effects. Such understatement could occur if, for example, defendants who have spent their funds paying bail are less able to afford a high-quality private attorney than a similarly situated (from the same zip code, charged with the same crime, et cetera) individual who did not pay bail. For all of the outcomes except jail days, however, the difference between the natural experiment and regression estimates is not statistically significant. This suggests that the regression approach yields reasonable causal estimates when sufficient controls are available.

**Table 7**

Effects of Pretrial Detention Based on the Natural Experiment

| | | **Estimated Effect of Pretrial Detention** | |
|---|---|---|---|
| **Outcome** | **Average for Those Released** | **Regression with Controls** | **Natural Experiment** |
| Conviction | 0.542 | 0.122**<br>(0.003) | 0.204**<br>(0.077) |
| Guilty plea | 0.510 | 0.116**<br>(0.003) | 0.234**<br>(0.078) |
| Received jail sentence | 0.410 | 0.142**<br>(0.003) | 0.227**<br>(0.078) |
| Jail sentence days | 7.5 | 7.33**<br>(0.18) | 19.3**<br>(5.39) |
| Received probation | 0.214 | -0.067**<br>(0.002) | -0.124*<br>(0.058) |
| Probation days | 71.2 | -2.2**<br>(0.81) | -42.3<br>(22.1) |

This table reports coefficients from ordinary least squares (column two) and IV (column three) regressions measuring the effect of pretrial detention on the listed outcome. In the IV regressions, the instrument is whether the bail hearing occurred on Tuesday, Wednesday, or Thursday; the unreported first-stage effect is in the expected direction and highly significant. Controls are as in Specification 6 of Table 2. Each reported estimated effect is from a unique regression. The sample size is 146,078, and the sample is limited to defendants with bail hearings on Tuesday, Wednesday, or Thursday. * indicates that the estimate is statistically significant at the 0.05 level. ** indicates that the estimate is statistically significant at the 0.01 level. Standard errors are reported in parentheses.

The natural experiment is not without drawbacks. The underlying assumption of the natural experiment—that those with Thursday bail hearings would have had similar case outcomes to those with Tuesday or Wednesday bail hearings were it not for their enhanced access to pretrial release—is not directly testable. Moreover, because the absolute difference in detention rates across the Tuesday, Wednesday, and Thursday groups is relatively modest—about 4 percentage points—to the extent that there are remaining uncontrolled, unobserved differences across the groups, even small ones, such differences could be the true causal source of what appear to be detention effects. Additionally, although the natural experiment does deliver statistically significant estimates, the confidence intervals on these estimates are much larger. This means that this approach allows us to make less definitive claims about the magnitude of the relationship between detention and outcomes. Thus, the results of this analysis are probably best interpreted as providing evidence that, after including a fairly rich set of controls, regression estimates approximate causal estimates of the effects of detention, and any remaining biases that may exist seem unlikely to fundamentally alter the conclusion that pretrial detention has significant adverse downstream consequences.

### C. Future Crime

In addition to the impacts in the immediate case, pretrial detention carries the potential to affect later criminal activity. Given that a primary policy purpose of pretrial detention is to enhance public safety, such downstream effects—to the extent they exist—should be an important component of any bail assessment.[133] Unfortunately, rigorous estimates of the downstream crime effects of pretrial detention are relatively uncommon in the existing empirical work on bail. This Subpart presents new estimates of the impact of misdemeanor detention on future crime in Harris County.

Downstream crime effects might occur through several mechanisms. Some of these mechanisms would reduce future offending. Most directly, pretrial detention generates an incapacitation effect over the period of pretrial custody. Thus, at least in the immediate period following arrest, detainees should commit fewer crimes than similarly situated releasees simply due to the fact that they are in custody. Second, the experience of being detained might change offender perceptions of the disutility of confinement. To the extent offenders discover that confinement is worse than expected, this could enhance the deterrent effect of criminal law. This mechanism seems more likely to operate for first-time offenders or those with relatively little prior experience with confinement. Lastly, if pretrial detention increases the conviction rate (as the prior analysis suggests) and a prior conviction increases the possible sanctions for additional crime, pretrial detention may augment the expected sanction following a new crime, which would also enhance deterrence.

133. For a discussion of the constitutional dimensions of this point, see Part IV below.

Other mechanisms would increase future offending (or arrests). If detention teaches offenders that confinement is less unpleasant than anticipated, it could reduce deterrence. Detention may also lead to job loss, disrupted interpersonal relationships, or other collateral consequences that change the relative attractiveness of crime in the future.[134] To take a simple example: if a detained defendant loses her job, acquisitive criminal activities such as larceny or robbery might become comparatively more attractive as a means of making up for lost income. Pretrial detainees may also make new social ties or learn new skills through their interactions with other jail inmates that change their propensity for crime.[135] Detention could also paradoxically lower expected sanctions for future crime if detention leads defendants to substitute custodial sentences for probation, because those on probation would face a supervision period where additional crime would trigger punishment for not only the new but also the prior offense. Finally, pretrial detention might alter the probability that future behavior is labeled by the criminal justice system as worthy of sanction. For instance, imagine that Defendant *A* is detained pretrial and then pleads guilty, while similar Defendant *B* is released, enrolls in a treatment program, and ultimately has the charge dismissed. Both are arrested in the future on allegations that the prosecutor views as presenting a marginal case. The prosecutor pursues charges against Defendant *A* because he has a prior conviction but not against Defendant *B*, who does not.

Given that these various potential mechanisms cut in opposite directions, it is not apparent on a theoretical level whether pretrial detention should increase or decrease future crime. This is thus an empirical question of considerable import. To measure recidivism, new charges for each defendant filed during the eighteen months following his or her initial misdemeanor bail hearing were examined. Future crime was measured relative to the date the bail hearing occurred rather than the date the case ended because released defendants' cases take considerably longer to clear than those of detained defendants.[136] Waiting until a case is resolved to start the clock would compare

134. *See supra* note 12 and accompanying text (discussing the collateral consequences of detention).

135. *See, e.g.*, Patrick Bayer et al., *Building Criminal Capital Behind Bars: Peer Effects in Juvenile Corrections*, 124 Q.J. ECON. 105, 108 (2009); Megan Stevenson, Breaking Bad: Mechanisms of Social Influence and the Path to Criminality in Juvenile Jails 3 (Oct. 12, 2015) (unpublished manuscript), http://ssrn.com/abstract=2627394 (presenting evidence of peer effects in juvenile incarceration facilities).

136. Unsurprisingly, the cases of defendants in detention tend to resolve much more quickly. For detained defendants, the median time to first judgment is 3 days, and 80% of defendants have their cases resolved within 18 days. For those who make bond, the median time to first judgment is 125 days. These large disparities are also apparent among defendants charged with the same offense. (These statistics are from the Authors' own calculations.)

released defendants months or in some cases even years after their initial arrest to detained defendants in the days and weeks after their arrest. The recidivism analysis was conducted using conventional regression modeling and continues to adjust for offense, defendant demographics, prior criminal record, zip code of residence, indigence, public defender representation, and time and court of adjudication.[137] Misdemeanor and felony charges were considered separately, and charges were measured cumulatively.

An important feature of this analysis is that, as before in the preferred specification, it fully controls for the bail amount assessed at the bail hearing. This means it compares detained defendants to similarly situated released defendants who were assigned the same bail. As a general matter, one might expect higher recidivism among those who are detained relative to those who are released simply as a result of the correct operation of the bail process. In particular, if the government is correctly assessing defendant risk, higher risk defendants (who will ultimately commit more crime) should be detained more often. This analysis, however, compares two defendants whom the bail process has determined to be of equal risk because their bail was set identically. Thus, the impacts documented here already net out any effects that might reflect the differential sorting of defendants through the bail system.

Figure 6 below plots results from a series of regressions where the outcome is the number of new misdemeanors recorded between the bail hearing and some number of days post-hearing. The actual average number of offenses for the released population is depicted in the figure along with the adjusted rate for the detained population. This adjusted rate is calculated by estimating regressions similar to those in Specification 6 of Table 2 but with new offenses as the outcome and then adding the resultant estimate for the effect of pretrial detention to the actual offending rate for releasees. This, in essence, depicts what the expected misdemeanor offending rate would be for the detainees if they were similar in demographics, case characteristics, prior criminal history, and all other relevant characteristics to the released population. Figure 6 includes bars denoting the 95% confidence intervals for the adjusted rates and shows impacts through the first thirty days post-hearing. The figure demonstrates a steady rise in the number of new charges for both groups over time. This increase over time is a direct consequence of the choice to define the outcome as the cumulative number of new charges. For the first nineteen days after the bail hearing, the incidence of misdemeanors for detainees is below that of releasees, which likely reflects criminal incapacitation from being in

137. We explored applying the natural experiment to recidivism outcomes, but the results, while not inconsistent with the results reported in this Article, were insufficiently precise to provide useful guidance. For example, the instrumental variables estimates implied that detention increases felonies committed as of eighteen months after the bail hearing by 15%, but the 95% confidence interval for this estimate was -59% to 219%.

jail. These differences are statistically significant through day thirteen. By day thirty, however, there is a statistically significantly higher incidence of misdemeanors among the detained population. Thus, despite the initial incapacitation, by one month after the hearing the average number of new charges for detainees has exceeded that of their similarly situated counterparts who were released. To the extent that the rich set of controls allows one to construe these differences as causal, they suggest that pretrial detention has a greater criminogenic than deterrent effect.

Figure 7 below plots similar differences between releasees and detainees in misdemeanor crime but expands the time window to a full eighteen months after the bail hearing. Throughout this later period, the disparity between detainees and releasees remains statistically significant and practically large. Table A.1 in the Appendix below, which reports the numeric estimates underlying Figure 7, shows that the gap between detainees and those released stabilizes at about one year post-hearing and represents a roughly 22% increase in misdemeanor crime associated with detention.

**Figure 6**

New Misdemeanor Charges by Pretrial Release Status During the First Thirty Days After the Bail Hearing

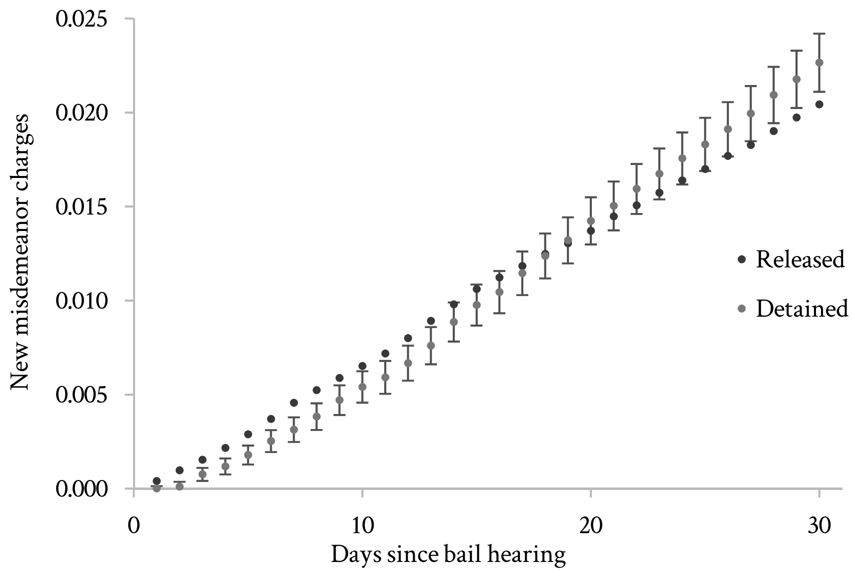


**Figure 7**
New Misdemeanor Charges by Pretrial Release Status During the First Eighteen Months After the Bail Hearing

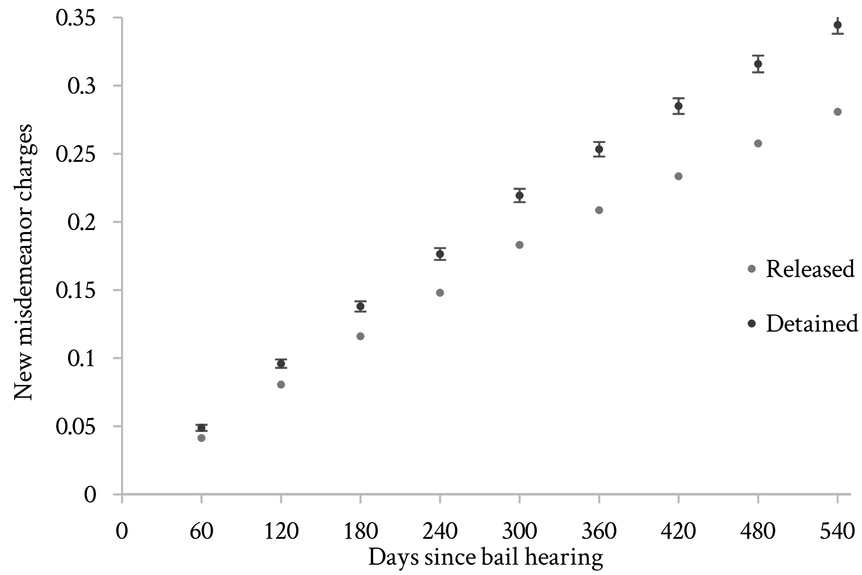



Figure 8 below depicts similar estimates, this time focusing on felonies and considering the time window from 0 to 100 days post-hearing. For felony offending, incapacitation effects from detention appear somewhat longer lasting, with detainees overtaking releasees only after several months. By three months post-hearing, however, there is a statistically significant positive effect of detention on felony offending.

Figure 9 below, which extends the analysis to a full eighteen months after the bail hearing, demonstrates continued heightened felony offending for those who are detained compared to similarly situated releasees. Table A.2 in the Appendix below, which reports the estimates used to construct Figures 8 and 9, demonstrates that the offending gap appears to stabilize toward the end of the sample period, with detainees committing nearly a third more felonies. By eighteen months after the conviction, a group of 100 detained defendants would be expected to have committed about four additional felonies as compared to an observationally similar group of 100 released defendants.

**Figure 8**
New Felony Charges by Pretrial Release Status During the First One Hundred Days After the Bail Hearing

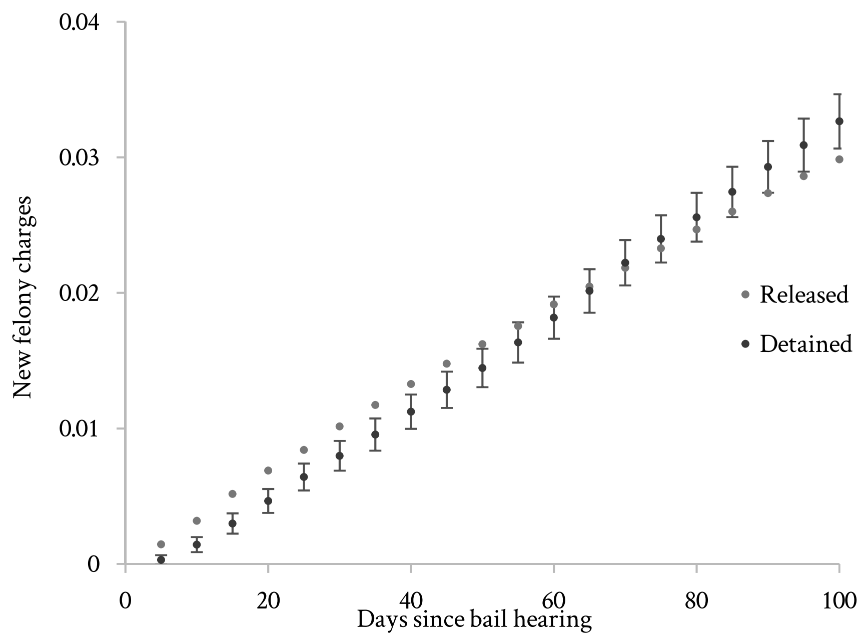
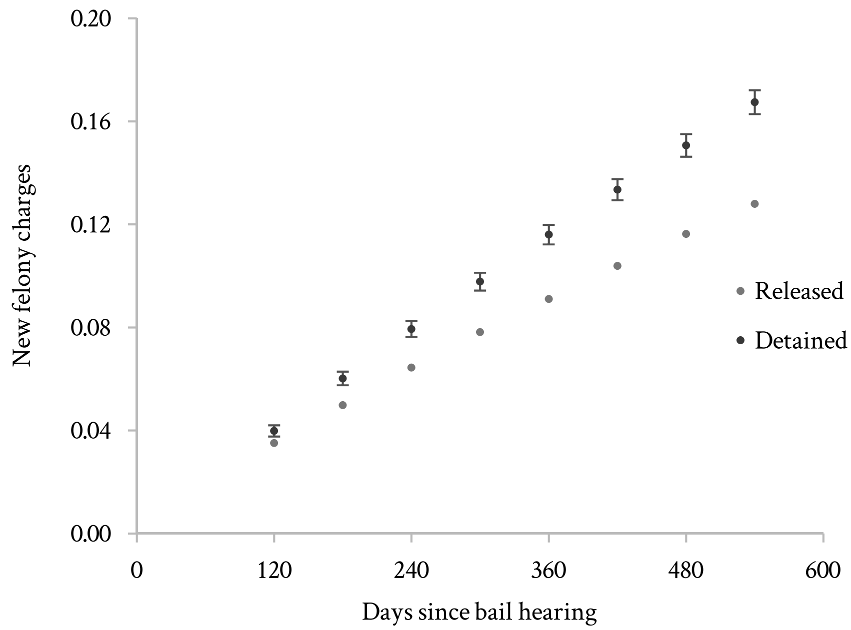


**Figure 9**
New Felony Charges by Pretrial Release Status During the First Eighteen Months After the Bail Hearing




The notion that pretrial detention might actually increase future crime is consistent with recent research that suggests incarceration might itself be criminogenic. A paper by Michael Mueller-Smith, also set in Harris County, uses a research design that leverages random assignment to judges to estimate the causal effect of incarceration on future crime.[138] He finds that incarceration for misdemeanor defendants—who are in jail for a median of ten days following the filing of charges—leads to a 6.0 percentage point increase in the likelihood of being charged with a new misdemeanor and a 6.7 percentage point increase in the likelihood of being charged with a new felony.[139] These

138. Michael Mueller-Smith, The Criminal and Labor Market Impacts of Incarceration 2 (Aug. 18, 2015) (unpublished manuscript), http://sites.lsa.umich.edu/mgms/wp-content/uploads/sites/283/2015/09/incar.pdf.

139. *Id.* at 24-27 (showing that in the quarter during which the misdemeanor defendant was in jail—the average jail time being only a tiny fraction of the quarter—there is a 4.6% increase in new misdemeanor charges and a 6.4% increase in new felony charges). Added to these are the average quarterly increases in new misdemeanor and felony charges of 1.4% and 0.3%, respectively. *Id.* at 27 tbl.5.

estimates are not dissimilar to those presented here, although the timing of the effects is somewhat different. Mueller-Smith finds most of the effect within the first three months after charges are filed, while this Article finds a larger effect somewhat further out.[140]

Stakeholders' assessment of the cost of increased crime from pretrial detention may depend in part on whether the increase represents individuals shifting into offending who otherwise would have maintained a clean record or more intensive offending by individuals who would have accumulated charges in any case. To address this question, Table 8 below reports estimates from regressions analogous to those used to produce Figures 6-9 that use an indicator variable for whether a particular defendant had any future charges as the outcome variable of interest. These regressions assess whether some individuals who would not have been expected to offend later if released do so after being detained.

Table 8 reveals that detention increases the share of defendants charged with new misdemeanors by 9.7% as of eighteen months post-hearing. Over the same period, the likelihood of any future felony charges increases by 32.2%. Comparing this eighteen-month estimate for felonies in Table 8 to the eighteen-month estimated effect on the total count of crimes reported in Table A.2 (30.9%) reveals that essentially all of the increase in felony offending can be explained by an increase in the number of individuals accumulating new felony charges. This suggests that detention has broad effects, shifting many defendants who would have avoided future criminal behavior (at least as captured by charges in Harris County) into further contact with the criminal justice system.

140. *See id.* at 26; *see also* Anna Aizer & Joseph J. Doyle, Jr., *Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges*, 130 Q.J. ECON. 759, 763 (2015) (finding that incarceration has a criminogenic effect); Rafael Di Tella & Ernesto Schargrodsky, *Criminal Recidivism After Prison and Electronic Monitoring* 4 (Nat'l Bureau of Econ. Research, Working Paper No. 15602, 2009), http://www.nber.org/papers/w15602.pdf (finding the same). Other papers, however, concluded that incarceration is not in fact criminogenic. *See, e.g.*, Charles E. Loeffler, *Does Imprisonment Alter the Life Course?: Evidence on Crime and Employment from a Natural Experiment*, 51 CRIMINOLOGY 137, 154 (2013).

**Table 8**
Effects of Pretrial Detention on Likelihood of Reoffending

| Crime Type | Follow-Up Time Since Bail Hearing | Fraction of Released Defendants with New Charges | Estimated Effect of Pretrial Detention | Percent Increase |
|---|---|---|---|---|
| Misdemeanor | Thirty days | 0.018 | 0.0024** (0.0006) | 13.7 |
| | One year | 0.152 | 0.0146** (0.0015) | 9.6 |
| | Eighteen months | 0.193 | 0.0186** (0.0017) | 9.7 |
| Felony | Thirty days | 0.009 | -0.0013** (0.0004) | -15.1 |
| | One year | 0.066 | 0.0209** (0.0012) | 31.5 |
| | Eighteen months | 0.088 | 0.0285** (0.0013) | 32.2 |

This table reports coefficients from regressions measuring the effect of pretrial detention on the likelihood defendants are charged with new crimes at various follow-up periods post-bail hearing. Controls are as in Specification 6 of Table 2. Each reported estimated effect is from a unique regression. The sample size is 352,573. * indicates that the estimate is statistically significant at the 0.05 level. ** indicates that the estimate is statistically significant at the 0.01 level. Standard errors are reported in parentheses.

These differences in recidivism are important from a policy perspective. To the extent the estimates identified in this Article can be construed as causal, they suggest that a representative group of 10,000 misdemeanor offenders who are released pretrial would accumulate 2800 new misdemeanor charges and roughly 1300 new felony charges in Harris County in the eighteen months after their release. If this same group were instead detained, they would accumulate 3400 new misdemeanors and 1700 felonies over the same time period—an increase of 600 misdemeanors and 400 felonies. While pretrial detention clearly exerts a protective effect in the short run, for misdemeanor defendants it may ultimately serve to compromise public safety.

## IV. Constitutional Implications

The results reported here are relevant to an array of constitutional questions. As the Supreme Court has affirmed, "[i]n our society liberty is the norm, and detention prior to trial or without trial is the carefully limited exception."[141] Whether or not that remains true as a descriptive matter, it remains the aspiration of the law. The constitutional principles that serve to safeguard pretrial liberty include the Sixth Amendment right to counsel,[142] the Eighth Amendment prohibition on excessive bail,[143] due process,[144] and equal protection.[145] The effects of pretrial detention should inform constitutional analysis in each of these arenas.[146]

This Article is limited, of course, to a particular dataset. It does not support generalization about the downstream effects of pretrial detention in all times and places and for all people. But it adds further evidence to the body of literature finding that pretrial detention causally affects conviction and future crime rates. This Part synthesizes the constitutional implications of such effects in Harris County and wherever else they might exist.

### A. Equal Protection/Due Process: Does Pretrial Detention Produce Class-Based Case Outcomes?

To begin with, the Harris County data and results illustrate the extent to which the Harris County pretrial system produces disparate outcomes for the poor and the wealthy. The principle of equal protection (as applied to the states

141. United States v. Salerno, 481 U.S. 739, 755 (1987).

142. *Id.* amend. VI ("In all criminal prosecutions, the accused shall enjoy the right to a speedy and public trial, . . . and to be informed of the nature and cause of the accusation . . . , and to have the Assistance of Counsel for his defence.").

143. *Id.* amend. VIII ("Excessive bail shall not be required . . . .").

144. *Id.* amend. V ("No person shall . . . be deprived of life, liberty, or property, without due process of law . . . ."); *id.* amend. XIV, § 1 ("[N]or shall any State deprive any person of life, liberty, or property, without due process of law . . . .").

145. *Id.* amend. XIV, § 1 ("[N]or shall any State . . . deny to any person within its jurisdiction the equal protection of the laws."); Bolling v. Sharpe, 347 U.S. 497, 499 (1954) (reading the equal protection principle into the Fifth Amendment Due Process Clause).

146. The Fourth Amendment also protects pretrial liberty. *See* U.S. CONST. amend. IV ("The right of the people to be secure in their persons, houses, papers, and effects, against unreasonable searches and seizures, shall not be violated, and no Warrants shall issue, but upon probable cause, supported by Oath or affirmation, and particularly describing the place to be searched, and the persons or things to be seized."); *see also* Gerstein v. Pugh, 420 U.S. 103, 125 n.27 (1975) ("The Fourth Amendment was tailored explicitly for the criminal justice system, and its balance between individual and public interests always has been thought to define the 'process that is due' for seizures of person or property in criminal cases, including the detention of suspects pending trial.").

by the Fourteenth Amendment[147] and to the federal government by the Fifth Amendment, as a component of due process[148]) prohibits invidious or irrational state discrimination.[149] As a general matter, a claimant must show intentional or facial discrimination in order to prevail on an equal protection claim.[150] When a person's liberty is at stake, however, the Supreme Court has held that conditioning liberty on payment of an amount she cannot afford violates due process and equal protection.[151] More precisely, the Court has prohibited detention for inability to pay a monetary amount unless there are no other means that can meet the state's interests.[152]

Bail schedules have recently drawn criticism—and litigation—for conditioning liberty on a fixed monetary amount. Since 2015, a nonprofit organization called Equal Justice Under Law has challenged the use of money bail schedules in ten jurisdictions on the ground that such schedules, if implemented without consideration of defendants' financial status, violate the Equal Protection and Due Process Clauses.[153] The organization has filed one such lawsuit in Harris County.[154] As of this writing, the Department of Justice

147. U.S. CONST. amend. XIV, § 1 ("No State shall . . . deny to any person within its jurisdiction the equal protection of the laws.").

148. *Id.* amend. V ("No person shall . . . be deprived of life, liberty, or property, without due process of law . . . .").

149. *See, e.g.*, City of Cleburne v. Cleburne Living Ctr., Inc., 473 U.S. 432, 439 (1985) ("The Equal Protection Clause of the Fourteenth Amendment commands that no State shall 'deny to any person within its jurisdiction the equal protection of the laws,' which is essentially a direction that all persons similarly situated should be treated alike." (quoting U.S. CONST. amend. XIV, § 1)); *Bolling*, 347 U.S. at 499 (noting that the Fifth Amendment due process principle includes the same prohibition vis-à-vis the federal government).

150. *See, e.g.*, Washington v. Davis, 426 U.S. 229, 240-42 (1976) (explaining "the basic equal protection principle that the invidious quality of a law claimed to be racially discriminatory must ultimately be traced to a racially discriminatory purpose").

151. *See, e.g.*, Bearden v. Georgia, 461 U.S. 660, 667 (1983) ("The rule of *Williams* and *Tate*, then, is that the State cannot 'impos[e] a fine as a sentence and then automatically conver[t] it into a jail term solely because the defendant is indigent and cannot forthwith pay the fine in full.'" (alterations in original) (quoting Tate v. Short, 401 U.S. 395, 398 (1971))); *id.* at 672-73 (holding that to "deprive the probationer of his conditional freedom simply because, through no fault of his own, he cannot pay the fine . . . would be contrary to the fundamental fairness required by the Fourteenth Amendment"); *see also* Statement of Interest of the United States at 1, Varden v. City of Clanton, No. 2:15-cv-34-MHT-WC (M.D. Ala. Feb. 13, 2015) ("Incarcerating individuals solely because of their inability to pay for their release . . . violates the Equal Protection Clause of the Fourteenth Amendment." (citing *Tate*, 401 U.S. at 398; Williams v. Illinois, 399 U.S. 235, 240-41 (1970); and Smith v. Bennett, 365 U.S. 708, 709 (1961))).

152. *Bearden*, 461 U.S. at 674.

153. *See Ending the American Money Bail System*, *supra* note 21.

154. *See* First Amended Class Action Complaint, *supra* note 78 (challenging the Harris County bail schedule on due process and equal protection grounds).

has submitted a statement of interest in one of the bail schedule lawsuits[155] and an amicus brief in another, asserting that "bail practices that incarcerate indigent individuals before trial solely because of their inability to pay for their release violate[] the Fourteenth Amendment."[156] It also issued a Dear Colleague Letter to state and local courts, making the same point.[157]

The data and results reported here do not directly demonstrate whether Harris County's misdemeanor bail practices result in detention for poverty alone. They do show that more than half the misdemeanor defendants with bail set were nonetheless detained pending trial.[158] The average bail amount for these detainees was $2786.[159] It is possible that some number of these people choose not to post the bail but unlikely that many do. The more likely explanation is that they simply do not have the money. The analysis of detention rates by zip code, furthermore, suggests that wealth is an important determinant of who is detained pending trial.[160]

This Article's results provide stronger evidence that any wealth-based inequality in pretrial detention translates into wealth-based inequality in case outcomes. In this dataset, detention increases the likelihood of pleading guilty by 25% for no reason relevant to guilt.[161] In other words, the results suggest that approximately 17% of the detained misdemeanor defendants in the Harris County dataset who pleaded guilty would not have been convicted at all had they been released pretrial. They pleaded guilty because they were detained.

While there are several possible explanations for this detention effect, it is likely that detention obligates many defendants to serve more time than the likely sentence prior to adjudication. If a guilty plea for "time served" or a noncustodial sentence is an option, many a detained person will take it; the costs of staying in jail to fight a charge are simply overwhelming.[162] More

155. Statement of Interest of the United States, *supra* note 151.

156. Brief for the United States as Amicus Curiae Supporting Plaintiff-Appellee & Urging Affirmance of the Issue Addressed Herein at 3, Walker v. City of Calhoun, No. 16-10521-HH (11th Cir. Aug. 18, 2016).

157. Letter from Vanita Gupta, Principal Deputy Assistant Att'y Gen., Civil Rights Div., U.S. Dep't of Justice, and Lisa Foster, Dir., Office for Access to Justice, U.S. Dep't of Justice, to Colleague 7 (Mar. 14, 2016), https://www.justice.gov/crt/file/832461/download ("[A]ny bail practices that result in incarceration based on poverty violate the Fourteenth Amendment.").

158. *Supra* Table 1.

159. *Supra* Table 1.

160. *See supra* Part II.D.

161. *See supra* Table 3.

162. *See, e.g.*, Curry v. Yachera, No. 15-1692, 2016 WL 4547188, at *3 (3d Cir. Sept. 1, 2016) ("Unable to post his bail, Curry was sent to jail and waited there for months for his case to proceed. While imprisoned, he missed the birth of his only child, lost his job, and feared losing his home and vehicle. Ultimately, he pled *nolo contendere* in order to

*footnote continued on next page*

broadly, the results suggest that the outcome of a bail hearing can profoundly impair the accused's ability to contest the charges against him.[163]

There is now a nationwide movement to make the pretrial system fairer by shifting from the money bail model to a "risk-based" model driven by actuarial assessment of a defendant's risk of flight and rearrest.[164] It is important to note that this shift will not eliminate inequality. Actuarial risk assessment will import the effects of race and class disparities earlier in the system.[165] Without violating the Equal Protection Clause, risk assessment may still result in the disproportionate pretrial detention of poor and minority communities.[166] To the extent detention also changes case outcomes, this means that a risk-based system of pretrial detention could continue to dispense unequal justice. In view of the cost of detention—both its immediate fiscal and human costs and its downstream effects—policymakers should work to avoid this result.

return home."); Sixth Amendment Ctr. & Pretrial Justice Inst., Early Appointment of Counsel: The Law, Implementation, and Benefits 9 (2014), http://sixthamendment.org/6ac/6ACPJI_earlyappointmentofcounsel_032014.pdf (noting that "those who work in criminal justice systems" report that this happens frequently (citing Joel M. Schumm, Am. Bar Ass'n Standing Comm. on Legal Aid & Indigent Defendants, National Indigent Defense Reform: The Solution is Multifaceted 26 (2012), http://www.americanbar.org/content/dam/aba/publications/books/ls_sclaid_def_national_indigent_defense_reform.authcheckdam.pdf)).

163. This is true of any of the potential mechanisms discussed above, *see supra* Part I.A, except if the detention effect results from the inability of detainees to obstruct justice. It seems unlikely, however, that misdemeanor defendants released pretrial routinely engage in obstructionist tactics.

164. *See, e.g.*, Pretrial Justice Inst., *Resource-Based to Risk-Based Pretrial Justice*, PREZI (Aug. 7, 2015), https://prezi.com/h6eboff0oyhx/resource-based-to-risk-based-pretrial-justice.

165. The most universal risk factors for future criminal behavior in current pretrial risk assessment tools are prior contacts with the criminal justice system. *See* Bernard E. Harcourt, *Risk as a Proxy for Race: The Dangers of Risk Assessment*, 27 FED. SENT'G REP. 237, 238-40 (2015); Sandra G. Mayson, Dangerous Defendants 11 tbl.1, app. at 47 (Aug. 10, 2016) (unpublished manuscript) (on file with authors).

166. As a general matter, the equal protection principle only prohibits facial (explicit) and intentional discrimination, not disparate impact alone, Washington v. Davis, 426 U.S. 229, 240-42 (1976), although the line of case law prohibiting incarceration for inability to pay a fine diverges from this framework, *see supra* note 151 and accompanying text. There is an argument that actuarial risk assessment is facially discriminatory if the variables used to predict risk include characteristics like race and income. *See* Sonja B. Starr, *Evidence-Based Sentencing and the Scientific Rationalization of Discrimination*, 66 STAN. L. REV. 803, 811-12, 821-36 (2014).

### B. Sixth Amendment Right to Counsel: Is Bail-Setting a "Critical Stage"?

The results of this Article also suggest that bail-setting should be deemed a "critical stage" of criminal proceedings at which accused persons have the right to the effective assistance of counsel.[167]

Despite arguments by scholars and advocates that accused persons should have the assistance of counsel at bail hearings,[168] that has not been the practical or legal reality. Some jurisdictions provide counsel at bail hearings, but many do not.[169] Federal statutory law does not guarantee the right to counsel at a bail hearing (although it prohibits federal courts from setting money bail that results in pretrial detention, and it requires an adversarial hearing at which the accused has the right to representation before a court can order the person detained).[170] A 2008-2009 survey of state practice found that only ten states uniformly provided counsel at an accused's first appearance.[171] Ten states

167. Rothgery v. Gillespie County, 554 U.S. 191, 212 (2008).

168. *See, e.g.*, Alexander Bunin, *The Constitutional Right to Counsel at Bail Hearings*, CRIM. JUST., Spring 2016, at 23, 47 ("[L]awyers are necessary at initial bail hearings."); Douglas L. Colbert, *Coming Soon to a Court Near You—Convicting the Unrepresented at the Bail Stage: An Autopsy of a State High Court's Sua Sponte Rejection of Indigent Defendants' Right to Counsel*, 36 SETON HALL L. REV. 653, 654-55 (2006) ("[T]he pretrial release or bail determination hearing should be considered a 'critical stage' of a criminal prosecution, triggering the Sixth and Fourteenth Amendments' right to counsel." (footnotes omitted)); Douglas L. Colbert et al., *Do Attorneys Really Matter?: The Empirical and Legal Case for the Right of Counsel at Bail*, 23 CARDOZO L. REV. 1719, 1763-83 (2002) [hereinafter Colbert et al., *Do Attorneys Really Matter?*] (demonstrating that defense representation significantly improves defendants' bail hearing outcomes and arguing for provision of such representation); Douglas L. Colbert, *Prosecution Without Representation*, 59 BUFF. L. REV. 333, 335 (2011) [hereinafter Colbert, *Prosecution Without Representation*] (urging the criminal and human rights law bars to encourage the Supreme Court to articulate a constitutional right to counsel at bail hearings); Douglas L. Colbert, *Thirty-Five Years After* Gideon*: The Illusory Right to Counsel at Bail Proceedings*, 1998 U. ILL. L. REV. 1, 7 [hereinafter Colbert, *Thirty-Five Years After* Gideon] (exploring "the constitutional basis for extending an accused's right to counsel to bail [hearings]"); Gerstein, *supra* note 19, at 1516 (arguing that, because the outcome of a bail hearing "can prejudice the outcome of a plea negotiation," defendants have the right to counsel at bail hearings); Constitution Project Nat'l Right to Counsel Comm., Don't I Need a Lawyer?: Pretrial Justice and the Right to Counsel at First Judicial Bail Hearing (2015), http://www.constitutionproject.org/wp-content/uploads/2015/03/RTC-DINAL_3.18.15.pdf (laying out constitutional and practical arguments for providing counsel at bail hearings); Sixth Amendment Ctr. & Pretrial Justice Inst., *supra* note 162 (explaining the ambiguity of relevant constitutional law but urging jurisdictions to provide counsel at bail hearings and describing how some have done so).

169. *E.g.*, Colbert et al., *Do Attorneys Really Matter?*, *supra* note 168, at 1719 ("Most states do not consider the right to counsel to apply until a later stage of a criminal proceeding—days, weeks or months after the pretrial release determination.").

170. *See* 18 U.S.C. § 3142(c)(2), (f) (2015).

171. Colbert, *Prosecution Without Representation*, *supra* note 168, at 389.

uniformly provided no counsel.[172] The remaining thirty provided appointed counsel "in select counties only."[173]

It remains an open question of constitutional law, meanwhile, whether the Sixth Amendment right to counsel extends to bail hearings. The Sixth Amendment provides that "[i]n all criminal prosecutions, the accused shall enjoy the right . . . to have the Assistance of Counsel for his defence."[174] The Supreme Court has held the right to include the "effective" assistance of counsel with respect to any charge that may carry a sentence of incarceration and the right to an appointed attorney if the accused cannot afford to hire one.[175] As a temporal matter, the right "attaches" at "the first appearance before a judicial officer at which a defendant is told of the formal accusation against him and restrictions are imposed on his liberty."[176] This is the nature of most bail hearings. But to say that the right attaches is not to say that counsel need be present. Rather, once the right attaches, "counsel must be appointed within a reasonable time . . . to allow for adequate representation at any critical stage before trial, as well as at trial itself."[177]

The open question is whether the bail hearing is itself a "critical stage."[178] Unfortunately, the term has no precise definition.[179] The Court has offered many formulations. It most recently described critical stages as those

172. *Id.* at 395-96.

173. *Id.* at 345, 400. *But see* Rothgery v. Gillespie County, 554 U.S. 191, 203-04 (2008) ("We are advised without contradiction that not only the Federal Government, including the District of Columbia, but 43 States take the first step toward appointing counsel 'before, at, or just after initial appearance.'" (quoting Brief of Amicus Curiae the National Ass'n of Criminal Defense Lawyers in Support of Petitioner app. at 1a, *Rothgery*, 554 U.S. 191 (No. 07-440))).

174. U.S. CONST. amend. VI; *see also* Gideon v. Wainwright, 372 U.S. 335, 344 (1963) ("The right of one charged with crime to counsel may not be deemed fundamental and essential to fair trials in some countries, but it is in ours.").

175. McMann v. Richardson, 397 U.S. 759, 771 n.14 (1970) ("It has long been recognized that the right to counsel is the right to the effective assistance of counsel."); *see also* Strickland v. Washington, 466 U.S. 668, 687 (1984) (articulating the test for an ineffective assistance claim); Argersinger v. Hamlin, 407 U.S. 25, 37 (1972) (holding that "absent a knowing and intelligent waiver, no person may be imprisoned for any offense . . . unless he was represented by counsel at his trial"); *Gideon*, 372 U.S. at 342-45 (incorporating the right to counsel, including appointed counsel for indigent persons, against the states).

176. *Rothgery*, 554 U.S. at 194, 199.

177. *Id.* at 212.

178. The *Rothgery* majority stopped short of deciding it. *Id.* at 212 n.15 (emphasizing that the Court was not deciding "the scope of an individual's post-attachment right to the presence of counsel").

179. *See* Van v. Jones, 475 F.3d 292, 312 (6th Cir. 2007) (noting that "[o]ne would welcome a comprehensive and final one-line definition of 'critical stage'" and providing a survey of varying Supreme Court formulations).

"proceedings between an individual and agents of the State . . . that amount to 'trial-like confrontations,' at which counsel would help the accused 'in coping with legal problems or . . . meeting his adversary.'"[180] It has also stated that "those pretrial procedures that would impair defense on the merits if the accused is required to proceed without counsel" constitute critical stages.[181] The Court has classified arraignments,[182] preliminary hearings,[183] pretrial lineups,[184] deliberate attempts to elicit incriminating information from an accused,[185] efforts to elicit consent to a psychiatric interview,[186] and plea bargaining[187] as critical stages.

Some of this case law supports the argument that a bail hearing is a critical stage. In *Coleman v. Alabama*, a plurality of the Court concluded that an Alabama preliminary hearing was a critical stage for reasons that could also apply to bail hearings. It reasoned that, at a preliminary hearing, an effective defense lawyer could (1) "expose fatal weaknesses in the State's case that may lead the magistrate to refuse to bind the accused over," (2) examine witnesses so as to "fashion a vital impeachment tool" for trial "or preserve testimony favorable to the accused," (3) "discover the case the State has against his client and make possible the preparation of a proper defense," and (4) make "effective arguments for the accused on such matters as the necessity for an early psychiatric examination or bail."[188] Three of these four reasons arguably apply to bail hearings as well. At a bail hearing, defense counsel can expose fatal weaknesses in the state's case, learn about the allegations in order to prepare "a proper defense," and make "effective arguments" for an early psychiatric examination or release. The only opportunity that defense counsel has at a preliminary hearing but not at a bail hearing is to examine witnesses.

On the other hand, *Gerstein v. Pugh* presents an obstacle to the argument that the bail hearing is a critical stage. *Gerstein* concerned a postarrest probable cause determination, which would also allow defense counsel—if she were present—to point out fatal flaws in the case, learn about the allegations in order to prepare an effective defense, and make arguments for release. In *Gerstein*,

180. *Rothgery*, 554 U.S. at 212 n.16 (second alteration in original) (quoting United States v. Ash, 413 U.S. 300, 312-13 (1973)).

181. Gerstein v. Pugh, 420 U.S. 103, 122 (1975).

182. Hamilton v. Alabama, 368 U.S. 52, 54-55 (1961).

183. Coleman v. Alabama, 399 U.S. 1, 9-10 (1970) (plurality opinion); White v. Maryland, 373 U.S. 59, 60 (1963) (per curiam).

184. United States v. Wade, 388 U.S. 218, 236-37 (1967).

185. Massiah v. United States, 377 U.S. 201, 204-06 (1964).

186. Estelle v. Smith, 451 U.S. 454, 470-71 (1981).

187. Lafler v. Cooper, 132 S. Ct. 1376, 1385-86, 1388 (2012).

188. *Coleman*, 399 U.S. at 9 (plurality opinion).

however, the Court held that a postarrest probable cause determination is not a critical stage.[189] It reasoned that a postarrest probable cause determination "is addressed only to pretrial custody."[190] The Court acknowledged that "pretrial custody may affect to some extent the defendant's ability to assist in preparation of his defense" but concluded that "this does not present the high probability of substantial harm identified as controlling in *Wade* and *Coleman*."[191]

This Article suggests that the Court's assumption about the limited effect of pretrial custody was incorrect. As noted above, pretrial custody does present a high probability of substantial harm, at least for Harris County misdemeanor defendants.[192] The rise of plea bargaining has only enhanced the importance of the bail hearing. As the Supreme Court has recognized, "[i]n today's criminal justice system, . . . the negotiation of a plea bargain, rather than the unfolding of a trial, is almost always the critical point for a defendant."[193] And pretrial detention puts defendants at a profound disadvantage in plea negotiations vis-à-vis the position they would be in if negotiating from freedom. The disadvantage is not just theoretical; the results reported here suggest that approximately 17% of the detained misdemeanor defendants who pleaded guilty would not have been convicted at all but for their detention. For these defendants, the bail hearing was *the* critical stage of criminal proceedings.[194]

Finally, there is reason to think that representation at bail hearings can reduce the likelihood of detention, and thus of conviction, for this subset of defendants.[195] As a matter of logic, defense counsel should be able to advocate for release by providing the judicial officer charged with pretrial custody determinations with a fuller picture of the accused's financial resources, connections to the community, and, if necessary, appropriate conditions of release.[196] Empirical data bear that logic out. In the mid-1980s, a controlled

189. Gerstein v. Pugh, 420 U.S. 103, 122 (1975).

190. *Id.* at 123. The Court also noted that a probable cause determination does not involve witness testimony, *id.*, but given that the Court has recognized plea bargaining as a critical stage, *see Lafler*, 132 S. Ct. at 1385, this cannot be determinative.

191. *Gerstein*, 420 U.S. at 123.

192. *But see* State v. Williams, 210 S.E.2d 298, 300 (S.C. 1974) ("There is no showing in this record, nor does appellant contend, that anything occurred at the bail hearing which in any way affected or prejudiced his subsequent trial or that was likely to do so.").

193. Missouri v. Frye, 132 S. Ct. 1399, 1407 (2012).

194. *See* Gerstein, *supra* note 19, at 1516 (laying out the argument that "a bail hearing is a critical stage because it can prejudice the outcome of a plea negotiation").

195. *Cf.* Colbert, *Thirty-Five Years After* Gideon, *supra* note 168, at 37 (noting that "a showing that counsel's absence at the bail hearing prejudiced the accused's fair trial rights" would provide grounds for finding that bail-setting is a critical stage).

196. *See, e.g.*, Constitution Project Nat'l Right to Counsel Comm., *supra* note 168, at 30-32 (explaining the opportunity for such advocacy).

experiment funded by the National Institute of Justice found that defendants who had public defenders assigned before the bail hearing spent significantly less time in pretrial detention.[197] In the 1990s, Douglas Colbert, Ray Paternoster, and Shawn Bushway ran a similar experiment in Baltimore, randomly assigning student lawyers to represent a treatment group of defendants for purposes of bail.[198] The assignment of student lawyers to bail hearings increased the pretrial release rate by 15 percentage points.[199] Given the evidence that pretrial release has a significant effect on case outcomes, it is difficult to maintain that the bail hearing is not a critical stage.[200]

### C. Eighth Amendment: When Is Bail or Detention "Excessive"?

#### 1. Cash bail

The raw data from Harris County suggest that Harris County bail officers may be regularly setting bail that violates the Eighth Amendment prohibition on "excessive bail."[201]

The Eighth Amendment requires that a bail determination be individualized. In *Stack v. Boyle*, the Supreme Court held that "the fixing of bail for any individual defendant must be based upon standards relevant to the purpose of assuring the presence of *that* defendant," including "the nature and circumstances of the offense charged, the weight of the evidence against him, the financial ability of the defendant to give bail and the character of the defendant."[202] Bail set higher than an amount "reasonably calculated" to assure

197. ERNEST J. FAZIO, JR. ET AL., NAT'L INST. OF JUSTICE, U.S. DEP'T OF JUSTICE, NCJ 97595, EARLY REPRESENTATION BY DEFENSE COUNSEL FIELD TEST: FINAL EVALUATION REPORT 208, 211 (1985), http://www.ncjrs.gov/pdffiles1/Digitization/97595NCJRS.pdf.

198. Colbert et al., *Do Attorneys Really Matter?*, *supra* note 168, at 1720 (explaining that the study generated "convincing empirical data that the benefits of representation are measurable and that representation is crucial to the outcome of a pretrial release hearing"); *id.* at 1728-31 (describing the appointment of student lawyers); *id.* at 1746-47 (describing the experiment's randomization).

199. *Id.* at 1728-31, 1757; *see also id.* at 1720 (reporting that "more than two and one half times as many represented defendants were released on recognizance from pretrial custody as were unrepresented defendants" and that "two and one half times as many represented defendants had their bail reduced to an affordable amount").

200. *See, e.g.*, Hurrell-Harring v. State, 930 N.E.2d 217, 223 (N.Y. 2010) ("There is no question that 'a bail hearing is a critical stage of the State's criminal process . . . .'" (quoting Higazy v. Templeton, 505 F.3d 161, 172 (2d Cir. 2007))); *cf.* Gonzalez v. Comm'r of Corr., 68 A.3d 624, 637 (Conn. 2013) ("[T]he petitioner had a sixth amendment right to effective assistance of counsel at the arraignment stage in which proceedings pertaining to the setting of bond and credit for presentence confinement occurred . . . .").

201. U.S. CONST. amend. VIII ("Excessive bail shall not be required . . . .").

202. Stack v. Boyle, 342 U.S. 1, 5 & n.3 (1951) (emphasis added) (quoting FED. R. CRIM. P. 46(c) (amended 1966)).

the presence of a particular defendant "is 'excessive' under the Eighth Amendment."[203]

The Supreme Court arguably expanded the permissible purposes of money bail in *United States v. Salerno*,[204] but it did not alter the *Stack* rule that the excessiveness inquiry is a particularized one. *Salerno* held that, in some circumstances, the state may constitutionally deny bail altogether on the grounds of a defendant's dangerousness.[205] Many lower courts have interpreted *Salerno* to authorize consideration of a defendant's dangerousness in setting money bail.[206] That interpretation is questionable but prevalent.[207] Texas, moreover, has maintained a conception of bail focused on ensuring appearance; it defines bail as "the security given by the accused that he will appear and answer before the proper court the accusation brought against him."[208]

Whatever the permissible purposes of money bail, the important point is that bail is an incentive mechanism, and the Excessive Bail Clause requires that it be calibrated to the particular circumstances of each case. Bail set higher than necessary to serve as a compelling incentive for *a particular individual* violates the Excessive Bail Clause.[209] And what constitutes a compelling incentive will vary. For a multimillionaire charged with murder, even hefty bail might be inadequate.[210] For a poor person charged with a misdemeanor, $500 may be excessive.

In Harris County, more than half of misdemeanor defendants with bail set are nonetheless detained pending trial. Their bail amounts appear to have been

203. *Id.* at 5.

204. 481 U.S. 739, 754 (1987) ("Nothing in the text of the Bail Clause limits permissible Government considerations solely to questions of flight.").

205. *Id.* at 752-55.

206. *See, e.g.*, Galen v. County of Los Angeles, 477 F.3d 652, 662 (9th Cir. 2007) ("We also reject Galen's argument that flight risk is the only factor the Commissioner was allowed to consider in setting bail . . . . *Salerno* holds that these non-flight-related considerations are permissible . . . .").

207. Rather, *Salerno* authorized the state to pursue pretrial crime prevention "through regulation of pretrial release." 481 U.S. at 753. Most obviously, this language authorizes the state to pursue pretrial crime prevention through regulation of *who* is released, which was the power at issue in *Salerno*. The Court's language might or might not authorize the use of money bail as a mechanism to discourage crime.

208. TEX. CODE CRIM. PROC. ANN. art. 17.01 (West 2015).

209. *See* United States v. Arzberger, 592 F. Supp. 2d 590, 605-06 (S.D.N.Y. 2008) ("[I]f the Excessive Bail Clause has any meaning, it must preclude bail conditions that are (1) more onerous than necessary to satisfy legitimate governmental purposes and (2) result in deprivation of the defendant's liberty.").

210. *See* Charles V. Bagli & Kevin Flynn, *Durst Jumps Bail, and a Nationwide Dragnet Is on*, N.Y. TIMES (Oct. 17, 2001), http://nyti.ms/2d7dKo4 (reporting that "New York real estate scion" Robert Durst fled a Texas homicide prosecution despite having posted $250,000 bail).

set according to a bail schedule. These facts suggest that their bail amounts do not reflect the individualized determination that the Eighth Amendment requires. To the extent that these bail amounts are greater than "reasonably calculated" to ensure the appearance of individual misdemeanor arrestees, they violate the Eighth Amendment.

It is at least arguable, furthermore, that whenever money bail results in detention because a defendant cannot pay, it is per se excessive. The premise of money bail is that the prospect of some financial loss is a sufficient deterrent to prevent pretrial flight. Detention is not necessary. If the bail is unaffordable and therefore results in detention, it is not functioning as a deterrent at all. It is functioning as an indirect means of detention. The use of unaffordable bail to detain pretrial defendants was precisely the practice that the original Excessive Bail Clause was intended to prevent.[211]

The counterargument is that, in some cases, an unaffordable bail amount is the only amount sufficient to create an adequate disincentive to flee.[212] But if that is so, the reality is that *no* bail can reasonably assure that particular defendant's appearance. In that case, judges should explicitly order detention and explain the reason for doing so.[213] Indeed, the Excessive Bail Clause arguably requires them to take this approach.[214]

211. *See* Samuel Wiseman, *Discrimination, Coercion, and the Bail Reform Act of 1984: The Loss of the Core Constitutional Protections of the Excessive Bail Clause*, 36 FORDHAM URB. L.J. 121, 127 (2008).

212. *See, e.g.*, United States v. McConnell, 842 F.2d 105, 110 (5th Cir. 1988) (rejecting the claim that unaffordable bail violated the Eighth Amendment on the basis that "[t]he court has found that only a substantial financial component will yield a reasonable assurance of McConnell's appearance"); White v. United States, 330 F.2d 811, 814 (8th Cir. 1964) ("The purpose for bail cannot in all instances be served by only accommodating the defendant's pocketbook and his desire to be free pending possible conviction.").

213. *Cf.* Hairston v. United States, 343 F.2d 313, 316 (D.C. Cir. 1965) (Bazelon, C.J., dissenting) ("It may be that [by setting unaffordable bail] the District Court intended to deny bail because there was no adequate assurance of appellant's presence . . . . If so, it should have specified the reason. To deny the reality of bail while maintaining the fiction, perverts and distorts the administration of bail.").

214. *See* Carlisle v. Landon, 73 S. Ct. 1179, 1182 (1953) (opinion of Douglas, J.) (interpreting the Excessive Bail Clause to mean "that a person may not be capriciously held" and that "[t]here must be an informed reason for the detention"); *cf.* 18 U.S.C. § 3142(e) (2015) ("If . . . the judicial officer finds that no condition or combination of conditions will reasonably assure the appearance of the person as required . . . , such judicial officer shall order the detention of the person before trial."). This is not possible in states that guarantee a right to bail, but the right to bail should be understood as a right to release. *See* Schnacke, *supra* note 24, at 63 ("[T]he right to bail should be read as a right to release through the bail process."); *id.* at 21-36, 42, 51-56 (making historical and legal arguments for this conclusion).

Few courts have held that unaffordable money bail is excessive per se. Many lower courts have held that it is not.[215] The Supreme Court has not weighed in one way or the other beyond the case law cited in this Article.[216] But federal statutory law and the American Bar Association's Standards on Pretrial Release are consistent with the argument that unaffordable bail is excessive. Both prohibit the setting of money bail in an amount that results in detention.[217] These authorities do not constitute constitutional law, but they reflect an understanding of the constitutional limits on money bail.

2. Pretrial detention

The results of this Article have implications for the question when pretrial detention itself (as distinct from a money bail amount) is unconstitutionally excessive. This question will become particularly topical as jurisdictions seeking to curtail the use of money bail adopt more explicit preventive detention regimes.[218] In *United States v. Salerno*, the Supreme Court held that the Excessive Bail Clause does not entail an absolute right to bail—that is, it does not prohibit detention without bail in some circumstances.[219] The Court also endorsed public safety as a basis for ordering the pretrial detention of some defendants.[220] But it suggested that the Excessive Bail Clause might require that "the Government's proposed conditions of release or detention not be 'excessive' in light of the perceived evil" they are designed to address.[221] To determine whether the intrusion on pretrial liberty is excessive, courts must

215. *E.g.*, *McConnell*, 842 F.2d at 107 ("[A] bail setting is not constitutionally excessive merely because a defendant is financially unable to satisfy the requirement."); White v. Wilson, 399 F.2d 596, 598 (9th Cir. 1968) ("The mere fact that petitioner may not have been able to pay the bail does not make it excessive."); Byrd v. Mascara, No. 4D16-1424, 2016 WL 3919078, at *1 (Fla. Dist. Ct. App. July 20, 2016) (per curiam) ("[A] defendant's inability to post a certain amount of bond does not render that amount per se unreasonable.").

216. *See* Scott W. Howe, *The Implications of Incorporating the Eighth Amendment Prohibition on Excessive Bail*, 43 HOFSTRA L. REV. 1039, 1039 (2015) ("The Eighth Amendment prohibition on 'excessive bail' is perhaps the least developed of the criminal clauses in the *Bill of Rights*." (quoting U.S. CONST. amend. VIII)).

217. 18 U.S.C. § 3142(c)(2) ("The judicial officer may not impose a financial condition that results in the pretrial detention of the person."); ABA STANDARDS: PRETRIAL RELEASE, *supra* note 36, § 10-1.4(e) ("The judicial officer should not impose a financial condition of release that results in the pretrial detention of a defendant solely due to the defendant's inability to pay.").

218. *See* Mayson, *supra* note 165, at 7-8 (describing third-generation bail reform, which seeks to "shift[] the entire pretrial paradigm from a cash-based to a risk-based model").

219. 481 U.S. 739, 754-55 (1987).

220. *Id.* at 755.

221. *Id.* at 754.

"compare" it "against the interest the Government seeks to protect by means of that response."[222]

The analysis of Eighth Amendment "excessiveness" thus requires a kind of cost-benefit analysis. A court analyzing a claim that pretrial detention is unconstitutionally excessive must first determine the "perceived evil" that detention is designed to address—presumably a risk of flight or pretrial crime.[223] It must then determine whether detention is "'excessive' in light of the perceived evil."[224] As a matter of logic, this requires an evaluation of whether the costs of detention are excessive in relation to risk (the likelihood that harm would occur without detention and the severity of that harm). That is, courts must determine whether the costs of detention to the detainee are excessive in relation to its benefit to the state.[225]

Pretrial detention has serious costs. In addition to the immediate costs to the detainee (loss of liberty and potential loss of employment, housing, et cetera), the results reported here demonstrate that detention can distort criminal adjudication.[226] That is a significant cost, both to the people who would not have been convicted but for their detention and to the legitimacy of the system as a whole.[227]

On the other side of the ledger, the benefit of detention lies in the number and severity of harms it prevents. If there is only a small risk that the defendant will abscond or commit a serious harm if released, then detention provides little benefit; it does not substantially promote the state's interests. Furthermore, detention may increase future criminal offending.[228] To the extent jurisdictions impose pretrial detention in order to prevent pretrial crime, its benefit—the crime averted—must be discounted by the increase in crime it produces. If it is not clear that the pretrial crime averted is worth the increase in future crime, detention might be an excessive response to the public safety threat. More generally, if the costs of detention vastly outweigh its

222. *Id.*

223. *Id.*

224. *Id.*; *see also* Galen v. County of Los Angeles, 477 F.3d 652, 660 (9th Cir. 2007) ("To determine whether the Excessive Bail Clause has been violated, we look to the valid state interests bail is intended to serve for a particular individual and judge whether bail conditions are excessive for the purpose of achieving those interests.").

225. For a recent effort to engage in systemic cost-benefit analysis of pretrial detention, see Shima Baradaran Baughman, *Costs of Pretrial Detention*, 97 B.U. L. REV. 1 (2017).

226. *See supra* Part III.A-B.

227. To the extent the cost of detention to taxpayers is relevant to this analysis, that cost is also substantial. *See* Pretrial Justice Inst., Pretrial Justice: How Much Does It Cost? 2 (2017), https://university.pretrial.org/viewdocument/pretrial-justice-how-much-does-it (estimating that pretrial detention costs taxpayers $38 million each day, which amounts to $14 billion annually).

228. *See supra* Part III.C.

expected benefit in preventing flight or pretrial crime, a court should conclude that it is an excessive response to the risk the defendant presents. This is even clearer if less restrictive alternatives like GPS monitoring can provide similar benefit at less cost.[229]

## D. Substantive Due Process: Is Pretrial Detention Punishment? Does It Impermissibly Infringe Liberty?

### 1. Pretrial punishment

Our results also support an argument that pretrial detention in some circumstances violates substantive due process by inflicting punishment before trial. "[U]nder the Due Process Clause, a detainee may not be punished prior to an adjudication of guilt in accordance with due process of law."[230] Pretrial detainees, that is, have the "right to be free from punishment."[231] The difficult question is when a restraint on liberty amounts to punishment.

Pursuant to current doctrine, the answer turns on whether the restraint is rationally related to a nonpunitive purpose and not "excessive" for that purpose.[232] Thus far, the Court has declined to classify any pretrial restraint as punishment. In *Bell v. Wolfish*, a challenge to certain conditions of pretrial confinement, the Court concluded that the conditions did not amount to punishment because they were rationally related to legitimate needs of the prison administration and not excessive for those ends.[233] In *Salerno*, the Court rejected the argument that pretrial detention pursuant to the federal Bail Reform Act constituted punishment per se on the basis that the detention regime was carefully tailored to the "legitimate" goal of preventing danger to the community and the "incidents" of detention were not "excessive in relation

229. *See* Samuel R. Wiseman, *Pretrial Detention and the Right to Be Monitored*, 123 YALE L.J. 1344, 1384 (2014) (arguing that pretrial detention "is clearly excessive if monitoring could serve the state's goals equally well (and equally efficiently)").

230. Bell v. Wolfish, 441 U.S. 520, 535 (1979). Note that this right against pretrial punishment is distinct from the presumption of innocence. *See id.* at 533 (stating that the presumption of innocence "is a doctrine that allocates the burden of proof in criminal trials" and "has no application to a determination of the rights of a pretrial detainee"). *But see* County of Riverside v. McLaughlin, 500 U.S. 44, 58 (1991) (alluding to the importance of minimizing "the time a presumptively innocent individual spends in jail").

231. *Bell*, 441 U.S. at 534.

232. *Id.* at 537-38 (quoting Kennedy v. Mendoza-Martinez, 372 U.S. 144, 168-69 (1963)); *see also* United States v. Salerno, 481 U.S. 739, 747 (1987).

233. 441 U.S. at 560-61. The challenged conditions were forced "double-bunking" in single cells, limitations on detainees' access to printed materials, a near-prohibition on receipt of packages, unannounced "shakedowns" of detainees' living areas, and strip searches after contact visits. *Id.* at 541-60.

to the regulatory goal Congress sought to achieve."[234] In both cases, however, the Court left open the possibility that in specific cases, or other circumstances, it might reach a different conclusion.[235]

The analysis whether a particular instance or regime of pretrial detention constitutes "punishment" effectively mirrors the Excessive Bail Clause analysis. Both require a court to determine whether the detention at issue is an "excessive" response to a risk of flight or pretrial crime.[236] The smaller the risk and the greater the costs of detention, the more likely it is to be an excessive response. Our results provide compelling evidence that the costs of detention include increasing the likelihood of conviction and future entanglement with the criminal justice system. Given these and other costs of pretrial detention, it may be an excessive response to low risks of pretrial flight and crime—and therefore constitute impermissible pretrial "punishment."

#### 2. Impermissible regulatory detention

Even if pretrial detention does not constitute punishment, it might, in some cases, violate substantive due process as an impermissible regulatory infringement on individual liberty. "Freedom from imprisonment . . . lies at the heart of the liberty that [the Due Process] Clause protects."[237] The state must therefore meet a high burden of justification when it seeks to detain individuals for regulatory, nonpunitive purposes. When challenges to regulatory detention have made their way to the Supreme Court, the Court has applied some type of heightened scrutiny.[238] Most relevant here, in *Salerno* the

234. 481 U.S. at 747-48.

235. *E.g.*, *id.* at 745 (noting "[t]he fact that the Bail Reform Act might operate unconstitutionally under some conceivable set of circumstances"); *id.* at 745 & n.3 (noting that the suit is a facial, not an as-applied, challenge); *id.* at 747 n.4 ("We intimate no view as to the point at which detention in a particular case might become excessively prolonged, and therefore punitive, in relation to Congress' regulatory goal."); *Bell*, 441 U.S. at 561-62 (acknowledging that "excessive" or "exaggerated" responses to security concerns at a pretrial detention facility would constitute impermissible pretrial punishment).

236. *See, e.g.*, Lopez-Valenzuela v. Arpaio, 770 F.3d 772, 791 (9th Cir. 2014) (en banc) (finding a "*severe* lack of fit between the asserted nonpunitive purpose and the actual operation" of an Arizona constitutional amendment denying bail to undocumented immigrants); *id.* at 792 (concluding that "the challenged laws are excessive in relation to the state's legitimate interest in assuring arrestees' presence for trial" and "therefore impermissibly impose punishment before an adjudication of guilt").

237. Zadvydas v. Davis, 533 U.S. 678, 690 (2001).

238. *See, e.g.*, *id.* at 690 (explaining that regulatory detention violates substantive due process rights except "in certain special and 'narrow' nonpunitive 'circumstances,' where a special justification, such as harm-threatening mental illness, outweighs the 'individual's constitutionally protected interest in avoiding physical restraint'" (citation omitted) (first quoting Foucha v. Louisiana, 504 U.S. 71, 80 (1992); and then quoting Kansas v. Hendricks, 521 U.S. 346, 356 (1997))).

Supreme Court rejected a substantive due process challenge to the federal preventive detention regime because the regime was "narrowly focuse[d]" on the "legitimate and compelling" state interest of preventing pretrial crime by an especially dangerous subset of defendants.[239]

Pursuant to *Salerno*'s analysis, a specific instance or regime of pretrial detention might violate substantive due process if it is not carefully tailored to its goal or if its costs vastly outweigh its benefits. Once again, the costs documented here should inform the calculation.[240] If a defendant poses little risk of flight or pretrial crime, then pretrial detention—given its attendant costs—is a blunt tool to mitigate the risk. Our analysis of the effect of pretrial detention on future crime suggests that it may even be counterproductive. Pretrial detention that exacerbates the harm it is supposed to prevent is not a "narrowly focused" means of protecting public safety, so it may violate substantive due process as an unjustified infringement on liberty.

### E. Procedural Due Process: Does Pretrial Detention Produce "Involuntary" Plea Bargains?

To the extent the causal effect of pretrial detention on conviction rates reflects a reality that detained people plead guilty simply to get out of jail, it raises the question whether such pleas are fully "voluntary." The Due Process Clauses of the Fifth and Fourteenth Amendments require that guilty pleas be "voluntary" and "intelligent," which requires that a defendant have and make a meaningful choice.[241]

Plea bargaining poses a dilemma because it is always in some sense coercive. The Supreme Court has confronted this question in two cases since 1970: *Brady v. United States* and *Bordenkircher v. Hayes*.[242] In *Brady*, the Court held that

239. 481 U.S. at 750-52 ("Given the legitimate and compelling regulatory purpose of the Act and the procedural protections it offers, we conclude that the Act is not facially invalid under the Due Process Clause of the Fifth Amendment.").

240. The tests the Court has articulated for impermissible pretrial "punishment" and impermissible regulatory detention are quite similar and overlap with the Eighth Amendment prohibition on "excessive" pretrial restraints on liberty; each requires courts to assess the fit between the state's goal (for example, preventing flight or pretrial crime) and the means taken to achieve it. *See supra* Part IV.C.2; *see also, e.g.*, *Lopez-Valenzuela*, 770 F.3d at 789, 791-92 (holding that an Arizona constitutional amendment denying bail to undocumented immigrants violated substantive due process rights on the two "independent grounds" that it failed heightened scrutiny and constituted pretrial punishment and acknowledging the very similar analysis for each).

241. Brady v. United States, 397 U.S. 742, 748 (1970) (holding that a plea must be a "knowing, intelligent act[] done with sufficient awareness of the relevant circumstances and likely consequences"); *see also* Boykin v. Alabama, 395 U.S. 238, 242 (1969) (holding, on procedural due process grounds, that a guilty plea must be knowing and voluntary).

242. 434 U.S. 357 (1978).

a plea is not rendered involuntary even if it were motivated by the defendant's fear of receiving the death penalty if convicted at trial.[243] In *Bordenkircher*, the Court held that it did not violate the Due Process Clause for a prosecutor to threaten to re-indict the defendant on more serious charges unless he pleaded guilty (and then to carry out the threat).[244] The Court reasoned that "'the imposition of these difficult choices [is] an inevitable'—and permissible—'attribute of any legitimate system which tolerates and encourages the negotiation of pleas.'"[245]

This precedent is clearly hostile to any argument that pretrial detention might render a guilty plea involuntary. But the Supreme Court did leave the door ajar. In *Brady*, the Court qualified its acceptance of bargains driven by fear: "Of course, the agents of the State may not produce a plea by actual or threatened physical harm or by mental coercion overbearing the will of the defendant."[246] And in *Bordenkircher*, the Court suggested that its decision was predicated on the assumption that the inducement at issue would not lead an innocent person to plead guilty. The Court reasoned that "[d]efendants advised by competent counsel and protected by other procedural safeguards are . . . unlikely to be driven to false self-condemnation."[247] It also noted that the case did not "involve the constitutional implications" of a prosecutor threatening harm or offering benefit to a third party, "which might pose a greater danger of inducing a false guilty plea by skewing the assessment of the risks a defendant must consider."[248]

These offhand caveats are hardly a firm foundation for a new jurisprudence of due process limits to coercion in plea bargaining, but they are suggestive. Evidence that pretrial detention leads to wrongful convictions by guilty plea might lead the Court to reconsider its due process conclusions. The empirical analysis in this Article suggests that approximately 17% of people detained pretrial in the Harris County dataset who pleaded guilty (or no contest) would not have been convicted but for their detention. This suggests

243. 397 U.S. at 750-51. The Court noted that "[t]he State to some degree encourages pleas of guilty at every important step in the criminal process," and it rejected the idea "that a guilty plea is compelled and invalid under the Fifth Amendment whenever motivated by the defendant's desire to accept the certainty or probability of a lesser penalty rather than face a wider range of possibilities" after trial. *Id.*; *see also id.* at 751 ("The issue we deal with is inherent in the criminal law and its administration . . . .").

244. *Bordenkircher*, 434 U.S. at 358, 365.

245. *Id.* at 364 (alteration in original) (quoting Chaffin v. Stynchcombe, 412 U.S. 17, 31 (1973)).

246. *Brady*, 397 U.S. at 750.

247. *Bordenkircher*, 434 U.S. at 363.

248. *Id.* at 364 n.8; *see also id.* at 363 ("[I]n the 'give-and-take' of plea bargaining, there is no such element of punishment or retaliation so long as the accused is *free* to accept or reject the prosecution's offer." (emphasis added)).

that they pleaded guilty simply to go home, not because of the strength of the case against them. It is impossible to tell how many of them were actually innocent. But the combination of statistics along these lines and evidence in an individual case might be compelling.

Consider, for instance, the case of Joseph Curry.[249] According to his allegations in a recent civil suit, Curry had discovered in 2012 that there was a warrant out for his arrest, accusing him of petty theft at a Walmart he had never entered. When he called the Pennsylvania state police to clarify the situation, he was arrested and jailed. Bail was set at $20,000, which he could not afford. In the months he was detained and waiting for his case to proceed, Curry "missed the birth of his only child, lost his job, and feared losing his home and vehicle. Ultimately, he pled *nolo contendere* in order to return home."[250]

Confronted with convincing evidence of facts like these in an appeal or postconviction proceeding, a court might find that the continued pretrial detention of a person in these circumstances constituted "mental coercion overbearing the will of the defendant,"[251] who was ultimately "driven to false self-condemnation."[252] The court could find that such a plea was not sufficiently voluntary to comport with due process. It would therefore vacate the plea and conviction. There would be some risk to pursuing this strategy for the defendant because the charge could be reinstated and he could again be arrested and jailed. If the evidence of his innocence were compelling, though, one hopes that this would not be the case.

There is little chance that a due process/coercion argument can serve as a useful vehicle for large-scale change in the pretrial system because it is necessarily individualized. But it might have traction in individual cases. And perhaps it might lead courts and prosecutors to question whether a pretrial detainee who can reasonably be released upon a guilty plea should be detained in the first place. Finally, individual court decisions grappling with the due process/coercion argument might begin to fill in the fuzzy outlines of the constitutional limit on coercive plea bargaining practices more generally.

## Conclusion

Pretrial detention has a significant impact on downstream criminal justice outcomes—both in the immediate case and through the future criminal activity

249. The facts of this case are recited in *Curry v. Yachera*, No. 15-1692, 2016 WL 4547188, at *1-2 (3d Cir. Sept. 1, 2016).

250. *Id.* at *3.

251. *Brady*, 397 U.S. at 750.

252. *Bordenkircher*, 434 U.S. at 363.

of detained defendants. Detention increases the rate of guilty pleas and leads detained individuals to commit more crime in the future. These findings not only carry import for Harris County; they also raise a host of broader empirical and constitutional questions that merit attention.

To appreciate the magnitude of the effects this Article documents, we offer the following thought experiment: imagine if, during the period of the sample, Harris County had released those defendants assigned the lowest amount of bail—$500—on personal bond (recognizance) rather than assessing bail. On the basis of the rate of detention among people with $500 bail set, the estimated effects of pretrial detention reported above, and other data carefully documenting the costs of detention and probation supervision in Harris County,[253] we predict the county would have released 40,000 additional defendants pretrial. These individuals would have avoided approximately 5900 criminal convictions, many of which would have come through possibly erroneous guilty pleas. Incarceration days in the county jail—severely overcrowded as of April 2016—would have been reduced by at least 400,000.[254] Over the next eighteen months after their release, these defendants would have committed 1600 fewer felonies and 2400 fewer misdemeanors. On net, after accounting for both reductions in jail time and increases in probation time, the county would have saved an estimated $20 million in supervision costs alone. Thus, with better pretrial detention policy, Harris County could save millions of dollars per year, increase public safety, and likely reduce wrongful convictions.

Our findings also carry import beyond the borders of Harris County. Many of the key features of Harris County's system—a heavy reliance on cash bail, assembly-line handling of bail hearings, and nonexistent representation for defendants at these hearings—are characteristic of misdemeanor bail systems across the country. This Article presents strong empirical evidence that under such circumstances, bail hearings influence later case outcomes. This evidence demands further guidance from the courts as to whether the Sixth Amendment guarantees the assistance of counsel at such hearings and whether such a process sufficiently protects the due process and Eighth Amendment rights of misdemeanor defendants.

253. *E.g.*, Tex. Criminal Justice Coal., Harris County, Texas: Adult Criminal Justice Data Sheet (n.d.), http://countyresources.texascjc.org/sites/default/files/adult_county_data_sheets/TCJC's%20Adult%20Harris%20County%20Data%20Sheet.pdf; Vera Inst. of Justice, The Price of Jails: Measuring the Taxpayer Cost of Local Incarceration 28 (2015), http://www.vera.org/sites/default/files/resources/downloads/price-of-jails.pdf.

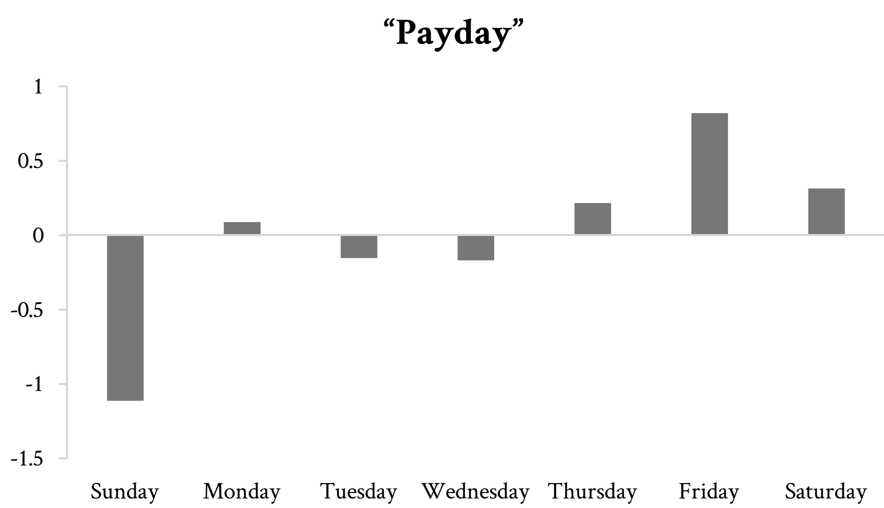
254. This is actually a conservative estimate because it is based on the estimate of the change in the jail sentence associated with detention and thus ignores time spent in pretrial detention that does not end up counting against the final sentence of the accused.

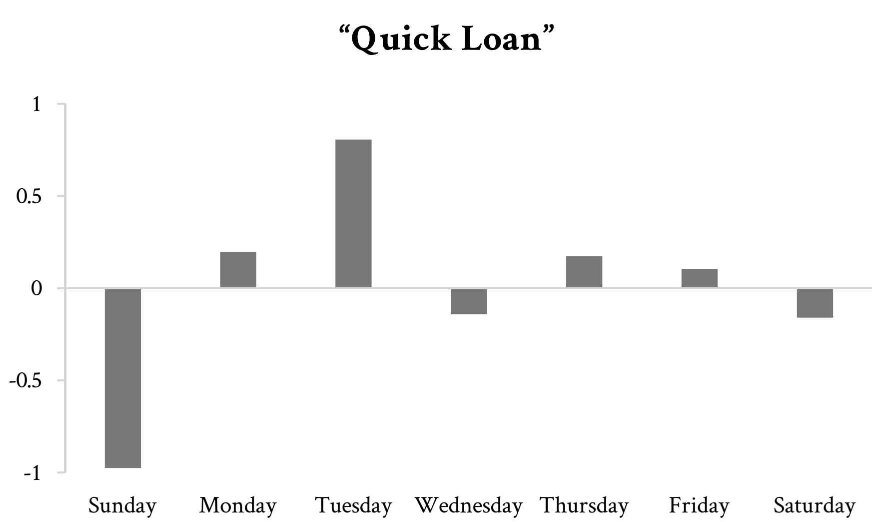
Our results also have important implications for the conduct of future empirical studies assessing the effects of pretrial detention. Our analysis suggests that prior work measuring the association between pretrial detention and case outcomes, which controlled for only a limited set of defendant and case characteristics, may have overestimated the causal effect of detention. After controlling for a broader set of characteristics, however—including the exact offense and the precise amount of bail set at the initial hearing—we are able to obtain correlational estimates that approach the causal estimates we observe using a natural experiment. In this respect, this Article's results mirror those of Stevenson.[255] Researchers therefore may be able to learn much about bail effects across many other jurisdictions operating under different systems without resorting to costly, and in some cases practically infeasible, randomized controlled trials so long as they account for preexisting differences between the pools of detained and released defendants. Such future work could help catalyze a shift toward bail systems that reduce wealth disparities, increase public safety, and minimize the lengthy periods of detention that have such high budgetary and human costs.

255. *See supra* notes 63-67 and accompanying text.

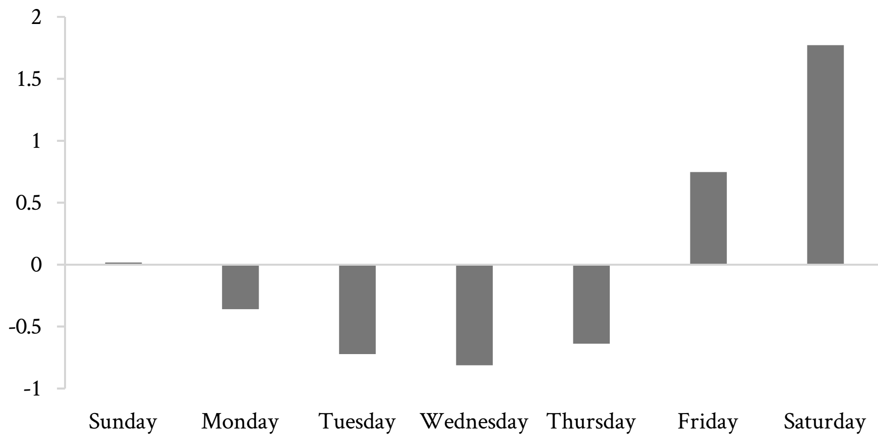
## Appendix

**Figure A.1**
Google Daily Keyword Search Volume by Day of Week (Standardized Score)



**"Check Cashing"**




This figure plots average daily Google search volume by day of week for several search terms that serve as proxies for liquidity. For each term, daily search volume was standardized and then averaged by day of week to construct the bars in the chart. Data were downloaded from Google Trends[256] and cover the period from January 31, 2016 to April 23, 2016.

256. GOOGLE TRENDS, https://www.google.com/trends (last visited Mar. 3, 2017).

**Figure A.2**
Distribution of Bail Assessments by Day of Week of Hearing

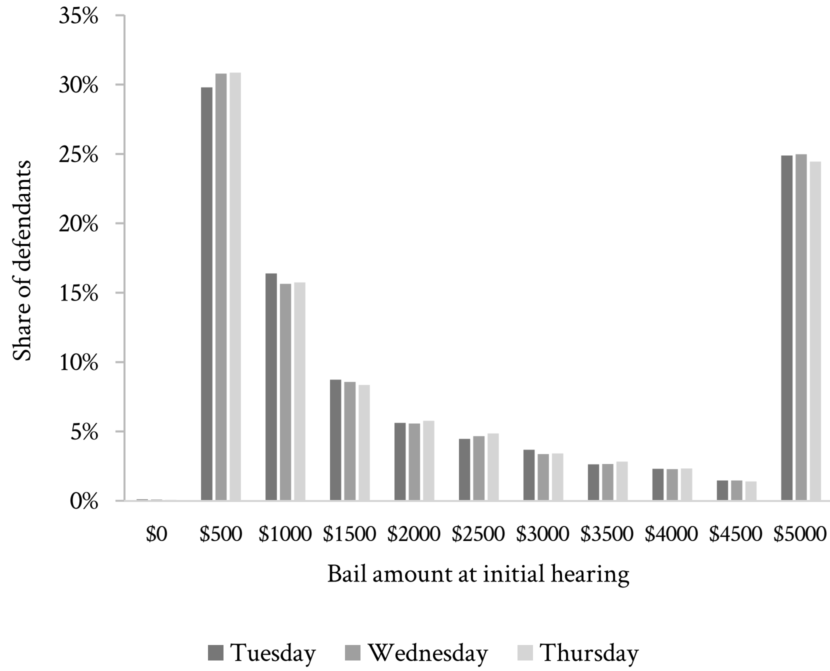


**Table A.1**
Numeric Results for Misdemeanor Recidivism Analysis

| Days Since Bail Hearing | Cumulative New Misdemeanors per Released Defendant | Estimated Effect of Detention | Standard Error | *p*-Value | Percent Change in Misdemeanors Due to Detention |
|---|---|---|---|---|---|
| 1 | 0.0004 | -0.0004 | 0.00006 | 4.56E-10 | -97.0 |
| 2 | 0.0010 | -0.0009 | 0.00013 | 4.55E-11 | -89.1 |
| 3 | 0.0015 | -0.0008 | 0.00018 | 1.12E-05 | -50.6 |
| 4 | 0.0022 | -0.0010 | 0.00022 | 5.52E-06 | -45.6 |
| 5 | 0.0029 | -0.0011 | 0.00026 | 1.74E-05 | -38.1 |
| 6 | 0.0037 | -0.0012 | 0.00030 | 7.28E-05 | -31.8 |
| 7 | 0.0046 | -0.0014 | 0.00033 | 2.14E-05 | -31.2 |
| 8 | 0.0052 | -0.0014 | 0.00036 | 0.000 | -26.8 |
| 9 | 0.0059 | -0.0012 | 0.00040 | 0.003 | -20.0 |
| 10 | 0.0065 | -0.0011 | 0.00043 | 0.009 | -17.0 |
| 11 | 0.0072 | -0.0013 | 0.00045 | 0.005 | -17.6 |
| 12 | 0.0080 | -0.0013 | 0.00048 | 0.005 | -16.6 |
| 13 | 0.0089 | -0.0013 | 0.00050 | 0.009 | -14.8 |
| 14 | 0.0098 | -0.0009 | 0.00053 | 0.079 | -9.5 |
| 15 | 0.0106 | -0.0008 | 0.00056 | 0.127 | -8.0 |
| 16 | 0.0112 | -0.0008 | 0.00057 | 0.178 | -6.9 |
| 17 | 0.0118 | -0.0004 | 0.00059 | 0.520 | -3.2 |
| 18 | 0.0125 | -0.0001 | 0.00061 | 0.870 | -0.8 |
| 19 | 0.0130 | 0.0002 | 0.00062 | 0.800 | 1.2 |
| 20 | 0.0137 | 0.0005 | 0.00064 | 0.406 | 3.9 |
| 21 | 0.0145 | 0.0006 | 0.00066 | 0.399 | 3.9 |
| 22 | 0.0151 | 0.0009 | 0.00068 | 0.197 | 5.8 |
| 23 | 0.0157 | 0.0010 | 0.00069 | 0.149 | 6.3 |
| 24 | 0.0164 | 0.0012 | 0.00071 | 0.097 | 7.1 |
| 25 | 0.0170 | 0.0013 | 0.00072 | 0.069 | 7.7 |
| 26 | 0.0177 | 0.0014 | 0.00074 | 0.054 | 8.0 |
| 27 | 0.0183 | 0.0017 | 0.00075 | 0.025 | 9.2 |
| 28 | 0.0190 | 0.0019 | 0.00076 | 0.012 | 10.1 |
| 29 | 0.0197 | 0.0020 | 0.00078 | 0.009 | 10.3 |
| 30 | 0.0204 | 0.0022 | 0.00079 | 0.005 | 10.9 |

| | | | | | |
|---|---|---|---|---|---|
| 60 | 0.0413 | 0.0075 | 0.00113 | 2.32E-11 | 18.2 |
| 120 | 0.0805 | 0.0154 | 0.00158 | 1.58E-22 | 19.2 |
| 180 | 0.1160 | 0.0219 | 0.00193 | 4.98E-30 | 18.9 |
| 240 | 0.1480 | 0.0284 | 0.00223 | 3.26E-37 | 19.2 |
| 300 | 0.1830 | 0.0364 | 0.00249 | 3.58E-48 | 19.9 |
| 360 | 0.2086 | 0.0447 | 0.00272 | 1.19E-60 | 21.4 |
| 420 | 0.2335 | 0.0515 | 0.00294 | 1.36E-68 | 22.0 |
| 480 | 0.2575 | 0.0584 | 0.00314 | 3.07E-77 | 22.7 |
| 540 | 0.2808 | 0.0638 | 0.00332 | 5.13E-82 | 22.7 |

**Table A.2**
Numeric Results for Felony Recidivism Analysis

| Days Since Bail Hearing | Cumulative New Felonies per Released Defendant | Estimated Effect of Detention | Standard Error | *p*-Value | Percent Change in Felonies Due to Detention |
|---|---|---|---|---|---|
| 5 | 0.0015 | -0.0012 | 0.00018 | 1.48E-10 | -79.5 |
| 10 | 0.0032 | -0.0018 | 0.00028 | 6.28E-10 | -55.1 |
| 15 | 0.0052 | -0.0022 | 0.00038 | 1.05E-08 | -42.2 |
| 20 | 0.0069 | -0.0022 | 0.00045 | 6.67E-07 | -32.5 |
| 25 | 0.0084 | -0.0020 | 0.00051 | 0.0001 | -23.7 |
| 30 | 0.0101 | -0.0022 | 0.00056 | 0.0001 | -21.3 |
| 35 | 0.0117 | -0.0022 | 0.00061 | 0.000 | -18.6 |
| 40 | 0.0133 | -0.0020 | 0.00065 | 0.002 | -15.4 |
| 45 | 0.0148 | -0.0019 | 0.00068 | 0.005 | -13.0 |
| 50 | 0.0162 | -0.0018 | 0.00072 | 0.015 | -10.8 |
| 55 | 0.0176 | -0.0012 | 0.00076 | 0.111 | -6.9 |
| 60 | 0.0192 | -0.0010 | 0.00079 | 0.212 | -5.2 |
| 65 | 0.0205 | -0.0003 | 0.00082 | 0.697 | -1.6 |
| 70 | 0.0218 | 0.0004 | 0.00085 | 0.650 | 1.8 |
| 75 | 0.0233 | 0.0007 | 0.00089 | 0.429 | 3.0 |
| 80 | 0.0247 | 0.0009 | 0.00092 | 0.328 | 3.6 |
| 85 | 0.0260 | 0.0014 | 0.00095 | 0.126 | 5.6 |
| 90 | 0.0274 | 0.0019 | 0.00097 | 0.046 | 7.1 |
| 95 | 0.0286 | 0.0023 | 0.00100 | 0.021 | 8.0 |
| 100 | 0.0298 | 0.0028 | 0.00102 | 0.006 | 9.4 |
| 120 | 0.0351 | 0.0047 | 0.00111 | 0.000 | 13.5 |
| 180 | 0.0498 | 0.0104 | 0.00136 | 0.000 | 20.9 |
| 240 | 0.0644 | 0.0150 | 0.00157 | 0.000 | 23.3 |
| 300 | 0.0782 | 0.0196 | 0.00177 | 0.000 | 25.1 |
| 360 | 0.0911 | 0.0250 | 0.00194 | 0.000 | 27.4 |
| 420 | 0.1039 | 0.0296 | 0.00210 | 0.000 | 28.5 |
| 480 | 0.1163 | 0.0343 | 0.00224 | 0.000 | 29.5 |
| 540 | 0.1280 | 0.0395 | 0.00237 | 0.000 | 30.9 |

# EXHIBIT E

# Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes

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This article uses a natural experiment to analyze whether incarceration during the pretrial period affects case outcomes. In Philadelphia, defendants randomly receive bail magistrates who differ widely in their propensity to set bail at affordable levels. Using magistrate leniency as an instrument, I find that pretrial detention leads to a 13% increase in the likelihood of being convicted, an effect largely explained by an increase in guilty pleas among defendants who otherwise would have been acquitted or had their charges dropped. I find also that pretrial detention leads to a 42% increase in the length of the incarceration sentence and a 41% increase in the amount of nonbail court fees owed. This latter finding contributes to a growing literature on fines-and-fees in criminal justice, and suggests that the use of money bail contributes to a “poverty-trap”: those who are unable to pay bail wind up accruing more court debt. (*JEL* K14)

> I have had the “you can wait it out or take the deal and get out” conversation with way too many clients.
>
> —a public defender, Philadelphia

## 1. Introduction

There are currently 434,000 people awaiting trial in jail in the United States (Minton and Zeng 2016). In fact, there are more people in jail awaiting trial than are incarcerated due to a drug sentence.[1] This number is particularly striking considering that our criminal justice system is founded on a presumption of innocence, where, at least in theory, “liberty is the norm, and detention prior to trial or without trial is the carefully limited exception.”[2] According to the Bureau of Justice Statistics, five out of six people detained before trial on a felony charge are held on money bail (Cohen and Reaves 2007). Some of these defendants

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1. The number of state and federal prisoners whose most serious offense was drug-related is found in Minton and Zeng (2015). The most recent information on the percentage of convicted jail inmates with a drug sentence is from James (2004).

2. Chief Justice Rehnquist in *United States v. Salerno*, 481 U.S. 739 (1987).

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are facing very serious charges, and accordingly have very high bail. But many have bail set at amounts that would be affordable for the middle or upper-middle class but are simply beyond the reach of the poor. In Philadelphia, the site of this study, more than half of pretrial detainees would be able to secure their release by paying a deposit of $1000 or less, most of which would be reimbursed if they appear at all court dates. Many defendants remain incarcerated even at extremely low amounts of bail, where the deposit necessary to secure release is only $50 or $100. Nor are the charges faced by many pretrial detainees particularly serious: 60% of those held for more than three days were charged with nonviolent crimes and 28% were charged only with a misdemeanor.

It has long been argued that pretrial detention puts a defendant at a disadvantage in their case (Ares et al. 1963; Rankin 1964; Goldkamp 1980; Williams 2003; Phillips 2007, 2008; Tartaro and Sedelmaier 2009; Sacks and Ackerman 2012; Lowenkamp et al. 2013; Oleson et al. 2014). A detained defendant may plead guilty to get out of jail, or accept an overly punitive plea deal because detention impaired her ability to gather evidence or meet with her lawyer. She may be less motivated to fight the charges when the fixed costs of incarceration have already been paid: stigma, loss of employment, housing or child custody, etc. Furthermore, the use of money bail to determine custody status suggests that pretrial detention may form a type of poverty trap, where defendants who are too poor to pay for pretrial release suffer economic consequences downstream. Such consequences include the stigma of a criminal record, the destabilization of incarceration, or the burdens of probation compliance. More directly, defendants who are too poor to pay for pretrial release may accrue more debt, owing hundreds or thousands of dollars to the courts through fees and fines.

This article contributes to a series of concurrent articles providing quasi-experimental evidence on the impacts of pretrial detention (Gupta et al. 2016; Heaton et al. 2017; Leslie and Pope 2017; Dobbie et al. 2018).[3] The research design takes advantage of the fact that defendants randomly receive bail magistrates who vary widely in their propensity to set bail at affordable levels. Those who receive a strict magistrate are statistically identical to those who receive a more lenient magistrate except in their likelihood of being detained pretrial. If those who receive a strict magistrate are also more likely to be convicted or receive unfavorable sentences, we can infer that this is due to differences in detention rates and not some other unseen difference in defendant or case characteristics.

Using web-scraped data from Philadelphia court records and the relative leniency of the bail magistrate as an instrument, I find that pretrial detention leads to a 13% increase in the likelihood of being convicted on at least one charge. The effect on conviction is largely explained by an

3. All five papers in the recent literature on the impacts of pretrial detention were developed in parallel and released publicly between May and August of 2016. A draft of this article was first released on May 2, 2016.

increase in the likelihood of pleading guilty among those who would otherwise have been acquitted, diverted, or had their charges dropped. These results are qualitatively consistent with the other recent papers, but the estimated effect sizes are significantly lower. This is particularly striking given that one of the other studies, Dobbie et al. (2018), is also largely based on Philadelphia data during a similar time period. (Gupta et al. (2016) also uses Philadelphia data but with a different independent variable: money bail instead of pretrial detention.) While some of this discrepancy may be due to cross jurisdictional differences, it may also be partly due to nonmonotonicity bias in specifications that assume that a magistrate's relative leniency does not vary across case or defendant characteristics.

I also find that pretrial detention leads to a 42% increase in the incarceration sentence, an effect that is only partially explained by release on time-served. This suggests that the impacts of pretrial detention extend beyond the classic example of defendants pleading guilty in order to get out of jail. Furthermore, it shows that the role pretrial detention plays in mass incarceration is bigger than its direct effects. Pretrial detainees constitute one in five of the total incarcerated population, but pretrial detention also contributes indirectly to mass incarceration through increased post-conviction sentences.[4]

Among the concurrent literature, only Heaton et al. (2017) (Harris County, Texas) and Leslie and Pope (2017) (New York City) find that pretrial detention increases the sentence length. Sentence outcomes were not evaluated in the other two recent Philadelphia-based papers. Compared to other settings, where the source of identifying variation is less clearly exogenous, the natural experiment in Philadelphia is particularly clean. There is one centralized bail hearing room for the entire city, and magistrates work a rotating schedule that creates random variation in which magistrate is on duty. Over time, each magistrate will work an equal number of night shifts, weekend shifts, etc. Furthermore, the duties of the bail magistrate are very limited and there are few plausible alternative channels through which they could affect case outcomes.

Finally, I find that pretrial detention has direct economic consequences: a 41% increase in courtroom debt. Since most people who are detained pretrial are detained due to an inability to pay bail, this provides support for poverty-trap theories of criminal justice. While the median defendant must pay only $250 to secure release, those who are convicted are expected to pay an average of $611 in court fees. The monetary bail system acts as a sort of regressive taxation: those who cannot afford to pay for pretrial release are required to pay a larger portion of the court's expenses.

This is the first study to evaluate pretrial detention's impacts on court fees, and contributes to a still-small literature on fines and fees in criminal

4. At any point in time there are 434,000 people detained pretrial (Minton and Zeng 2016) and 2.172 million people incarcerated in total (Kaeble and Cowhig 2018).

justice. Although monetary punishments have historically received little attention in academic literature, the "Ferguson report" put out by the Department of Justice has led to renewed interest (DOJ 2015). This report found that the revenue-generating practices of Ferguson Police Department imposed "a particular hardship upon Ferguson's most vulnerable residents, especially upon those living in or near poverty." Such a statement has resonance in Philadelphia as well.

In Section 2 I give a brief overview of the pretrial process, in Section 3 I describe the natural experiment, and in Section 4 I discuss the data and provide descriptive statistics and graphs. Section 5 discusses the empirical strategy for identifying the impacts of pretrial detention and provides evidence that magistrate assignment is as-good-as-random. Section 6 presents the results and provides several robustness checks. Section 7 concludes.

## 2. The Pretrial Process

Pretrial detention is the act of keeping a defendant confined during the period between arrest and disposition for the purposes of ensuring their appearance in court and/or preventing them from committing another crime. The vast majority of jurisdictions use a money bail system to govern whether or not a defendant is detained (PJI 2009). In such a system a judge or a magistrate determines the amount of the bail required for release and the defendant is only released if she pays that amount. In some cases the defendant will be released without having to pay anything, in others (usually only the most serious cases) she will be denied bail and must remain detained. While the defendant is liable for the full amount of the bail bond if she fails to appear at court or commits another crime during the pretrial period, she usually does not need to pay the full amount in order to secure release. In many jurisdictions she will borrow this sum from a bail bondsman, who charges a fee and holds cash or valuables as collateral (Cohen and Reaves 2007). In some jurisdictions, Philadelphia included, the courts act as a bail bondsman and will release the defendant after the payment of a deposit.

Bail hearings are generally quite brief—in Philadelphia most last only a minute or two—and often do not have any lawyers present.[5] After the bail hearing there are a series of pretrial court appearances that defendants must attend. Although the exact procedure varies across jurisdictions these usually include at least an arraignment (where formal charges are filed) and some sort of preliminary hearing or pretrial conference (where the case is discussed and plea deals can be negotiated). Plea bargaining

5. PJI (2009) shows 40% of respondent districts do not have defense attorneys at bail hearings. While there is no systematic survey of the length of bail hearing, they are reported to be very short in many jurisdictions: three minutes long in North Dakota (VandeWalle 2013), less than two minutes in Cook County (Staff 2016) and only a couple minutes long in Harris County (Heaton et al. 2017).

usually begins around the time of arraignment and can continue throughout the criminal proceedings. In some jurisdictions, like New York City, the arraignment happens simultaneous to the bail hearing and it is not uncommon to strike a plea deal at this first appearance (Barry et al. 2012). In other jurisdictions, such as New Orleans, arraignments for felony defendants often do not happen until four months after the bail hearing and a defendant who is unable to make bail must wait until then to file a plea.[6] In Philadelphia, arraignments usually happen within a month of the bail hearing.

Plea negotiation is a process in which the defendant receives reduced charges or shorter sentences in return for pleading guilty and waiving her right to a trial. Since defendants often face severe sentences if found guilty at trial, the incentives to plead are strong. It is estimated that 90–95% of felony convictions are reached through a plea deal (Devers 2011). Philadelphia differs from many other jurisdictions in its wide use of bench trials on felony cases. Since sentencing tends to be more lenient in bench trials than jury trials, this reduces the incentive to plead guilty.[7] Only about 78% of felony convictions are reached through plea in Philadelphia. Trial by jury is not constitutionally required if the maximum incarceration sentence is less than six months, and the use of bench trials for misdemeanors, as is the custom in Philadelphia, is more common across jurisdictions.

There are a number of reasons why a detained defendant might be more likely to be convicted, or receive a more punitive sentence. Any plea deal that involves immediate release from jail would be very tempting, even if the deal involved onerous probation requirements, heavy fines, and negative impacts on future labor market prospects or access to public benefits (Bibas 2004). It may be that since some of the disruptions of incarceration have already occurred—loss of job/housing, the initial adjustment to life behind bars—the incentives to fight the charges are lower. Jail may affect optimism about the likelihood of winning the case, or, by changing the reference point, may affect risk preferences in such a way that the certainty of a plea deal seems preferable to the gamble of a trial. Detention also impairs the ability to gather exculpatory evidence, makes confidential communication with attorneys more difficult, and limits opportunities to impress the judge with gestures of remorse or improvement (taking an anger management course, entering rehab, etc.) (Goldkamp 1980). Detained defendants may attend pretrial court appearances in handcuffs and/or prison garb, creating superficial impressions of criminality. Furthermore, if a defendant must await trial behind bars he may be

6. Based on discussions with former New Orleans Parish defenders.

7. In Philadelphia, a bench trial is the default for all but the most serious felonies. The right to a jury trial can be asserted upon request, but this is uncommon. Although there is no formal mechanism that ensures that a bench trial will lead to better outcomes for the defendant than a jury trial, all defense attorneys interviewed assured me that this was the case.

reluctant to employ legal strategies that involve delay. Although a released defendant may file continuances in the hopes that the prosecution's witnesses will fail to appear, memories will blur, or charges eventually get dropped, a detained defendant pays a much steeper price for such a strategy. More nefariously, those detained have less opportunity to coerce witnesses, destroy evidence or otherwise impede the investigation (Laudan and Allen 2010).

These different mechanisms through which pretrial detention could affect case outcomes are likely to vary in importance by defendant and according to the local characteristics of criminal procedure. Although there is little reason to believe that the results shown in this article are unique to Philadelphia, the magnitude of the effects may differ across jurisdictions.

## 3. The Natural Experiment

Immediately after arrest, arrestees are brought to one of seven police stations around the city. There, the arrestee will be interviewed via videoconference by Pretrial Services. Pretrial Services collects information about various risk factors as well as financial information to determine eligibility for public defense. Using risk factors and the current charge, Pretrial Services will determine the arrestee's place in a 4 by 10 grid of bail recommendations. Although these bail guidelines suggest a wide range of appropriate bail, they are only followed about 50% of the time (Shubik-Richards and Stemen 2010). Once Pretrial Services has entered the bail recommendation and the financial information into the arrest report the arrestee is ready for her bail hearing.

Once every four hours the magistrate will hold bail hearings (in Philadelphia these are called Preliminary Arraignments) for all arrestees who are ready. The bail hearing will be conducted over videoconference by the magistrate, with a representative from the district attorney's office, a representative from the Defender Association of Philadelphia (the local public defender), and a clerk also present. In general, none are attorneys. The magistrate makes the bail determination on the basis of information in the arrest report, the pretrial interview, criminal history, bail guidelines, and advocacy from the district attorney and public defender representatives.

There are four things that happen during the bail hearing: the magistrate will read the charges to the arrestee, inform her of her next court appearance, determine whether the arrestee will be granted a court-appointed defense attorney, and set the bail amount. The first two activities are formalities that ensure the defendant is aware of what she is being charged with and where her next court date is. Eligibility for public defense is determined by income. If the defendant is deemed eligible, she will be assigned either to the Defender Association or to a private attorney who has been approved to accept court appointments by the City of

Philadelphia. The default is to appoint the Defender Association; if procedural rules require the court to appoint an attorney outside of the Defender Association the magistrate's clerk will appoint the attorney at the top of a rotating list of eligible attorneys known as a "wheel."[8]

A typical bail hearing lasts only a minute or two and the magistrate has broad authority to set bail as she sees fit.[9] Bail decisions fall into three categories: release with no payment required, cash bail or no bail.[10] Those with cash bail will be required to pay a 10% deposit on the total bail amount in order to be released. After disposition, and assuming that the behavioral conditions of the pretrial period were met, 70% of this deposit will be returned. The City of Philadelphia retains 30% of the deposit, even if charges get dropped or the defendant is acquitted on all charges. Those who do not have the 10% deposit in cash can borrow this amount from a commercial bail bondsman, who will accept cars, houses, jewelry and other forms of collateral for their loan. If the defendant's arrest occurred while she is already on probation or parole, her probation officer may choose to file a detainer. If a detainer is filed she may not bail out until a judge removes the detainer.[11]

The research design uses variation in the propensity of the magistrates to assign affordable bail as an instrument for detention status. The validity of the instrument rests on several factors, including that the magistrate received is essentially random and that the instrument will not affect outcomes through a channel other than pretrial detention. The following details help ease concerns along these lines.

Philadelphia employs six Arraignment Court Magistrates at a time, and one of the six will be on duty 24 hours a day, 7 days a week, including holidays. Each day is composed of three work shifts: graveyard (11:30 p.m.–7:30 a.m.), morning (7:30 a.m.–3:30 p.m.) and evening (3:30 p.m.–11:30 p.m.). Each magistrate will work for five days on a particular shift, take five days off, then do five days on the next shift, five days off, and so forth. For example, a magistrate may work the graveyard shift from January 1st to January 5th, have January 6th–10th off, then work

8. If there are multiple codefendants, such that representing all of them would pose a conflict of interest, one defendant will be randomly selected to be served by the Defender Association and the others will receive a court-appointed attorney. For opaque historical reasons, four out of five defendants charged with murder will be represented by court-appointed attorneys and the fifth will be represented by the homicide division of the Defender Association (Anderson and Heaton 2012). This decision is made by the order in which defendants are entered into the data system and the court-appointed attorney is chosen by a Municipal Court Judge, not a magistrate.

9. If either the defense or the prosecution is unhappy with the decision they can make an appeal to a judge immediately after the bail hearing. However, the bar is high for overturning the original bail decision so this is not very common.

10. Holding a defendant without bail is uncommon, although bail is sometimes set at prohibitively high rates.

11. The detainer hearing usually happens within a week of arrest. Detainer cases are evenly distributed across magistrates and should not bias the results.

the morning shift from January 11th–15th, have the 16th–20th off, do the evening shift from January 21st–25th, take the next five days off, and then start the cycle all over again.

This rotation relieves concerns that certain magistrates set higher bail because they work during shifts that see higher-risk defendants. Over time, each magistrate will be scheduled to work a balanced number of weekends, graveyard shifts, and so forth. However the magistrates do not always work their appointed shifts; in fact, about 20% of the time there is a substitute (usually one of the other magistrates). To avoid potential confounds I instrument with the magistrate who was scheduled to work instead of the magistrate who actually worked. Furthermore, arrestees do not have latitude to strategically postpone their bail hearing to receive a more lenient magistrate. The process from arrest to bail hearing has been described as a conveyor belt: on average the time from arrest to the bail hearing is 17 hours and defendants are seen as soon as Pretrial Services notifies the Arraignment Court that they are ready (Clark et al. 2011). Thus the magistrate received by each defendant is essentially random, at least in that the sample of defendants who are seen by each magistrate should be statistically identical. I confirm this empirically in Section 5.

Since the duties of the bail magistrate are so limited, there are few channels outside of the setting of bail through which the magistrate could affect outcomes. One concern would be a correlation between the schedules of the magistrates and the likelihood of receiving a particular judge, prosecutor or defense attorney later on in the criminal proceedings. However, the peculiar schedule of the magistrates does not align with the schedule of any other actors in the criminal justice system. For one, this is because the other courts are not open on weekends. This is also because Philadelphia predominantly operates on a horizontal system, meaning that a different prosecutor handles each different stage of the criminal proceedings. Likewise, if the defendant is represented by the Defender Association (~60% of the sample), she will have a different defense attorney at each stage.[12] While attorneys often rotate duties, their rotations are based on a Monday–Friday work week and not the "five days on, five days off" schedule of the magistrates.

Eligibility for public defense is another potential channel through which the magistrate could affect outcomes; 75% of the sample has a public defender at the time of disposition. However, there is no correlation between the leniency of the bail magistrate and having a public defender. This can be seen in Figure 1, where the $x$ and $y$ axes show residuals from regressions of detention and having a public defender (respectively) on controls for the time and season of the bail hearing. The time controls account for the fact that certain magistrates do not work through the entire time period of my data, and each dot represents the average per

12. The most serious cases are not handled horizontally; however, the choice of attorney to handle these cases has nothing to do with the magistrate.

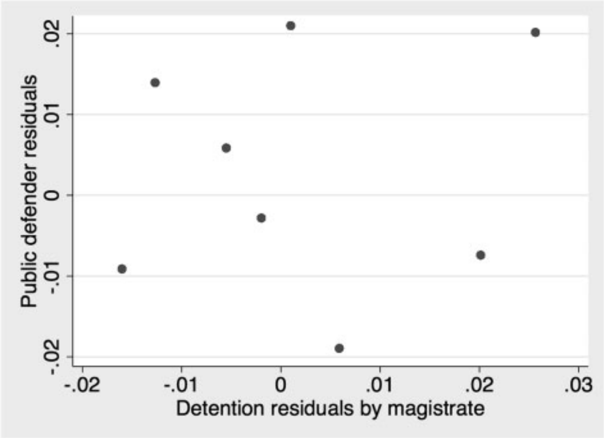


Figure 1. This figure shows the relationship between pretrial detention and having a public defender. Each dot represents the per-magistrate average. Both pretrial detention and public defense have been residualized against time controls to account for the fact that some magistrates work in different time periods.

magistrate. There is no visible correlation between the likelihood of receiving a lenient magistrate and the likelihood of having a public defender. (Nor is there any statistically significant relationship between the two in a regression.) In Section Appendix Table A1, I show that controlling for whether or not the defendant is represented by a public defender has no meaningful effect on the main results.

The only other condition of release that the magistrates are responsible for is determining whether the defendant must phone in periodically with Pretrial Services. As of 2009, approximately 9% of defendants were required to call into pretrial services either once or twice a week as a part of their condition of release (Clark et al. 2011). These phone calls are made to an interactive voice-response system, and there is no therapeutic element involved. Those who violate the call-in requirement do so with impunity: no violation notice is sent to the court, nor are any sanctions applied (Clark et al. 2011). It is unlikely that these calls will have more than a minor effect on case outcomes. In robustness tests, I find that the main results are robust to the inclusion of controls for the telephone call-in requirement (results not shown).

More invasive conditions of release are available to judges later in the criminal proceedings, but not to the magistrate who makes the initial bail assignment. These include electronic monitoring, drug testing, substance abuse counseling, in-person meetings with pretrial services or house arrest. As of 2009, only about 1% of arrestees were assigned to any of these conditions (Clark et al. 2011). The schedules of the judges who assign these conditions of release do not correlate with the rotating schedule of magistrates.

## 4. Data and Descriptive Statistics

The data for this analysis come from the court records of the Pennsylvania Unified Judicial System. PDF files of case dockets and court summaries were acquired by web-scraping public records; these were converted into data suitable for statistical analysis by text-parsing. The data covers all the Philadelphia arrests in which charges were filed between September 13, 2006 and February 18, 2013. Before September 13, 2006, Philadelphia used a different data management system and the data from that time period is of much lower quality. I do not look at cases which began after February 18, 2013 both because I wanted to leave ample time for all cases to resolve and because one of the magistrates was replaced by a new one on that date.

Each observation in my data set refers to a particular criminal case. A case can have multiple charges and a defendant can have multiple cases. Information about the bail amount, the magistrate, the bail hearing, and the charges at the time of the bail hearing comes from the Municipal Court (lower court) dockets. Information about court fees and whether the defendant is held pretrial on a detainer can be found in the Municipal Court dockets as well as the Court of Common Pleas (felony court) dockets. In addition, each defendant has a Court Summary Report, which summarizes the outcomes of each criminal case in which charges were filed in Pennsylvania. This provides both criminal history and recidivism information, as well as other general descriptors of each case (outcomes, sentencing, attorneys, dates of arrest/disposition, etc.). Average gross income for each ZIP code in 2010 was acquired from IRS.gov.[13]

A few constraints of the data should be noted. First, criminal history and recidivism is only available for crimes committed within Pennsylvania. Of these, I have the full range of past charges, and all post-release charges before December, 2015. Second, the data does not allow me to distinguish between concurrent and consecutive incarceration sentences. The definition of the length of incarceration that is used in this article is the longest sentence received. Finally, a small subset of the data got lost in the web-scraping process. I am missing key data sources for about 0.33% of the sample (about 1000 cases), these have been dropped. Since these missing variables are due to technical errors in the download, they should not result in any systematic selection of cases and are not expected to affect the results. The final sample consists of 331,971 cases.

Figure 2a shows a histogram of the number of days defendants are detained before disposition, conditional on being detained more than three days and less than 600 days. The left tail of the distribution is omitted since the primary definition of "detainees" used in this article is being unable to make bail within three days; the long right-hand tail of the distribution is omitted for visual simplicity. The median number of days

13. https://www.irs.gov/uac/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi

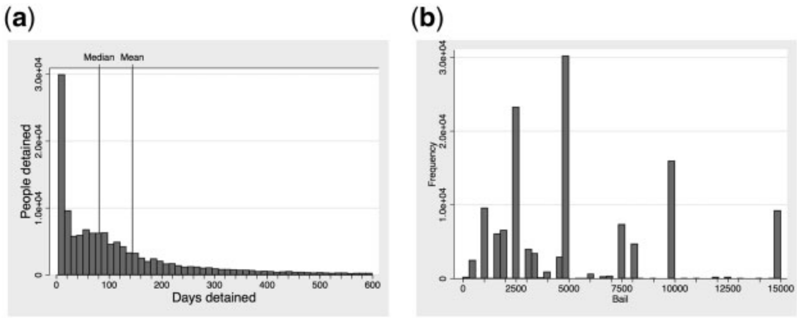


Figure 2. (a) The average number of days detained for those who are detained for more than three days after the bail hearing, truncated at 600 days for visual clarity. (b) The distribution of nonzero bail amounts, truncated at $150,000 (95th percentile).

detained for those who are unable to make bail within three days is 78, the mean is 146.

Summary statistics for the released group, the detained group, and the whole sample are shown in Table 1. Defendants are predominantly male, with an average age of 32 years. In all, 57% of the defendants are black, 28% are white and, with the exception of a tiny group of Asians, the rest are either missing race information or marked as unknown-race. Those detained tend to have longer criminal histories and are facing more serious charges than those released. It should be noted, however, that 28% of the detained sample are only facing misdemeanor charges.[14]

Almost half the sample have their charges dropped, dismissed, or are placed in some sort of diversion program.[15] Almost everyone else was convicted, through plea or at trial, on at least one charge. In all, 90% of cases resolved at trial result in convictions, suggesting that prosecutors will not bring a case to trial if they do not believe they have a strong chance of winning. If a detained defendant pleads quickly to avoid more time waiting in jail, she may be pleading guilty on a case that otherwise would not have proceeded to court.

One third of the sample is released without being required to pay bail and an additional 26% are able to pay their way out within three days of the bail hearing. Figure 2b shows the distribution of bail amounts for defendants with monetary bail set. About 10% of the sample has bail

14. The offense information used in this article is taken from the charge at the time of the bail hearing. Many of those who were originally charged with felonies subsequently had the felony charge downgraded to a misdemeanor.

15. Diversion programs are designed for those with low-level misdemeanor charges; if the defendant agrees to requirements such as paying restitution to victims, entering rehab, or performing community service, they are generally able to avoid a formal adjudication of guilt.

Table 1. Summary Statistics

| | Released | Detained | Total |
|---|---|---|---|
| Age | 32.8 | 32.0 | 32.5 |
| Male | 0.79 | 0.88 | 0.83 |
| White | 0.30 | 0.26 | 0.28 |
| Black | 0.52 | 0.65 | 0.57 |
| Unknown/missing race | 0.15 | 0.06 | 0.11 |
| Charged with selling drugs | 0.12 | 0.13 | 0.12 |
| Charged with robbery | 0.02 | 0.14 | 0.07 |
| Charged with drug possession | 0.18 | 0.06 | 0.13 |
| Charged with aggravated assault | 0.07 | 0.11 | 0.09 |
| Charged with first offense DUI | 0.10 | 0.02 | 0.06 |
| Number of prior cases | 3.90 | 6.28 | 4.88 |
| Has felony charge at time of bail hearing | 0.36 | 0.72 | 0.51 |
| Case proceeds to felony court | 0.19 | 0.40 | 0.28 |
| Bail | $3413 | $61,974 | $26,844 |
| Nonfinancial release | 0.54 | 0.01 | 0.33 |
| Detained >3 days | 0 | 1 | 0.41 |
| All charges dropped or dismissed | 0.48 | 0.48 | 0.48 |
| Case went to trial | 0.32 | 0.19 | 0.27 |
| Not guilty on all charges | 0.03 | 0.03 | 0.03 |
| Guilty of at least one charge | 0.49 | 0.49 | 0.49 |
| Pled guilty to at least one charge | 0.21 | 0.33 | 0.26 |
| Court fees charged | $387 | $206 | $312 |
| Sentenced to incarceration | 0.18 | 0.32 | 0.24 |
| Maximum days of incarceration sentence | 94 | 576 | 292 |
| Minimum days of incarceration before parole eligibility | 39 | 322 | 155 |
| Observations | 195,340 | 136,631 | 331,971 |
| Conditional summary statistics | | | |
| Court fees charged (cond. on conviction) | $409 | $753 | $611 |
| Sentenced to incarceration (cond. on conviction) | 0.46 | 0.67 | 0.49 |
| Max. days of incarc. sentence (cond. on incarceration) | 529 | 1736 | 1213 |
| Min. days before parole eligibility (cond. on incarceration) | 220 | 971 | 645 |

*Notes*: "Released" is defined as released from pretrial custody within three days after the bail hearing, and "Detained" is defined as detained pretrial for at least four days. The statistic shown is the mean and, unless otherwise indicated, variables are dummies where 1 indicates the presence of a characteristic. Age is measured in years, those marked "Number..." are count variables, and those expressed in dollar amounts are currency. The sentence is coded as zero if the defendant did not receive an incarceration sentence. The summary statistics in the bottom panel are limited to those who are convicted (top two rows) or receive an incarceration sentence (bottom two rows).

set at an amount greater than $0 but less than or equal to $2000. Among this low-bail sample—77% of whom are charged only with misdemeanors—the average number of days detained pretrial is 28, and 40% are detained for at least four days. This group would need to pay a deposit of $200 or less to secure their freedom. The median amount of bail for those who do not post bond is $10,000.

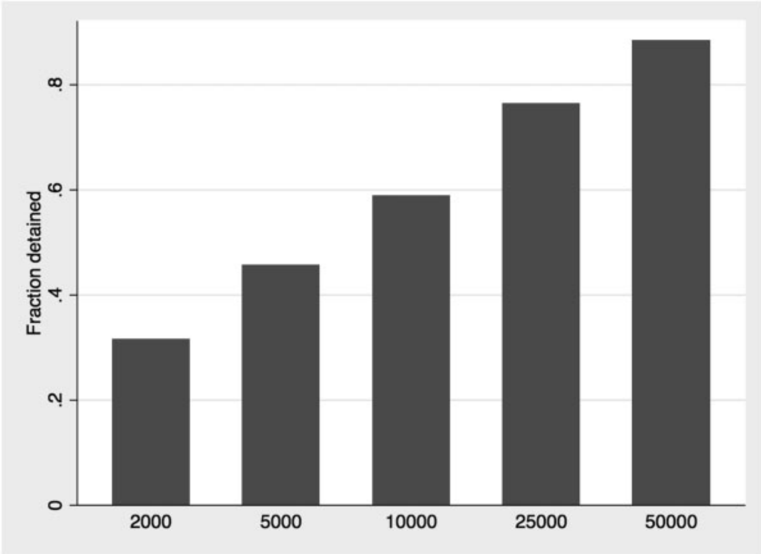


Figure 3. This figure shows the percentage released and detained at a variety of bail levels among defendants who did not have a detainer placed on them (i.e. were free to leave if they posted bail).

Figure 3 shows the percentage detained and released at various levels of bail. This subsample is limited to defendants who do not have a detainer placed on them—in other words, these defendants would be free to leave if they posted bail. Almost half of the defendants with bail set at $5000 do not post bail within three days of the bail hearing. These defendants would only need to post a deposit of $500 in order to secure release. Although a percentage may prefer to stay in jail, it is reasonable to infer that many would post bail if they could afford it. As of 2008, Philadelphia's jails housed 44% more inmates than they were designed to, and 20% of inmates were living in "triple cells" (three inmates in a cell designed for one or two people).[16] "Lock-downs" and restrictions on movement are common, and despite the heat and humidity which characterize Philadelphia's summers, many buildings lacked air conditioning.

## 5. Empirical Strategy

Instrumenting for sentencing outcomes using varying propensities of randomly assigned or rotating judges is a popular method of identifying causal effects in criminal justice (Kling 2006; Aizer and Doyle 2009; Loeffler 2013; DiTella and Schargrodsky 2013; Mueller-Smith 2015). My empirical specification follows in that tradition. I use a jackknife

16. From *Williams v. City of Philadelphia*, 2008.

(leave-one-out) instrumental variables method, in which magistrate leniency for case *i* is calculated using all observations except *i*. This is a commonly used method to reduce bias due to instrument endogeneity, particularly when there are many instruments (Angrist et al. 1999). Since pretrial detention status is a function of both magistrate leniency and unobserved characteristics that might be correlated with the outcome, these unobserved characteristics will be correlated with the instrument if the pretrial detention status of case *i* is included in the instrument calculation for case *i*.

My specification follows in the tradition of Mueller-Smith (2015) and a robustness test in Aizer and Doyle (2009), and allows the preferences of the magistrate to vary across three time periods and according to the offense, criminal history, race and gender of the defendant. The first stage of this specification is shown in equation (1) where a dummy for pretrial detention in case *i* ($Detention_i$) is regressed on the magistrate dummy ($Magistrate_i$) interacted with a subset of covariates ($Cov_i^{sub}$)[17] and with indicators for three time periods ($T_i$), as divided by February 23, 2009 and February 23, 2011.[18] Other offense, criminal history, and demographic controls are included in $X_i$,[19] and controls for the time and date of the bail hearing are included in $Time_i$.[20] The instrument for pretrial detention for the defendant in case *i* is thus the average detention rate of all other individuals with a similar offense, criminal history, race and

17. $Cov^{sub}$ consists of the following variables: dummies for the 17 most common offenses (murder, robbery, aggravated assault, burglary, theft, shoplifting, simple assault, drug possession, drug sale, drug purchase, marijuana possession, second-degree felony firearm possession, third-degree felony firearm possession, vandalism, prostitution, first offense DUI, motor vehicle theft), a dummy for being labeled black, a dummy for being female, the number of prior cases, the number of prior violent crimes, a dummy for having at least one prior and a dummy for having a detainer.

18. These dates provide a natural break point since certain magistrates left were replaced by others at these times.

19. $X_i$ includes controls for age, age squared, age cubed, the number of prior felony cases, prior cases where the defendant was found guilty of at least one charge, dummies for having at least one prior case, having at least three prior cases, awaiting trial on another charge, and having a prior arrest within five years of the bail hearing. Offense variables include dummies for having a charge in the following category: rape, possession of stolen property, second offense DUI, resisting arrest, stalking, indecent assault, arson, solicitation of prostitutes, disorderly conduct, pedophilia, intimidation of witnesses, accident due to negligence, false reports to a police officer, fleeing an officer, and reckless endangerment. Additional offense controls include dummies for being charged with a first-, second- or third-degree felony, an unclassified felony, a first-, second- or third-degree misdemeanor, an unclassified misdemeanor, or a summary offense. I also control for the total number of charges, the total number of felony charges, the total number of misdemeanor charges, and the total "offense gravity score" of the charges (the offense gravity score is used by Philadelphia to measure the seriousness of a charge on a scale of 1–8).

20. $Time_i$ includes dummies for each year, a cubic in the day of the year (1–365), dummies for each day of the week, and for each shift in the day (graveyard, morning, evening).

gender who had their bail set by the same magistrate during a two year period.

$$Detention_i = \alpha_1 + Magistrate_i * T_i * \omega_1 + Magistrate_i * Cov_i^{sub} * \phi_1 + Cov_i^{sub} * T_i * \delta_1 + X_i * \gamma_1 + Time_i * \psi_1 + e_i. \quad (1)$$

The second stage of the two stage least squares regression is shown in equation (2) where $Case\_Outcome_i$ represents a variety of case outcomes, $\widehat{Detention}_i$ is the fitted value from the jackknifed first stage, and $Cov_i^{sub}$, $X_i$, $T_i$ and $Time_i$ are as described above.

$$Case\_Outcome_i = \alpha_2 + \widehat{Detention}_i * \beta_2 + Cov_i^{sub} * T_i * \delta_2 + X_i * \gamma_2 + Time_i * \psi_2 + \epsilon_i. \quad (2)$$

Each magistrate sees about 17,000 cases during a two year period. Since the interaction effects are additive, the instrument for each case will be estimated off of many thousands of other defendants. For example, the instrument for a white female with an aggravated assault charge who had bail set by Magistrate 3 will be calculated *not just* using others with the exact same characteristics, but rather the cumulative differential effect that Magistrate 3 has on the detention status of whites, females, and those facing aggravated assault charges, compared to the sample average.

The inclusion of magistrate interactions in the first stage increases the power of the instrument, but it also eases concerns about monotonicity violations (Imbens and Angrist 1994). In this setting, a monotonicity violation would occur if some defendants are *less* likely to be detained pretrial if they have bail set by a usually-strict magistrate. If magistrates have heterogeneous bail preferences—in other words, if they are relatively strict for certain types of defendants but relatively lenient for other types of defendants—the monotonicity assumption would not hold. The data show ample evidence of heterogeneous bail preferences. Figure 4a shows detention rates by magistrate across the entire sample. The $y$ axis shows residuals from a regression of the pretrial detention dummy on a set of time controls; the whiskers show the 95% confidence intervals. Each bar shows the average residuals per magistrate. Figures 4b shows the same per-magistrate average detention residuals among a sample limited to those charged with robbery. The magistrate that is most lenient overall is actually strictest when it comes to robbery: magistrate preferences are not consistent across offense types. This is confirmed by conducting a series of difference-in-means tests, where the null hypothesis is that the average detention residuals for defendants who had bail set by the strictest magistrate (as measured by overall detention rates) will be larger than the average detention-residuals for defendants who saw the most-lenient magistrate. This one-sided test is conducted separately for defendants charged with the 17 most common offense types. Of these 17 different

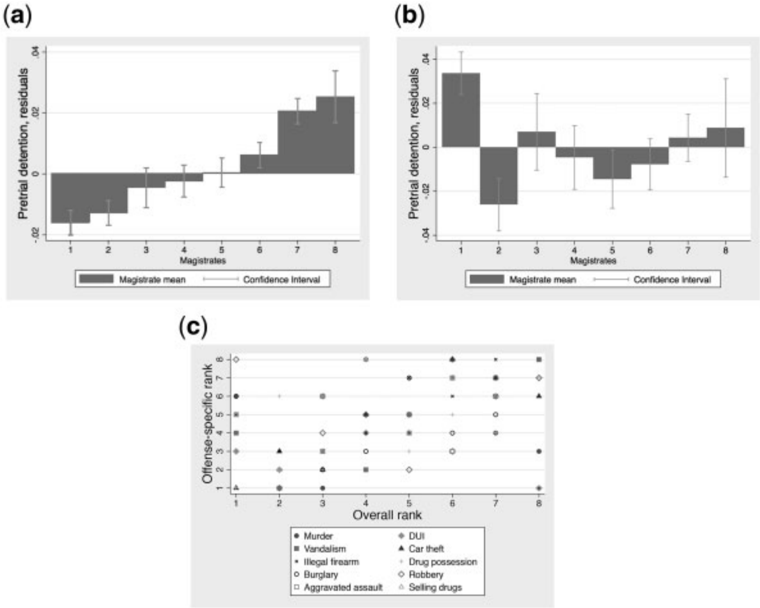


Figure 4. The top two figures show pretrial detention rates by magistrate over the whole sample (a), and for defendants charged with robbery (b). The numbers 1 through 8 delineate the different magistrates by ranking, where magistrate 1 is the most lenient magistrate across the entire sample. The *y* axes show the residuals from a regression of pretrial detention on time controls; each bar represents the per-magistrate average of the residuals. The error bars indicate the 95% confidence intervals for the mean. The numbering of the magistrates is consistent across both figures. (c) Plots the overall magistrate leniency ranking on the *x* axis against various crime-specific magistrate leniency rankings on the *y* axis.

tests, there are four (including robbery) for which I reject the null hypothesis. All four rejecting tests have *p*-values less than 0.03; two of them have *p*-values less than 0.000001. Thus for 4 of the 17 most common offenses, being assigned to the magistrate who is most lenient overall would actually *increase* the likelihood of being detained pretrial relative to being assigned the most strict magistrate.

Figure 4c provides additional evidence that magistrate leniency varies by offense type. Figure 4c plots the overall leniency ranking of each of the eight magistrates on the *x* axis against the leniency ranking of the eight magistrates on the subsample of defendants facing different charges on the *y* axis. The ranking for each subsample is indicated by a different marker. Under the monotonicity assumption, each magistrate would have the same ranking within each offense category, and the graph would show a single 45 degree line of overlaid symbols. However, as is evidenced in this chart, there is considerable variance in ranking across different offenses. For instance, the magistrate who is most lenient overall (with a leniency-ranking of 1 on the *x* axis) has the leniency-ranking of 1, 3, 4, 5, 6, and 8 across 10 different offense types.

Violations of the monotonicity assumption will lead to biased estimates if there are heterogeneous treatment effects (Angrist and Krueger 1995). In fact, the combination of a monotonicity violation and heterogeneous treatment effects could even generate a treatment effect estimate with the wrong sign. Consider a simple example in which there are only two offense categories: DUI and robbery. Suppose that pretrial detention had no effect on case outcomes for defendants who are charged with DUI, but increased the likelihood of conviction for people charged with robbery. If the instrument for pretrial detention *increases* the likelihood that DUI defendants will be detained pretrial, but *decreases* the likelihood that a robbery defendant is detained pretrial,[21] then the instrumental variables (IV) approach would estimate that pretrial detention makes a defendant *less* likely to be convicted. This is because the instrument works "backwards" for the group of defendants for whom pretrial detention has an effect: being assigned a generally-strict magistrate decreases instead of increases the likelihood of being detained pretrial.

The inclusion of magistrate interaction terms in the first stage allows magistrates to have different bail-setting preferences over a variety of defendant characteristics. Although this may not entirely eliminate nonmonotonicity bias, it should ameliorate it substantially. In tests, I found that the estimates tended to stabilize as more interaction terms were added. This is discussed more in Section 6.

Without further assumptions, the magistrate received by each defendant must be essentially random to allow for a causal interpretation of the results. Table 2 shows that pretrial detention is endogenous but that the instrument for pretrial detention is uncorrelated with observable characteristics. Each cell of the table comes from a separate regression. The dependent variables of each regression—various covariates describing the case and the defendant—are shown in the left-hand side of the table. Each cell shows the coefficient on pretrial detention (Column 1) or the instrument for pretrial detention (Columns 2 and 3). Column 1 shows results for ordinary least squares (OLS) regressions of each covariate on a dummy for pretrial detention, controlling only for a small set of time controls: fixed effects for each year and a cubic in the day of the year (1–365). As can be seen, pretrial detention is strongly endogenous. Those detained are facing more serious charges, have longer criminal histories, are more likely to be male, and more likely to have a graveyard-shift bail hearing. Column 2 shows results from regressing covariates on the "simple instrument," that is the predicted likelihood of pretrial detention based on the leave-me-out average detention rate per

21. One could imagine an instrument that works this way if there are more DUI cases than robbery cases and if magistrates who are relatively harsh on DUIs are relatively lenient on robberies.

Table 2. Randomization Test

| | (1) OLS | (2) Simple instrument | (3) Interacted instrument |
|---|---|---|---|
| White | −0.0399**** | 0.0834 | |
| | (0.00158) | (0.0631) | |
| Male | 0.0905**** | −0.00484 | |
| | (0.00126) | −0.00484 | |
| At least one prior charge | 0.140**** | −0.0485 | |
| | (0.00143) | (0.0600) | |
| Robbery | 0.127**** | 0.00994 | |
| | (0.00101) | (0.0364) | |
| First time DUI | −0.0833**** | −0.0429 | |
| | (0.000760) | (0.0335) | |
| Selling drugs | 0.00634**** | 0.0170 | |
| | (0.00117) | (0.0466) | |
| Aggravated assault | 0.0444**** | −0.00302 | |
| | (0.00105) | (0.0395) | |
| Age | −0.901**** | −1.700 | 0.377 |
| | (0.0398) | (1.574) | (0.602) |
| Prior felony arrests | 0.819**** | 0.559** | −0.0623 |
| | (0.00772) | (0.274) | (0.108) |
| Prior convictions | 0.779**** | −0.127 | −0.0796 |
| | (0.00902) | (0.337) | (0.128) |
| Offense gravity score | 9.107**** | −0.675 | 0.158 |
| | (0.0422) | (1.673) | (0.365) |
| Number felony charges | 3.193**** | −0.494 | −0.0167 |
| | (0.0168) | (0.673) | (0.184) |
| Rape | 0.0156**** | −0.0104 | 0.00116 |
| | (0.000372) | (0.0128) | (0.00457) |
| Resisting arrest | 0.0108**** | −0.0273 | −0.00407 |
| | (0.000591) | (0.0225) | (0.00878) |
| Disorderly conduct | −0.00712**** | 0.00861 | 0.00254 |
| | (0.000420) | (0.0171) | (0.00274) |
| Graveyard shift | 0.0311**** | 0.0753 | 0.00799 |
| | (0.00165) | (0.0650) | (0.0284) |
| Weekend shift | −0.000252 | 0.0262 | 0.0197* |
| | (0.000635) | (0.0252) | (0.0113) |
| Observations | 331971 | 331971 | 331971 |

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.

*Notes*: The dependent variables are shown on the left-hand side. In Column 1 the independent variable is a dummy for pretrial detention, in Column 2 it's the "simple instrument" for pretrial detention (the predicted likelihood of detention based on the magistrate dummies) and in Column 3 it's the "interacted instrument" (the predicted likelihood of detention based on the magistrate dummies interacted with three time periods, offense, criminal history, and demographics). Each regression controls for the year and season of the bail hearing to account for the fact that some magistrates work in different time periods. Heteroskedastic-robust standard errors in parentheses.

magistrate. Fixed effects for each year, and a cubic in the day of the year, are included to account for the fact that some magistrates work in different time periods. Although pretrial detention is strongly endogenous, this simple instrument for pretrial detention is not. Of the 17 tests conducted, only one is statistically significant at the 5% level, no more than would be expected by chance.

Column 3 shows regressions of various covariates on the "interacted instrument" for pretrial detention, that is the leave-me-out predicted likelihood of detention based on the magistrate dummies interacted with three time periods, offense, criminal history, and demographics of the defendants, as described above. Once again, fixed effects for each year, and a cubic in the day of the year, are included to account for the fact that some magistrates work in different time periods. The dependent variables in Column 3 are from $X_i$: variables that are included as controls in the main regression but are not included as interactions with magistrate fixed effects in the first stage. These include less common crime types, general descriptors of the charges (such as the total number of felony charges), indicators for shift times or weekends, and additional measures of criminal history. Also included as a dependent variable is the "offense gravity score," which is a measure used in Philadelphia to evaluate the seriousness of the charges. Once again, the results show that the instrument for pretrial detention is exogenous to a wide variety of observable characteristics.

Figure 5 shows graphical evidence of the relationship between magistrate leniency and conviction status. It consists of two overlaid graphs; in the first graph, with circles as markers, the axes represent residuals from a regression of conviction and pretrial detention respectively on the set of time controls described by *Time*. The second graph, represented by diamonds, is similar except that conviction and pretrial detention are residualized over $Cov^{sub} * T^3, X$ and *Time*. Each marker represents the average detention and conviction residuals of one of the eight magistrates. A linear fit between the per-magistrate conviction residuals and the per-magistrate detention residuals are also shown: the slope of this line is an approximation of the simple instrumental variables regression.[22] As can be seen, there is a clear positive correlation between conviction and detention which is not qualitatively altered once the effect of covariates have been removed.

## 6. Impacts of Pretrial Detention

Table 3 shows how pretrial detention affects both conviction and the likelihood of pleading guilty using a variety of different jackknife IV specifications. The specifications vary in two ways. First, Columns 1 and 2 exclude covariates from both the first and second stages, whereas Columns 3–6 include covariates in both stages. Second, the instrument set used in the first stage expands as we move to the right (except for

22. Given the nonmonotonicity concerns discussed in this article, the slope represented in Figure 5 may not be an accurate representation of the magnitude of the causal relationship between pretrial detention and conviction. Nonetheless, it can be useful to see a visual representation of the relationship with relatively unprocessed data.

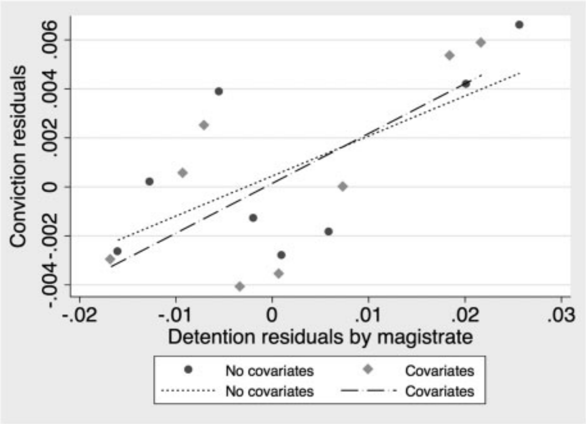


Figure 5. This figure consists of two overlaid graphs. In the first graph, with circles as markers, the axes represent residuals from a regression of conviction and pretrial detention respectively on the set of time controls described by *Time*. The second graph, represented by diamonds, is similar except that conviction and pretrial detention are residualized over $Cov^{sub} * T^3$, $X$ and *Time*. Each marker represents the average detention and conviction residuals of one of the eight magistrates. A linear fit between the per-magistrate conviction residuals and the per-magistrate detention residuals are also shown.

Column 3, which includes the same instrument set as Column 2, but with the addition of covariates in both stages). As discussed above, the larger instrument sets effectively allow magistrate preferences to vary more flexibly over case and defendant characteristics. Column 1 uses only the eight magistrate dummies as instruments. The instruments in Columns 2 and 3 consist of the eight magistrate dummies interacted with dummies for the three time periods. Column 4 adds additional instruments: the interactions between the magistrate dummies and the five most common lead charges, which are drug possession, first offense DUI, robbery, selling drugs and aggravated assault. Column 5 adds interactions between magistrate dummies and the number of prior cases/prior violent charges, dummies for having at least one prior case, having a detainer, and being black or female. Finally, Column 6 allows for more nuanced variation in magistrate preferences across offense categories by adding first-stage interactions between the eight magistrates and the 12 next-most-common lead charges: murder, burglary, theft, shoplifting, simple assault, buying drugs, marijuana possession, second- and third-degree felony firearm possession, vandalism, prostitution, and motor vehicle theft.

Two patterns emerge from evaluating the estimates across the six different specifications. First, standard errors decrease as the instrument becomes more flexible. This is as expected: since magistrates are not uniformly strict or lenient, allowing their bail-setting preferences to vary according to offense, criminal history, race and gender increases the power

Table 3. How Does Pretrial Detention Affect Conviction Rates and Guilty Pleas?

| Outcomes | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Conviction | 0.167** | 0.180*** | 0.282*** | 0.119*** | 0.0907** | 0.0620** |
| | (0.0736) | (0.0655) | (0.0868) | (0.0412) | (0.0364) | (0.0291) |
| | | | | | | {0.016} |
| | | | | | | ((0.032)) |
| Guilty plea | 0.124** | 0.174*** | 0.177** | 0.102*** | 0.0536* | 0.0469* |
| | (0.0619) | (0.0563) | (0.0776) | (0.0366) | (0.0324) | (0.0262) |
| | | | | | | {0.052} |
| | | | | | | ((0.073)) |
| Instrument set: | | | | | | |
| Eight magistrate dummies | Y | Y | Y | Y | Y | Y |
| Magistrate × 3 time periods | | Y | Y | Y | Y | Y |
| Magistrate × top 5 crimes | | | | Y | Y | Y |
| Magistrate × crim. history | | | | | Y | Y |
| Magistrate × demographics | | | | | Y | Y |
| Magistrate × top 6–17 crimes | | | | | | Y |
| Variables included in both stages: | | | | | | |
| Time controls | Y | Y | Y | Y | Y | Y |
| Defendant and case covariates | | | Y | Y | Y | Y |
| First stage *F*-stat. | 34.68 | 19.46 | 25.71 | 21.82 | 14.99 | 11.56 |

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.

*Notes*: This table shows how pretrial detention affects conviction and guilty pleas using various jackknife instrumental variables specifications. The exogenous variables in the first column are the eight magistrate dummies; in the subsequent columns they include interactions between the magistrate dummies and three time period fixed effects, the five most common crime types, a variety of criminal history variables, defendant demographics, and the remainder of the 17 most common crime types. The first two columns control only for the time and date of the bail hearing, all subsequent columns include the full set of controls for offense, criminal history, and demographics as described in Section 5. Heteroskedastic-robust standard errors are in parentheses, empirical *p*-values as derived from a permutation test are shown in curly brackets and parametrically estimated *p*-values are shown in double parentheses. There are 331,971 observations in all regressions. The first-stage *F* statistic on all magistrate dummies and interaction terms is provided in the bottom row.

of the research design. Second, the magnitude of the effect also decreases as the instrument becomes more flexible. If the treatment effects are heterogeneous—in other words, if the impacts of pretrial detention are greater for certain types of defendants than others—then nonmonotonicity bias will be lower in the interacted specifications than in the simple IV. In particular, if treatment effects are smaller among crime types for which the monotonicity assumption is violated, then the estimates in Columns 1–3 will be biased upwards. The specification shown in Column 6 may still be subject to some nonmonotonicity bias. However I have found that adding additional interactions to the first stage does not substantially change the results, suggesting that any remaining bias should be minimal.

My preferred specification, Column 6, allows magistrates' preferences to vary across all 17 of the most common crime types, across the criminal history, race, and gender of the defendant, and over the three time periods.[23] The power of the instrument is greatest in this specification, the standard errors are smallest, and nonmonotonicity is less likely to be a concern when magistrates preferences are allowed to vary. It should also be noted that this is the most conservative specification: the effect sizes are smaller than in the simpler specifications. I estimate that pretrial detention leads to a 6.2 percentage point increase in the likelihood of being convicted and a 4.7 percentage point increase in the likelihood of pleading guilty. Compared to the means for each dependent variable, that estimate converts into a 13% increase in the probability of conviction and a 18% increase in the likelihood of pleading guilty.

The estimated effects in my preferred specification are smaller than those found in the concurrent literature. The quasi-experimental estimates shown in Dobbie et al. (2018), Heaton et al. (2017), and Leslie and Pope (2017) suggest that pretrial detention leads to a 15, 20 (misdemeanor), and 13 (felony) percentage point increase, respectively, in the likelihood of conviction.[24] Some of this discrepancy could be due to sample differences or cross-jurisdictional variation. It is also possible that there remains some omitted variable bias in Heaton et al. (2017) and Leslie and Pope (2017), as the source of identifying variation in Harris County and New York City is less clearly exogenous. The quasi-experimental analysis in Heaton et al. (2017) relies on the fact that defendants are more likely to make bail on if they are arrested on Thursday, close to the weekend, than if they are arrested on Tuesday. However, there may be other differences in Tuesday/Thursday cases that affect conviction rates. Leslie and Pope (2017) instrument for pretrial detention using judge leniency, but many of the bail judges in New York City (at least during the time period of their analysis) were assigned to work in fixed shifts in courtrooms that relate to a particular geographic area of the city. The authors account for courtroom and the time of the bail hearing in building the instrument, but is is unclear exactly where the remaining variation comes from, making it hard to ascertain whether there could be a confounding factor.

Dobbie et al. (2018), however, relies primarily on Philadelphia data. Roughly three-fourth of the data used in their analysis should be the same as that used here. The different effect sizes between Dobbie et al. (2018) and this article is thus likely due to different specifications.[25] In particular, the specification used in Dobbie et al. (2018), which shows similar effect size as shown in Column 1 of Table 3, does not allow

23. The most-common crime types are defined as all offenses for which at least 2% of the sample have that charge.

24. Leslie and Pope (2017) also show results for misdemeanors, but admit to significant confounds in the research design for this subsample.

25. Following the specification used in Dobbie et al. (2018), and using the Philadelphia data only, I am able to generate results that are similar to theirs: being released within three days of the bail hearing leads to a 16 p.p. decrease in conviction and a 13 p.p. decrease in pleading guilty. In comparison, their paper shows a 14 p.p. decrease in conviction and a 11 p.p. decrease in pleading guilty.

magistrate leniency to vary across different case types and thus may produce upward-biased estimates due to violations of the monotonicity assumption. Dobbie et al. (2018) refer to the discrepancy between their results and those found in this study in Footnote 18, but conclude that any potential bias from monotonicity violations is likely to be small. They do so on the basis of two arguments. Referring to a previous draft of this article, Stevenson (2016, unpublished working paper), they state that the results are similar and same-signed regardless of whether magistrate fixed effects are interacted with crime and defendant characteristics. However, "similar" may be in the eyes of the beholder. The estimated effect in the non-interacted specifications is three times larger than the estimated effect in the interacted specifications.[26] Some observers may consider a three-fold difference in magnitude to be a meaningful difference, even if it is same-signed.

Dobbie et al. (2018) also argue that monotonicity bias is not a concern because treatment effects do not vary much across various subsamples. (Monotonicity violations only result in bias if there are heterogeneous treatment effects.) While neither this article nor theirs find statistically significant differences in effect sizes across subsamples, this does not mean that treatment effects are homogenous. Subgroup analysis necessarily entails much smaller sample sizes, reducing power. Unless the research design is very high powered, heterogeneity in treatment effects can be hard to detect at the standard 5% level. Given the strong evidence of monotonicity violations in the first stage, a lack of statistically-significant heterogeneity in treatment effects should not equate to a lack of concern about monotonicity bias.

The bottom panel of Table 3 shows the *F*-statistic of joint significance on the set of first-stage instruments. This statistic is generally decreasing as interaction terms are added. This is as expected; the marginal information content of adding more interaction terms decreases as the first stage becomes more flexible.

Research designs with many instruments are rightly subject to increased scrutiny due to concerns about bias and incorrect standard errors. Bias concerns are mitigated by the use of the jackknifed first stage (Angrist et al. 1999). I verify the statistical significance of the results using a permutation test. This permutation test entails building a number of "false" work schedules for the magistrates. Like the real schedules, each false work schedule has a magistrate working for five days in a row on the same shift, and each magistrate only works one shift per five day period. Within these constraints, work schedules are randomly assigned to create 500 unique false work schedules. This preserves much of the correlational structure of the research design: defendants who have bail set during the same shift, who may have similar characteristics and may

26. This can been seen in Table 5 of Stevenson (2016, unpublished working paper), which is similar to Table 3 in this version of the article.

even be codefendants on the same case, will also have the same false-schedule magistrate. I calculate the two-stage-least-squares results for each of the false schedules and collect the *t*-statistics on the instrument for pretrial detention in the second stage. The empirical *p*-values are the fraction of false-schedule *t*-statistics which are greater in absolute value than the *t*-statistic from the real data. Since this process is computationally intensive, I only conduct it for select specifications. The empirical *p*-values shown in Column 6 are smaller than those estimated parametrically, confirming that the estimated effects are unlikely to be due to chance.

Table 4 shows how pretrial detention affects conviction rates, guilty pleas, court fees, the likelihood of being incarcerated, and both the maximum and minimum incarceration sentence.[27] Column 1 shows results from the jackknife instrumental variables method with the most fully interacted specification; the first two rows are identical to the final column of Table 3. Column 2 shows results from an OLS regression controlling for the full set of offense, criminal history, demographic, and time controls.

The IV estimates show that pretrial detention leads to an average increase of $129 in nonbail court fees owed, which translates into a 41% increase over the mean. In general, defendants who are convicted in Philadelphia are required to pay court fees to cover a variety of expenses associated with the case, including court costs, victim restitution, lab tests, probation expenses, etc. Conditional on being convicted, court fees average at $611. For the tens of thousands of people convicted as a result of pretrial detention—many of whom were unable to pay even fairly small amounts of bail—these court fees may pose a significant challenge. Most defendants pay only a portion of these fees, remaining in debt to the city. A total of 82% of defendants who were charged court fees are still in debt five years later, with an average debt of $691, or 85% of the total amount.[28] In 2011, Philadelphia hired a collection agency and began an aggressive campaign of collecting unpaid court debt dating back to 1971. This collection effort was controversial, partly because the court lacked records to back up computerized debt claims. Those who do not pay court fees face the threat of criminal prosecution, with a jail sentence of up to six months. There is no evidence, however, that criminal charges were ever filed against Philadelphia debtors (Denvir 2012). Facing public backlash and civil rights lawsuits, Philadelphia scaled back on debt collection in 2014.

The IV results for the likelihood of being incarcerated are positive but noisy; however, the results for the incarceration sentence length are more precise. Pretrial detention leads to an expected increase of 124 days in the

27. Sentence length is coded as zero for individuals who do not receive an incarceration sentence.

28. These results pertain to defendants for whom I have at least five years of post-arrest data: those arrested in 2010 or earlier.

Table 4. Full Sample Results—Jackknife IV and OLS

| | (1) IV | (2) OLS | (3) Mean dep. var. |
|---|---|---|---|
| Conviction | 0.0620** | 0.0333**** | 0.49 |
| | (0.0291) | (0.00197) | |
| | {0.016} | | |
| Guilty plea | 0.0469* | 0.0566**** | 0.26 |
| | (0.0262) | (0.00181) | |
| | {0.052} | | |
| Court fees | 129.5**** | −103.5**** | 312 |
| | (33.26) | (2.618) | |
| | {0.000} | | |
| Any incarc. | 0.0186 | 0.0976**** | 0.24 |
| | (0.0249) | (0.00166) | |
| | {0.466} | | |
| Max. days | 124.7* | 133.7**** | 292 |
| | (74.40) | (3.463) | |
| | {0.054} | | |
| Min. days | 136.4** | 67.78**** | 155 |
| | (62.61) | (2.539) | |
| | {0.008} | | |

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.

*Notes*: This table shows how pretrial detention affects various case outcomes using both a jackknife IV regression (Column 1) and an OLS regression (Column 2). Column 3 shows the mean of the outcome variables: dummies for being convicted/pleading guilty, total nonbail court fees in dollars, a dummy for whether or not the defendant receives an incarceration sentence, the maximum days of that incarceration sentence, and the minimum days the defendant must serve before being eligible for parole. Heteroskedastic-robust standard errors are in parentheses and empirical $p$-values are in the curly brackets. In all of the IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history, and demographics of defendants. There are 331,971 observations per regression. All regressions include the full set of controls as described in Section 5.

maximum days of the incarceration sentence, a 42% increase over the mean. Detention leads to a 136 day increase in the minimum number of days before being eligible for parole. Some defendants who have been detained get released on “time-served”—in other words, the time they spent detained pretrial is considered punishment for the crime. Since it was retrospectively considered punishment, I include time-served as part of the incarceration sentence. Using alternative definitions, in which time-served is not included as part of the sentence length, I estimate that pretrial detention leads to a 92 day increase in the maximum sentence and a 107 day increase in the minimum sentence.

With the exception of court fees, the OLS estimates and the IV estimates are same-signed. The negative correlation between pretrial detention and court fees could be due to the relative poverty of detainees—court fees can be waived for the indigent. The IV estimates for the other outcomes are sometimes smaller and sometimes larger than the OLS estimates; for guilty pleas and the maximum sentence length the two estimates are quite similar in magnitude.

Empirical $p$-values for all the IV results are shown in curly brackets. Again, the empirical $p$-values are generally smaller than those estimated parametrically. Additionally, I conduct a wild cluster bootstrap test as proposed in Cameron et al. (2008). For this test, I define a cluster as a magistrate during a two year period. Compared to the parametrically estimated $p$ values, the wild cluster $p$ values change very little for conviction, court fees or incarceration. The $p$ value increases for guilty pleas, such that this estimate is no longer statistically significant at the 10% level. They decrease for the minimum/maximum days of incarceration, such that both estimates are now statistically significant at the 1% level.

Table A1 in the Appendix provides evidence that variation in eligibility for public defense does not confound the estimates of the impacts of pretrial detention. Panel A of Table A1 is identical to Column 1 of Table 4 except that there are two endogenous variables that are instrumented for with magistrate dummies: pretrial detention and a dummy for having a public defender at the time of disposition.[29] I find no statistically significant effect on having a public defender in any specification, and the coefficients on pretrial detention change only trivially. Panel B is similar to Column 1 except that I add the controls for having a public defender in the second stage. Once again, the coefficients on pretrial detention change only trivially; if anything, they increase slightly in both magnitude and precision.

In Table 5 I show the impacts of pretrial detention separately for misdemeanor and felony defendants using the interacted instrumental variable method.[30] The IV effect sizes of the felony sample are similar in magnitude to the full sample, but are noisy. The IV effects among misdemeanors are more precisely measured and, at least in relation to the means of the dependent variables, are larger than the full sample estimates. In fact, pretrial detention among misdemeanor defendants leads to a statistically significant increase in all outcomes. The effects on punishment are particularly large: those detained will be 7.6 percentage points more likely to receive a sentence of incarceration over a mean of 16% incarceration rate. While the expected increase in sentence length is only a month or two, this represents more than a 100% increase relative to the mean. The large incarceration effects among misdemeanor defendants may be partly explained by defendants who are released on time-served, which is more common among misdemeanors. Using alternative definitions of sentence length in which time spent detained pretrial is subtracted from the incarceration sentence, pretrial detention is estimated to lead to a 38 day increase in the maximum days and an 11 day increase in the minimum days.

29. The dummy is equal to one if the defender has a public defender or a court appointed attorney; 86% of public defense is handled by a public defender. The magistrate has no say over which type of public defense is received.

30. The felony sample is defined as those who were charged with at least one felony at the time of the bail hearing; many of these had their charges downgraded to misdemeanors only by the time of the arraignment.

Table 5. Results for Misdemeanors and Felonies

| | Misdemeanor | | Felony | |
|---|---|---|---|---|
| | (1) IV | (2) Mean dep. var. | (3) IV | (4) Mean dep. var. |
| Conviction | 0.0766** (0.0363) | 0.50 | 0.0513 (0.0434) | 0.47 |
| Guilty plea | 0.0577* (0.0295) | 0.16 | 0.0391 (0.0414) | 0.35 |
| Court fees | 77.55** (38.03) | $351 | 139.3*** (53.69) | $274 |
| Any incarc. | 0.0759*** (0.0281) | 0.16 | −0.0257 (0.0398) | 0.32 |
| Max. days | 55.82** (21.95) | 48 | 182.3 (139.9) | 528 |
| Min. days | 26.62** (12.09) | 18 | 207.0* (119.3) | 288 |
| Observations | 163236 | | 168735 | |

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.

*Notes*: This table shows effect sizes for defendants charged with only misdemeanor crimes (Column 1) and those charged with felonies (Column 3). The means of the outcome variables are shown in Columns 2 and 4: dummies for being convicted/pleading guilty, total nonbail court fees in dollars, receiving an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. Heteroskedastic-robust standard errors are shown in parenthesis. In all IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history, and demographics of defendants. The means of the dependent and independent variables are shown in the subpanel. All regressions include the full set of controls as described in Section 5.

The estimated impact on sentence lengths is not dissimilar to that found in Leslie and Pope (2017) and Heaton et al. (2017).[31] Leslie and Pope (2017) find that pretrial detention leads to a 157 day increase in the minimum sentence for felonies and Heaton et al. (2017) find that pretrial detention leads to a 19 day increase in the sentence length for misdemeanors.

In Table A2 in the Appendix, I test for evidence of treatment effect heterogeneity across defendant characteristics. Generally, the IV estimates are too noisy to provide definitive evidence on this question. However, there are commonsense reasons why the effects of pretrial detention may vary. Certain offense types, such as DUI, shoplifting, or drug possession, rely on difficult-to-refute evidence and thus leave little room for extralegal factors to influence the outcome. True guilt is often harder to verify for offense categories such as assault or robbery. Conviction in these cases is contingent upon the time and resources devoted to building a strong defense; if pretrial detention limits the ability to gather evidence or meet with the lawyer, it is expected to impact the outcome of the case. Treatment

31. The average sentence length is reasonably similar across the different jurisdictions: average minimum felony sentences are 212 days in New York City (Leslie and Pope 2017) and 288 days in Philadelphia. Average minimum misdemeanor sentences for released defendants are 7 days in Harris County (Heaton et al. 2017) and 12 days in Philadelphia.

effects may also vary according to the defendant's prior experience with the criminal justice system. Jail is likely to be a particularly adverse experience for those who are incarcerated for the first time, thus increasing the pressure to plead guilty in order to get out of jail. Conversely, those who are more savvy with the criminal justice system may know better than to accept a bad plea deal just because they are detained pretrial.

## 7. Conclusion

There is currently a broad-reaching movement to reform bail systems across the United States. In recent years, New Jersey, Kentucky, Colorado, Maryland, New Mexico, Chicago, New York City, Harris County, San Francisco and many other places have committed to or implemented pretrial reform. Dozens of jurisdictions are implementing new pretrial risk assessment regimes in partnership with the Laura and John Arnold Foundation and 20 cities have developing pretrial reform proposals with a $75 million fund from the MacArthur Foundation. Philadelphia is also implementing significant changes to their pretrial system: they have instituted an early bail review for defendants who are detained pretrial, and Philadelphia's jail population has fallen by 18% from July 2015 to March 2017 (Gambacorta and Melamed 2017). Their newly elected DA has promised to end the use of monetary bail for those charged with nonviolent offenses (krasnerforda.com 2017).

The renewed interest in the front end of the criminal justice system is welcome. As shown in this article, pretrial detention is not only impactful in its own right, but it has significant downstream consequences: a detained defendant is more likely to be convicted, to receive a lengthy incarceration sentence, and to accrue more courtroom debt. The repercussions entailed with the loss of freedom in the beginning of the criminal proceedings underline the importance of making the pretrial custody decision with care.

## Appendix

Table A1. Robustness Checks

Panel A: instrumenting for public defender (full sample, IV)

| | (1) Conviction | (2) Guilty plea | (3) Court fees | (4) Any incarc. | (5) Max days | (6) Min days |
|---|---|---|---|---|---|---|
| Pretrial detention | 0.0625** | 0.0470* | 120.5**** | 0.0230 | 147.5* | 149.0** |
| | (0.0304) | (0.0271) | (33.17) | (0.0255) | (79.02) | (66.97) |
| Public defender | 0.00339 | 0.00115 | −67.48 | 0.0329 | 169.6 | 93.54 |
| | (0.0539) | (0.0481) | (72.23) | (0.0477) | (197.2) | (170.7) |

Panel B: controlling for public defender (full sample, IV)

| | (1) Conviction | (2) Guilty plea | (3) Court fees | (4) Any incarc. | (5) Max days | (6) Min days |
|---|---|---|---|---|---|---|
| Pretrial detention | 0.0688** | 0.0520** | 126.0**** | 0.0246 | 119.9 | 131.9** |
| | (0.0285) | (0.0257) | (33.18) | (0.0246) | (73.50) | (61.78) |
| Public defender | 0.0394**** | 0.0292**** | −36.43**** | 0.0421**** | 11.65 | −4.382 |
| | (0.00366) | (0.00330) | (4.531) | (0.00314) | (10.03) | (8.544) |
| Observations | 331971 | 331971 | 331971 | 331971 | 331971 | 331971 |
| Mean dep. var. | 0.49 | 0.26 | 312 | 0.24 | 292 | 155 |

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.

*Notes*: This table presents robustness checks for the main results. Panel A instruments for two endogenous variables: a dummy for having a public defender and the pretrial detention dummy. Panel B includes adds the controls for having a public defender into the second stage. The outcome variables are dummies for being convicted/pleading guilty, total nonbail court fees in dollars, receiving an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all specifications, magistrate preferences are allowed to vary across three time periods and according to offense, criminal history, and demographics of defendants. The means of the dependent variables are shown in the subpanel. All regressions include the full set of controls as described in Section 5. Heteroskedastic-robust standard errors are in parentheses.

Table A2. Comparing Results Across Defendant Subgroups

| | (1) White | (2) Black | (3) Young | (4) Old | (5) Few priors | (6) Many priors |
|---|---|---|---|---|---|---|
| Conviction | 0.0802 | 0.0664* | 0.0359 | 0.0716** | 0.118 | 0.0625** |
| | (0.0590) | (0.0392) | (0.0636) | (0.0358) | (0.0788) | (0.0317) |
| Guilty pleas | 0.0223 | 0.0204 | 0.0608 | 0.0521 | 0.0916 | 0.0445 |
| | (0.0549) | (0.0353) | (0.0578) | (0.0324) | (0.0727) | (0.0284) |
| Court fees | 88.64 | 113.8*** | 82.73 | 179.0**** | 40.44 | 151.8**** |
| | (75.38) | (44.05) | (76.43) | (40.79) | (105.1) | (35.15) |
| Any incarc. | −0.0285 | −0.00911 | −0.00439 | 0.0217 | −0.123** | 0.0721** |
| | (0.0532) | (0.0338) | (0.0556) | (0.0306) | (0.0624) | (0.0284) |
| Maximum days | 195.8 | 53.83 | 264.3 | 28.99 | 169.7 | 183.6** |
| | (135.4) | (112.6) | (209.4) | (76.78) | (213.0) | (78.48) |
| Minimum days | 236.4** | 107.0 | 245.3 | 57.72 | 245.2 | 181.8*** |
| | (109.8) | (95.66) | (182.4) | (62.86) | (173.5) | (66.94) |
| Observations | 94076 | 191379 | 167615 | 164356 | 124344 | 297963 |

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.

*Notes*: This table shows effect sizes among white defendants, black defendants, those who are under the age of 30 years, those over 30 years, those with zero/one prior arrests, and those with two or more prior arrests. The outcome variables are dummies for being convicted/pleading guilty, total nonbail court fees in dollars, a dummy for whether or not the defendant receives an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. All estimates come from jackknife independent variable specifications where magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants. Heteroskedastic-robust standard errors are in parentheses.

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# EXHIBIT F

# UNSECURED BONDS: THE AS EFFECTIVE AND MOST EFFICIENT PRETRIAL RELEASE OPTION

PRETRIAL JUSTICE INSTITUTE

Michael R. Jones
Washington, D.C.
October 2013

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## STUDY SUMMARY

This study was done to provide judicial officers, prosecutors, defense attorneys, sheriffs, jail administrators, county commissioners, pretrial services program directors, and other decision-makers in Colorado as well as in other states empirical evidence that can directly inform their pretrial release and detention policies and practices. Specifically, the simultaneous influence of unsecured bonds (personal recognizance bonds with a monetary amount set) and of secured bonds (surety and cash bonds) on the three most important pretrial outcomes: (1) public safety; (2) court appearance; and (3) jail bed use, were compared. The study, using data from over 1,900 defendants from 10 Colorado counties, found the following:

For defendants who were lower, moderate, or higher risk:

- Unsecured bonds are as effective at achieving public safety as are secured bonds.
- Unsecured bonds are as effective at achieving court appearance as are secured bonds.
- Unsecured bonds free up more jail beds than do secured bonds because: (a) more defendants with unsecured bonds post their bonds; and (b) defendants with unsecured bonds have faster release-from-jail times.
- Higher monetary amounts of secured bonds are associated with more pretrial jail bed use but not increased court appearance rates.
- Unsecured bonds are as effective at "fugitive-return" for defendants who have failed to appear as are secured bonds.
- Many defendants are incarcerated for the pretrial duration of their case and then released to the community upon case disposition.
- Jurisdictions can make data-guided changes to local pretrial case processing that would achieve their desired public safety and court appearance results while reserving more jail beds for unmanageably high risk defendants and sentenced offenders.
- Judicial officers now have data and law to support changing their bail setting practices to maintain their effectiveness while increasing their efficiency.

This study provides empirical evidence about the effectiveness of secured and unsecured bonds. Findings support judicial officers changing their practices to use more unsecured releases, to include unsecured bonds if currently permitted by law, to achieve the same public safety and court appearance rates while using far fewer jail beds. These unsecured bonds could be used in conjunction with an individualized bond setting hearing.

## INTRODUCTION

Multiple criminal justice and government decision-makers have a role in the decision to release or detain defendants on pretrial status, either at the policy level or on a case-by case basis. Jail administrators are commonly granted authority by the court to release many defendants on their own recognizance or through the use of a money bond schedule, and those administrators are responsible for housing defendants who are not released. Pretrial services staff members perform risk assessment and information gathering, and provide the results and any release-condition recommendations to the court. Prosecutors and defense attorneys at pretrial hearings often request certain release conditions, including substance testing, electronic monitoring, or changes to a previously set monetary bond amount, based on their perception of the defendant's pretrial risk to court appearance or public safety. Judges make the final decisions about the types of bond and conditions of bond, including financial and non-financial release conditions. County commissioners or state legislators fund the staff and court and jail facilities that comprise the pretrial system and/or pass laws, but often do so with little or no evaluative feedback about the system's effectiveness or efficiency.

Whether in the role of making daily, case-by-case pretrial release or detention decisions or policy-level funding decisions, many of these criminal justice decision-makers have had to do so without scientific evidence to help guide their decisions. As a result, they may assume that the current pretrial justice process meets their standards for effectiveness and efficiency, and that the money bail system motivates defendants to return to court or to refrain from criminal activity upon release from jail pending the disposition of their case.

Researchers have recently attempted to determine to what extent, if any, secured monetary forms of pretrial release (e.g., surety or cash bonds) improve court appearance and public safety over non-monetary or unsecured forms of pretrial release (e.g., recognizance bonds). Unfortunately, for the reasons that Cohen and Kyckelhahn (2010) and Bechtel, Clark, Jones, and Levin (2012) have recently explained, researchers have not had access to data that has allowed them to determine simultaneously the effect of different bond types on the three most important pretrial outcomes: (1) public safety; (2) court appearance; and (3) pretrial release and jail bed use. To summarize, previous research has either: (a) had data or methodological limitations that limit the generalizability of the findings to other jurisdictions (see, for example, Morris, 2013; Krahl & New Direction Strategies, 2011); (b) has not sufficiently accounted for possible alternate explanations of the findings (see, for example, Block, 2005); and/or (c) was limited to measuring the effect of various forms of pretrial release on a singular outcome - court appearance, but not on both of the other two important pretrial outcomes - public safety and jail bed use (see, for example, Helland & Tabarrok, 2004; Morris, 2013). Indeed, as Bechtel et al. (2012) explain, the optimal outcome for any pretrial justice system from both an effectiveness (justice system goals) and efficiency (resource management) perspective is to:

(1) Maximize public safety
and
(2) Maximize court appearance
while
(3) Maximizing release from custody.

Achieving only one or two of these pretrial outcomes without or at the expense of realizing the remainder would be less optimal than achieving all three simultaneously. Indeed, Osborne and Hutchinson (2004) make a compelling case for governments to maximize results while expending the minimal public resources to achieve those results.

The purpose of this study is to overcome some of the limitations of previous research and provide information to pretrial release decision-makers and criminal justice funding decision-makers that will enable them to accomplish a win-win situation: to achieve their desired public safety and court appearance outcomes while most efficiently using their costly jail resources. Because the study uses data from multiple Colorado counties, the results are generalizable throughout Colorado. Factors that may affect the extent to which the results are generalizable outside of Colorado are addressed later in the paper.

Furthermore, due to Colorado statute's requirement of financial conditions of release, this study is an evaluation of the effect of different types of monetary bonds on public safety, court appearance, and jail bed use. As described in more detail later, some of these monetary bonds in Colorado require the defendant to post the entire monetary amount in cash or some portion thereof through a commercial bail bondsman prior to leaving jail custody, whereas other monetary bonds do not require any money to be posted prior to release.[1]

After each statistical analysis, a brief explanation of the meaning of the findings is provided. Practical implications of this study for pretrial release decision-making and policy-making are discussed in the final section.

1 This study does not evaluate the effectiveness of commercial bail bonding in achieving court appearance results, nor does it evaluate the effectiveness of pretrial services program supervision in achieving certain court appearance or public safety results. Rather, the focus is on outcomes associated with various forms of monetary bonds set by the court.

# METHOD

Data for this study came from the dataset used to develop Colorado's 12-item empirically-derived pretrial risk assessment instrument, the Colorado Pretrial Assessment Tool (CPAT; Pretrial Justice Institute & JFA Institute, 2012). The dataset has hundreds of case processing and outcome variables collected on 1,970 defendants booked into 10 Colorado county jails over a 16-month period.[2] Each local jurisdiction collected data on a pre-determined, "systematic random sampling" selection schedule to minimize bias in selecting defendants and to enhance the generalizability of the findings. For example, each jurisdiction collected data at an interval of every 2nd, 4th, or 7th defendant who was booked into the jail on new charges. Over 80% of the state's population resides in the 10 counties that participated: Adams, Arapahoe, Boulder, Denver, Douglas, El Paso, Jefferson, Larimer, Mesa, and Weld.

**DEFENDANTS WERE ASSESSED FOR THEIR PRETRIAL RISK, AND NEARLY 70% SCORED IN THE LOWER TWO OF FOUR RISK CATEGORIES.**

Based on the CPAT's scoring procedures, 1,970 defendants in the dataset were assigned a CPAT risk score, ranging from 0 (lower risk) to 82 (higher risk), and to a corresponding risk category, ranging from 1 (lower risk) to 4 (higher risk). Some relevant data were missing for 51 defendants, so they were removed from all analyses. Thus, the final sample used in the analyses was 1,919 defendants, with 1,309 (68%) of them having been released on pretrial status prior to case disposition. Table 1 shows the percentage of released defendants and the public safety and court appearance success rates associated with each risk category.

**Table 1. Average Risk Score, Percent and Number of Defendants, and Public Safety and Court Appearance Rates by Released Defendants' Risk Category**

| CPAT PRETRIAL RISK CATEGORY | CPAT RISK SCORE RANGE | AVERAGE CPAT RISK SCORE | PERCENT (AND NUMBER) OF DEFENDANTS | PUBLIC SAFETY RATE[a] | COURT APPEARANCE RATE[b] |
|---|---|---|---|---|---|
| 1 (lower) | 0 to 17 | 8 | 20% (265) | 92% (243/265) | 95% (252/265) |
| 2 | 18 to 37 | 28 | 49% (642) | 81% (517/642) | 86% (549/642) |
| 3 | 38 to 50 | 44 | 23% (295) | 70% (205/295) | 78% (231/295) |
| 4 (higher) | 51 to 82 | 57 | 8% (107) | 59% (63/107) | 51% (55/107) |
| Average/Total | 0 to 82 | 30 | 100% (1,309) | 79% (1,028/1,309) | 83% (1,087/1,309) |

a. On the CPAT and for this study, the public safety rate is defined as the percentage of defendants who did not have a prosecutorial filing in court for any new felony, misdemeanor, traffic, municipal, or petty offense that allegedly occurred during the pretrial release time period. Thus, public safety is defined very broadly as any new filing and is not limited to physical harm against a person or to felony or misdemeanor charges.
b. The court appearance rate is defined as the percentage of defendants who attended all of their court hearings during their pretrial release (i.e., they did not have any notations of failure to appear indicated in the Colorado Judicial Branch's statewide database).

2 Risk assessment data were collected over the 16-month period from February 2008 to May 2009, and pretrial outcome data were collected after cases closed up until December 2010, thus allowing at least 19 months for all cases to close after defendants were booked into jail because of new charges. Ninety-nine percent (99%) of the cases closed within the minimum 19-month time period.

## Summary of Findings

The CPAT effectively sorts defendants into one of four risk categories, with each category having different rates for the desired outcomes of public safety and court appearance. Nearly 70% of defendants scored in the lower two risk categories. These risk categories can be used when examining the impact of different forms of money bonds on public safety, court appearance, and jail bed use.

**DEFENDANTS RECEIVED EITHER UNSECURED OR SECURED BONDS, AND WERE SEPARATED INTO FOUR GROUPS TO ENABLE ANALYSIS OF BOND-TYPE COMPARISONS.**

Table 2 shows the percentage of released defendants who received unsecured or secured (surety or cash) money bonds within each of the four risk categories. Statutorily, all bonds in Colorado must have a financial condition.[3]

**Table 2: Percent and Number of Released Defendants by Bond Type and Risk Category**

| PRETRIAL RISK CATEGORY | BOND TYPE | |
|---|---|---|
| | UNSECURED[a] | SECURED[b] |
| 1 (lower) | 52% (137/265) | 48% (128/265) |
| 2 | 32% (208/642) | 68% (434/642) |
| 3 | 15% (45/295) | 85% (250/295) |
| 4 (higher) | 13% (14/107) | 87% (93/107) |
| Average | 31% (404/1,309) | 69% (905/1,309) |

a. Unsecured bonds do not require defendants to post money prior to their pretrial release from jail. While Colorado law uses the term "personal recognizance," the term "unsecured" is used in this paper to distinguish these bonds from "pure" personal recognizance bonds (or "own recognizance" bonds), as they are called in many other states. Financial conditions are rarely allowed or used with "pure" or "own" recognizance bonds.

b. Secured bonds require defendants to post some amount of money prior to their pretrial release from jail.[4]

3 Unsecured bonds in Colorado are known in statute as personal recognizance bonds and although they are required to have a financial condition in some monetary amount, they do not require the defendant to post any money with the court prior to pretrial release from jail. If the defendant fails to appear, the court can hold the defendant liable for the full amount of the bond. The court can also require the signature of a co-signor on unsecured bonds prior to the defendant's release from jail. The co-signor is typically a family member who promises the court that he or she will assist the defendant in appearing in court and who may be held liable for the full monetary amount if the defendant fails to appear. In this study, as noted above, these personal recognizance bonds are called "unsecured" bonds because they have a financial condition for which the defendant or co-signor could be fully liable. The unsecured bond group is for the most part a "defendant-only (with no co-signor) unsecured" group because 344 (85%) of the 404 unsecured bonds did not require a co-signor.

4 Secured bonds in Colorado require money to be posted with the court on the defendant's behalf prior to pretrial release, and can be in the form of cash, surety, or property. If the defendant fails to appear, the court can hold the defendant or a commercial bail bondsman (for a surety bond) liable for the full amount of the bond. The secured bond group is for the most part a "surety bond" group because 849 (94%) of the 905 secured bond defendants posted a surety bond rather than a cash bond. Surety bonds were the most prevalent form of bond set by the court during the time this study's data were collected. Property bonds are very rarely used in Colorado, and were not used for any of the defendants in this study.

## Summary of Findings

Data show that judicial officers set both unsecured and secured bonds for defendants in each of the four risk groups. All of these bonds carry the possibility that the court could hold the defendant or other party (i.e., co-signor or bail bondsman) legally liable for the bond's full monetary amount if the defendant fails to appear in court. For surety bonds, defendants are still liable for the full monetary amount, albeit indirectly. If a defendant released on surety bond fails to appear, the court, within the confines of statute, may hold the bail bondsman liable for the full monetary amount. If so, then the bail bondsman may offset this expense by collecting the full monetary amount of the bond pursuant to the contract with the defendant or the defendant's family member or friend, and turn over the full bond amount to the court.

Placing defendants into one of four risk categories stratifies defendants based on their overall level of risk, thus helping increase the chances that defendants' bond type, rather than their degree of pretrial risk, accounts for the observed results. Specifically, the stratification was done because in the total sample there was a relatively higher proportion of lower risk defendants in the unsecured bond group and a relatively higher proportion of higher risk defendants in the secured bond group. This pattern of data is found across most criminal justice systems nationwide. In addition, the total sample size of defendants in this study and in the four separate risk groups is large enough to detect statistical differences between the two bond-type groups if differences indeed do exist (see Cohen, 1988).[5]

Moreover, the Colorado jurisdictions that have already implemented the CPAT or that will be implementing it in the near future use the CPAT's four-category risk scheme to guide daily pretrial release and detention decision-making, so using the CPAT's risk scheme in this study enables the study to provide decision-makers with findings that directly inform their daily practice.

5 The social science conventional standard of 0.05 for statistical significance testing was used throughout this study. Statistical significance at the 0.05 level means that we can be at least 95% confident that the observed results are not due to chance. To statistically determine that defendants with unsecured bonds were similar in pretrial risk to defendants with secured bonds, stratification, or the separation of the defendants into incremental groups, was done. Separate t-tests (tests used to determine if two groups have different averages on a measure) were performed on the four pretrial risk groups. These analyses showed that the average risk score for defendants with unsecured bonds was not statistically significantly different than the average risk score for defendants with secured bonds in risk categories 1, 3, and 4 (all $p > 0.19$). For risk category 2, the average score for defendants with unsecured bonds (27) was two points less than the average score for defendants with secured bonds (29) ($p < .001$). However, given that there was no significant difference for the other three risk categories, including the categories both below (i.e., category 1) and above (i.e., categories 3 and 4) category 2, and because the two-point score difference was no larger than the non-significant score difference in the other three risk categories, the statistically significant difference observed in category 2 is determined not to be practically significant. That is, the difference is likely not meaningful enough to be useful for purposes of informing practice. Additionally, there were no significant differences in the percentages of defendants who were ordered to pretrial supervision among the four risk groups (ranging from 48% to 50% for each of the four groups), indicating that pretrial supervision likely did not interfere with the effects of bond type on the outcome measures.

## GOALS OF THE STUDY

This study evaluates the extent to which, if at all, one type of money bond (unsecured) is associated with better pretrial outcomes than is the other type of money bond (secured, in the form of cash or surety) while also accounting for jail bed use. Because all bonds in Colorado have a monetary condition, this study was not able to test whether bonds with no financial condition could have achieved the same public safety or court appearance outcomes as did bonds with a financial condition.

For the following analyses, defendants were sorted into two groups depending on the type of money bond they received – unsecured or secured. Defendants' performance on the three pretrial outcomes most important to pretrial decision-makers - public safety, court appearance, and jail bed use - was examined. Defendants in the two bond-type groups were compared separately within each of the four pretrial risk categories to mitigate the influence of defendants' risk levels on the observed outcomes.

# RESULTS

**UNSECURED BONDS ARE AS EFFECTIVE AS SECURED BONDS AT ACHIEVING PUBLIC SAFETY.**

Table 3 shows the percentage of defendants who were not charged with a new crime during pretrial release (i.e., the public safety rate) for the unsecured and secured bond groups in each of the four risk categories.

**Table 3: Public Safety Outcomes by Bond Type and Risk Category**

| PRETRIAL RISK CATEGORY | PUBLIC SAFETY RATE | |
|---|---|---|
| | UNSECURED BOND | SECURED BOND |
| 1 (lower)+ | 93% (128/137) | 90% (115/128) |
| 2+ | 84% (174/208) | 79% (343/434) |
| 3+ | 69% (31/45) | 70% (174/250) |
| 4 (higher)+ | 64% (9/14)* | 58% (54/93) |
| Average** | 85% (342/404) | 76% (686/905) |

+ All statistical comparisons showed no statistically significant differences. All $p > 0.16$.
* The 64% observed in this cell is based on a small sample size (n=14) and thus should be interpreted with caution. For example, if one more defendant in the unsecured bond group had no new charges, the percentage would increase to 71%. If one more of these defendants had a new charge, the percentage would decrease to 57%.
** The public safety rate for all unsecured bond defendants was not compared to the rate for all secured bond defendants because that analysis would fail to control for defendants' degree of pretrial risk.

Chi-square tests[6] revealed that there were no statistically significant differences in defendants' public safety outcomes for the two different types of bond in each of the four risk categories. This finding also holds when only person crimes are analyzed. That is, defendants from both bond-type groups did not significantly differ from one another in their rate of receiving new charges for alleged crimes against a person while on pretrial release ($p > 0.65$).

### Summary of Findings

Whether released defendants are higher or lower risk or in-between, unsecured bonds offer the same public safety benefit as do secured bonds. This finding is expected because although defendants can have their bond revoked if they receive a new charge while on pretrial release, they legally cannot be ordered to forfeit any amount of money or property under any bond type. Thus, the financial condition of an unsecured or secured bond cannot legally have an impact on defendants' criminal behavior. This study's failure to find a public safety benefit for one bond type over another is consistent with previous research (Helland & Tabarrok, 2004; Morris, 2013).

6 The Chi-square statistic tests the degree of agreement between observed data and the data expected under a certain hypothesis. It can be used to compare the differences in frequencies on a measure between two groups.

**UNSECURED BONDS ARE AS EFFECTIVE AS SECURED BONDS AT ACHIEVING COURT APPEARANCE.**

Table 4 shows the percentage of defendants who made all of their court appearances during pretrial release (i.e., the court appearance rate) for the unsecured and secured bond groups in each of the four risk categories.

**Table 4: Court Appearance Outcomes by Bond Type and Risk Category**

| PRETRIAL RISK CATEGORY | COURT APPEARANCE RATE | |
|---|---|---|
| | UNSECURED BOND | SECURED BOND |
| 1 (lower)+ | 97% (133/137) | 93% (119/128) |
| 2+ | 87% (181/208) | 85% (368/434) |
| 3+ | 80% (36/45) | 78% (195/250) |
| 4 (higher)+ | 43% (6/14)* | 53% (49/93) |
| Average** | 88% (356/404) | 81% (731/905) |

+ All statistical comparisons showed no statistically significant differences. All $p > 0.12$.

* The 43% observed in this cell is based on a small sample size (n=14) and thus should be interpreted with caution. For example, if one more defendant in the unsecured bond group made all court appearances, the percentage would increase to 50%. If one more of these defendants had a failure to appear, the percentage would decrease to 36%.

** The court appearance rate for all unsecured bond defendants was not compared to the rate for all secured bond defendants because that analysis would fail to control for defendants' risk.

Chi-square tests revealed that there were no statistically significant differences in defendants' court appearance outcomes for the two different types of bond in each of the four risk categories.

## Summary of Findings

Whether released defendants are higher or lower risk or in-between, unsecured bonds offer decision-makers the same likelihood of court appearance as do secured bonds. The lack of benefit from using one financial bond type versus another is not surprising given that both bond types carry the potential for the defendant to lose money for failing to appear.

**UNSECURED BONDS FREE UP MORE JAIL BEDS THAN DO SECURED BONDS BECAUSE MORE DEFENDANTS WITH UNSECURED BONDS POST THEIR BONDS.**

Table 5 shows the percentage of defendants who were released from jail on pretrial status for the unsecured and secured bond groups in each of the four risk categories.[7]

**Table 5: Pretrial Release Rates by Bond Type and Risk Category**

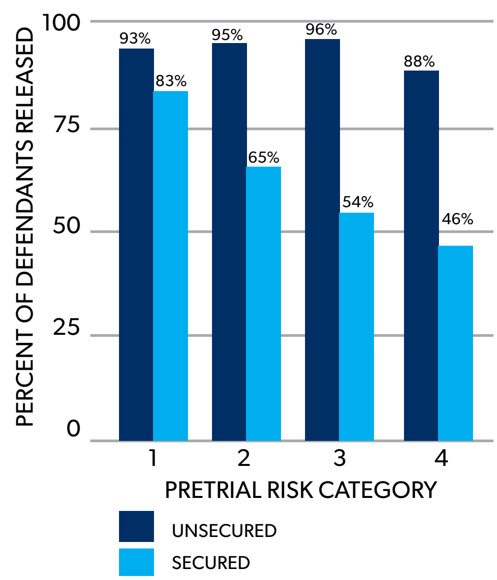
| PRETRIAL RISK CATEGORY | RELEASE RATE+ | |
|---|---|---|
| | UNSECURED BOND | SECURED BOND |
| 1 (lower)+ | 93% (137/147) | 83% (128/155) |
| 2+ | 95% (208/220) | 65% (434/669) |
| 3+ | 96% (45/47) | 54% (250/464) |
| 4 (higher)+ | 88% (14/16)* | 46% (93/201) |
| Average** | 94% (404/430) | 61% (905/1,489) |

+ All statistical comparisons were statistically significant. All $p < 0.006$.
* The 88% observed in this cell is based on a small sample size (n=16) and thus should be interpreted with caution. For example, if one more defendant in the unsecured bond group were released, the percentage would increase to 94%. If one more of these defendants were not released, the percentage would decrease to 81%.
** The release rate for all unsecured bond defendants was not compared to the rate for all secured bond defendants because that analysis would fail to control for defendants' risk.

Chi-square tests revealed that the release rates for unsecured bond defendants were statistically significantly higher than the release rates for secured bond defendants for all four of the pretrial risk categories.

The findings shown in Table 5 are illustrated in Figure 1.

**Figure 1: Pretrial Release Rates by Bond Type and Risk Category**




7 The number of defendants who post their bonds and the time to post those bonds, as opposed to the number of defendants released on pretrial status and their time to release, are better measures for more accurately determining pretrial jail bed use because once a bond is posted, the defendant is no longer utilizing a jail bed for pretrial reasons. The defendant may or may not remain in jail after bond-posting because of other cases or holds. However, for this study, like in most pretrial research, data on dates that bonds were posted were not available, so the next best measures for determining pretrial jail bed use - release on pretrial status and time to pretrial release - were used.

Both Table 5 and Figure 1 show that judicial officers used both unsecured and secured bonds with defendants of all risk levels - higher risk, lower risk, and those in between. For defendants at all risk levels, defendants with an unsecured bond were statistically significantly more likely to be released than defendants with a secured bond.[8]

### Summary of Findings

Whether released defendants are higher or lower risk or in-between, unsecured bonds enable more defendants to be released from jail than do secured bonds. Findings show that many defendants of all risk levels never post their secured bond. This finding is expected because defendants who receive unsecured bonds, or their family or friends, do not have to pay some monetary amount to the court or a commercial bail bondsman prior to the defendants' release from jail custody. Secured bonds, however, do require pre-release payment. Consequently, secured bonds used more jail beds. This finding is consistent with previous research using data from across the United States that shows that secured bond defendants are much more likely to be detained for their entire pretrial period than are unsecured bond defendants (Cohen & Reaves, 2007).

**THE MONETARY AMOUNT OF SECURED BONDS AFFECTED PRETRIAL RELEASE RATES BUT NOT COURT APPEARANCE RATES.**

Table 6 shows the percentage of defendants who were released from jail on secured bonds of select monetary amounts.

**Table 6: Pretrial Release Rates by Secured Bond Amount**

| SECURED MONETARY BOND AMOUNT | PERCENT (AND NUMBER) OF RELEASED DEFENDANTS |
|---|---|
| \$500 (12th Percentile) | 64% (52/81) |
| \$5,000 (65th Percentile) | 58% (100/191) |
| \$50,000 (97th Percentile) | 49% (37/76) |

Frequency analyses revealed that when the secured bond amount was set relatively very low at \$500 (12th percentile of secured bond amounts set by Colorado judicial officers in this study), 64% of defendants were released. When the secured bond amount was set at \$5,000 (65th percentile of secured bond amounts), 58% of defendants were released. When the secured bond amount was set at \$50,000 (97th percentile of secured bond amounts), 49% of defendants were released. However, correlational analyses revealed that the monetary amount of posted secured bonds was not statistically significantly related to court appearance for any of the four risk groups ($p > 0.09$).

8 It is possible that the lower release rate for secured bond defendants could have been in part associated with judicial officers having accounted for an unmeasured risk factor in these defendants, and thus the public safety and court appearance rates would have been lower for these defendants had they been released. The mechanism for achieving this increase in pretrial detention would have been judicial officers setting secured bonds in a monetary amount the defendant could not post. Several judicial officers have told this author that this practice is not uncommon in Colorado, but have acknowledged its questionable lawfulness given Colorado's constitutional and statutory law. Nonetheless, as indicated by this study's analyses, if more secured bond defendants had been released, the secured bonds would likely not have associated with increased public safety or court appearance.

### Summary of Findings

As the monetary amount of secured bonds increases, fewer defendants post their bonds. However, regardless of whether defendants are higher or lower risk or in-between, higher bond amounts are not associated with better court appearance outcomes for released defendants. Thus, higher secured bond amounts are associated with more pretrial incarceration but not more court appearances. The finding of increased incarceration associated with secured bonds is consistent with previous research using data from across the United States: As the monetary amount of secured bonds increases, the probability of release decreases (Cohen & Reaves, 2007).

**UNSECURED BONDS ALSO FREE UP MORE JAIL BEDS THAN DO SECURED BONDS BECAUSE DEFENDANTS WITH UNSECURED BONDS HAVE FASTER RELEASE TIMES.**

Table 7 shows the cumulative percent of defendants who were released on pretrial status for the unsecured and secured bond groups by the amount of time in jail that elapsed prior to pretrial release.

**Table 7: Time to Pretrial Release by Bond Type**

| DAYS TO PRETRIAL RELEASE* | CUMULATIVE PERCENT OF DEFENDANTS RELEASED ON UNSECURED BONDS | CUMULATIVE PERCENT OF DEFENDANTS RELEASED ON SECURED BONDS |
|---|---|---|
| <1 to 1.9⁺ | 80% (325/404) | 58% (525/905) |
| 2 to 2.9⁺ | 83% (336/404) | 68% (611/905) |
| 3 to 3.9⁺ | 85% (344/404) | 73% (663/905) |
| 4 to 4.9⁺ | 86% (348/404) | 77% (699/905) |
| 5 to 5.9⁺ | 87% (351/404) | 80% (721/905) |
| 6 to 6.9⁺ | 88% (356/404) | 81% (731/905) |
| 7 to 7.9⁺ | 88% (356/404) | 82% (741/905) |
| 8 to 8.9⁺ | 89% (358/404) | 84% (758/905) |
| 9 to 9.9⁺ | 89% (360/404) | 85% (768/905) |
| 10 to 10.9** | 89% (360/404) | 86% (774/905) |
| 11 to 11.9** | 89% (361/404) | 86% (781/905) |
| 12 to 12.9** | 90% (362/404) | 87% (784/905) |

⁺ All statistical comparisons were statistically significant. All $p < 0.05$.

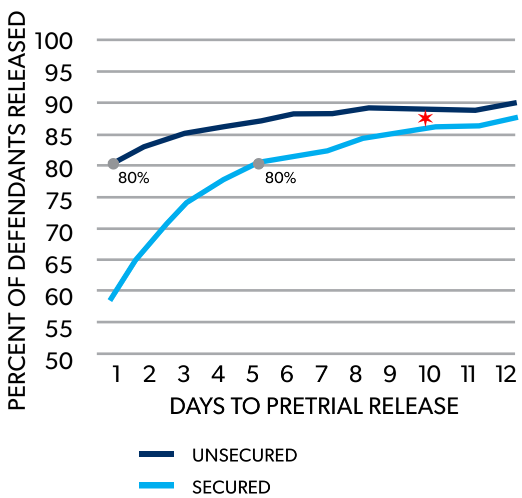
* Defendants across all risk categories were grouped together for this analysis because a defendant's pretrial risk level can have no legal bearing on the amount of time a defendant remains in pretrial incarceration after a judicial officer sets the bond. In contrast, the monetary amount of a secured bond, holds from other jurisdictions, or requirements from a defendant's other cases can affect whether and when the defendant can be released from jail even if the defendant has posted his bond, regardless of bond type and regardless of his pretrial risk level.

** Beginning on the tenth day of pretrial incarceration, the percent of defendants in the two bond type groups who had not been released on pretrial status was no longer statistically significantly different ($p > 0.07$). Because there was no significant difference after day 9, it was assumed for the purposes of this analysis that after day 9 other factors, such as the defendants' other cases or possible holds, contributed to defendants' continued pretrial incarceration to the degree that the bond type was no longer the primary factor contributing to continued pretrial incarceration. In addition, a t-test revealed that the average time to pretrial release for the unsecured bond group (0.7 days) was statistically significantly lower than that for the secured bond group (1.5 days) when the analysis of pretrial incarceration was capped at 9 days for the reasons described above ($p < 0.0001$). The 9-day cap also makes it likely that the 1.5-day average for the secured bond defendants is an underestimate because 10 or more days may actually elapse before a defendant or his family can meet the court's cash bond or bondsman's surety bond requirements; however, this cap was derived from the best data available for this study. Moreover, the use of this average for the secured bond defendants is still sufficient for statistically demonstrating the increased jail use that results from secured bonds, and is sufficient for demonstrating practical significance for policy-making.

Chi-square tests revealed that statistically significantly more defendants with unsecured bonds were released on pretrial status than were defendants with secured bonds for each of the first nine days after defendants' bonds were set. A t-test revealed that the average number of days spent in jail on pretrial status was statistically significantly less for defendants with unsecured bonds than the average for defendants with secured bonds up to the first nine days after defendants' bonds were set.

The findings shown in Table 7 are illustrated in Figure 2.

**Figure 2: Time to Pretrial Release by Bond Type**




**Note. The ✶ symbol denotes that after day 9, the difference in the percent of released defendants between the two groups was no longer statistically significant. The time at which the 80% threshold was achieved is indicated for both groups.**

Figure 2 depicts that released defendants with unsecured bonds spent fewer days incarcerated on pretrial status than did defendants with secured bonds. Moreover, Figure 2 depicts:

- Five days of jail incarceration were required for defendants with cash or surety bonds to achieve the same release threshold of 80% that defendants with unsecured bonds experienced by day one.
- Ten days of jail incarceration were required for defendants with cash or surety bonds to achieve the same overall release threshold as defendants with unsecured bonds because there were statistically significant differences for the first nine days.

## Summary of Findings

After judicial officers set defendants' bonds, unsecured bonds enable defendants to be released from jail more quickly than do secured bonds. This finding is expected because nearly all defendants who receive unsecured bonds can be released from custody immediately upon signing their bond, whereas defendants with secured bonds must wait in custody until they or a family member or friend negotiates a payment contract with a commercial bail bondsman or their family member or friend posts the full monetary amount of a cash bond at the jail. This finding indicates that the process of posting a secured bond takes much longer than the process of posting a unsecured bond for released defendants. Furthermore, this finding is consistent with previous research using data from across the United States that shows released defendants with secured bonds remained in jail longer than did released defendants with bonds that did not require a prerelease payment (Cohen & Reaves, 2007).

**UNSECURED BONDS ARE AS EFFECTIVE AS SECURED BONDS AT "FUGITIVE-RETURN" FOR DEFENDANTS WHO HAVE FAILED TO APPEAR.**

Table 8 shows the percent of defendants whose case was still open up to 19 months after they were released from jail and who were at-large because of a failure to appear warrant, among all released defendants who had failed to appear (i.e., the at-large rate), for the unsecured and secured bond groups.

**Table 8: At-Large Rate by Bond Type**

| AT-LARGE RATE+* | |
|---|---|
| UNSECURED BOND | SECURED BOND |
| 10% (5/48) | 9% (15/174) |

+ The comparison was not statistically significantly different ($p > 0.69$). Non-significance was also found when data from just the surety bond defendants were compared to the unsecured bond defendants - that is, when the cash-only bond defendants were removed from the secured bond group ($p > 0.48$).
* There were too few at-large cases in each of the four risk categories to permit analyses within each of the risk categories.

Chi-square tests revealed that there were no statistically significant differences in defendants' at-large rates for the two different types of bond, as well as for surety-bond-only defendants.

## Summary of Findings

When released defendants fail to appear, unsecured bonds offer the same probability of fugitive-return as do secured (including surety-only) bonds. Because the commercial bail bond industry often claims that it locates and captures defendants who have failed to appear or who are fugitives on the run (see Professional Bail Agents of the United States, 2013; Tabarrok, 2011), this topic is discussed in detail.

Nationally, the fugitive-return function has received minimal attention in the empirical research literature, and no empirical research prior to the current study has been done in Colorado. This study failed to find support for the commercial bail bond industry's fugitive-return claim for defendants released on surety bonds because there was no difference in the percent of defendants who were released on surety bonds, who failed to appear, and who still had an open case, when compared to the percent of defendants who were released on unsecured bonds, who failed to appear, and who still had an open case. All defendants who had an open case at the time this study's data collection was completed were at-large on a failure to appear warrant and not in jail custody. If commercial bail bondsmen or hired bounty hunters return defendants at a greater rate than the rate for which defendants on unsecured bonds return to custody or court, then the percent of at-large surety bond defendants would be statistically significantly less than it is for unsecured bond defendants. That difference was not found in this study.

This study's failure to find a fugitive-return benefit for one bond type over another is consistent with previous research designed to measure directly the fugitive-return function allegedly associated with surety bonds. Jones, Brooker, and Schnacke (2009) found no empirical support for Colorado commercial bail bondsmen's claim that they locate or apprehend surety bond defendants who had failed to appear, as indicated by local jail booking data, the court's bondsman-contact tracking logs, and by law enforcement officials' report (p. 83).

Furthermore, in 2012 a committee that consisted of several justice system stakeholders and Colorado bail agents' representatives studied Colorado pretrial case processing and decision-making for a year. A portion of that review included discussion about fugitive-return evidence in Colorado.

Committee members acknowledged that there are no data to support the bondsmen's fugitive-return claim, and that the extent to which bondsmen return defendants to jail, court, or to law enforcement officers in Colorado remains empirically undemonstrated.[9]

**MANY DEFENDANTS ARE INCARCERATED FOR THE PRETRIAL DURATION OF THEIR CASE AND THEN RELEASED TO THE COMMUNITY UPON CASE DISPOSITION.**

Because some judicial officers, sheriffs, and defense attorneys have expressed concern or puzzlement to this author about their observation that apparently many defendants spend the pretrial duration of their case in custody, sometimes for several weeks or months, and then are released to the community upon conviction or sentencing, data on case dispositions were analyzed to determine the extent to which this phenomenon occurs in Colorado.

Table 9 shows the collective percentage of never-released, secured-bond defendants by type of case disposition from all 10 Colorado jurisdictions.

**Table 9: Never-Released Defendants by Case Disposition**

| CASE DISPOSITION | PERCENT (AND NUMBER) OF DEFENDANTS OR OFFENDERS* |
|---|---|
| Department of Corrections | 14% (79) |
| Jail, Work Release, or Time Served in the Local Jail | 34% (194) |
| Community-Based Option (Diversion, Probation, Community Corrections, Home Detention) | 37% (210) |
| Dismissed or Not Filed | 13% (76) |
| Still Open or Had Some Other Sentence | 2% (9) |
| Total | 100% (568) |

* Each percentage changes 1% or less when unreleased defendants with recognizance bonds were included in the analysis.

9 See the Colorado Commission on Criminal and Juvenile Justice's Bail Subcommittee's March 2012 Meeting Minutes at http://www.colorado.gov/cs/Satellite/CDPS-CCJJ/CBON/1251617151523.

## Summary of Findings

These findings have implications for pretrial jail bed use because 50% (37% + 13%) of defendants return to the community upon conviction or case closure.[10] This percentage increases to 84% (50% + 34%) when defendants who return to the community after completing a jail sentence (including those who received sentences for time served while in pretrial custody) are included. This pattern of findings suggests that when judges and other decision-makers consider the likelihood of a defendant's conviction and the most likely type of sentence, they can further reduce pretrial jail bed use by using more unsecured bonds in lieu of secured bonds for defendants who will likely return to the community upon case disposition (i.e., for those defendants who are not likely to be transported to the Department of Corrections to start a sentence).

10 With the exception of some defendants for whom another case results in continued detention.

## DISCUSSION AND IMPLICATIONS FOR POLICY MAKING

The findings from this study provide strong evidence that the type of monetary bond posted does not affect public safety or defendants' court appearance, but does have a substantial effect on jail bed use. Specifically, when posted, unsecured bonds (personal recognizance bonds with a financial condition) achieve the same public safety and court appearance results as do secured (cash and surety) bonds. This finding holds for defendants who are lower, moderate, or higher risk for pretrial misconduct. However, unsecured bonds achieve these public safety and court appearance outcomes while using substantially (and statistically significantly) fewer jail resources. That is, more unsecured bond defendants are released than are secured bond defendants, and unsecured bond defendants have faster release times than do secured bond defendants. The amount of the secured monetary bond was associated with increased pretrial jail use but not increased court appearance. Finally, the type of monetary bond did not affect the fugitive-return rate as measured by the percent of cases with a failure to appear warrant remaining open up to one-and-a-half years later.

**THE TYPE OF BOND SET BY THE COURT HAS A DIRECT IMPACT ON THE AMOUNT OF JAIL BEDS CONSUMED, BUT IT DOES NOT IMPACT PUBLIC SAFETY AND COURT APPEARANCE RESULTS.**

A three-jurisdiction example demonstrates this study's implications for jail bed use. If there were three jurisdictions that use different rates of unsecured and secured bonds, they each would use their local jail resource very differently to achieve the same public safety and court appearance outcomes.[11] Table 10 demonstrates this scenario.

**Table 10: Differential Jail Bed Use Resulting from Different Bond Setting Practices in Three Jurisdictions**

| JURISDICTION | PERCENT OF UNSECURED BONDS | PERCENT OF SECURED BONDS | PRETRIAL BEDS NEEDED FOR UNSECURED BONDS* | PRETRIAL BEDS NEEDED FOR SECURED BONDS* | TOTAL PRETRIAL BEDS NEEDED* | PUBLIC SAFETY RATE** | COURT APPEARANCE RATE** |
|---|---|---|---|---|---|---|---|
| Status Quo[a] | 31% | 69% | 34 | 430 | **464** | 79% | 83% |
| Moderate Unsecured[b] | 61% | 39% | 67 | 243 | **310** | 79% | 83% |
| High Unsecured[c] | 91% | 9% | 100 | 56 | **156** | 79% | 83% |

c. The "Status Quo" jurisdiction's use of unsecured bonds was selected to be the same as the average unsecured bond use in the 10 jurisdictions that contributed data to this study (see Table 2).
d. The "Moderate Unsecured" jurisdiction's percent of unsecured bonds was selected to be 30 percentage points higher than that of the Status Quo jurisdiction and centered between the other two jurisdictions. Its bond type percentages are nearly the inverse of the Status Quo jurisdiction.
e. The "High Unsecured" jurisdiction's percent of unsecured bonds was selected to be 30 percentage points higher than that of the Moderate Unsecured jurisdiction. It also uses nearly the same percent of unsecured bonds as there are defendants in the three lowest Colorado Pretrial Assessment Tool (CPAT) risk categories (i.e., categories 1, 2, and 3). This would approximately be the case, for example, if a jurisdiction were to use unsecured bonds for defendants whose pretrial risk score is in CPAT risk categories 1 through 3 and use secured bonds for defendants whose pretrial risk score is in CPAT risk category 4.
* Per 10,000 defendants booked into jail on new charges.
** The public safety rate of 79% and the court appearance rate of 83% were averages for all 1,309 released defendants, regardless of their bond type or risk level.

As seen in Table 10, secured bonds require more jail beds than do unsecured bonds when a relatively high number (69% or 39%) of secured bonds are used. In particular, the Status Quo jurisdiction would need 464 jail beds allocated for pretrial detention for every 10,000 defendants booked into jail on new charges, whereas the Moderate Unsecured jurisdiction would need 310 jail beds allocated for pretrial detention for this same pool of defendants.

11 The average length of time that defendants spent in detention for pretrial reasons (calculated for this study as 0.7 days for unsecured bond defendants and 1.5 days for secured bond defendants) and the average length of time of 58 days for all in-custody cases to close were used to calculate the number of beds that defendants would use. See Cunniff (2002) for the formulas used (p. 30).

The Status Quo jurisdiction's higher amount of jail bed use is caused by fewer secured bond defendants being released and when they are released, taking more time to do so when compared to unsecured bond defendants (refer back to Tables 5 and 7).

In contrast, the High Unsecured (i.e., high use of personal recognizance bonds) jurisdiction would need only 156 jail beds allocated for pretrial detention for every 10,000 defendants booked into jail on new charges. In this jurisdiction, more jail beds are actually required for unsecured bond defendants than for secured bond defendants because of the very high volume of unsecured bond defendants. However, this jurisdiction uses substantially fewer pretrial jail beds overall than do the other two jurisdictions because fewer defendants remain incarcerated, and when defendants are released, they are released much more quickly.

In summary, the High Unsecured jurisdiction achieves the same court appearance and public safety outcomes as does the Status Quo jurisdiction, but does so while reserving 197% more jail beds for other purposes (e.g., incarcerating sentenced inmates, reducing jail expenses by closing one or more housing sections). Similarly, the Moderate Unsecured jurisdiction achieves the same court appearance and public safety outcomes as does the High Unsecured jurisdiction, but consumes twice as many jail beds while doing so.

## JURISDICTIONS CAN MAKE DATA-GUIDED CHANGES TO LOCAL PRETRIAL CASE PROCESSING THAT WOULD ACHIEVE THEIR DESIRED PUBLIC SAFETY AND COURT APPEARANCE RESULTS WHILE RESERVING MORE JAIL BEDS FOR UNMANAGEABLY HIGH RISK DEFENDANTS AND SENTENCED OFFENDERS.

Criminal justice policy-makers, such as judges, sheriffs and jail administrators, district attorneys, defense attorneys, and county commissioners or city council members, in each local jurisdiction (e.g., county or city-county) could benefit from convening to discuss and analyze their current practices and to identify opportunities for improving their pretrial practices. Colorado jurisdictions use secured money bonds for over two-thirds (69%) of their cases. However, this study provides compelling evidence that the same level of public safety and court appearance that these jurisdictions experience today can be achieved at considerably lower costs to taxpayers who fund local jails, and this finding occurs for defendants of all risk levels.[12] Moreover, this study's findings provide empirical support for a Colorado jurisdiction changing its pretrial practices to be consistent with Colorado's new bail statute enacted in May of 2013.[13]

It will be important for local decision-makers to collaborate to hold each other accountable to maximize their desired public safety, court appearance, and jail bed use outcomes. Judges, sheriffs, district attorneys, and other justice system decision-makers desire to achieve the highest levels of public safety and court appearance as possible, and they rely on county commissioners and legislators to provide them with the resources (e.g., jail and court facilities, staff, programs) to make those outcomes possible. Similarly, county commissioners or legislators fund the jail and program resources, and they rely on judges and other system decision-makers to engage in effective practices that most efficiently

12 The higher financial cost to each local jail created by the use of secured bonds can be demonstrated whether short-run marginal costs and/or step-fixed costs are used in cost calculations (see Henrichson & Galgano, 2013).

13 See House Bill 13-1236 at http://www.leg.state.co.us/.

use those resources. This study indicates that Colorado jurisdictions have the opportunity to be much more effective and efficient with the pretrial use of local jails by using an empirically-based risk assessment instrument such as the Colorado Pretrial Assessment Tool and by maximally using personal recognizance bonds with a financial condition. In this decision-making scenario, defendants' risk for pretrial misconduct would be known prior to defendants' release from custody, and all released defendants would have a personal recognizance bond with a financial condition that the court could enforce if the defendant were to fail to appear.

**COLORADO JUDICIAL OFFICERS NOW HAVE DATA AND LAW TO SUPPORT CHANGING THEIR BAIL SETTING PRACTICES TO BE AS EFFECTIVE BUT MUCH MORE EFFICIENT.**

This study does not address the question of whether or when judicial officers should use monetary bonds or not use them (i.e., bonds with a financial condition or bonds with no financial condition). That is a research question beyond the scope of this study and is not currently relevant in Colorado, given that statute requires all bonds to have a financial condition. Rather, this study's results, combined with the new bail statute enacted in May of 2013, provide Colorado judicial officers with both empirical and legal justification for changing their bail setting practices to achieve their desired levels of public safety and court appearance while incarcerating only higher risk individuals and no longer incarcerating lower risk defendants who cannot pay their cash or surety bonds. The pretrial release mechanism created in Colorado's new bail statute for achieving all of these outcomes simultaneously are personal recognizance bonds with an unsecured financial condition found in Colorado Revised Statutes Sections 16-4-104(1) (a) and (b). These bonds are the only ones in Colorado that simultaneously (1) allow judicial officers to set an amount of money that they believe may give defendants sufficient incentive to return to court, *and* (2) do not prevent those defendants' release because the amount is too high for them or their family or friends to post.[14]

The new statute and this study's findings also converge to imply two features of a money bond schedule if a jurisdiction's decision-makers choose to have one: (1) The schedule should have the defendant's risk integrated into the formula that is to guide or determine a specific monetary amount of bond for each individual defendant; and (2) the scheduled monetary amounts should only be used for financial conditions associated with recognizance bonds and not for cash or surety bonds. If these two features are not incorporated and integrated into money bail bond schedules and pretrial decision-making, then the jurisdiction is likely to achieve its desired public safety and court appearance outcomes while failing to minimize pretrial detention because of the number of lower risk defendants who will be incarcerated for their lack of pre-release financial resources.

This study shows that defendants who are released from jail on personal recognizance bonds with a financial condition return to court and avoid new charges at the same rate as do defendants who bond out on cash or surety bonds, and they are as unlikely to remain at-large on fugitive status. Nonetheless, as one pretrial legal scholar has proposed (T. Schnacke, personal communication, August 1,

14 The Colorado Commission on Criminal and Juvenile Justice's Bail Subcommittee discussed the possibility that defendants are more likely to appear in court when they have "skin in the game" because of a financial condition of their bond (see http://www.colorado.gov/cs/Satellite/CDPS-CCJJ/CBON/1251617151523). Several justice system decision-makers in other states have suggested the same to this author. This study could not test this hypothesis; however, this study does provide empirical support that if defendants are more likely to appear in court because of a financial condition, this "motivation" is achieved just as effectively with a personal recognizance bond with a financial condition than it is with a cash or surety bond, but without the accompanying unnecessary pretrial jail bed use.

2013), even if the fugitive-return rate were some degree higher for surety bond defendants than for unsecured bond defendants, criminal justice decision-makers in each jurisdiction would need to decide if this gain offsets other costs. Specifically, if commercial bail bondsmen were to return defendants to custody sooner than law enforcement does, these cases could be closed more quickly. However, this benefit needs to be weighed against the high financial cost the local justice system incurs from the pretrial jail bed use that results from the large percent of surety bond defendants who are never released from jail or who take much longer to be released when they are released.

Finally, the pretrial decision-making supported by this study and the new statute has a precedent in Colorado. In early 2010 during Jefferson County's Bail Impact Study, which was a pilot project in which judges set more recognizance bonds with the support from the local criminal justice coordinating committee, a First Judicial District Court Judge set personal recognizance bonds with a financial condition for 75% of defendants who appeared before him at initial advisement. This Bail Impact Study, among initiatives in other jurisdictions and an earlier version of the research done for this paper, ultimately led to the introduction and passage of House Bill 13-1236, which rewrote Colorado's bail statute to encourage more recognizance releases and to reduce unnecessary pretrial detention while still emphasizing public safety and court appearance.[15]

**THIS STUDY'S FINDINGS ARE LIKELY MORE GENERALIZABLE TO JURISDICTIONS THAT USE BOND SETTING PRACTICES SIMILAR TO THOSE USED IN COLORADO.**

Colorado jurisdictions' pretrial case processes are very similar to one another and are typical of the processes used nationwide. When defendants are booked into jail, typically within a day or two most of them have the opportunity to leave custody after posting their bond via a money bail bond schedule or after first appearing before a judicial officer. Colorado judicial officers use unsecured, cash, and surety bonds in varying proportions, but not in a "sequential' manner as is done in some jurisdictions. For example, Dallas County's (Texas) use of non-financial release occurs almost exclusively in instances when defendants cannot first post their secured bond (L. Gamble, personal communication, March 4, 2013). In Colorado, judicial officers order unsecured bonds regardless of defendants' initial ability to post a secured bond. This non-sequential use, combined with this study's statistical controls for defendants' pretrial risk level, allow for methodologically sound bond-type comparisons on public safety, court appearance, and jail bed use.

Finally, research methods similar to those used in this study should be replicated in jurisdictions outside of Colorado to determine to what extent similar findings emerge. Criminal justice officials in many jurisdictions outside of Colorado also heavily rely on secured money bonds without any data showing the effect, pro or con, of these secured bonds on all three pretrial outcomes simultaneously. These decision-makers could likely improve the efficiency of their systems without detriment to their public safety and court appearance outcomes by using more recognizance bonds with a financial condition in lieu of cash or surety bonds.[16]

15 See C.R.S. 16-4-103(4) (c) (2013), "The Court shall . . . consider all methods of bond and conditions of release to avoid unnecessary pretrial incarceration."

16 As previously noted, the effect on court appearance of recognizance bonds that have no financial condition compared to unsecured or secured bonds could not be examined in this study. If studies show that recognizance bonds with no financial condition outperform unsecured or secured bonds, then they would provide an effective release option for jurisdictions that seek, voluntarily or through statute or court rule, to impose the least restrictive conditions that assure public safety and/or court appearance.

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## ABOUT THE AUTHOR

Dr. Michael R. Jones has been a senior project associate at the non-profit Pretrial Justice Institute (PJI) since 2010. At PJI, he has assisted dozens of states and local jurisdictions in understanding and implementing more legal and empirically-based pretrial policies and practices. In Colorado, he led the project to develop Colorado's first empirically-based pretrial risk assessment tool, coordinated pretrial services programs' statutorily mandated performance measurement, and assisted justice system decision-makers in their efforts to defeat regressive legislation and pass progressive legislation. He currently provides strategic planning, training, technical assistance, and consulting to a variety of justice system stakeholders in Colorado and nationwide. Prior to PJI, he worked for nine years as a criminal justice planner and manager in Jefferson County, Colorado, where he was lead staff for the local criminal justice coordinating committee. He has also worked as a technical resource provider for the National Institute of Corrections since 2004, providing justice system assessments and assisting local jurisdictions in developing or improving their capacity for systemic collaboration and data-guided policy-making. Mike has a Ph.D. in Clinical Psychology from the University of Missouri-Columbia.

# EXHIBIT G

UCHICAGO URBANLABS CRIME LAB

ideas42

# Using Behavioral Science to Improve Criminal Justice Outcomes

## Preventing Failures to Appear in Court

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## About ideas42

We're a leader in our field with unique expertise and experience at the forefront of behavioral science. We use this to innovate, drive social change, and improve millions of lives. We create fresh solutions to tough issues based on behavioral insights that can be scaled up for the greatest impact. ideas42 also educates leaders and helps institutions improve existing programs and policies.

Our work spans 30 countries and encompasses consumer finance, economic mobility, education, energy and the environment, health, international development, and safety and justice. As a global nonprofit organization, our partners include governments, foundations, companies, NGOs, and many other institutions.

At its core, behavioral science helps us understand human behavior and why people make the decisions they do. It teaches us that context matters, that asking the right questions is critical, and that simple solutions are often available, but frequently overlooked or dismissed. We work to identify the subtle but important contextual details that can have a disproportionate impact on outcomes.

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## About the University of Chicago Crime Lab

The U.S. has the highest rate of homicide among any developed nation in the world. The U.S. also has by far the highest rate of incarceration among any high-income nation, with over 2.2 million people currently incarcerated nationwide. Both of these problems disproportionately affect our most economically disadvantaged and socially marginalized communities.

Taken together, all levels of government in the U.S. spend well over $200 billion per year on the criminal justice system (including police, courts, and corrections). Yet we have made little long-term progress on these problems. The homicide rate in America today is about the same as it was in 1950, or even 1900. This stands in stark contrast to the enormous progress the U.S. has made toward reducing mortality rates from almost every other leading cause of death. One key reason we have not made more progress on these problems is a striking lack of rigorous evidence about what actually works, for whom, and why.

The University of Chicago Crime Lab and sister organization Crime Lab New York aim to change this by doing the most rigorous research possible in close collaboration with city government and non-profits. Using randomized controlled trials, insights from behavioral economics, and predictive analytics, the Crime Lab partners with government agencies and frontline practitioners to design and test promising ways to prevent violence and reduce the social harms of the criminal justice system, with the ultimate goal of helping the public sector deploy its resources more effectively (and humanely) to improve lives.

Building on the model of the Crime Lab, the University of Chicago launched Urban Labs in 2015 to help cities identify and test the policies and programs with the greatest potential to improve human lives at scale. Under the direction of leading social scientists, Urban Labs utilizes this approach across five labs that tackle urban challenges in the crime, education, energy & environment, health, and poverty domains.

Visit urbanlabs.uchicago.edu/labs/crime

# Executive Summary

In 2014, nearly 41% of the approximately 320,000 cases from tickets issued to people for low-level offenses in New York City (NYC) had recipients who did not appear in court or resolve their summons by mail. This represents approximately 130,000 missed court dates for these offenses. Regardless of the offense severity (summonses are issued for offenses ranging from things like littering on the street or sidewalk to drinking in public), failure to appear in court automatically results in the issuance of an arrest warrant. Because warrants are costly and burdensome for both the criminal justice system and recipients, the NYC Mayor's Office of Criminal Justice—in partnership with the New York City Police Department and New York State Unified Court System Office of Court Administration—asked ideas42 and the University of Chicago Crime Lab to design and implement inexpensive, scalable solutions to reduce the failure to appear (FTA) rate.

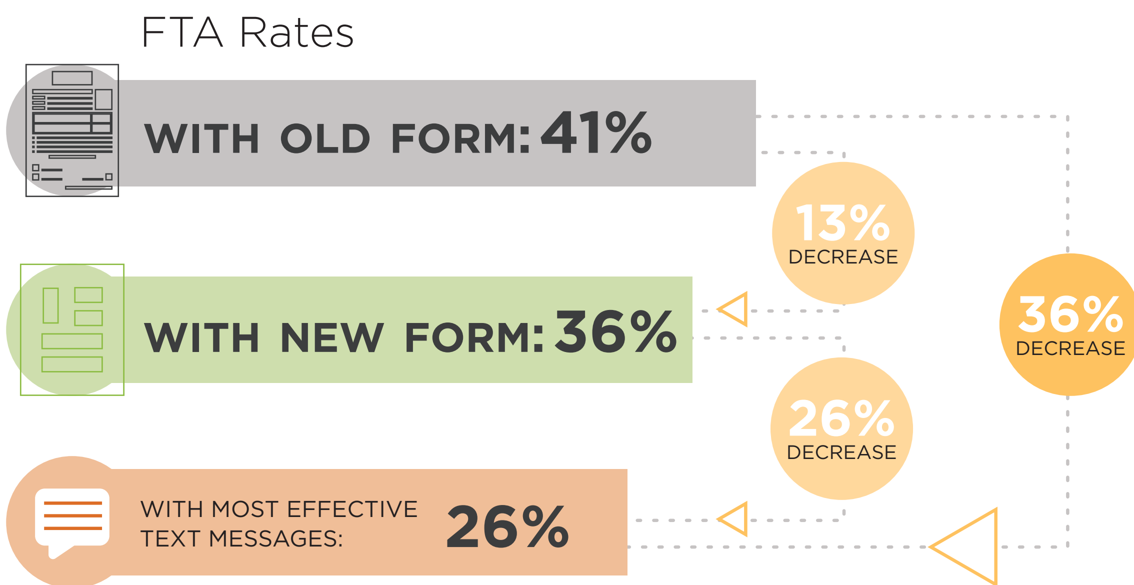
We tackled this problem using a two-sided approach. First, we redesigned the NYC summons form to make the most relevant information stand out, making it easier for people to respond appropriately. In the new form, important information about one's court date and location is moved to the top, the negative consequence of failing to act is boldly displayed, and clear language encourages recipients to show up to court or plead by mail.

Second, we created text message reminders. We identified behavioral barriers leading many to miss their court dates: people forget, they have mistaken beliefs about how often other people skip court, they see a mismatch between minor offenses and the obligation to appear in court, and they overweigh the immediate hassles of attending court and ignore the downstream consequences. We then designed different reminders targeted at helping recipients overcome these barriers.

From March 2016 to September 2017 we implemented and evaluated our interventions, and showed that both have significant and positive effects on appearance rates. **We found that behavioral redesign of the form reduced FTA by 13%.** This form has already been scaled system-wide to all criminal court summonses, and, based on 2014 figures, translates to preventing roughly 17,000 arrest warrants per year.

Using a randomized controlled trial, **we found that the most effective reminder messaging reduced FTA by 26%** relative to receiving no messages. **Looking 30 days after the court date, the most effective messaging reduced open warrants by 32% relative to receiving no messages.** This stems from both reducing FTA on the scheduled court date as well as court appearances after the FTA to clear the resulting warrant. These results are in addition to the gains already realized from the summons form redesign. The most effective messaging combined information on the consequences of not showing up to court, what to expect at court, and plan-making elements.

# Improvements in timely court appearance




Estimates for summons recipients who provide a phone number

Traditionally, criminal justice policy is informed by the assumption that people make an explicit decision to offend, and so most approaches aim to make crime less worthwhile. But our interventions are built on the view that people who miss their court date do not necessarily make an active choice to skip it. Rather, they may have failed to consider the decision at all due to a number of obstacles. The results indicate that crime policies that focus on behavioral barriers can offer humane approaches to reduce negative consequences for both citizens and the criminal justice system, without resorting to the traditional lever of increasing enforcement.

# Introduction

To bring about behavior change and crime prevention, policymakers within the criminal justice system have traditionally focused on deterrence. For example, longer prison sentences are often used to discourage crime by making crimes more costly for offenders.

However, these policies will only be effective if people carefully consider the costs and benefits of their actions. Yet a growing body of literature in the behavioral sciences suggests that people often do not think systematically about costs and benefits before acting. Instead, people often base their decisions on intuitive or automatic processes that falter in predictable ways. Fortunately, the predictability of these processes opens up additional levers for generating behavior change. For example, behavioral science has shown people will reduce their energy consumption if told how much energy they use relative to their neighbors[1] or that medical adherence can be boosted with simple reminders to reduce forgetting.[2] However, insights from behavioral science have yet to be methodically applied to criminal justice, where they hold promise for making the system fairer and more efficient.

To illustrate this, we focus on one problem: failures to appear in court (FTA). The criminal justice system cannot work if people fail to appear in court, which is why the system places great weight on ensuring that people attend required hearings and enforces prescribed responses if they fail to do so. Nationally, the FTA rate is approximately 21-24% for felony cases.[3] FTA rates for misdemeanor and low-level offenses are even higher: historically this rate is around 40% for summons cases in New York City (NYC), which in 2014 represented about 130,000 missed court dates. In many jurisdictions, failing to appear can result in an arrest warrant; in NYC this is the default response in accordance with state law.

To reduce FTAs, a traditional policy approach would propose stricter enforcement of warrants, based on the assumption that people skip court because they weren't deterred by existing penalties. However, a behavioral science perspective suggests many other factors could lead people to miss court. For example, they may not have paid close attention to information about their court date when they got it, they may have simply forgotten, or they may not have planned for taking time off from work in order to attend their court date. If these behavioral barriers account for some instances of FTA, then behavioral interventions may help courts reduce FTA rates without resorting to stricter enforcement.

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¹ Allcott, Hunt, and Sendhil Mullainathan. "Behavior and energy policy." Science 327, no. 5970 (2010): 1204-1205. http://science.sciencemag.org/content/327/5970/1204

² Dai, Hengchen, Katherine L. Milkman, John Beshears, James J. Choi, David Laibson, and Brigitte C. Madrian. "Planning prompts as a means of increasing rates of immunization and preventive screening." Public Policy & Aging Report 22, no. 4 (2012): 16-19. http://nber.org/aging/roybalcenter/planning_prompts.pdf

³ Cohen, T. H. (2010). Pretrial release of felony defendants in state courts: State court processing statistics, 1990-2004. https://www.bjs.gov/content/pub/pdf/prfdsc.pdf

This policy brief outlines the process and results of a joint project with ideas42 and the University of Chicago Crime Lab, in partnership with the New York City Mayor's Office of Criminal Justice (MOCJ), New York City Police Department (NYPD), and the New York State Unified Court System Office of Courts Administration (OCA). The project's aim was to develop and test two behavioral approaches to addressing the common issue of FTA, which plagues court systems across the country. Instead of applying traditional approaches to increase compliance with court summonses (via stiffer enforcement), we looked for opportunities to address contextual factors that were contributing to missed appearances in NYC courts.

In the following sections, we outline the extent of the FTA problem in NYC, the contextual factors we identified as contributing to the problem, and two simple, cost-effective solutions we designed and tested to address it. After presenting results of each intervention, we conclude with thoughts and recommendations for moving forward.

## What is behavioral science?

Behavioral science is the study of how people make decisions and act within a complex and textured world where details matter. It draws from decades of research in the social sciences to create a more realistic framework for understanding people. The standard approach to predicting human behavior suggests that we consider all available information, weigh the pros and cons of each option, make the best choice, and then act on it. The behavioral approach suggests something different. We make decisions with imperfect information and do not always choose what's best for us. Seemingly small and inconsequential details undermine our intentions to act. Behavioral science has been used across a variety of fields to realign policies, programs, and products with how we really behave in order to improve outcomes.

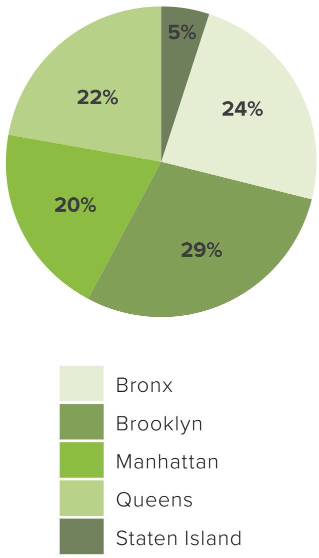
# Behavioral Reasons People Fail to Appear in Court

Court appearance tickets are issued for low-level offenses, which range from public consumption of alcohol and public urination to riding a bicycle on the sidewalk and spitting. Among summonses requiring an in-person court appearance (that were not resolved through plea by mail[4]), historically, around 40% end in FTA.

## Who Receives Summonses? Descriptive Statistics of Summons Recipients[5]

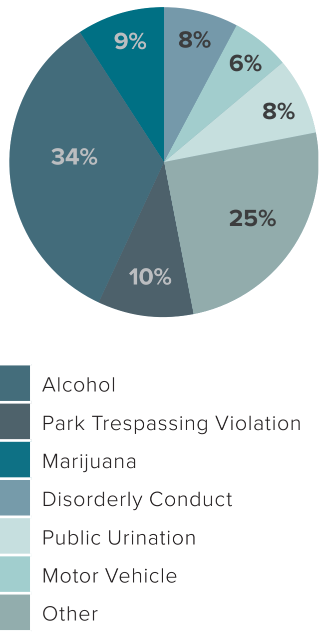
*Summons recipients between January 2016 and June 2017*

### Borough

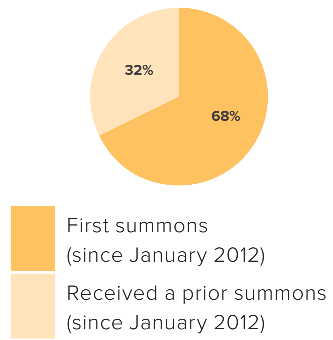



**34 years old** *Average age of summons recipients*

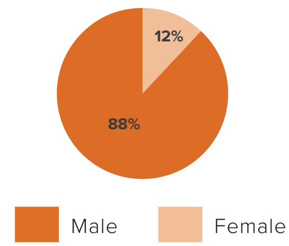
### Offense




### Prior Summons Recipients




### Gender Breakdown




[4] The plea by mail option is available for two offenses: public consumption of alcohol and public urination.

[5] Source: New York State Unified Court System data

In NYC, hundreds of thousands of arrest warrants are currently open due to FTA, which is problematic for both civilians (who can be taken into custody upon future interaction with police officers) and for law enforcement (whose time and resources are spent taking individuals into custody who might otherwise walk away from interactions with the police). The negative consequences for recipients could include time in police custody, potential immigration issues, and disruptions to work and family life—not to mention the psychological costs of worrying about being picked up on an open warrant. Dealing with these warrants also places a burden on the police and court systems.

To uncover the psychological and contextual features contributing to FTAs, ideas42 and the Crime Lab conducted quantitative and qualitative research using our behavioral diagnosis methodology. We uncovered four main barriers contributing to FTAs.

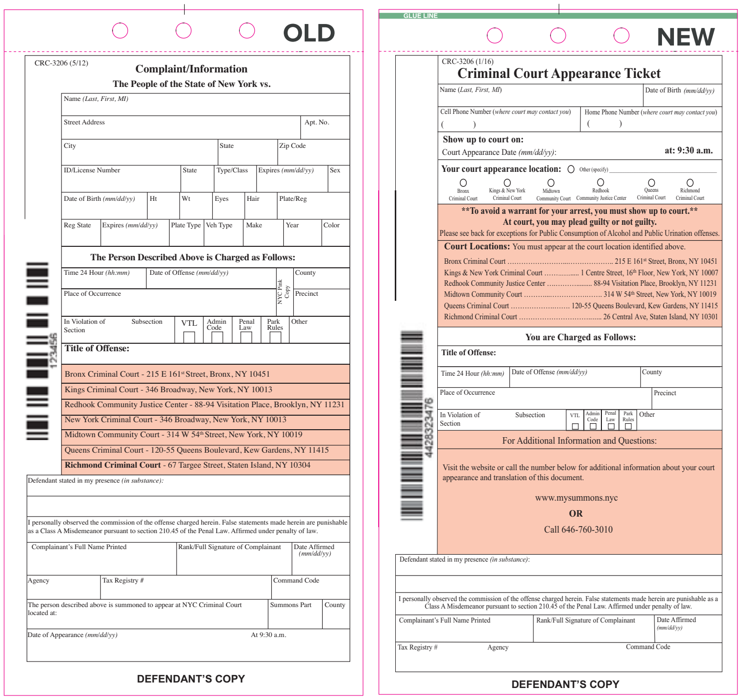
| | |
|---|---|
| **Mental Models** | First, some recipients believe that receiving a ticket for a minor offense and having to attend court for it is unfair. The crime feels misaligned with the punishment. Other recipients receiving a ticket for a minor offense did not expect a warrant to be issued for not attending court. That is, having to go to court for a seemingly minor offense (e.g., being in a park after hours) doesn't match with people's "**mental model**" of what necessitates a court appearance, much less an arrest warrant. |
| **Present Bias** | Second, the immediate financial or psychological costs of attending court, such as taking time off work or fears around what may happen at court, outweigh the (often unknown) consequences of not appearing. Many people we interviewed weren't aware that a warrant was a consequence of FTA, but even among those who were aware of the warrant, some still reported missing court because immediate costs of going loomed larger than the risk of getting arrested in the future. This focus on immediate costs over future ones, even when those future costs are objectively larger, is known as "**present bias.**" |
| **Social Norms** | Third, there is a misperception about court attendance. A majority of interviewees hold the misperception that most people do not attend their court dates, which (consciously or unconsciously) may influence their own decision to attend or not. Prior work from behavioral science shows that the perceived behavior of peers ("**social norms**") can have a strong influence on our decisions and actions. |
| **Inattention** | Fourth, the long lag time between receiving the summons and attending court leads many to forget. In NYC, the court date is typically 60 to 90 days after the ticket was issued, which is plenty of time for people to forget about their court date or the summons altogether. This forgetting can be attributed to "**inattention.**" |

# Behavioral Interventions to Reduce FTA

With our understanding of the contextual and psychological barriers influencing court attendance, we designed two simple, low-cost, scalable solutions to increase appearances.[6] Our **first** touch point was the summons form itself, which is the recipients' main source of information regarding where and when they must attend court. One reason for FTA could be that people do not take the time to carefully read the form. We redesigned it to limit the attention needed to acquire the most important information by putting the essential details near the top of the form and clearly stating the consequences of missing court.

## *Comparing the old and new summons forms*

We made several changes to the recipient copy of the summons form. Some of the main changes of the front page of the form are described in the call out boxes on the next page.[7]

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[6] Most recently, the Mayor's Office worked with four district attorneys (Bronx, Brooklyn, Manhattan and Queens) to dismiss over 644,000 outstanding summons warrants that were over 10 years old for minor offenses like drinking alcohol in public or entering a park after hours.

[7] See idea42's website for more details on the form redesign: http://www.ideas42.org/summons

**OLD**

CRC-3206 (5/12)

**Complaint/Information**

**The People of the State of New York vs.** 1

Name (*Last, First, MI*)

Street Address | Apt. No.

City | State | Zip Code

ID/License Number | State | Type/Class | Expires (*mm/dd/yy*) | Sex

Date of Birth (*mm/dd/yy*) | Ht | Wt | Eyes | Hair | Plate/Reg

Reg State | Expires (*mm/dd/yy*) | Plate Type | Veh Type | Make | Year | Color

**The Person Described Above is Charged as Follows:**

Time 24 Hour (*hh:mm*) | Date of Offense (*mm/dd/yy*) | NYC Pink Copy | County

Place of Occurrence | Precinct

In Violation of Section | Subsection | VTL | Admin Code | Penal Law | Park Rules | Other

**Title of Offense:**

123456

Bronx Criminal Court - 215 E 161st Street, Bronx, NY 10451

Kings Criminal Court - 346 Broadway, New York, NY 10013

Redhook Community Justice Center - 88-94 Visitation Place, Brooklyn, NY 11231

New York Criminal Court - 346 Broadway, New York, NY 10013

Midtown Community Court - 314 W 54th Street, New York, NY 10019

Queens Criminal Court - 120-55 Queens Boulevard, Kew Gardens, NY 11415

**Richmond Criminal Court** - 67 Targee Street, Staten Island, NY 10304

Defendant stated in my presence (*in substance*):

I personally observed the commission of the offense charged herein. False statements made herein are punishable as a Class A Misdemeanor pursuant to section 210.45 of the Penal Law. Affirmed under penalty of law.

Complainant's Full Name Printed | Rank/Full Signature of Complainant | Date Affirmed (*mm/dd/yy*)

Agency | Tax Registry # | Command Code

The person described above is summoned to appear at NYC Criminal Court located at: | Summons Part | County

Date of Appearance (*mm/dd/yy*) | At 9:30 a.m. 2

**DEFENDANT'S COPY**

GLUE LINE

**NEW**

CRC-3206 (1/16)

**Criminal Court Appearance Ticket** 1

Name (*Last, First, MI*) | Date of Birth (*mm/dd/yy*)

Cell Phone Number (*where court may contact you*) ( ) | Home Phone Number (*where court may contact you*) ( )

2 **Show up to court on:**

Court Appearance Date (*mm/dd/yy*): **at: 9:30 a.m.**

**Your court appearance location:** ○ Other (specify) ____

○ Bronx Criminal Court ○ Kings & New York Criminal Court ○ Midtown Community Court ○ Redhook Community Justice Center ○ Queens Criminal Court ○ Richmond Criminal Court

3 ****To avoid a warrant for your arrest, you must show up to court.****

**At court, you may plead guilty or not guilty.**

Please see back for exceptions for Public Consumption of Alcohol and Public Urination offenses.

**Court Locations:** You must appear at the court location identified above.

Bronx Criminal Court .................................... 215 E 161st Street, Bronx, NY 10451

Kings & New York Criminal Court ................ 1 Centre Street, 16th Floor, New York, NY 10007

Redhook Community Justice Center ............... 88-94 Visitation Place, Brooklyn, NY 11231

Midtown Community Court .............................. 314 W 54th Street, New York, NY 10019

Queens Criminal Court ............................ 120-55 Queens Boulevard, Kew Gardens, NY 11415

Richmond Criminal Court ...................................... 26 Central Ave, Staten Island, NY 10301

**You are Charged as Follows:**

**Title of Offense:**

Time 24 Hour (*hh:mm*) | Date of Offense (*mm/dd/yy*) | County

Place of Occurrence | Precinct

In Violation of Section | Subsection | VTL | Admin Code | Penal Law | Park Rules | Other

4428323476

For Additional Information and Questions:

Visit the website or call the number below for additional information about your court appearance and translation of this document.

www.mysummons.nyc

**OR**

Call 646-760-3010

Defendant stated in my presence (*in substance*):

I personally observed the commission of the offense charged herein. False statements made herein are punishable as a Class A Misdemeanor pursuant to section 210.45 of the Penal Law. Affirmed under penalty of law.

Complainant's Full Name Printed | Rank/Full Signature of Complainant | Date Affirmed (*mm/dd/yy*)

Tax Registry # | Agency | Command Code

**DEFENDANT'S COPY**

1. Clear title describes the purpose and required action.
2. The date, time, and location of the appearance is moved from the bottom to the top, where it is more likely to be read.
3. The consequence of missing is clearly articulated and framed to spur loss aversion, the human tendency to feel losses more severely than equivalent gains.

The **second** touch point addressed the lag time between receipt of the summons and the court date. We designed text message reminders tailored to address the bottlenecks described above. Compared to other forms of reminders, such as letters or robo-calls, text messages are inexpensive, and information is easily received and retrievable later.

We designed multiple sets of text messages to determine which messaging is most effective at reducing FTA. Some were sent before a person's scheduled court date (pre-court messages) and some were only sent if they had missed their court date (post-FTA messages). In order to test which messages were most impactful on FTA rates, recipients were randomly assigned to receive some combination of pre-court and/or post-FTA messages, or no message at all.

The pre-court message sets consist of three different texts, sent seven, three, and one day(s) before the scheduled court date. This schedule was chosen in order to prompt recipients to take preemptive action for attending court (i.e. scheduling time away from work or securing childcare) without reminding them too early, which could lead to procrastination.

Some pre-court messages emphasized the consequences of failing to appear and provided information about what to expect at court ("consequences"), while others focused on helping people develop concrete plans for appearing in court ("plan-making"). A third set combined consequences and plan-making messages. All messages helped to address inattention or forgetting the court date.

## Pre-Court Messages

### CONSEQUENCES MESSAGES

*7 days before court*

Helpful reminder: go to court Mon Jun 03 9:30AM. We'll text to help you remember. [Show up to avoid an arrest warrant.] 1 Reply STOP to end texts. www.mysummons.nyc

*3 days before court*

Remember, you have court on Mon Jun 03 at 346 Broadway Manhattan. 2 [Tickets could be dismissed or end in a fine (60 days to pay).] [Missing can lead to your arrest.] 3

*1 day before court*

At court tomorrow at 9:30AM [a public defender will help you through the process.] 2 [Resolve your summons (ID##########) to avoid an arrest warrant.] 4

1 Makes the costs of FTA more salient to overcome present bias.

2 Reduces the ambiguity and perceived costs of attending court.

3 Highlights penalties to overcome present bias and the mental model that you don't need to go to court for minor violations.

4 Repeats the consequence to keep the cost of missing court top-of mind, reinforcing that despite the mismatch between crime and punishment, you must attend to avoid a warrant.

## PLAN-MAKING MESSAGES

*7 days before court*

Helpful reminder: go to court on Mon Jun 03 9:30AM. [Mark the date on your calendar and set an alarm on your phone.] Reply STOP to end messages. www.mysummons.nyc 1

*3 days before court*

You have court on Mon Jun 03 at 346 Broadway Manhattan. [What time should you leave to get there by 9:30AM? Any other arrangements to make? Write out your plan.] 2

*1 day before court*

You have court tomorrow for summons ID##########. [Did you look up directions to 346 Broadway Manhattan?] 3 Know how you're getting there? Please arrive by 9:30AM.

1 Encourages people to set reminders to help them remember.

2 Aids people to think ahead and overcome potential barriers (or costs) to showing up to court.

3 Helps plan how to get there and makes the act of going more concrete.

## COMBINATION MESSAGES

*7 days before court*

Helpful reminder: go to court Mon Jun 03 9:30AM. We'll text to help you remember. Show up to avoid an arrest warrant. Reply STOP to end texts. www.mysummons.nyc

*3 days before court*

You have court on Mon Jun 03 at 346 Broadway Manhattan. What time should you leave to get there by 9:30AM? Any other arrangements to make? Write out your plan.

*1 day before court*

Remember, you have court tomorrow at 9:30AM. Tickets could be dismissed or end in a fine (60 days to pay). Missing court for ########## can lead to your arrest.

*These messages, combining elements from both sets above, address present bias, mental models, and plan-making as previously described.*

In addition to the pre-court reminders, we developed two types of messages sent only if a person had missed the court appearance and a warrant had been issued. The first type focused on consequences, letting recipients know that a warrant was issued, but that they wouldn't be arrested if they clear it at the court. The second type relied on the power of social norms and informed recipients that most people actually had attended their court date. Again, both addressed inattention or forgetting.

## Post-FTA Messages

### CONSEQUENCE MESSAGE

1 [Since you missed court on Jun 03 (ID##########), a warrant was issued.]
2 [You won't be arrested for it if you clear it at 346 Broadway Manhattan.]
www.mysummons.nyc

***Sent when a warrant is triggered by an FTA***

1 Notifies of the serious consequence that has occurred.

2 Encourages action to resolve the open warrant.

### SOCIAL NORMS MESSAGE

1 [Most people show up to clear their tickets but records show you missed court for yours (ID##########).]
Go to court at 346 Broadway Manhattan.
www.mysummons.nyc

***Sent when a warrant is triggered by an FTA***

1 Provides feedback that their behavior goes against the norm.

# Results

## Solution 1: Summons Form Behavioral Redesign

The redesigned summons form was first introduced to replace old forms in March 2016 and universally adopted by July 2016. The rollout period culminated in a rapid adoption of the new form across NYC between June and July 2016. Once the new form was issued citywide, the old forms were revoked and collected for destruction.

In order to isolate the impact of the redesigned summons form from other contributing factors to FTA, we compared outcomes between people issued an old form and a new form using a quasi-experimental approach called a regression discontinuity design. We focused on the narrow time-window around new form adoption, comparing people who received summonses just before and just after their issuing officer switched to the new form. The intuition behind this research design is that within a few weeks of the switch, the form version a recipient received was as good as random: they happened to get whichever form the officer was using at that time. This means that any change in FTA is likely caused by the new forms.[8]

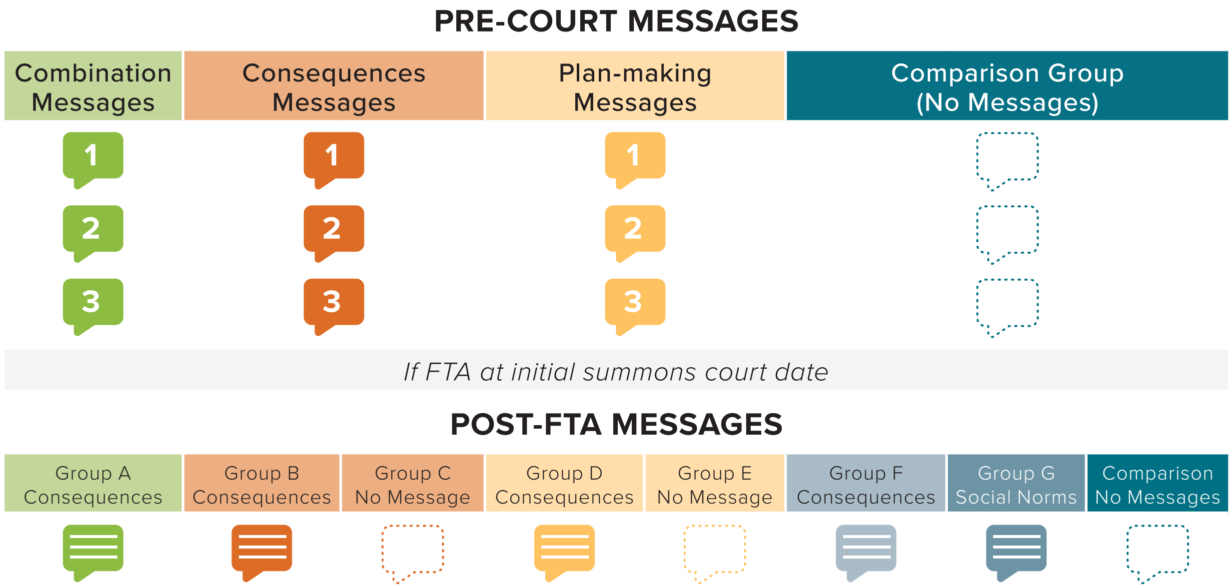
**Those who happened to receive the new summons form have an FTA rate that is 13%, or 6.4 percentage points, lower** than those who happened to receive the old summons form because their issuing officer had not switched yet. As the key variable between these two similar groups of summons recipients, we can determine that the new forms caused this reduction in FTA.

## Solution 2: Behavioral Text Messages to Reduce FTA

We evaluated the effect of behavioral text messages using a randomized controlled trial. Anyone in NYC who was issued a summons and provided their cell phone number was eligible to receive text message reminders. Approximately 20,000 summons recipients were randomized to receive one of the pre-court or post-FTA message sets, or no messages (the “comparison group”). All effects seen here are in addition to the gains in court attendance already realized through the behavioral summons form redesign.

[8] In fact, the characteristics of summons recipients were very similar just before and just after officers switched forms, in terms of the kinds of offenses they received summonses for, their age and gender composition, and their likelihood of having received summonses in the past. Thus, any difference in FTA rate between those who received the old and new forms would suggest that the new forms were responsible for the change.

# TEXT MESSAGE SETS



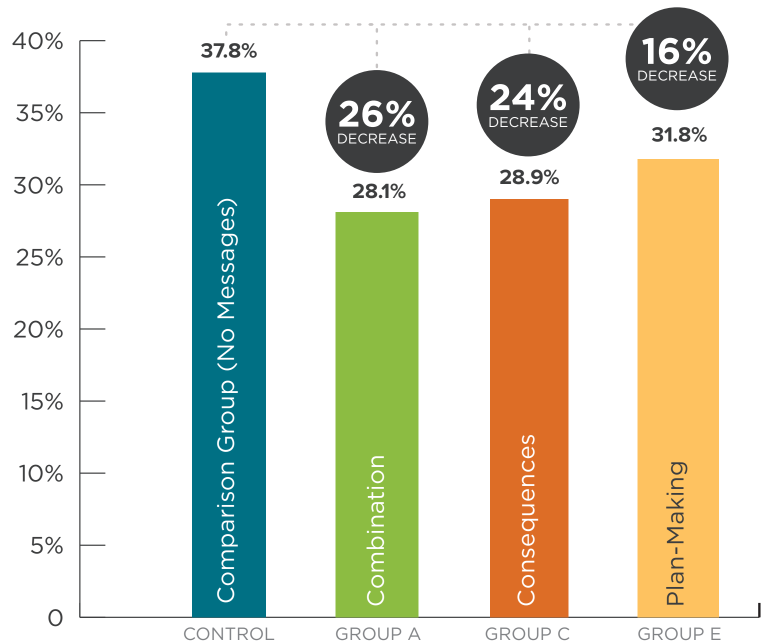

We found that receiving any pre-court message reduces FTA on the court date by 21%. **The combination messages, using elements of both the consequences and plan-making sets, were the most effective, reducing FTA by 26% (from 38% to 28%).** This 26% FTA reduction is measured on the court date, and comes after receiving the sequence of three pre-court messages.

We also looked at the impact 30 days after the court date, as some summons recipients show up to court to clear their warrants after their scheduled court date. Individuals receiving the combination messages receive a post-FTA message if they fail to appear in court on their scheduled date. Relative to receiving no text message, **we find a 32% reduction in open warrants for people who received a combination message set and a post-FTA message (from 24% to 17%).** This reflects both the change in FTA on the court date, as well as subsequent court appearances to clear warrants within 30 days of the scheduled court date.

There is also a question of whether timing of messages matters for reducing FTA—are messages more effective when they are sent before missing a court date or after? We find that post-FTA messages alone are helpful, leading to a 15% reduction in failures to return to court within 30 days, but not as helpful as pre-court messages. Among post-FTA messages, the consequences message (16% reduction) was more effective than the social norms message (14% reduction).[9]

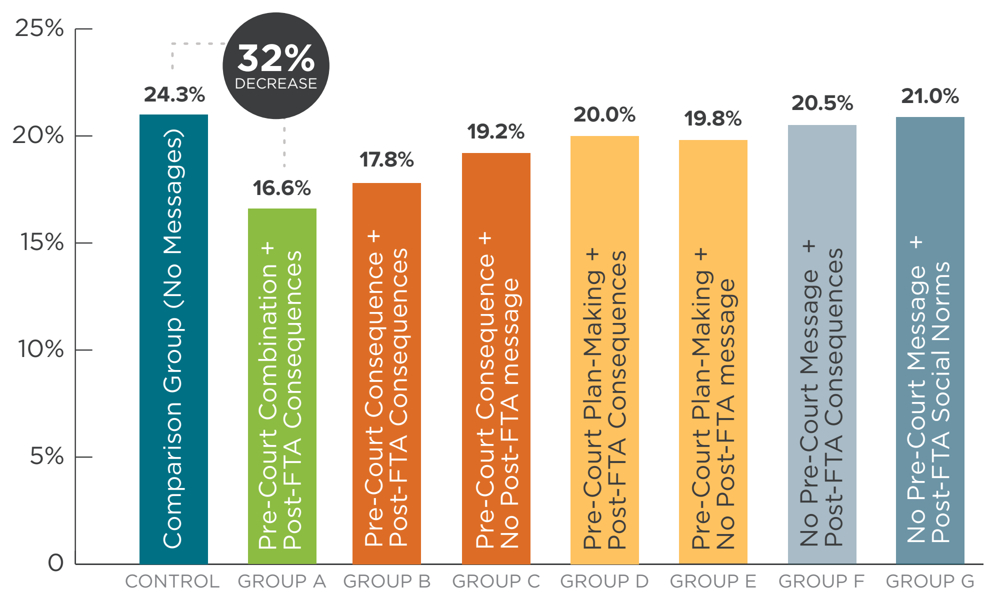
[9] We also compared sending just the pre-court messages vs. pre-court *plus* post-FTA messages. Here, we find that for people who received pre-FTA messages the effect of receiving an additional post-FTA message is encouragingly in the right direction, but not yet statistically significant at the typical 5% level.

**FTA Rate by Type of Message**



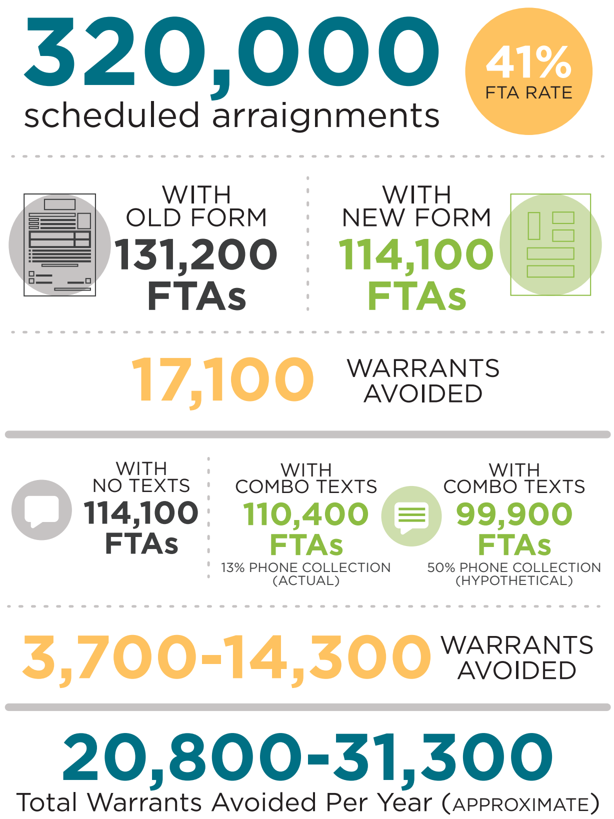

*The difference in FTA rates between the comparison group and any treatment arm is significant at the 1% level (p<0.01)*

**Open Warrant Rate 30 Days After Court Dates**




*The difference in FTA rates between the comparison group and any treatment arm is significant at the 1% level (p<0.01)*

# WARRANTS THAT COULD HAVE BEEN AVOIDED IN 2014




## Cost-Benefit Analysis

Both the redesign and text message interventions are inexpensive and scalable. Using the redesigned form has exactly the same cost as using the old form, and the only cost is incurred during a one-time change. The text messages are also inexpensive, at less than one cent ($0.0075) per message. For example, sending all 2014 summons recipients three messages would have cost less than $7,500.

By contrast, the costs of failing to appear in court are much higher. Entry into the criminal justice system—as would be the case if a person was arrested for having an FTA warrant—can have major adverse impacts on people's lives, regardless of the severity of the initial offense. Failures to appear in court also divert time and resources in both courts and policing. The benefits could be even larger if these kinds of messages also reduce FTA for more severe offenses, since this could result in a lesser use of pre-trial detention. By reducing FTA rates,[10] behavioral interventions might make it possible to allow more people to await trial outside of jail—an important goal for NYC and other U.S. jurisdictions that are concerned about the racial and social disparities of pre-trial detention. Because they are so inexpensive and easy to replicate, both interventions could easily be adapted for other locations and for other types of courts and offenses.

[10] Prior failures to appear in court are among the strongest predictors of future missed court appearances, and similarly the most heavily weighted factors in considering pre-trial release https://www.bjs.gov/content/pub/pdf/prfdsc.pdf http://www.nycja.org/lwdcms/doc-view.php?module=reports&module_id=678&doc_name=doc

# Next Steps

The interventions described here are among the first applications of behavioral science to criminal justice policy.[11] Promisingly, not only are these solutions impactful, their effects are as large or larger than some of the most successful similar behavioral interventions in other domains. We see these interventions as an encouraging first step toward incorporating insights from behavioral science into criminal justice reform.

An immediate next step is to build off of the results and continue to scale the most effective interventions to reach more people. As a measure of the potential for future growth, a recent survey found that 87% of adults nationwide own a cell phone, with ownership reaching nearly 96% in NYC.[12] While text messages are very effective, only about 13% of summons recipients in NYC currently provide a cell phone number, which represents a significant opportunity to expand reach. Enabling more recipients to get these messages would increase the potential impact of this intervention.

Another promising avenue we are exploring is “personalized reminders.” The usual approach in behavioral science is to identify the intervention with the largest average effect and administer the same “nudge” to everyone. We might achieve larger gains by tailoring reminders to individuals, so that a given individual receives messages specific to the barriers that they are experiencing. For instance, busy people may be particularly responsive to plan-making messages, while first-time summons recipients may be more responsive to consequences messages.

Our findings have the potential for impact beyond low-level offenses and beyond NYC. Another aim is to scale both the redesign of other complex forms that recipients receive and text message reminders across different court systems and cities. Future work could specifically investigate the gains to behavioral enhancements at criminal courts that handle more serious misdemeanors and felonies in jurisdictions across the country.

The work we describe here represents an early success in using behavioral science to improve the criminal justice system. Because behavioral approaches to criminal justice reform have been largely overlooked, we believe that there are many “easy wins” to be had. Of course, effective nudges are not substitutes for substantial policy change, but they could be an effective complement and can be more readily implemented and scaled than broad policy changes. A concerted effort toward low-cost, incremental benefits could add up to make a significant difference both for the criminal justice system and for people’s lives.

This research by ideas42 and the University of Chicago Crime Lab, in collaboration with MOCJ, NYPD, and OCA, is a promising step toward incorporating behavioral science in criminal justice. We are eager to continue efforts to better understand how novel, low-cost strategies could be used by NYC and other jurisdictions to make progress on persistent policy challenges.

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[11] Another early example is the work of Bornstein et al (2013)." http://ppc.unl.edu/wp-content/uploads/2012/08/Bornstein-et-al-Reducing-courts-failure-to-appear-rate-by-written-reminders-Psychology-Public-Policy-and-the-Law-2013.pdf

[12] https://www1.nyc.gov/assets/dca/MobileServicesStudy/Research-Brief.pdf