

The Effects of Pre-Trial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges*

Will Dobbie[†]

Jacob Goldin[‡]

Crystal Yang[§]

October 2016

Abstract

Over 20 percent of prison and jail inmates in the United States are currently awaiting trial, but little is known about the impact of pre-trial detention on defendants. This paper uses the detention tendencies of quasi-randomly assigned bail judges to estimate the causal effects of pre-trial detention on subsequent defendant outcomes. Using data from administrative court and tax records, we find that pre-trial detention significantly increases the probability of conviction, primarily through an increase in guilty pleas. Pre-trial 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) pre-trial detention weakening defendants' bargaining positions during plea negotiations, and (ii) a criminal conviction lowering defendants' prospects in the formal labor market.

*We thank Amanda Agan, Adam Cox, Hank Farber, Paul Goldsmith-Pinkham, Louis Kaplow, Adam Looney, Alex Mas, Magne Mogstad, Michael Mueller-Smith, Erin Murphy, Steven Shavell, Megan Stevenson, and numerous seminar participants for helpful comments and suggestions. Molly Bunke, Kevin DeLuca, Sabrina Lee, and Amy Wickett provided excellent research assistance. The views expressed in this article are those of the authors and do not necessarily reflect the view of the U.S. Department of Treasury.

[†]Princeton University and NBER. Email: wdobbie@princeton.edu

[‡]Stanford Law School and U.S. Department of the Treasury. Email: jsgoldin@law.stanford.edu

[§]Harvard Law School. Email: cyang@law.harvard.edu

Each year, more than eleven 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 each year, nearly double the next highest country—China (Walmsley 2013). The high rate of pre-trial 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 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 pre-trial 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 pre-trial detention can disrupt defendants' lives, putting jobs at risk and increasing the pressure to accept unfavorable plea bargains.¹ There are also concerns that pre-trial 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...also constitute bad public policy." (Department of Justice 2016). Others claim that the bail system is operating as designed, and that releasing more defendants would increase pre-trial flight and crime rates. This debate is currently playing out across the country, with a number of jurisdictions exploring alternatives to pre-trial 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 pre-trial detention 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 pre-trial may be more likely to be guilty or more

¹As 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 <http://www.nytimes.com/2015/08/16/magazine/the-bail-trap.html>.

²For example, New York City has earmarked substantial funds to supervise low-risk defendants instead of requiring them to post bail or face pre-trial detention, and Illinois lawmakers passed a bill in May 2015 requiring that a nonviolent defendant be released pre-trial 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.

³Data tracking defendants often contain some information on pre-trial 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.

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 pre-trial detention on criminal case outcomes, pre-trial flight, future crime, and foregone earnings and social benefits. Our empirical strategy exploits plausibly exogenous variation in pre-trial release 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 pre-trial release 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 pre-trial release on case outcomes. We find that pre-trial release decreases the probability of being found guilty by 15.1 percentage points, a 25.8 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 11.8 percentage points, a 26.4 percent change. Conversely, pre-trial 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 pre-trial release improves 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 pre-trial release on pre-trial flight and new crime, two frequently cited costs of release. We find that pre-trial release (mechanically) increases the probability of failing to appear in court by 15.2 percentage points, a 125.6 percent increase, with smaller effects

⁴Prior work based on cross-sectional comparisons has yielded mixed results, with some papers suggesting little impact of pre-trial detention on conviction rates (Goldkamp 1980), and others finding a significant relationship between pre-trial 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).

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

for defendants with no prior offenses. In contrast, we find no detectable effect of pre-trial release on new crime up to two years after the bail hearing. This null result is driven by offsetting incapacitation and criminogenic effects. While pre-trial release (mechanically) increases the likelihood of rearrest prior to case disposition by 13.4 percentage points, a 68.4 percent change, it also decreases the likelihood of rearrest following case disposition by 15.0 percentage points, a 46.9 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 suggest that the most empirically relevant cost of pre-trial release is increased flight, not new crime.

Finally, we examine the effects of pre-trial release on formal sector employment and social benefits receipt. We find evidence that pre-trial 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. Pre-trial release increases the probability of employment in the formal labor market three to four years after the bail hearing by 10.8 percentage points, a 28.6 percent increase from the detained defendant mean. Pre-trial release also increases the amount of Unemployment Insurance (UI) benefits received over the same time period by \$315, a 130.2 percent increase, and the amount of Earned Income Tax Credit (EITC) benefits received by \$220, a 61.8 percent increase. The probability of having any formal sector income over this time period increases by 10.5 percentage points, a 22.8 percent increase, and the probability of filing a tax return increases by 4.9 percentage points, a 16.1 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 mechanical 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 pre-trial release at the margin is between \$37,031 and \$40,048 per defendant. The large net benefit of pre-trial 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. Mueller-Smith (2015) finds that post-conviction incarceration reduces employment and increases future crime using data on defendants from Harris County, Texas. Aizer and Doyle (2015) find that juvenile incarceration reduces high school completion and increases adult incarceration using data on juveniles from Chicago. None of these papers, however, have been able to shed light on the effects of a criminal conviction independent of incarceration or the effects of pre-conviction detention.⁶

Our paper is also related to a number of recent working 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, and Stevenson 2016). However, none of these papers are able to examine non-criminal outcomes such as formal sector employment or social benefits take-up, nor do any of these papers assess the costs of pre-trial detention relative to benefits, which is critical to evaluating recent bail reforms. To the best of our knowledge, our paper is the first to provide causal estimates of the impact of bail decisions on defendants’ labor market outcomes and take-up of public assistance.

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.

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 U.S. 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

⁶Our 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 any costs of pre-trial detention are disproportionately borne by black defendants.

⁷For instance, the Eighth Amendment to the U.S. 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.”

all court proceedings, known broadly as release on recognizance (ROR). Second, defendants may be released subject to some non-monetary 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 ten 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 ten 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 bondsmen also charge a non-refundable fee for their services, generally ten 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 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. 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.

⁸A 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.

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 Pre-Trial Services Bail Unit. The Bail Unit operates 24 hours a day, seven 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 Appendix Figure A2) that is presented to the bail judge. However, these bail guidelines are only followed by the bail judge about half the time, with judges often imposing monetary bail instead of the recommended non-monetary options (Shubik-Richards and Stemen 2010).

After the Pre-Trial 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).

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 defense counsel and the prosecutor. While monetary bail amounts at this stage often follow the standard bail schedule, the choice between monetary versus non-monetary bail conditions varies widely across judges in Miami-Dade (Goldkamp and Gottfredson 1988).

Mapping to Empirical Design: Our empirical strategy exploits variation in the pre-trial 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 (7:30AM-3:30PM), another serving the afternoon shift (3:30PM-11:30PM), and the final judge serving the night shift (11:30PM-7:30AM). While it may be endogenous whether a defendant is arrested in the morning or at night or on a specific day of 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 9:00AM, 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.⁹

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

⁹There 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 be 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 pre-trial release and bail judge assignment, it has the advantage of not capturing any (potential) non-random 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.

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.

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 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.

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 pre-trial 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.¹⁰ 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 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 pre-trial 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 99th and 1st 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.

¹⁰Undocumented immigrants without a valid SSN are not included in these data.

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.¹¹ 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 detained pre-trial and those who are released pre-trial. We measure pre-trial release based on whether a defendant is released within three days of the bail hearing, as recent policy initiatives focus on this time period.¹² In Section IV.E, we explore the robustness of our results to alternative measures of pre-trial release. Additional summary statistics by mutually exclusive bail types are presented in Appendix Table A1.

Panel A of Table 1 provides summary statistics on bail decisions in our setting. Among defendants who are released pre-trial within the first three days, 36.6 percent are released ROR, 21.6 percent are released on non-monetary bail, and 41.7 percent are released on monetary bail with an average bail amount of \$12,497 and median bail amount of \$5,000. In contrast, among those who are detained for at least three days, 94.3 percent are detained on monetary bail with an average bail amount of \$48,318 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 three days after the bail hearing, 43.5 percent petition for bail modification, 9.3 percent are released within 14 days and 36.6 percent are released at some point prior to case disposition. In contrast, among defendants released within three days of the bail hearing, 7.2 percent petition for bail modification.

Panel C presents demographic characteristics of defendants in our sample. In our sample, 38.3 percent of detained defendants are white and 60.7 percent are black. Among released defendants, 42.4 percent are white and 55.6 percent are black. 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 detained and released defendants are approximately 33 years of age at the time of bail. Panel C also presents

¹¹Specifically, defendants were first 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 (1) whether the defendant ever filed a tax return or received an information return reporting residence in the state of residence, (2) whether the first three letters of the defendant’s first name matched a first name reported on a tax return or other informational return, and (3) whether the defendant’s zip code matched a zip code reported with a tax return or informational return. Remaining duplicates were dropped from the sample. Because the filing of tax and information returns may be related to pre-trial release, we restrict the matching process to tax information submitted before the year of the defendant’s arrest.

¹²See, for example, the 3DaysCount project at the Pretrial Justice Institute.

selected baseline labor market outcomes by three-day detention status. Among defendants detained for at least three days, 31.9 percent are employed in the year prior to arrest, 77.1 percent have any income, and the average annual income is \$4,511. Among defendants released within three days, 42.3 percent are employed in the year prior to arrest, 81.4 percent have any income, and the average annual income is \$7,205.

Panel D presents offense characteristics of defendants in our sample. 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 detained defendants, 29.2 percent are charged with a violent offense and 34.4 percent are charged with a property offense. In contrast, only 19.2 percent of released defendants are charged with a violent offense and 18.5 percent are charged with a property offense. In general, released defendants are substantially less likely to be charged with felonies compared to detained defendants.

Finally, Panel E presents case outcomes, future crime, and labor market outcomes by three-day detention status. In our sample, 58.5 percent of detained defendants are found guilty of at least one charge compared to 48.7 percent of released defendants. Forty-five percent of detained defendants plead guilty compared to just 20.8 percent of released defendants.¹³ Detained defendants are also 15.8 percentage points more likely to be incarcerated compared to released defendants.

Defendants released within three days are more likely to fail to appear in court, with 17.9 percent of released defendants failing to appear compared to 12.1 percent of 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 47.0 percent of detained defendants rearrested compared to 40.4 percent of released defendants.

In terms of labor market outcomes, released defendants earn substantially more in the two years after the bail hearing compared to detained defendants and are more likely to be employed. In our sample, 37.7 percent of detained defendants are employed compared to 50.9 percent of released defendants. Given these low rates of employment, annual wage earnings of all defendants are also low, with detained defendants making \$5,212 in reported earnings compared to \$7,891 for released defendants. Released defendants are also more likely to receive any income in the first two years after the bail hearing compared to detained defendants. Differences in earnings outcomes of released and detained defendants also persist three to four years after the bail hearing. During this time period, 37.7 percent of detained defendants are employed in the formal labor market compared to 48.2 percent of released defendants, with detained defendants making annual reported earnings of \$5,873 compared to \$8,354 for released defendants.

¹³In 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.

III. Research Design

Overview: For individual i , consider a model that relates outcomes such as future crime to an indicator for whether the individual was released before his or her trial for case c , $Released_{ic}$:

$$Y_{ict} = \beta_0 + \beta_1 Released_{ic} + \beta_2 \mathbf{X}_{ict} + \varepsilon_{ict} \quad (1)$$

where Y_{ict} is the outcome of interest for individual i in court c in year t , \mathbf{X}_{ict} is a vector of case- and defendant-level control variables, and ε_{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 pre-trial 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 towards a finding that pre-trial release lowers future crime.

To address this issue, we estimate the causal impact of pre-trial release using a measure of the tendency of a quasi-randomly-assigned bail judge to release a defendant pre-trial 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 pre-trial 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). 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.

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

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

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

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 ε_{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:

$$Z_{ctj} = \left(\frac{1}{n_{tj} - 1} \right) \left(\sum_{k=0}^{n_{tj}} (Released_{ikt}^*) - Released_{ict}^* \right) \quad (3)$$

where n_{tj} is the number of cases seen by judge j in year t . We calculate the instrument across all case types (i.e. both felonies and misdemeanors), but allow the instrument to vary across years. In robustness checks, we allow judge tendencies to vary by case severity and by crime type.

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 pre-trial. 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 pre-trial.

Judge Variation: Figure 1 presents the distribution of our residualized judge leniency measure for pre-trial release at the judge-by-year level. Our sample includes nine total bail judges in Philadelphia and 170 total bail judges in Miami-Dade. In any given year, there are six bail judges serving in Philadelphia and approximately 60 serving in Miami-Dade. In Philadelphia, the average number of cases per judge is 36,499 during the sample period of 2007-2014, with the typical judge-by-year cell including 6,596 cases. In Miami-Dade, the average number of cases per judge is 573 during the sample period of 2006-2014, with the typical judge-by-year cell including 187 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 pre-trial 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 pre-trial 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 non-monetary release. Finally, some judges may impose monetary bail that a defendant is able to post to secure his or her release. 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 IV.E explores the impact of other margins such as being assigned monetary bail.

To determine which bail decisions are most predictive of whether a defendant is released pre-trial, we regress pre-trial release on each residualized judge leniency measure separately calculated for ROR, non-monetary bail, monetary bail, and bail amount (including zeros). See Appendix Table A2. We find that defendants assigned to judges who are more likely to use conditional non-monetary 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 pre-trial. In contrast, we find no significant relationship between our residualized judge leniency measure for ROR and the probability of pre-trial release. In combination, these results suggest that defendants on the margin of pre-trial release are those for whom judges disagree about the appropriateness of conditional non-monetary bail versus monetary bail, not those for whom judges disagree about the appropriateness of ROR versus other bail decisions.

One question might be why judges differ in their bail decisions. We have few detailed characteristics of judges to help illuminate this question. While interesting for thinking about the design of the bail determination process, it is not critical to our analysis to know precisely why some judges are more lenient than others. What is critical is that some judges are systematically more lenient than others, that cases are randomly assigned to judges conditional on our court-by-time fixed effects, and 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 released pre-trial (*Released*), we estimate the following equation for individual i and case c , assigned to judge j at time t using a linear probability model:

$$Released_{ictj} = \alpha_0 + \alpha_1 Z_{ctj} + \alpha_2 \mathbf{X}_{ict} + \varepsilon_{ict} \quad (4)$$

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 pre-trial release rate is 0.560 and far from zero or one. Robust standard errors are two-way clustered at the individual and judge-by-year level.

Figure 1 provides a graphical representation of the first stage relationship between our residualized measure of judge leniency and the probability of pre-trial 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 pre-trial release against judge leniency. The individual rate of pre-trial 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 pre-trial 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 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. Finally, column 4 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 pre-trial, with an F-statistic for the instrument of 569.1. Including controls in columns 3 and 4 does not change the magnitude of the estimated first stage effect, consistent with the quasi-randomness of bail judge assignment. With all controls (column 4), our results show that a defendant assigned to a bail judge that is 10 percentage points more likely to release a defendant pre-trial is 6.8 percentage points more likely to be released pre-trial.

The probability of pre-trial 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 pre-trial is 4.1 percentage points less likely to petition for bail modification, 6.8 percentage points more likely to be released within 14 days of the bail hearing, and 5.8 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 pre-trial release: (1) bail judge assignment only impacts defendant outcomes through the probability of pre-trial release, and (2) the impact of judge assignment on the probability of pre-trial 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 pre-trial 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-by-year level. We find that male defendants are 11.6 percentage points less likely to be released pre-trial compared to similar female defendants, a 20.7 percent decrease from the mean pre-trial 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 15.4 percentage points less likely to be released compared to defendants with no prior offense, a 27.5 percent decrease. Additionally,

defendants arrested for felonies are 25.4 percentage points less likely to be released than those arrested for misdemeanors, a 45.4 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 pre-trial. 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 = 0.72).

Nevertheless, the exclusion restriction could also be violated if bail judge assignment impacts future outcomes through channels other than pre-trial 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 pre-trial 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 assignment of a bail judge is correlated with the assignment of other criminal justice actors, who may independently affect defendant outcomes. 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 also estimate whether release with and without conditions differentially affects defendant outcomes to partially explore this potential threat to the exclusion restriction.¹⁴

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 Appendix Table A3. 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 pre-trial and one's own detention outcome.

The second condition needed to interpret our estimates as the LATE of pre-trial release is that the impact of judge assignment on the probability of pre-trial 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

¹⁴Our exclusion restriction would also 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.

weights would not sum to one (Angrist, Imbens, and Rubin 1996, Heckman and Vytlačil 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. Appendix Table A4 and Appendix Table A5 present these first stage results 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.

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, and employment status. 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).

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 bail outcome had their case been assigned to the most lenient judge instead of the most strict judge. In comparison, 53 percent of our sample are “never takers,” meaning that they would be detained by all judges, and 34 percent are “always takers,” meaning that they would be released pre-trial regardless of the judge assigned to the case. Compliers in our sample are 12 percentage points more likely to be charged with a misdemeanor and 15 percentage points more likely to be charged with non-violent offenses compared to the average defendant. Compliers are not systematically different from the average defendant by race, prior criminal history, or baseline employment status, however.

IV. Results

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

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 pre-trial. Columns 2-4 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 and defendant controls: race, gender, age, whether the defendant had a prior 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. Finally, column 4 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, indicators for any W-2 earnings, household income, UI, and EITC, and indicators for missing IRS data. Columns 5-7 report analogous two-stage least squares results where we instrument for pre-trial release within three days using the leave-out measure of judge leniency described in Section III. Robust standard errors two-way clustered at the individual and judge-by-year level are reported throughout.

The OLS estimates show that released defendants have significantly better case outcomes than detained defendants. In all specifications, 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 released pre-trial is 19.3 percentage points less likely to plead guilty, a 43.2 percent decrease from the mean for detained defendants. When we add baseline crime and defendant controls (column 3), the magnitude of the estimate is approximately halved, dropping to 10.1 percentage points. In contrast, adding baseline IRS controls (column 4) does not change the size of the estimate, which remains at 10.1 percentage points. These results suggest that, at least for case outcomes, crime and defendant controls are important for addressing potential omitted variable bias.

The two-stage least squares estimates in columns 5-7 improve upon our OLS estimates by exploiting plausibly exogenous variation in pre-trial release from the quasi-random assignment of cases to bail judges. These two-stage least squares results confirm that defendants released before trial have significantly better case outcomes than otherwise similar defendants detained before trial. With the full set of controls (column 7), we find that the marginal released defendant is 15.1 percentage points

less likely to be found guilty, a 25.8 percent decrease from the mean, and 11.8 percentage points less likely to plead guilty, a 26.4 percent decrease from the mean. These results are consistent with the theory that pre-trial release improves a defendant’s bargaining position in plea negotiations. In Appendix Table A6, 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 3.3 percentage points less likely to be incarcerated after case disposition, a 10.9 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 Appendix Table A7, we also find that pre-trial release significantly reduces the number of days detained prior to disposition by 13.3 days but has no significant effect on the number of days incarcerated after disposition. These findings suggest that pre-trial release primarily reduces time spent in jail at the pre-trial stage.

B. Failures to Appear and Future Crime

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

Panel B of Table 4 examines the impact of pre-trial release on flight in our Philadelphia sample, as we do not observe these measures in our Miami-Dade data. We find that pre-trial release leads to substantial increases in failing to appear for required court appearances. Controlling for our full set of controls (column 7), we find that the marginal released defendant is 15.2 percentage points more likely to fail to appear in court, a 125.6 percent increase from the mean. The probability of fleeing from the jurisdiction also increases by 0.6 percentage points, a 300 percent increase from the detained defendant mean, but the estimate is not statistically significant due to the relative infrequency of this outcome. These findings indicate that pre-trial detention reduces missed court appearances and flight through a largely mechanical incapacitation effect.¹⁵

Panel C of Table 4 presents estimates of the impact of pre-trial 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

¹⁵In Appendix Table A6, we also find that the marginal released defendant waits for an extra 41.8 days between bail and case disposition, a 20.9 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 pre-trial, and the fact that marginal released defendants may wait longer between bail and case disposition because they are less likely to plead guilty.

results suggest no detectable net effect on future crime up to two years after the bail hearing, although the large standard errors make definitive conclusions difficult. We find similar null results on the intensive margin of recidivism using the number of rearrests (see Appendix Table A6).

To better understand this null effect, we estimate the impact of pre-trial 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 7), we find that the marginal released defendant is 13.4 percentage points more likely to be rearrested for a new crime prior to disposition, a 68.4 percent increase from the mean, but 15.0 percentage points less likely to be arrested after case disposition, a 46.9 percent decrease from the mean. Taken together, we interpret these results as suggesting that pre-trial detention has two main opposing effects on future crime. First, pre-trial detention prevents new criminal activity prior to case disposition through a short-run incapacitation effect. Second, pre-trial 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 pre-trial 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, pre-trial 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 towards 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, pre-trial 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 pre-trial 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 335,373 defendants matched to IRS data with cases before 2013, and for outcomes measured over the third to fourth years after the bail hearing, our sample is limited to the 224,319 defendants matched to IRS data with cases before 2011.

The OLS estimates in Table 5 show that 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 7), we find that marginal released defendants are 11.0 percentage points more likely to have any income two years after bail, a 24.1 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, released defendants are 10.8 percentage points more likely to be employed in the formal labor sector, a 28.6 percent increase from the mean. Formal sector earnings are \$1,104 higher per year over the same time period, an 18.8 percent increase from the mean, and the probability of having any income is 10.5 percentage points higher, a 22.8 percent increase from the mean, broadly consistent with the more precise OLS estimates.

A valid question is why we find a significant impact of pre-trial release on the extensive margin of employment, but insignificant effects on the intensive margin. One possible explanation is that our intensive margin estimates are 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 75th 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 7), we find that released defendants are 10.1 percentage points more likely to file a tax return one to two years after the bail hearing, a 24.0 percent increase from the mean. Pre-trial release also increases the receipt of EITC benefits by \$191 per year over the same time period, a 57.9 percent increase. Three to four years after the bail hearing, released defendants are 4.9 percentage points more likely to file a tax return, a 16.1 percent increase from the mean, and receive an additional \$315 in UI benefits and \$220 in EITC benefits per year, 130.2 and 61.8 percent increases from the mean, respectively. These results suggest that pre-trial 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. 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 pre-trial release are generally largest for those without a prior offense in the past year. For individuals without a recent prior offense, released defendants are 20.3 percentage points less likely to be found guilty, 15.3 percentage points less likely to plead guilty, and 16.0 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 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 Appendix Tables A8 and A9, 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,

our results are consistently larger for defendants charged with misdemeanor, drug, and property offenses. Our labor market results are also larger for individuals who were employed prior to the bail hearing. Overall, these results suggest that the social costs imposed by pre-trial detention are larger for those with more limited ties to the criminal justice system and stronger ties to the formal labor sector.

E. Robustness Checks

Threats to Exclusion Restriction: As discussed previously, interpreting our two-stage least squares estimates as the causal impact of pre-trial 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 release without any conditions (ROR) and release with conditions. By separately estimating these two decision margins relative to pre-trial detention, we can test whether our results are driven solely by a defendant being released before trial, or by some combination of pre-trial 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 Pre-Trial Services officer to more intensive conditions like electronic monitoring or home confinement. In Appendix Table A10, we first document a strong first stage relationship between a defendant's pre-trial release conditions and the assigned judge's propensity for release with or without conditions, with judges independently varying across these two margins.

In Appendix Table A11, 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 and without conditions. Our two-stage least squares estimates show no statistically significant differences in the effect of pre-trial release on any of our main outcomes with and without conditions. Our standard errors are also precise enough that we can rule out any large differences by release type. These findings indicate that pre-trial release by itself improves case outcomes, suggesting that the exclusion restriction is unlikely to be violated by release conditions having an independent effect on outcomes.

Alternative Specifications: Appendix Table A12 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 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 ever released pre-trial, and column 4 estimates results on whether the defendant is assigned non-monetary bail. Column 5 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 6 calculates judge leniency based on the scheduled bail judge, which differs from the

assigned bail judge approximately 30 percent of the time, and column 7 presents results using a full set of judge fixed effects as instruments. Appendix Table A13 presents our main results separately for Philadelphia (column 1), Miami-Dade (column 2), and the sample matched to the IRS (column 3). Results across all specifications are similar to our preferred specification. 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: Pre-trial release could improve case outcomes through at least two main channels. First, pre-trial release may strengthen a defendant’s bargaining position during plea negotiations. For example, it is possible that pre-trial 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 pre-trial release affects a defendant’s ability to prepare an adequate defense or negotiate a settlement with prosecutors. The second way that pre-trial 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 pre-trial 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 (Appendix Table A6). The fact that so many of our results are driven by changes in the plea bargaining phase, and not the trial phase, suggests that pre-trial release improves case outcomes primarily through a strengthening of defendants’ bargaining positions.

Future Crime: Pre-trial release may decrease future crime following case disposition through two main channels. First, pre-trial release may decrease crime if pre-trial detention is criminogenic because of harsh prison conditions and negative peer effects (e.g. Chen and Shapiro 2007, Bayer, Hjalmarsson and Pozen 2009). Second, pre-trial release can reduce future crime through an increased likelihood of employment, which subsequently discourages further criminal activity. To assess whether pre-trial 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 Appendix Table A14, we present estimates of the joint probability of future crime and employment in the several years after the bail hearing. We find that in the first two years after the bail

hearing, pre-trial release significantly increases the joint probability of not being rearrested and of being employed. Although less precisely estimated, we find a similar 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 are driven by the same defendants who are employed, suggesting that pre-trial release may decrease future crime through the channel of increased labor market attachment.

Labor Market Outcomes: Pre-trial release could improve labor market outcomes through at least three main channels. First, pre-trial release might increase labor market attachment through a mechanical incapacitation effect since defendants cannot work in the formal sector while detained pre-trial or incarcerated post-conviction. Defendants who are imprisoned are also ineligible to claim UI benefits and EITC benefits for wages earned while incarcerated. Second, pre-trial release might improve 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, pre-trial 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 Appendix Figure A5, we graphically present two-stage least squares estimates of the impact of pre-trial 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, pre-trial 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 pre-trial release on incarceration becomes statistically insignificant from zero. Given that we find evidence that pre-trial 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 pre-trial 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 pre-trial release improves 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 Appendix Table A15, we present estimates of the joint probability of conviction and employment in the several years after the bail hearing. 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. 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. We conclude from these results that pre-trial release primarily affects future

labor market outcomes through the channel of a criminal conviction.

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 pre-trial release significantly decreases the probability of conviction, primarily through a decrease in guilty pleas. Pre-trial release mechanically increases pre-trial 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 pre-trial 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) pre-trial 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 pre-trial 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.

An important open question is whether the benefits of pre-trial 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.¹⁶ See Online Appendix D for a description of this exercise. Based on these tentative calculations, we estimate that the total net benefit of pre-trial release for the marginal defendant is anywhere between \$37,031 and \$40,048. Intuitively, pre-trial 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.¹⁷ These calculations suggest that unless there is a large general deterrence effect of pre-trial detention, which we are unable to measure in our paper, detaining more individuals on the margin is unlikely to be welfare-improving even under the most conservative assumptions.

Instead, our results suggest that it may be welfare-enhancing to use alternatives to pre-trial detention, at least on the margin. For example, Di Tella and Schargrodsky (2013) find that recidi-

¹⁶The welfare implications of an increase in guilty pleas is unclear. On the one hand, if a defendant would have been found guilty at trial and pre-trial 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 pre-trial 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/>).

¹⁷Recall that the benefits of pre-trial 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 pre-trial release is even larger for this subsample.

vism rates are substantially lower among individuals who randomly receive electronic monitoring compared to detention. Electronic monitoring is also substantially less costly compared to jail, with the annual cost of electronic monitoring in the United States at roughly \$3,650 compared to over \$30,000 for incarceration. To the extent that their results extend to our setting, electronic monitoring may provide many of the same benefits of pre-trial 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 pre-trial detention. Second, we are unable to measure the impacts of pre-trial detention on informal sector earnings or consumption. If lost formal sector earnings are largely replaced by informal earnings, the case against pre-trial detention is perhaps weaker. Finally, given these concerns, we are unable to draw any sharp welfare conclusions about the 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.

References

- [1] Abadie, Alberto. 2003. "Semiparametric Instrumental Variable Estimation of Treatment Response Models." *Journal of Econometrics*, 113(2): 231-263.
- [2] Abrams, David S., Marianne Bertrand, and Sendhil Mullainathan. 2012. "Do Judges Vary in Their Treatment of Race?" *Journal of Legal Studies*, 41(2): 347-383.
- [3] Abrams, David S., and Chris Rohlfs. 2011. "Optimal Bail and the Value of Freedom: Evidence From the Philadelphia Bail Experiment." *Economic Inquiry*, 49(3): 750-770.
- [4] Agan, Amanda Y., and Sonja B. Starr. 2016. "Ban the Box, Criminal Records, and Statistical Discrimination: A Field Experiment." *University of Michigan Law & Econ Research Paper No. 16-012*.
- [5] Aizer, Anna, and Joseph Doyle, Jr. 2015. "Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly-Assigned Judges." *Quarterly Journal of Economics*, 130(2): 759-803.
- [6] Alesina, Alberto, and Eliana La Ferrara. 2014. "A Test of Racial Bias in Capital Sentencing." *American Economic Review*, 104(11): 3397-3433.
- [7] Angrist, Joshua, Guido Imbens, and Donald Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association*, 91(434): 444-455.
- [8] Anwar, Shamena, Patrick Bayer, and Randi Hjalmarsen. 2012. "The Impact of Jury Race in Criminal Trials." *Quarterly Journal of Economics*, 127(2): 1017-1055.

- [9] Ares, Charles E., and Anne Rankin, and Herbert Sturz. 1963. "The Manhattan Bail Project: An Interim Report on the Use of Pre-trial Parole." *New York University Law Review* 38(1): 67-95.
- [10] Autor, David, Andreas Ravndal Kostol, and Magne Mogstad. 2015. "Disability Benefits, Consumption Insurance, and Household Labor Supply." MIT Working Paper.
- [11] Ayres, Ian, and Joel Waldfogel. 1994. "A Market Test for Race Discrimination in Bail Setting." *Stanford Law Review*, 46(5): 987-1047.
- [12] Bayer, Patrick, Randi Hjalmarsson, and David Pozen. 2009. "Building Criminal Capital Behind Bars: Peer Effects in Juvenile Corrections." *Quarterly Journal of Economics*, 124(1): 105-147.
- [13] Bhuller, Manudeep, Gordon B. Dahl, Katrine V. Loken, and Magne Mogstad. 2016. "Incarceration, Recidivism and Employment." Unpublished Working Paper.
- [14] Bushway, Shawn D., and Jonah B. Gelbach. 2011. "Testing for Racial Discrimination in Bail Setting Using Nonparametric Estimation of a Parametric Model." Unpublished Working Paper.
- [15] Chang, Tom, and Antoinette Schoar. 2008. "Judge Specific Differences in Chapter 11 and Firm Outcomes." Unpublished Working Paper.
- [16] Chen, M. Keith, and Jesse Shapiro. 2007. "Do Harsher Prison Conditions Reduce Recidivism? A Discontinuity-based Approach." *American Law and Economics Review*, 9(1):1-29.
- [17] Chetty, Raj, John N. Friedman, and Jonah Rockoff. 2014. "Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates." *American Economic Review*, 104(9): 2593-2632.
- [18] Clarke, Stevens H., Jean L. Freeman, and Gary G. Koch. 1976. "Bail Risk: A Multivariate Analysis." *Journal of Legal Studies*, 5(2):341-385.
- [19] Cohen, Thomas H., and Brian A. Reaves. 2007. "State Court Processing Statistics, 1990-2004: Pretrial Release of Felony Defendants in State Courts." Washington, DC: U.S. Department of Justice.
- [20] Dahl, Gordon B., Andreas Ravndal Kostol, and Magne Mogstad. 2014. "Family Welfare Cultures." *Quarterly Journal of Economics*, 129 (4): 1711-1752.
- [21] Department of Justice. 2016. Brief for the United States as Amicus Curiae in *Maurice Walker v. City of Calhoun*, Georgia.
- [22] Di Tella, Rafael, and Ernesto Schargrofsky. 2013. "Criminal Recidivism after Prison and Electronic Monitoring." *Journal of Political Economy*, 121(1): 28-73.
- [23] Dobbie, Will, Paul Goldsmith-Pinkham, and Crystal Yang. 2015. "Consumer Bankruptcy and Financial Health." NBER Working Paper No. 21032.

- [24] Dobbie, Will, and Jae Song. 2015. "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection." *American Economic Review*, 105(3): 1272-1311.
- [25] Doyle, Joseph. 2007. "Child Protection and Child Outcomes: Measuring the Effects of Foster Care." *American Economic Review*, 97(5): 1583-1610.
- [26] Doyle, Joseph. 2008. "Child Protection and Adult Crime: Using Investigator Assignment to Estimate Causal Effects of Foster Care." *Journal of Political Economy*, 116(4): 746-770.
- [27] French, Eric, and Jae Song. 2014. "The Effect of Disability Insurance Receipt on Labor Supply." *American Economic Journal: Economic Policy*, 6(2): 291-337.
- [28] Foote, Caleb. 1954. "Compelling Appearance in Court: Administration of Bail in Philadelphia." *University of Pennsylvania Law Review*, 102: 1031-1079.
- [29] Goldkamp, J. S. 1980. "The Effects of Detention on Judicial Decisions: A Closer Look." *Justice System Journal*, 5: 234-257.
- [30] Goldkamp, J. S., and M.R. Gottfredson. 1988. "Development of Bail/Pretrial Release Guidelines in Maricopa County Superior Court, Dade County Circuit Court and Boston Municipal Court. Washington, D.C.:National Institute of Justice.
- [31] Gould, Eric D., Bruce A. Weinberg, and David B. Mustard. 2002. "Crime Rates and Local Labor Market Opportunities in the United States: 1979-1997." *The Review of Economics and Statistics*, 84(1): 45-61.
- [32] Grogger, Jeffrey. 1998. "Market Wages and Youth Crime." *Journal of Labor Economics*, 16(4): 756-791.
- [33] Gupta, Arpit, Christopher Hansman, and Ethan Frenchman. 2016. "The Heavy Costs of High Bail: Evidence from Judge Randomization." Unpublished Working Paper.
- [34] Heckman, James, and Edward Vytlacil. 2005. "Structural Equations, Treatment Effects, and Econometric Policy Evaluation." *Econometrica*, 73(3): 669-738.
- [35] Kling, Jeffrey. 2006. "Incarceration Length, Employment, and Earnings." *American Economic Review*, 96(3): 863-876.
- [36] Landes, William M. 1973. "The Bail System: An Economic Approach." *Journal of Legal Studies*, 2: 79-105.
- [37] Leslie, Emily, and Nolan G. Pope. 2016. "The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from NYC Arraignments." Unpublished Working Paper.
- [38] Maestas, Nicole, Kathleen Mullen, and Alexander Strand. 2013. "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt." *American Economic Review*, 103(5): 1797-1829.
- [39] McIntyre, Frank, and Shima Baradaran. 2013. "Race, Prediction, and Pretrial Detention." *Journal of Empirical Legal Studies*, 10(4): 741-770.

- [40] Mueller-Smith, Michael. 2015. "The Criminal and Labor Market Impacts of Incarceration." Unpublished Working Paper.
- [41] Myers, Samuel L., Jr. 1981. "The Economics of Bail Jumping." *Journal of Legal Studies*, 10(2): 381-396.
- [42] Oleson, James C., Christopher T. Lowenkamp, John Wooldredge, Marie VanNostrand, and Timothy P. Cadigan. 2014. "The Sentencing Consequences of Federal Pretrial Supervision." *Crime & Delinquency*: 1-21.
- [43] Pager, Devah. 2003. "The Mark of a Criminal Record." *American Journal of Sociology*, 108: 937-975.
- [44] Phillips, Mary T. 2008. "Bail, Detention, and Felony Case Outcomes." New York Criminal Justice Agency, Inc.
- [45] Raphael, Steven, and Rudolf Winter-Ebmer. 2001. "Identifying the Effect of Unemployment on Crime." *Journal of Law and Economics*, 44(1): 259-283.
- [46] Reaves, Brian A. 2013. "Felony Defendants in Large Urban Counties, 2009 - Statistical Tables." Washington, DC: U.S. Department of Justice.
- [47] Rehavi, M. Marit, and Sonja B. Starr. 2014. "Racial Disparity in Federal Criminal Sentences." *Journal of Political Economy*, 122(6): 1320-1354.
- [48] Shubik-Richards, Claire and Don Stemen. 2010. "Philadelphia's Crowded, Costly Jails: The Search for Safe Solutions." Technical Report, Pew Charitable Trusts Philadelphia Research Initiative.
- [49] Stevenson, Megan. 2016. "Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes." Unpublished Working Paper.
- [50] Travis, Jeremy, Bruce Western, and Steve Redburn, eds. 2014. "The Growth of Incarceration in the United States: Exploring Causes and Consequences." The National Academies Press: Washington, D.C.
- [51] Walmsley, Roy. 2013. "World Prison Population List (tenth edition)," International Centre for Prison Studies, London, United Kingdom.
- [52] Williams, Marian R. 2003. "The Effect of Pretrial Detention on Imprisonment Decisions." *Criminal Justice Review*, 28: 299-316.

Table 1: Descriptive Statistics

	Initial Bail Decision	
	Detained	Released
	(1)	(2)
<i>Panel A: Bail Type</i>		
Release on Recognizance	0.018	0.366
Non-Monetary Bail	0.039	0.216
Monetary Bail	0.943	0.417
Bail Amount (in thousands)	48.318	12.479
<i>Panel B: Subsequent Bail Outcomes</i>		
Bail Modification Petition	0.435	0.072
Released in 14 Days	0.093	1.000
Released Before Trial	0.366	0.999
<i>Panel C: Defendant Characteristics</i>		
Male	0.877	0.785
White	0.383	0.424
Black	0.607	0.556
Age at Bail Decision	33.931	33.465
Prior Offense in Past Year	0.355	0.199
Baseline Earnings (thousands)	4.511	7.205
Baseline Employed	0.319	0.423
Baseline Any Income	0.771	0.814
<i>Panel D: Charge Characteristics</i>		
Number of Offenses	3.748	2.513
Felony Offense	0.628	0.327
Misdemeanor Only	0.372	0.673
Any Drug Offense	0.284	0.420
Any DUI Offense	0.025	0.116
Any Violent Offense	0.292	0.192
Any Property Offense	0.344	0.185
<i>Panel E: Outcomes</i>		
Any Guilty Offense	0.585	0.487
Guilty Plea	0.447	0.208
Any Incarceration	0.304	0.146
Failure to Appear in Court	0.121	0.179
Rearrest in 0-2 Years	0.470	0.404
Earnings (thousands) in 1-2 Years	5.212	7.891
Employed in 1-2 Years	0.377	0.509
Any Income in 1-2 Years	0.457	0.521
Earnings (thousands) in 3-4 Years	5.873	8.354
Employed in 3-4 Years	0.377	0.482
Any Income in 3-4 Years	0.461	0.507
Observations	185,575	236,275

Note: 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 data appendix for additional details on the sample and variable construction.

Table 2: Judge Leniency and Pre-Trial Release

	Sample	Judge Leniency		
	Mean	(2)	(3)	(4)
<i>Panel A: Initial Release</i>				
Released in 3 Days	0.560 (0.496)	0.687*** (0.031)	0.685*** (0.030)	0.682*** (0.030)
<i>Panel B: Subsequent Bail Outcomes</i>				
Bail Modification Petition	0.208 (0.406)	-0.407*** (0.042)	-0.407*** (0.046)	-0.407*** (0.045)
Released in 14 Days	0.601 (0.490)	0.680*** (0.027)	0.678*** (0.027)	0.675*** (0.027)
Released Before Trial	0.720 (0.449)	0.584*** (0.028)	0.580*** (0.027)	0.577*** (0.027)
Court x Year FE	-	Yes	Yes	Yes
Crime Controls	-	No	Yes	Yes
IRS Controls	-	No	No	Yes
Observations	421,850	421,850	421,850	421,850

Note: 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 crime and defendant baseline controls: defendant race, defendant gender, defendant age, whether the defendant had a prior offense with 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, and indicators for missing characteristics. Column 4 adds a full set of baseline IRS controls: 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 income data. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses in columns 2-4. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3: Test of Randomization

	Pre-Trial Release	Judge Leniency
	(1)	(2)
Male	-0.11630*** (0.00348)	-0.00001 (0.00015)
Black	-0.03965*** (0.00359)	0.00005 (0.00017)
Age at Bail Decision	-0.01278*** (0.00144)	-0.00006 (0.00005)
Prior Offense in Past Year	-0.15411*** (0.00407)	0.00012 (0.00011)
Number of Offenses	-0.02437*** (0.00067)	-0.00000 (0.00002)
Felony Offense	-0.25420*** (0.01177)	0.00010 (0.00010)
Any Drug Offense	0.12182*** (0.00415)	0.00015 (0.00017)
Any DUI Offense	0.10681*** (0.00886)	0.00019 (0.00021)
Any Violent Offense	-0.01561** (0.00792)	0.00009 (0.00016)
Any Property Offense	0.01053 (0.00780)	-0.00026 (0.00017)
Matched to IRS Data	0.00858*** (0.00212)	-0.00002 (0.00014)
Baseline Earnings	0.00111*** (0.00008)	-0.00001 (0.00001)
Baseline UI	0.00289*** (0.00040)	-0.00002 (0.00002)
Baseline EITC	0.01227*** (0.00110)	0.00002 (0.00007)
Baseline Filed Return	0.05002*** (0.00351)	-0.00020 (0.00017)
Baseline Employed	0.02521*** (0.00244)	0.00018 (0.00013)
Baseline Any EITC	-0.01746*** (0.00395)	0.00004 (0.00022)
Baseline Any Income	0.00000 (0.00000)	0.00000 (0.00000)
Baseline Any UI	0.02350*** (0.00440)	0.00029 (0.00025)
Joint F-Test	[0.00000]	[0.72374]
Observations	421,850	421,850

Note: 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 pre-trial 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. The p-value reported at the bottom of columns 1-2 is for a F-test of the joint significance of the variables listed in the rows. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the data appendix for additional details on the sample and variable construction.

Table 4: Pre-Trial Release and Criminal Outcomes

	Detained			OLS Results			2SLS Results		
	Mean	(2)	(3)	(4)	(5)	(6)	(7)		
<i>Panel A: Case Outcomes</i>									
Any Guilty Offense	0.585 (0.493)	-0.079*** (0.006)	-0.062*** (0.004)	-0.060*** (0.004)	-0.140*** (0.041)	-0.152*** (0.038)	-0.151*** (0.038)		
Guilty Plea	0.447 (0.497)	-0.193*** (0.006)	-0.101*** (0.006)	-0.101*** (0.006)	-0.108** (0.044)	-0.118*** (0.041)	-0.118*** (0.041)		
Any Incarceration	0.304 (0.460)	-0.165*** (0.009)	-0.110*** (0.004)	-0.107*** (0.004)	-0.018 (0.030)	-0.034 (0.029)	-0.033 (0.030)		
<i>Panel B: Court Process Outcomes</i>									
Failure to Appear in Court	0.121 (0.326)	0.063*** (0.005)	0.006 (0.008)	0.010 (0.008)	0.154*** (0.032)	0.151*** (0.031)	0.152*** (0.031)		
Absconded	0.002 (0.045)	0.005*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.006 (0.005)	0.006 (0.005)	0.006 (0.005)		
<i>Panel C: Future Crime</i>									
Rearrest in 0-2 Years	0.470 (0.499)	-0.050*** (0.007)	-0.025*** (0.004)	-0.018*** (0.004)	-0.046 (0.053)	-0.045 (0.053)	-0.041 (0.053)		
Rearrest Prior to Disposition	0.196 (0.397)	0.043*** (0.007)	0.057*** (0.005)	0.060*** (0.005)	0.132*** (0.044)	0.132*** (0.045)	0.134*** (0.044)		
Rearrest After Disposition	0.320 (0.467)	-0.071*** (0.004)	-0.054*** (0.003)	-0.049*** (0.003)	-0.157*** (0.045)	-0.152*** (0.045)	-0.150*** (0.045)		
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes		
Crime Controls	-	No	Yes	Yes	No	Yes	Yes		
IRS Controls	-	No	No	Yes	No	No	Yes		
Observations	185,575	421,850	421,850	421,850	421,850	421,850	421,850		

Note: 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 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-by-year level are reported in parentheses in columns 2-7. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5: Pre-Trial Release and Labor Market Outcomes

	Detained						
	OLS Results			2SLS Results			
Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Years 1-2</i>							
Earnings (thousands)	5.212 (15.178)	2.686*** (0.077)	2.238*** (0.082)	0.395*** (0.053)	0.602 (1.485)	1.028 (1.464)	-0.324 (0.918)
Household Income (thousands)	10.170 (22.818)	2.699*** (0.135)	2.199*** (0.128)	0.237*** (0.089)	2.109 (1.977)	2.820 (1.936)	-0.051 (1.477)
Employed	0.377 (0.485)	0.135*** (0.002)	0.102*** (0.002)	0.051*** (0.002)	0.089* (0.047)	0.081* (0.047)	0.050 (0.041)
Any Income	0.457 (0.498)	0.104*** (0.003)	0.081*** (0.003)	0.036*** (0.003)	0.141*** (0.047)	0.134*** (0.047)	0.110*** (0.044)
<i>Panel B: Years 3-4</i>							
Earnings (thousands)	5.873 (15.884)	2.418*** (0.103)	1.984*** (0.095)	0.200*** (0.067)	0.594 (1.680)	1.459 (1.754)	1.104 (1.180)
Household Income (thousands)	10.921 (23.968)	2.440*** (0.172)	1.946*** (0.156)	0.020 (0.103)	1.915 (2.225)	2.897 (2.291)	0.520 (1.762)
Employed	0.377 (0.485)	0.105*** (0.003)	0.078*** (0.003)	0.032*** (0.003)	0.121** (0.051)	0.125** (0.052)	0.108*** (0.049)
Any Income	0.461 (0.498)	0.090*** (0.004)	0.067*** (0.004)	0.030*** (0.003)	0.130*** (0.050)	0.128** (0.051)	0.105*** (0.049)
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	-	No	Yes	Yes	No	Yes	Yes
IRS Controls	-	No	No	Yes	No	No	Yes
Observations	143,089	335,373	335,373	335,373	335,373	335,373	335,373

Note: 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 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-by-year level are reported in parentheses in columns 2-7. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6: Pre-Trial Release and Social Benefits Take-Up

	Detained			OLS Results			2SLS Results		
	Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
<i>Panel A: Years 1-2</i>									
Filed Return	0.420 (0.494)	0.092*** (0.005)	0.072*** (0.004)	0.032*** (0.003)	0.131*** (0.036)	0.128*** (0.035)	0.101*** (0.034)		
UI (thousands)	0.279 (1.524)	0.417*** (0.028)	0.381*** (0.028)	0.208*** (0.017)	0.175 (0.149)	0.190 (0.147)	0.068 (0.145)		
EITC (thousands)	0.330 (0.947)	0.191*** (0.006)	0.157*** (0.004)	0.094*** (0.004)	0.249** (0.110)	0.239** (0.110)	0.191* (0.100)		
Any UI	0.066 (0.248)	0.068*** (0.003)	0.059*** (0.002)	0.030*** (0.002)	0.059** (0.024)	0.060** (0.024)	0.037* (0.022)		
Any EITC	0.218 (0.413)	0.070*** (0.002)	0.056*** (0.002)	0.033*** (0.002)	0.119** (0.047)	0.113** (0.046)	0.101** (0.043)		
<i>Panel B: Years 3-4</i>									
Filed Return	0.304 (0.460)	0.057*** (0.006)	0.044*** (0.004)	0.019*** (0.002)	0.071** (0.030)	0.069** (0.030)	0.049* (0.029)		
UI (thousands)	0.242 (1.324)	0.279*** (0.023)	0.256*** (0.025)	0.156*** (0.017)	0.319* (0.175)	0.356** (0.177)	0.315* (0.173)		
EITC (thousands)	0.356 (0.996)	0.180*** (0.006)	0.142*** (0.005)	0.092*** (0.005)	0.310** (0.141)	0.282** (0.140)	0.220* (0.129)		
Any UI	0.064 (0.245)	0.055*** (0.003)	0.048*** (0.003)	0.029*** (0.002)	0.028 (0.030)	0.031 (0.031)	0.021 (0.030)		
Any EITC	0.232 (0.422)	0.057*** (0.003)	0.043*** (0.002)	0.026*** (0.002)	0.137*** (0.049)	0.131*** (0.050)	0.114** (0.049)		
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes		
Crime Controls	-	No	Yes	Yes	No	Yes	Yes		
IRS Controls	-	No	No	Yes	No	No	Yes		
Observations	143,089	335,373	335,373	335,373	335,373	335,373	335,373		

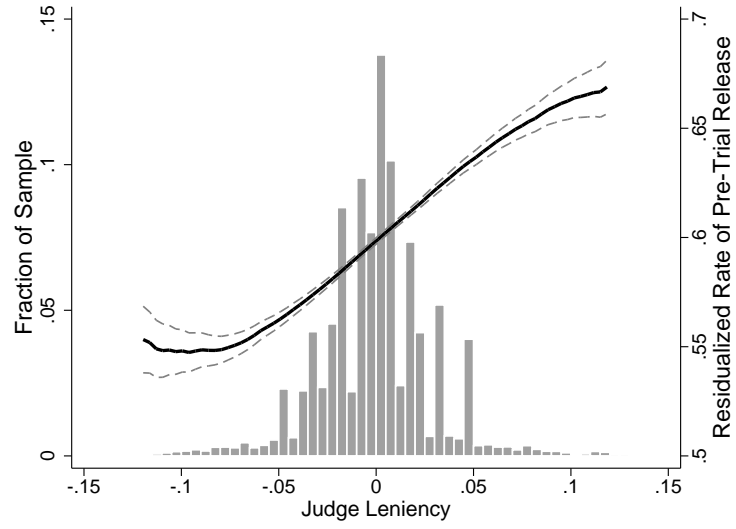
Note: This table reports OLS and two-stage least squares results of the impact of pre-trial release. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Two-stage least squares models instrument for pre-trial detention using 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-by-year level are reported in parentheses in columns 2-7. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7: Results by Prior Criminal History

	No Priors	Priors	p-value
	(1)	(2)	(3)
Any Guilty Offense	-0.203*** (0.046) [0.499]	-0.050 (0.055) [0.615]	0.052
Guilty Plea	-0.153*** (0.046) [0.283]	-0.053 (0.057) [0.394]	0.275
Any Incarceration	-0.012 (0.037) [0.191]	-0.068 (0.051) [0.283]	0.236
Failure to Appear in Court	0.139*** (0.040) [0.149]	0.181*** (0.031) [0.181]	0.023
Rearrest in 0-2 Years	-0.059 (0.059) [0.366]	0.018 (0.099) [0.624]	0.443
Rearrest Prior to Disposition	0.136*** (0.047) [0.191]	0.134 (0.103) [0.304]	0.785
Rearrest After Disposition	-0.167*** (0.050) [0.223]	-0.107 (0.087) [0.417]	0.382
Employed in 1-2 Years	0.085 (0.053) [0.487]	-0.033 (0.065) [0.360]	0.185
Any Income in 1-2 Years	0.166*** (0.052) [0.507]	-0.001 (0.073) [0.458]	0.074
Employed in 3-4 Years	0.126** (0.061) [0.465]	0.051 (0.086) [0.365]	0.447
Any Income in 3-4 Years	0.160*** (0.057) [0.509]	-0.018 (0.107) [0.428]	0.108
Court x Year FE	Yes	Yes	-
Crime Controls	Yes	Yes	-
IRS Controls	Yes	Yes	-
Observations	309,003	112,847	-

Note: This table reports two-stage least squares results of the impact of pre-trial 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 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. 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-by-year level are reported in parentheses and the mean of the dependent variable is reported in brackets in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

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.

Appendix A: Additional Results
[FOR ONLINE PUBLICATION ONLY]

Table A1: Descriptive Statistics by Bail Type

	Initial Bail Decision		
	ROR	Non-Monetary	Monetary
	(1)	(2)	(3)
<i>Panel A: Detention Outcomes</i>			
Released in 3 Days	0.963	0.877	0.402
Bail Modification Petition	0.058	0.071	0.295
Released in 14 Days	0.967	0.898	0.466
Released Before Trial	1.000	0.952	0.648
<i>Panel B: Defendant Characteristics</i>			
Male	0.765	0.785	0.847
White	0.422	0.433	0.381
Black	0.555	0.557	0.605
Age at Bail Decision	34.157	33.974	33.223
Prior Offense in Past Year	0.148	0.294	0.293
Baseline Earnings (thousands)	7.446	5.259	5.760
Baseline Employed	0.432	0.369	0.367
Baseline Any Income	0.817	0.805	0.791
<i>Panel C: Charge Characteristics</i>			
Number of Offenses	1.887	2.039	3.744
Felony Offense	0.134	0.414	0.629
Misdemeanor Only	0.866	0.586	0.371
Any Drug Offense	0.490	0.505	0.303
Any DUI Offense	0.190	0.050	0.045
Any Violent Offense	0.042	0.091	0.351
Any Property Offense	0.165	0.275	0.266
<i>Panel D: Outcomes</i>			
Any Guilty Offense	0.516	0.577	0.499
Guilty Plea	0.140	0.266	0.346
Any Incarceration	0.132	0.186	0.270
Failure to Appear in Court	0.251	0.263	0.099
Rearrest in 0-2 Years	0.378	0.496	0.425
Earnings (thousands) in 1-2 Years	8.015	5.891	6.487
Employed in 1-2 Years	0.514	0.444	0.439
Any Income in 1-2 Years	0.510	0.496	0.489
Earnings (thousands) in 3-4 Years	8.658	6.396	7.075
Employed in 3-4 Years	0.495	0.421	0.428
Any Income in 3-4 Years	0.504	0.494	0.484
Observations	89,867	58,300	268,293

Note: This table reports descriptive statistics by bail decision. The sample is described in the notes to Table 1. See the data appendix for additional details on the sample and variable construction.

Table A2: Predictors of Pre-Trial Release

	(1)	(2)	(3)	(4)
Residualized ROR Rate	0.019 (0.041)			
Residualized Non-Monetary Rate		0.148*** (0.032)		
Residualized Monetary Rate			-0.483*** (0.032)	
Residualized Monetary Amount				-0.003*** (0.001)
Observations	421,850	421,850	421,850	421,850

Note: This table reports OLS estimates of pre-trial release on residualized judge leniency measures separately calculated for each bail type. 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 the same year following the procedure described in Section III. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A3: Reduced Form Effect of Judge Leniency

	Detained			
	Mean	OLS Results		
	(1)	(2)	(3)	(4)
Any Guilty Offense	0.585 (0.493)	-0.096*** (0.028)	-0.105*** (0.026)	-0.103*** (0.026)
Guilty Plea	0.447 (0.497)	-0.074** (0.030)	-0.081*** (0.028)	-0.080*** (0.028)
Any Incarceration	0.304 (0.460)	-0.012 (0.021)	-0.024 (0.020)	-0.022 (0.020)
Failure to Appear in Court	0.121 (0.326)	0.143*** (0.029)	0.140*** (0.028)	0.140*** (0.028)
Rearrest in 0-2 Years	0.470 (0.499)	-0.029 (0.034)	-0.028 (0.033)	-0.025 (0.033)
Rearrest Prior to Disposition	0.196 (0.397)	0.084*** (0.029)	0.081*** (0.028)	0.082*** (0.028)
Rearrest After Disposition	0.320 (0.467)	-0.099*** (0.030)	-0.094*** (0.029)	-0.092*** (0.029)
Employed in 1-2 Years	0.377 (0.485)	0.059* (0.031)	0.053* (0.030)	0.032 (0.027)
Any Income in 1-2 Years	0.457 (0.498)	0.093*** (0.029)	0.088*** (0.029)	0.071*** (0.027)
Employed in 3-4 Years	0.377 (0.485)	0.081** (0.035)	0.081** (0.034)	0.069** (0.032)
Any Income in 3-4 Years	0.461 (0.498)	0.087*** (0.033)	0.083** (0.033)	0.067** (0.031)
Court x Year FE	-	Yes	Yes	Yes
Crime Controls	-	No	Yes	Yes
IRS Controls	-	No	No	Yes
Observations	185,575	421,850	421,850	421,850

Note: This table reports reduced form OLS estimates of case outcomes on our residualized judge leniency measure described in Section III. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses in columns 2-4. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A4: First Stage Results by Case Characteristics

	Crime Severity		Crime Type			
	Misd.	Felony	Drug	DUI	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Initial Release</i>						
Released in 3 Days	0.853*** (0.053) [0.697]	0.505*** (0.047) [0.399]	0.700*** (0.065) [0.647]	1.286*** (0.305) [0.856]	0.932*** (0.084) [0.402]	0.114* (0.065) [0.494]
<i>Panel B: Subsequent Bail Outcomes</i>						
Bail Modification Petition	-0.508*** (0.043) [0.135]	-0.272*** (0.084) [0.300]	-0.444*** (0.061) [0.179]	-0.676*** (0.156) [0.135]	-0.978*** (0.125) [0.268]	0.013 (0.076) [0.226]
Released in 14 Days	0.842*** (0.052) [0.727]	0.500*** (0.042) [0.453]	0.672*** (0.061) [0.680]	1.153*** (0.281) [0.878]	0.926*** (0.078) [0.439]	0.203*** (0.057) [0.555]
Released Before Trial	0.716*** (0.050) [0.793]	0.432*** (0.040) [0.636]	0.604*** (0.056) [0.798]	0.360*** (0.077) [0.967]	0.742*** (0.066) [0.587]	0.174*** (0.052) [0.721]
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	Yes	Yes	Yes	Yes	Yes	Yes
IRS Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	227,998	193,852	115,745	20,786	78,836	72,637

Note: This table reports first stage results for selected case types. The regressions are estimated on the sample as described in the notes to Table 1. Judge leniency is estimated using all 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-by-year level are reported in parentheses and the mean of the dependent variable is reported in brackets in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A5: First Stage Results by Defendant Characteristics

	No Prior	Prior	Black	White	Employed	Non-Emp.
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Initial Release</i>						
Released in 3 Days	0.641***	0.786***	0.671***	0.601***	0.635***	0.718***
	(0.032)	(0.081)	(0.038)	(0.048)	(0.040)	(0.045)
	[0.612]	[0.417]	[0.457]	[0.500]	[0.628]	[0.524]
<i>Panel B: Subsequent Bail Outcomes</i>						
Bail Modification Petition	-0.363***	-0.551***	-0.381***	-0.549***	-0.430***	-0.394***
	(0.046)	(0.082)	(0.078)	(0.068)	(0.043)	(0.052)
	[0.195]	[0.246]	[0.254]	[0.221]	[0.185]	[0.220]
Released in 14 Days	0.628***	0.793***	0.691***	0.618***	0.607***	0.707***
	(0.028)	(0.077)	(0.039)	(0.048)	(0.041)	(0.043)
	[0.653]	[0.460]	[0.498]	[0.540]	[0.668]	[0.565]
Released Before Trial	0.527***	0.700***	0.612***	0.518***	0.494***	0.602***
	(0.033)	(0.055)	(0.040)	(0.045)	(0.039)	(0.041)
	[0.763]	[0.605]	[0.663]	[0.678]	[0.776]	[0.696]
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	Yes	Yes	Yes	Yes	Yes	Yes
IRS Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	309,003	112,847	153,014	103,035	159,173	176,200

Note: This table reports first stage results for selected defendant types. The regressions are estimated on the sample as described in the notes to Table 1. Judge leniency is estimated using all 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-by-year level are reported in parentheses and the mean of the dependent variable is reported in brackets in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A6: Additional Results

	OLS Results			2SLS Results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Detained							
Mean							
Number of Guilty Offenses	0.950 (1.763)	-0.256*** (0.009)	-0.096*** (0.006)	-0.093*** (0.006)	-0.182** (0.073)	-0.198*** (0.068)	-0.197*** (0.068)
Guilty Plea to Time Served	0.215 (0.411)	-0.143*** (0.003)	-0.065*** (0.003)	-0.063*** (0.003)	-0.125* (0.071)	-0.116* (0.069)	-0.116* (0.069)
Offense Charged Down	0.459 (0.498)	0.026*** (0.006)	0.067*** (0.004)	0.064*** (0.004)	0.098** (0.041)	0.105*** (0.039)	0.104*** (0.040)
Days to Case Decision	199.745 (249.782)	13.455*** (5.021)	34.282*** (3.405)	34.225*** (3.502)	46.954*** (17.418)	41.879** (18.059)	41.774** (18.150)
Rearrest Counts in 0-2 Years	2.650 (4.620)	-0.509*** (0.054)	-0.180*** (0.035)	-0.129*** (0.036)	-0.355 (0.426)	-0.315 (0.426)	-0.292 (0.427)
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	-	No	Yes	Yes	No	Yes	Yes
IRS Controls	-	No	No	Yes	No	No	Yes
Observations	185,575	421,850	421,850	421,850	421,850	421,850	421,850

Note: This table reports additional 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 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-by-year level are reported in parentheses in columns 2-7. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A7: Pre-Trial Release and Number of Days Detained

	Detained Mean		OLS Results			2SLS Results		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Days Detained Prior to Disposition	40.829 (110.548)	-36.376*** (2.353)	-35.574*** (2.175)	-35.724*** (2.183)	-12.133** (5.452)	-13.333** (5.447)	-13.282** (5.466)	
Days Detained After Disposition	330.355 (752.202)	-294.504*** (8.208)	-147.869*** (3.216)	-145.614*** (3.267)	-44.931 (41.675)	-46.227 (40.561)	-44.895 (40.626)	
Days Detained Ever	371.184 (779.152)	-330.880*** (9.773)	-183.443*** (4.611)	-181.338*** (4.672)	-57.064 (44.769)	-59.560 (43.889)	-58.178 (43.960)	
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes	
Crime Controls	-	No	Yes	Yes	No	Yes	Yes	
IRS Controls	-	No	No	Yes	No	No	Yes	
Observations	161,400	370,427	370,427	370,427	370,427	370,427	370,427	

Note: This table reports additional 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 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-by-year level are reported in parentheses in columns 2-7. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A8: Results by Crime Characteristics

	Crime Severity		Crime Type			
	Misd.	Felony	Drug	DUI	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Any Guilty Offense	-0.187*** (0.046) [0.509]	-0.094 (0.072) [0.554]	-0.099 (0.070) [0.678]	0.079 (0.109) [0.528]	-0.125** (0.054) [0.561]	-0.744 (0.542) [0.293]
Guilty Plea	-0.181*** (0.054) [0.214]	-0.013 (0.070) [0.429]	-0.087 (0.073) [0.354]	-0.017 (0.117) [0.264]	-0.092* (0.054) [0.421]	-0.340 (0.356) [0.201]
Any Incarceration	0.016 (0.023) [0.112]	-0.119* (0.068) [0.337]	-0.114** (0.058) [0.259]	0.106 (0.121) [0.430]	0.000 (0.042) [0.228]	-0.385 (0.379) [0.149]
Failure to Appear in Court	0.118*** (0.045) [0.201]	0.210*** (0.038) [0.101]	0.194** (0.092) [0.237]	0.097 (0.084) [0.182]	0.140*** (0.033) [0.187]	0.126 (0.370) [0.082]
Rearrest in 0-2 Years	-0.086 (0.060) [0.434]	0.019 (0.090) [0.432]	-0.058 (0.079) [0.487]	0.076 (0.207) [0.280]	-0.156* (0.087) [0.529]	0.112 (0.367) [0.339]
Rearrest Prior to Disposition	0.112*** (0.043) [0.188]	0.166* (0.093) [0.259]	0.132 (0.088) [0.269]	0.045 (0.208) [0.181]	0.067 (0.083) [0.255]	0.330 (0.330) [0.153]
Rearrest After Disposition	-0.179*** (0.053) [0.316]	-0.112 (0.076) [0.222]	-0.166** (0.081) [0.287]	0.076 (0.104) [0.140]	-0.167** (0.072) [0.354]	-0.220 (0.291) [0.224]
Employed in 1-2 Years	0.036 (0.043) [0.481]	0.064 (0.072) [0.418]	0.152* (0.080) [0.424]	-0.060 (0.080) [0.601]	0.014 (0.066) [0.396]	0.471 (0.354) [0.491]
Any Income in 1-2 Years	0.124** (0.050) [0.509]	0.089 (0.071) [0.476]	0.141* (0.078) [0.455]	-0.025 (0.093) [0.591]	0.046 (0.077) [0.481]	0.659* (0.337) [0.524]
Employed in 3-4 Years	0.093* (0.056) [0.458]	0.128 (0.084) [0.414]	0.122 (0.103) [0.407]	-0.050 (0.182) [0.568]	-0.080 (0.095) [0.386]	0.875** (0.416) [0.469]
Any Income in 3-4 Years	0.064 (0.053) [0.492]	0.153* (0.090) [0.483]	0.029 (0.094) [0.457]	-0.195 (0.166) [0.556]	0.088 (0.101) [0.473]	0.666* (0.398) [0.500]
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	Yes	Yes	Yes	Yes	Yes	Yes
IRS Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	227,998	193,852	115,745	20,786	72,637	34,313

Note: This table reports two-stage least squares results of the impact of pre-trial release for selected case types. 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 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, crime controls, and IRS controls. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses and the mean of the dependent variable is reported in brackets in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A9: Results by Defendant Characteristics

	Black	White	Not Emp.	Employed
	(1)	(2)	(3)	(4)
Any Guilty Offense	-0.095*	-0.252***	-0.093*	-0.182***
	(0.056)	(0.073)	(0.056)	(0.061)
	[0.574]	[0.592]	[0.552]	[0.506]
Guilty Plea	-0.119**	-0.189**	-0.101*	-0.109*
	(0.059)	(0.076)	(0.056)	(0.063)
	[0.383]	[0.392]	[0.326]	[0.292]
Any Incarceration	-0.050	-0.054	-0.017	-0.080*
	(0.041)	(0.057)	(0.043)	(0.048)
	[0.280]	[0.244]	[0.233]	[0.197]
Failure to Appear in Court	0.213***	0.165**	0.139***	0.114***
	(0.044)	(0.073)	(0.039)	(0.040)
	[0.201]	[0.209]	[0.171]	[0.143]
Rearrest in 0-2 Years	-0.222***	0.027	-0.038	-0.086
	(0.085)	(0.107)	(0.072)	(0.076)
	[0.497]	[0.435]	[0.498]	[0.400]
Rearrest Prior to Disposition	-0.027	0.186*	0.098	0.144**
	(0.083)	(0.095)	(0.066)	(0.068)
	[0.242]	[0.203]	[0.246]	[0.211]
Rearrest After Disposition	-0.194**	-0.105	-0.139**	-0.205***
	(0.079)	(0.088)	(0.062)	(0.066)
	[0.316]	[0.284]	[0.324]	[0.244]
Employed in 1-2 Year	0.080	0.072	0.033	0.059
	(0.067)	(0.086)	(0.052)	(0.057)
	[0.404]	[0.444]	[0.235]	[0.690]
Any Income in 1-2 Years	0.115*	0.222**	0.085	0.136**
	(0.067)	(0.106)	(0.065)	(0.053)
	[0.478]	[0.488]	[0.355]	[0.646]
Employed in 3-4 Years	0.023	0.068	0.087	0.131*
	(0.096)	(0.122)	(0.066)	(0.072)
	[0.389]	[0.425]	[0.246]	[0.634]
Any Income in 3-4 Years	-0.009	0.318**	0.082	0.148**
	(0.098)	(0.152)	(0.073)	(0.069)
	[0.462]	[0.492]	[0.354]	[0.624]
Court x Year FE	Yes	Yes	Yes	Yes
Crime Controls	Yes	Yes	Yes	Yes
IRS Controls	Yes	Yes	Yes	Yes
Observations	153,014	103,035	176,200	159,173

Note: This table reports two-stage least squares results of the impact of pre-trial release for selected defendant types. 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 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, crime controls, and IRS controls. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses and the mean of the dependent variable is reported in brackets in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A10: First Stage Results by Release Type

	Sample Mean	Judge Leniency	
		No	With
		Conditions	Conditions
	(1)	(2)	(3)
Released with No Conditions	0.205 (0.404)	0.937*** (0.014)	-0.041*** (0.013)
Released with Conditions	0.355 (0.479)	-0.268*** (0.027)	0.725*** (0.029)
Court x Year FE	-	Yes	Yes
Crime Controls	-	Yes	Yes
IRS Controls	-	Yes	Yes
Observations	421,850	421,850	421,850

Note: This table reports additional first stage results for two types of pre-trial release: release ROR with no conditions and release with conditions. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Estimates in columns 2 and 3 are from the same OLS specification. 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. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A11: Results by Release Type

	Detained Mean	OLS Results		2SLS Results	
		No Conditions	With Conditions	No Conditions	With Conditions
		(1)	(2)	(3)	(4)
Any Guilty Offense	0.585 (0.493)	-0.084*** (0.007)	-0.053*** (0.004)	-0.165*** (0.041)	-0.149*** (0.038)
Guilty Plea	0.447 (0.497)	-0.144*** (0.008)	-0.088*** (0.005)	-0.128*** (0.044)	-0.116*** (0.041)
Any Incarceration	0.304 (0.460)	-0.114*** (0.004)	-0.105*** (0.004)	-0.018 (0.031)	-0.034 (0.028)
Failure to Appear in Court	0.121 (0.326)	0.060*** (0.006)	-0.006 (0.010)	0.181*** (0.032)	0.153*** (0.028)
Rearrest in 0-2 Years	0.470 (0.499)	-0.054*** (0.007)	-0.008* (0.005)	-0.022 (0.052)	-0.044 (0.052)
Rearrest Prior to Disposition	0.196 (0.397)	0.049*** (0.005)	0.064*** (0.006)	0.138*** (0.045)	0.134*** (0.044)
Rearrest After Disposition	0.320 (0.467)	-0.086*** (0.005)	-0.039*** (0.003)	-0.133*** (0.043)	-0.152*** (0.044)
Employed in 1-2 Years	0.377 (0.485)	0.053*** (0.003)	0.050*** (0.002)	0.054 (0.041)	0.050 (0.041)
Any Income in 1-2 Years	0.457 (0.498)	0.036*** (0.004)	0.036*** (0.003)	0.108** (0.047)	0.110** (0.044)
Employed in 3-4 Years	0.377 (0.485)	0.041*** (0.003)	0.029*** (0.003)	0.093* (0.052)	0.109** (0.050)
Any Income in 3-4 Years	0.461 (0.498)	0.032*** (0.004)	0.029*** (0.003)	0.065 (0.049)	0.106** (0.048)
Court x Year FE	-	Yes	Yes	Yes	Yes
Crime Controls	-	Yes	Yes	Yes	Yes
IRS Controls	-	Yes	Yes	Yes	Yes
Observations	185,575	421,850	421,850	421,850	421,850

Note: This table reports OLS and two-stage least squares results of the impact of two types of pre-trial release: release ROR with no conditions and release with conditions. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Estimates in columns 2 and 3 are from the same OLS specification, and estimates in columns 4 and 5 are from the same two-stage least squares specification. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A12: Robustness of Two-Stage Least Squares Results

	Judge Leniency					Scheduled Judge	Judge FE
	Crime Severity	Crime Type	Release Ever	Non-Money Bail	Split Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any Guilty Offense	-0.128*** (0.040)	-0.169*** (0.049)	-0.195*** (0.048)	-0.076*** (0.023)	-0.158*** (0.047)	-0.144*** (0.043)	-0.191*** (0.039)
Guilty Plea	-0.126*** (0.040)	-0.139*** (0.053)	-0.192*** (0.051)	-0.046** (0.023)	-0.111** (0.050)	-0.128*** (0.045)	-0.164*** (0.041)
Any Incarceration	0.015 (0.036)	-0.044 (0.034)	-0.033 (0.033)	-0.012 (0.018)	-0.045 (0.038)	-0.035 (0.030)	-0.048* (0.026)
Failure to Appear in Court	0.116*** (0.037)	0.150*** (0.042)	0.235*** (0.054)	0.081*** (0.017)	0.164*** (0.032)	0.120*** (0.038)	0.096*** (0.029)
Rearrest in 0-2 Years	-0.006 (0.047)	-0.007 (0.046)	-0.042 (0.072)	0.016 (0.033)	-0.057 (0.068)	-0.052 (0.058)	-0.008 (0.048)
Employed in 1-2 Years	0.045 (0.032)	0.028 (0.036)	0.017 (0.061)	0.027 (0.022)	0.010 (0.055)	0.063 (0.046)	0.027 (0.033)
Any Income in 1-2 Years	0.077** (0.037)	0.053 (0.038)	0.140** (0.062)	0.006 (0.024)	0.075 (0.054)	0.139*** (0.048)	0.063* (0.034)
Employed in 3-4 Years	0.030 (0.042)	0.067 (0.043)	0.095 (0.074)	0.082*** (0.029)	0.055 (0.066)	0.097* (0.055)	0.078** (0.039)
Any Income in 3-4 Years	0.064 (0.045)	0.033 (0.041)	0.176** (0.073)	0.044 (0.027)	0.066 (0.068)	0.117* (0.061)	0.039 (0.041)
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IRS Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	421,850	421,850	421,850	421,850	316,529	421,850	421,850

Note: This table reports robustness checks of our two-stage least squares results. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Column 1 allows judge leniency to vary across felonies and misdemeanors. Column 2 allows judge leniency to vary across the 5 mutually exclusive crime types. Column 3 uses an indicator for ever being released before trial to calculate judge leniency. Column 4 uses an indicator for the assignment of non-monetary bail. Column 5 calculates judge leniency using a 25 percent random subset of the data and estimates the treatment effects in the opposing subset of data. Column 6 uses the scheduled bail judge to calculate judge leniency. Column 7 uses an exhaustive set of judge fixed effects as instruments. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A13: Robustness of Two-Stage Least Squares Results by Sample

	Judge Leniency		
	Philadelphia	Miami	IRS
	Only	Only	Sample
	(1)	(2)	(3)
Any Guilty Offense	-0.124*** (0.029)	-0.173** (0.071)	-0.136*** (0.042)
Guilty Plea	-0.040 (0.026)	-0.197*** (0.074)	-0.110** (0.045)
Any Incarceration	-0.011 (0.038)	-0.045 (0.046)	-0.046 (0.033)
Failure to Appear in Court	0.152*** (0.031)	-	0.127*** (0.032)
Rearrest in 0-2 Years	0.004 (0.065)	-0.098 (0.084)	-0.067 (0.055)
Rearrest Prior to Disposition	0.189*** (0.044)	0.093 (0.074)	0.116** (0.052)
Rearrest After Disposition	-0.173*** (0.046)	-0.154** (0.074)	-0.175*** (0.049)
Employed in 1-2 Years	-0.003 (0.041)	0.112 (0.079)	0.050 (0.041)
Any Income in 1-2 Years	-0.020 (0.038)	0.306*** (0.084)	0.110** (0.044)
Employed in 3-4 Years	0.174*** (0.048)	0.009 (0.091)	0.108** (0.049)
Any Income in 3-4 Years	0.069* (0.042)	0.167 (0.103)	0.105** (0.049)
Court x Year FE	Yes	Yes	Yes
Crime Controls	Yes	Yes	Yes
IRS Controls	Yes	Yes	Yes
Observations	328,492	93,358	335,373

Note: This table reports robustness checks of our two-stage least squares results across different samples. The dependent variable is listed in each row. Column 1 restricts the sample to Philadelphia. Column 2 restricts the sample to Miami-Dade. Column 3 restricts the sample to cases matched to the IRS data. All specifications control for court-by-time fixed effects. Robust standard errors two-way clustered at the individual and judge-by-year level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A14: Effects on Joint Probabilities of Rearrest and Employment

	Detained						
	OLS Results			2SLS Results			
Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Years 1-2</i>							
Rearrested and Employed	0.122 (0.327)	0.010*** (0.002)	0.013*** (0.002)	0.003 (0.002)	-0.013 (0.028)	-0.017 (0.028)	-0.024 (0.028)
Rearrested and Not Employed	0.236 (0.424)	-0.057*** (0.004)	-0.039*** (0.003)	-0.020*** (0.002)	-0.036 (0.031)	-0.035 (0.030)	-0.024 (0.029)
Not Rearrested and Employed	0.153 (0.360)	0.084*** (0.006)	0.062*** (0.004)	0.036*** (0.002)	0.071** (0.031)	0.072** (0.031)	0.056** (0.028)
Not Rearrested and Not Employed	0.205 (0.403)	-0.038*** (0.005)	-0.036*** (0.004)	-0.018*** (0.003)	-0.022 (0.034)	-0.019 (0.034)	-0.007 (0.033)
<i>Panel B: Years 3-4</i>							
Rearrested and Employed	0.104 (0.305)	0.006*** (0.002)	0.009*** (0.002)	0.001 (0.002)	0.010 (0.028)	0.006 (0.028)	0.000 (0.028)
Rearrested and Not Employed	0.214 (0.410)	-0.043*** (0.004)	-0.028*** (0.002)	-0.012*** (0.002)	-0.056* (0.030)	-0.056* (0.029)	-0.047 (0.029)
Not Rearrested and Employed	0.139 (0.346)	0.061*** (0.005)	0.042*** (0.003)	0.019*** (0.002)	0.055** (0.027)	0.055** (0.027)	0.042 (0.026)
Not Rearrested and Not Employed	0.188 (0.391)	-0.024*** (0.004)	-0.023*** (0.003)	-0.008*** (0.002)	-0.008 (0.028)	-0.005 (0.029)	0.005 (0.030)
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	-	No	Yes	Yes	No	Yes	Yes
IRS Controls	-	No	No	Yes	No	No	Yes
Observations	143,089	335,373	335,373	335,373	335,373	335,373	335,373

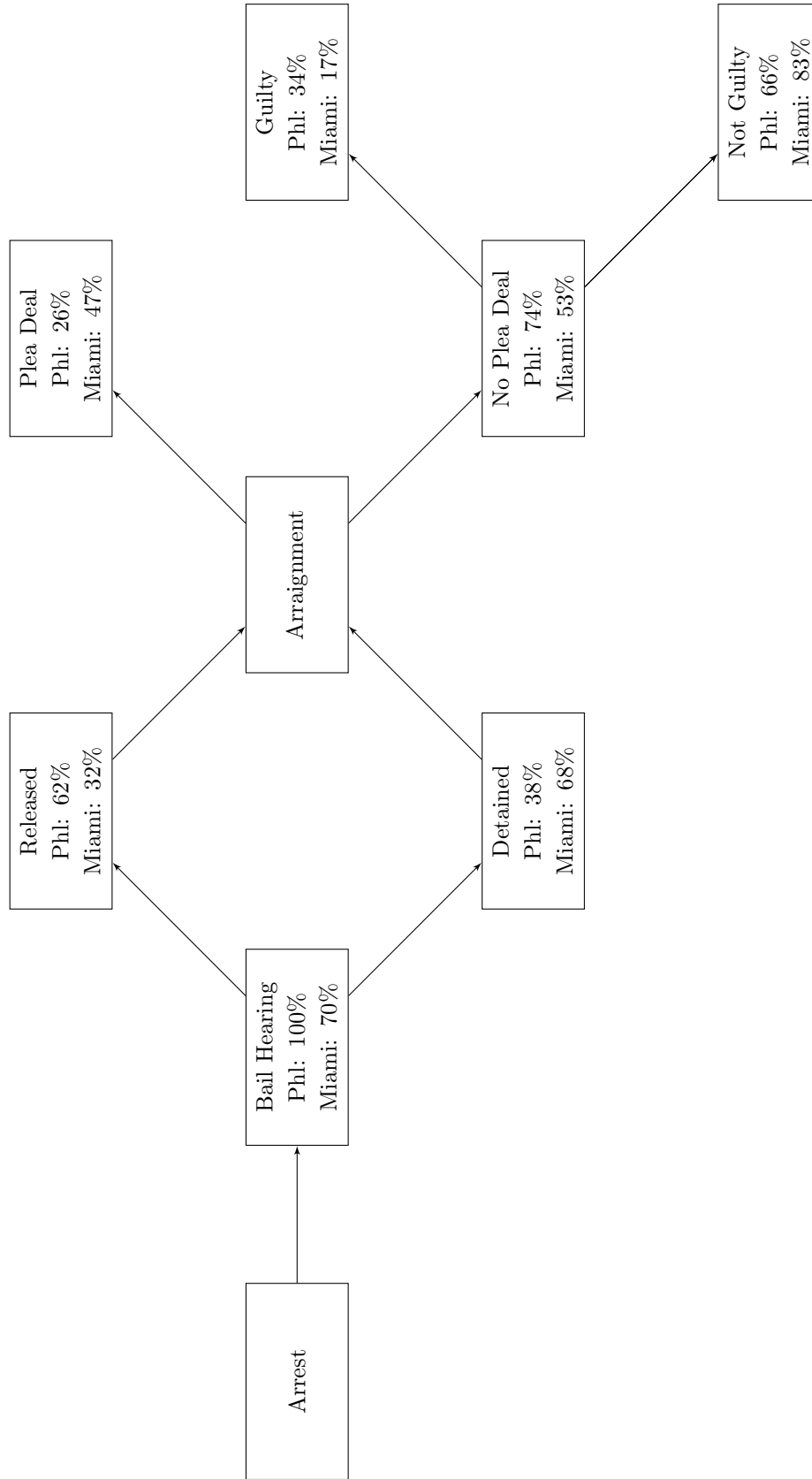
Note: This table reports additional OLS and two-stage least squares results of the impact of pre-trial release on the joint probability of rearrest and employment. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. Two-stage 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-by-year level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A15: Effects on Joint Probabilities of Conviction and Employment

	Detained						
	OLS Results			2SLS Results			
Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Years 1-2</i>							
Guilty and Employed	0.192 (0.394)	0.026*** (0.003)	0.019*** (0.003)	-0.004 (0.002)	-0.050 (0.032)	-0.056* (0.031)	-0.071** (0.032)
Guilty and Not Employed	0.341 (0.474)	-0.101*** (0.005)	-0.076*** (0.003)	-0.051*** (0.003)	-0.074** (0.034)	-0.076** (0.033)	-0.060* (0.032)
Not Guilty and Employed	0.146 (0.353)	0.094*** (0.004)	0.073*** (0.003)	0.050*** (0.003)	0.113*** (0.036)	0.116*** (0.035)	0.101*** (0.033)
Not Guilty and Not Employed	0.217 (0.412)	-0.019*** (0.003)	-0.015*** (0.003)	0.005** (0.003)	0.011 (0.030)	0.017 (0.029)	0.030 (0.029)
<i>Panel B: Years 3-4</i>							
Guilty and Employed	0.141 (0.348)	0.007*** (0.002)	0.006*** (0.002)	-0.009*** (0.002)	-0.002 (0.028)	-0.006 (0.027)	-0.015 (0.027)
Guilty and Not Employed	0.243 (0.429)	-0.058*** (0.005)	-0.041*** (0.003)	-0.025*** (0.002)	-0.030 (0.032)	-0.031 (0.032)	-0.020 (0.031)
Not Guilty and Employed	0.103 (0.304)	0.060*** (0.005)	0.045*** (0.003)	0.029*** (0.002)	0.067** (0.027)	0.068** (0.027)	0.057** (0.027)
Not Guilty and Not Employed	0.159 (0.366)	-0.010*** (0.003)	-0.010*** (0.003)	0.004** (0.002)	-0.035 (0.027)	-0.031 (0.026)	-0.022 (0.026)
Court x Year FE	-	Yes	Yes	Yes	Yes	Yes	Yes
Crime Controls	-	No	Yes	Yes	No	Yes	Yes
IRS Controls	-	No	No	Yes	No	No	Yes
Observations	143,089	335,373	335,373	335,373	335,373	335,373	335,373

Note: This table reports additional OLS and two-stage least squares results of the impact of pre-trial release on the joint probability of conviction and employment. 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-by-year level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Figure A1: Criminal Case Process



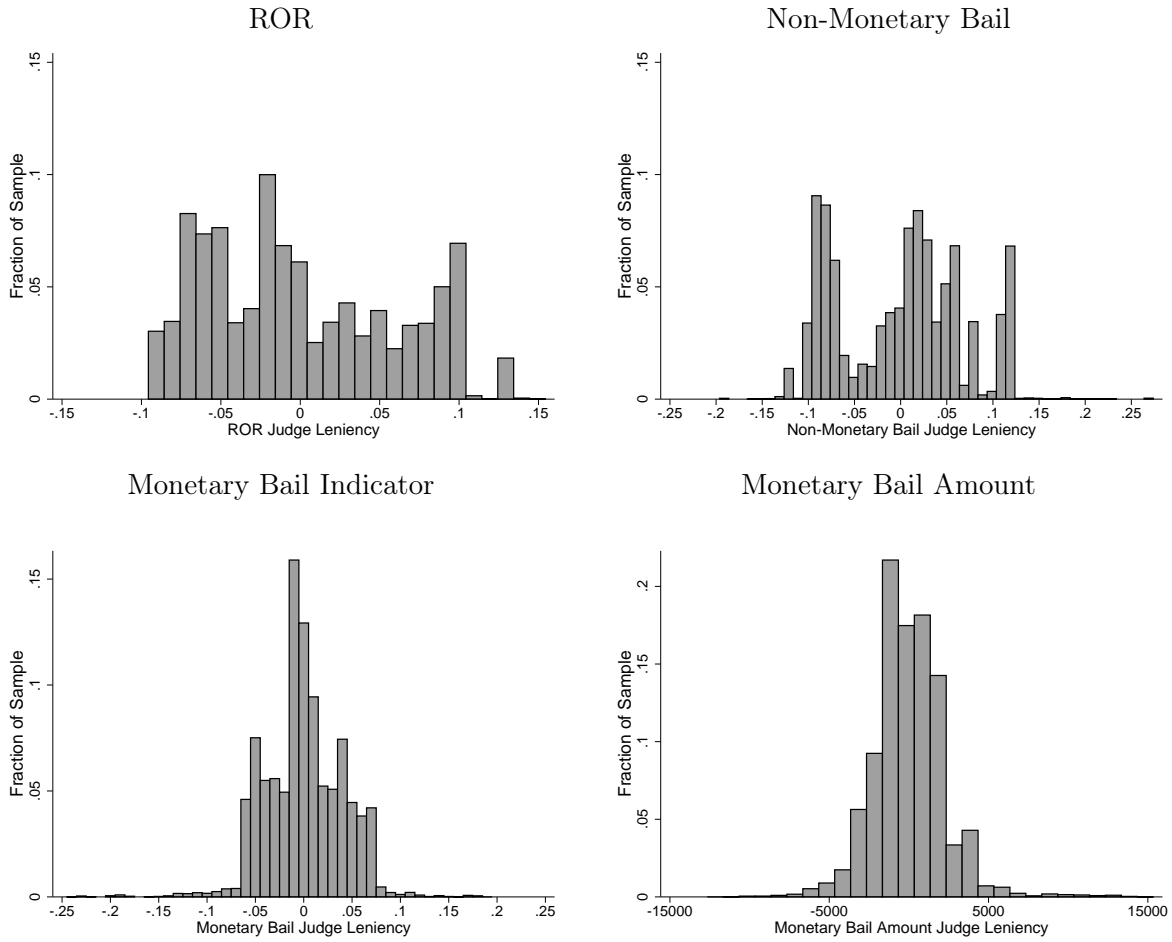
Note: This figure illustrates the criminal case process in Philadelphia and Miami-Dade. Following Reaves (2013), we include case dismissals, acquittals, diversion, and deferred prosecutions in our category of "Not Guilty."

Figure A2: Bail Guidelines in Philadelphia

	Charge Seriousness									
	1	2	3	4	5	6	7	8	9	10
1	ROR	ROR	ROR	ROR	ROR	ROR	ROR	\$1,500-\$4,500	\$2,200-\$6,700	\$3,000-\$12,000
	1	5	9	13	17	21	25	29	33	37
2	ROR	ROR	ROR	ROR	ROR	ROSC 1	ROSC 1	\$3,000-\$6,700	\$3,700-\$8,200	\$3,700-\$12,000
	2	6	10	14	18	22	26	30	34	38
3	ROR	ROSC 1	ROSC 1	ROSC 1	ROSC 2	ROSC 2	ROSC 2	\$3,700-\$7,500	\$4,500-\$9,700	\$6,700-\$22,500
	3	7	11	15	19	23	27	31	35	39
4	ROSC 1	ROSC 1	ROSC 1	ROSC 2	ROSC 2	ROSC 2	\$3,700-\$6,700	\$4,500-\$7,500	\$4,500-\$12,000	\$9,000-\$75,000
	4	8	12	16	20	24	28	32	36	40

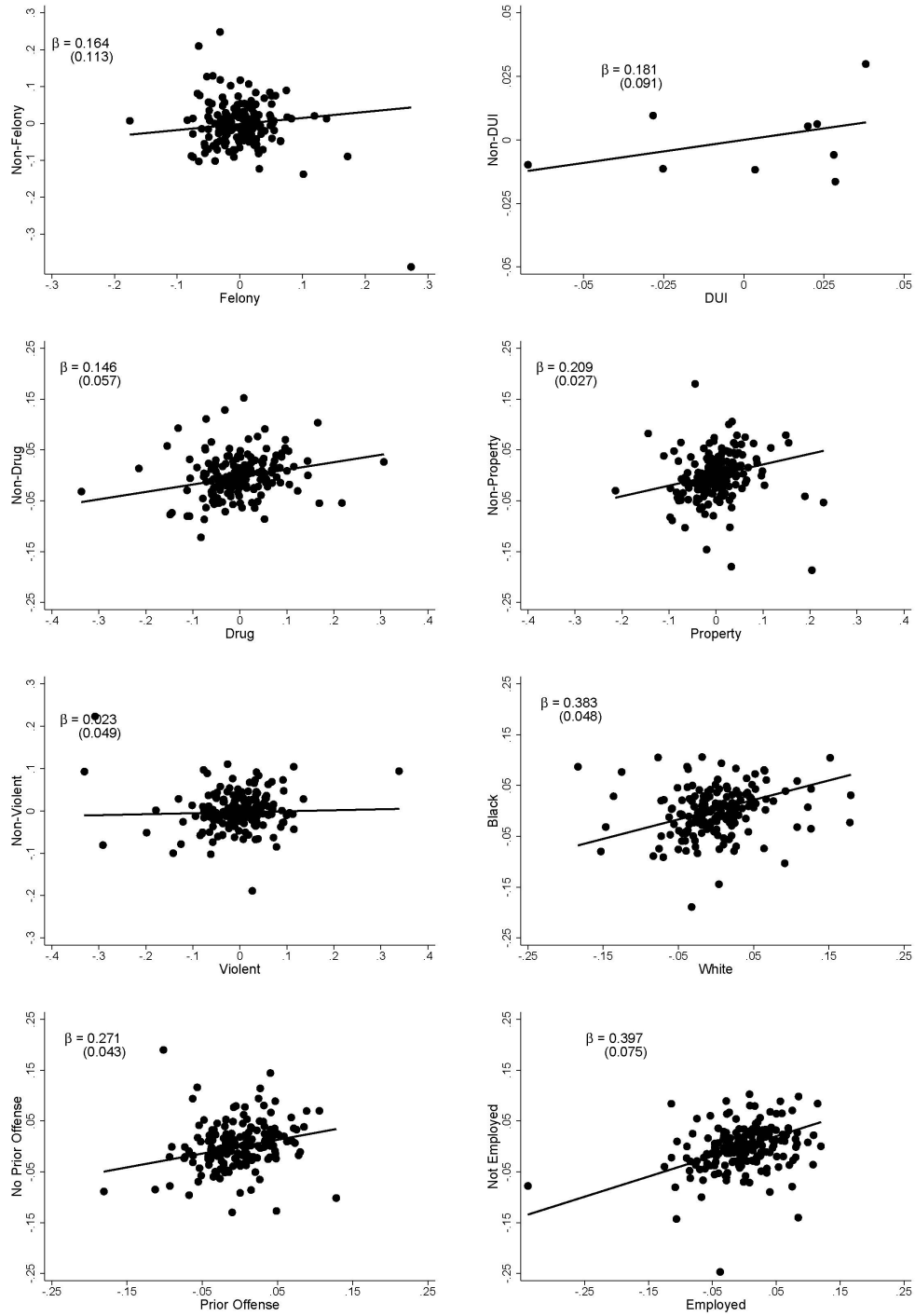
Note: This figure illustrates the bail guidelines in Philadelphia. The guidelines classify defendants into 40 possible categories and suggest four basic types of release options. Charge seriousness ranges from one to 10, with 10 the most serious. Risk level ranges from one to four, with four the highest risk, and is based on the risk of a defendant failing to appear in court and committing new offenses. ROR is release on recognizance. ROSC is release on special conditions.

Figure A3: Distribution of Alternative Judge Leniency Measures



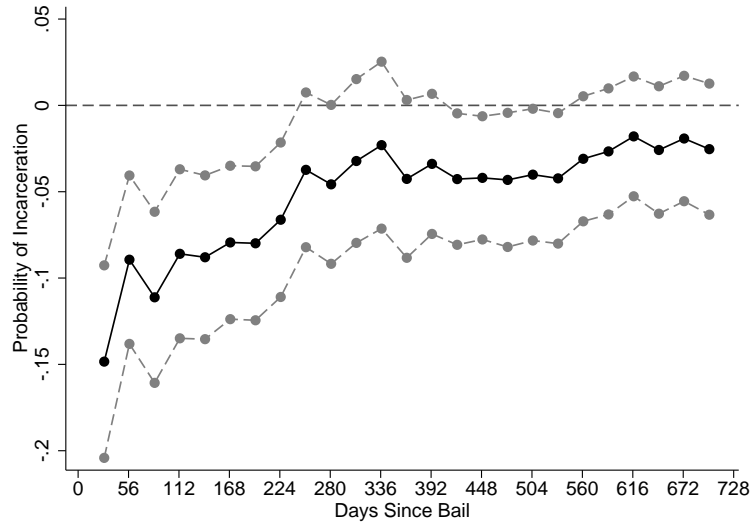
Note: These figures report the distribution of various judge leniency measures that are estimated using data from cases assigned to a bail judge in other dates following the procedure described in Section III.

Figure A4: Judge Leniency by Defendant and Case Characteristics



Note: These figures show the correlation between our residualized measure of judge leniency for different groups of defendants. DUI cases are only available in Philadelphia. We take the average leniency for each group over all available years of data. The solid line shows the best linear fit estimated using OLS relating each judge leniency measure.

Figure A5: Probability of Incarceration by Days Since Bail



Note: This figure reports two-stage least squares estimates and corresponding 95 percent confidence intervals for the impact of pre-trial release on the probability of either pre- or post-disposition incarceration at different points in time after the bail hearing. 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.

Appendix B: Data Dictionary

[FOR ONLINE PUBLICATION ONLY]

A. Judge Leniency

Judge Leniency: We calculate judge leniency as the leave-one-out mean residualized pre-trial release decisions of the assigned judge within a bail year. We use the residual pre-trial release decision after removing court-by-time fixed effects. In our main results, we define pre-trial release based on whether a defendant was released within the first three days after the bail hearing.

B. Bail Type

Release on Recognizance: An indicator for whether the defendant was released on recognizance (ROR), where the defendant secures release on the promise to return to court for his next scheduled hearing. ROR is used for offenders who show minimal risk of flight, no history of failure to appear for court proceedings, and pose no apparent threat of harm to the public.

Non-Monetary Bail: An indicator for whether the defendant was released on non-monetary bail, also known as conditional release. Non-monetary conditions include monitoring, supervision, halfway houses, and treatments of various sorts, among other options.

Monetary Bail: An indicator for whether the defendant was assigned monetary bail. Under monetary bail, a defendant is generally required to post a bail payment to secure release, typically 10 percent of the bail amount, which can be posted directly by the defendant or by sureties such as bail bondsman.

Bail Amount: Assigned monetary bail amount in thousands, set equal to missing for defendants who receive non-monetary bail or ROR.

C. Defendant Characteristics

Race: Information on defendant race is missing for the Philadelphia data prior to 2010.

Prior Offense in Past Year: An indicator for whether the defendant had been charged for a prior offense in the past year of the bail hearing within the same county, set to missing for defendants who we cannot observe for a full year prior to their bail hearing.

D. Charge Characteristics

Number of Offenses: Total number of charged offenses.

Felony Offense: An indicator for whether the defendant is charged with a felony offense.

Misdemeanor Offense: An indicator for whether the defendant is charged with only misdemeanor offenses.

E. Case Outcomes

Any Guilty Offense: An indicator for whether the defendant is found guilty of any charged offense. A defendant is guilty of an offense if found guilty at trial, or if he pleads guilty or nolo contendere (no contest).

Guilty Plea: An indicator for whether the defendant pleads guilty or nolo contendere to any charged offense.

Offense Charged Down: An offense is charged down if the defendant is found not guilty, or if the highest conviction offense is less severe than the highest charged offense.

Any Incarceration: Any indicator for whether the defendant receives a term of imprisonment following conviction, equal to zero if a defendant is found not guilty.

Max Days Incarcerated: Maximum days incarcerated calculated by taking the maximum prison sentence across all convicted offenses, equal to zero if a defendant is found not guilty.

Bail Modification Petition: An indicator for whether the defendant petitions for a bail modification.

Days to Case Decision: Number of days from the bail hearing to case disposition, set to missing for cases still pending.

Failure to Appear in Court: An indicator for whether the defendant fails to appear in court, as proxied by the issuance of a bench warrant.

Absconded: An indicator for whether the defendant absconded from the jurisdiction, defined as a case that is still pending but inactive, and for which the defendant failed to appear.

Rearrest: An indicator for whether the defendant was rearrested and charged in the same county at a given point in time.

New Conviction: An indicator for whether the defendant was convicted for another offense in the same county at a given point in time.

F. IRS Outcomes

Wage Earnings: The individual's wages as reported by the employer to the IRS on Form W-2. This information is available whether or not the individual files a tax return, and is aggregated across jobs if the individual works more than one job during the year. All dollar amounts are in terms of year 2013 dollars and reported in thousands. We top- and bottom-code earnings in each year at the 99th and 1st percentiles, respectively, to reduce the influence of outliers.

Employed: An indicator for whether W-2 wages are greater than zero in a given year.

Adjusted Gross Income (AGI): Total household income from all sources (wage, interest, self-employment, UI, etc.) as reported on the individual's tax return. This measure is only available if the individual files a tax return. For individuals who did not file a tax return, we impute adjusted gross income to equal W-2 wages + UI income. All dollar amounts are in terms of year 2013 dollars and reported in thousands. We top- and bottom-code earnings in each year at the 99th and 1st percentiles, respectively, to reduce the influence of outliers.

Any Income: An indicator for whether AGI is greater than zero in a given year.

Filed Return: An indicator for whether the individual filed a tax return in the given year.

UI Benefits: Amount of UI benefits the individual receives during the tax year, as reported to the IRS from the state UI agency. This measure is available whether the individual files a tax return or not. All dollar amounts are in terms of year 2013 dollars and reported in thousands. We top- and bottom-code earnings in each year at the 99th and 1st percentiles, respectively, to reduce the influence of outliers.

EITC Benefits: Amount of EITC claimed by the individual (and spouse if filing jointly) on his or her tax return. This measure is only available for individuals who file a tax return. All dollar amounts are in terms of year 2013 dollars and reported in thousands. We top- and bottom-code earnings in each year at the 99th and 1st percentiles, respectively, to reduce the influence of outliers.

G. Data Characteristics

Matched to IRS data: Indicator for whether the 421,850 defendants in our court data are matched to IRS data. We match the court data to administrative tax data from the IRS using first and last name, date of birth, gender, and state of residence. We were able to successfully match approximately 77 percent of individuals in the court data. Our match rate in Philadelphia is 81 percent and our match rate in Miami-Dade is 73 percent.

Missing Race: Indicator for whether defendant race is missing.

Appendix C: Interpreting our LATE [FOR ONLINE PUBLICATION ONLY]

This section includes additional details on how we calculate the number and characteristics of defendants who are always takers, never takers, and compliers in our sample.

Overview: Following Dahl et al. (2014), we define compliers as defendants whose pre-trial release decision would have been different had their case been assigned to the most lenient instead of the most strict judge:

$$\pi_c = Pr(Released_i = 1|Z_i = \bar{z}) - Pr(Released_i = 1|Z_i = \underline{z}) = Pr(Released_i(\bar{z}) > Released_i(\underline{z}))$$

where \bar{z} represents the maximum value of our judge instrument (the most lenient judge) and \underline{z} represents the minimum value of our instrument (the most strict judge).

Always takers are defendants who would always be released before trial regardless of the bail judge assigned to their case. Because of the monotonicity and independence assumptions, the fraction of always takers is given by the probability of being released pre-trial for the most strict judge:

$$\pi_a = Pr(Released_i = 1|Z_i = \underline{z}) = Pr(Released_i(\bar{z}) = Released_i(\underline{z}) = 1)$$

Finally, never takers are defendants who would never be released before trial, with the fraction of never takers given by the probability of being detained pre-trial by the most lenient judge:

$$\pi_n = Pr(Released_i = 0|Z_i = \bar{z}) = Pr(Released_i(\bar{z}) = Released_i(\underline{z}) = 0)$$

Number of Compliers: We calculate the shares of defendants in each category by looking at the pre-trial release rates for defendants assigned to the “most lenient” and “most strict” judges. Following Dahl et al. (2014), we begin by defining the “most strict” judge as the bottom 1 percentile of judge leniency and the “most lenient” judge as the top 1 percentile of judge leniency.

In the first three columns of Table C.1, we estimate a local linear regression of pre-trial release on our residualized measure of judge leniency controlling for our exhaustive court-by-time fixed effects. Under this more flexible analog to our first stage equation, we find that 13 percent of our sample are compliers, 53 percent are never takers, and 34 percent are always takers.

In the last three columns of Table C.1, we estimate our linear specification of the first stage, given by Equation (4). Under this specification, we can recover π_c as $\hat{\alpha}_1(\bar{z} - \underline{z})$, π_a as $\hat{\alpha}_0 + \hat{\alpha}_1\underline{z}$, and π_n as $1 - \hat{\alpha}_0 - \hat{\alpha}_1\bar{z}$ where $\hat{\alpha}_0$ and $\hat{\alpha}_1$ are the estimated first stage coefficients. Under this linear specification, we find that 12 percent of our sample are compliers, 52 percent are never takers, and 36 percent are always takers. We also explore the sensitivity of the estimated share of compliers, always takers, and never takers to the exact choice of cutoff for the most lenient and most strict judge. Our results are robust to the particular model specification and cutoff.

Table C.1: Sample Share by Compliance Type

Model Specification:	Local Linear Model			Linear Model		
Leniency Cutoff:	1%	1.5%	2%	1%	1.5%	2%
Compliers	0.13	0.13	0.13	0.12	0.11	0.09
Never Takers	0.53	0.53	0.53	0.52	0.53	0.54
Always Takers	0.34	0.34	0.34	0.36	0.36	0.37

Characteristics of Compliers: We also characterize our population of compliers by observable characteristics, which can be recovered by calculating the fraction of compliers in different subsamples (Abadie 2003, Dahl et al. 2014). We find that compliers are significantly more likely to be charged with misdemeanor offenses and less likely to be charged with violent offenses compared to the sample average.

Table C.2: Characteristics of Marginal Defendants

	$P[X = x]$	$P[X = x \text{complier}]$	$\frac{P[X=x \text{complier}]}{P[X=x]}$
White	0.402 (0.001)	0.383 (0.018)	0.953 (0.044)
Non-White	0.598 (0.001)	0.617 (0.018)	1.032 (0.030)
Drug	0.274 (0.001)	0.286 (0.015)	1.042 (0.055)
Non-Drug	0.726 (0.001)	0.714 (0.015)	0.984 (0.021)
Violent	0.172 (0.001)	0.018 (0.012)	0.102 (0.070)
Non-Violent	0.828 (0.001)	0.982 (0.012)	1.187 (0.015)
Felony	0.460 (0.001)	0.340 (0.017)	0.741 (0.037)
Misdemeanor	0.540 (0.001)	0.660 (0.017)	1.221 (0.031)
Prior Last Year	0.268 (0.001)	0.303 (0.014)	1.133 (0.051)
No Prior	0.732 (0.001)	0.697 (0.014)	0.952 (0.019)
Employed	0.475 (0.001)	0.458 (0.018)	0.966 (0.037)
Non-Employed	0.525 (0.001)	0.542 (0.018)	1.031 (0.033)

Note: This table presents the sample distribution, complier distribution, and relative likelihood for different subgroups. Bootstrapped standard errors in parentheses are obtained using 500 replications.

Appendix D: Cost-Benefit Analysis

[FOR ONLINE PUBLICATION ONLY]

Table D.1 below presents our relevant two-stage least squares estimates and lower and upper bounds on each cost.

A. Social Benefits of Release

Direct Costs of Jail: According to the Pew Trusts, the daily cost of keeping someone in jail in Philadelphia is \$95. According to the Miami-Dade corrections system, the daily cost of housing an inmate in jail is \$155 (see, for example, <http://www.miamiherald.com/news/local/community/miami-dade/article1985206.html>). We therefore assume an average jail cost of \$125 per day. Taking our two-stage least squares estimates on the number of days incarcerated, we find that the marginal released defendant spends 13.3 days less in jail, saving taxpayers \$1,662 in direct administrative costs.

Costs of Future Crime: We estimate the net impact of crime, which combines short-run incapacitation effects and medium-term criminogenic effects. To capture the fact that the social costs of crime differ across crime type, we separately estimate our net crime results by major crime type. Over the first two years after the bail hearing, the marginal released defendant is rearrested for 0.000 murders, 0.001 more rapes, 0.009 fewer robberies, 0.012 fewer assaults, 0.018 fewer burglaries, 0.006 fewer thefts, 0.087 fewer drug crimes, and 0.024 fewer DUIs. Using the social costs by crime type compiled by Mueller-Smith (2015), we estimate a benefit to pre-trial release ranging from \$2,768 to \$5,785.

Costs of Decreased Earnings and Social Assistance: Taking our two-stage least squares estimates, the marginal released defendant earns roughly \$1,104 per year and has \$315 more in UI income and \$220 in EITC income, for a total average annual income gain of \$1,639, 24.4 percent of mean earnings in the sample. Following Chetty et al. (2011), we assume that the percentage gain in earnings remains constant over the working lifecycle and discount annual earnings at a 3 percent discount rate back to age 34, the mean age in the sample. Under these assumptions, the marginal released defendant gains \$32,781 over a lifetime relative to the marginal detained defendant.

B. Social Costs of Release

Failure to Appear: There are very few estimates of the costs of re-apprehending a defendant who misses a required court appearance, but we follow Abrams and Rohlfs (2013) in assuming that the cost is roughly five percent of the bail amount, or approximately \$625 in our sample, and that the cost of additional bail hearings is roughly \$560 (Bierie 2007) to equal \$1,185. Combined with our two-stage least squares estimates which find that the marginal released defendant is 15.2 percentage points more likely to fail to appear, the expected cost of failing to appear is \$180 for the marginal released defendant.

C. Cost-Benefit Calculation

We combine our estimates of the social costs and benefits of pre-trial release with the monetary costs associated with each. Based on these assumptions, the lower-bound net benefit of release for the marginal individual is $\$1,662 + \$2,768 + \$32,781 - \$180 = \$37,031$. The upper-bound net benefit is $\$1,662 + \$5,785 + \$32,781 - \$180 = \$40,048$.

Table D.1: Details of Cost-Benefit Calculation

	2SLS Estimate	Lower Bound	Upper Bound
	(1)	(2)	(3)
<i>Panel A: Days in Jail</i>			
Days Detained Before Trial	-13.282	\$125	\$125
<i>Panel B: Future Crime (Counts)</i>			
Murder	0.000	\$4,301,817	\$11,559,713
Rape	0.001	\$187,680	\$343,859
Robbery	-0.009	\$73,196	\$333,701
Assault	-0.012	\$41,046	\$109,903
Burglary	-0.018	\$50,291	\$50,291
Theft	-0.006	\$9,598	\$9,974
Drug Crime	-0.087	\$2,544	\$2,544
DUI	-0.024	\$25,842	\$25,842
<i>Panel C: Earnings and Social Assistance (Thousands)</i>			
Earnings	1.104		
UI	0.315		
EITC	0.220		
<i>Panel D: Failure to Appear</i>			
Failure to Appear	0.152	\$1,185	\$1,185