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THE EFFECTS OF PRE-TRIAL DETENTION ON CONVICTION, FUTURE CRIME, AND EMPLOYMENT: EVIDENCE FROM RANDOMLY ASSIGNED JUDGES

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

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## 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 being detained before trial significantly increases the probability of a conviction, primarily through an increase in guilty pleas. Pre-trial detention has no detectable effect on future crime, but decreases pre-trial crime and failures to appear in court. We also find suggestive evidence that pre-trial detention decreases formal sector employment and the receipt of employment- and tax-related government benefits. We argue that these results are consistent with (i) pre-trial detention weakening defendants' bargaining position during plea negotiations, and (ii) a criminal conviction lowering defendants' prospects in the formal labor market.

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"The defendant with means can afford to pay bail. He can afford to buy his freedom. But the poorer defendant cannot pay the price. He languishes in jail weeks, months, and perhaps even years before trial. He does not stay in jail because he is guilty. He does not stay in jail because any sentence has been passed. He does not stay in jail because he is any more likely to flee before trial. He stays in jail for one reason only – he stays in jail because he is poor."

– President Lyndon Johnson, at the signing of the Bail Reform Act of 1966

Each year, the United States imprisons more than half a million individuals who have never been convicted of a crime, largely because they are unable to post bail (Walmsley 2013). Over the past twenty years, the proportion of felony defendants released with no conditions decreased from 26 percent to 14 percent. The average bail amount has also doubled from $25,400 to $55,400 over this time period, with over 70 percent of felony defendants now assigned bail amounts greater than $5,000 (Reaves 2013). Even when the bail amount is set at a relatively low level, the majority of defendants cannot afford to post bail. For example, in Philadelphia and Miami-Dade, the setting of our study, only about 50 percent of defendants were able to post bail when it was set at $5,000 or less.

In theory, the bail system is meant to balance three competing objectives: (1) allow all but the most dangerous criminal defendants to go free before trial, (2) ensure that defendants appear at all required court proceedings, and (3) protect the public by preventing new crime. Consider, for example, monetary bail, which allows a defendant to go free before trial by posting a percentage of the bail amount. If a defendant fails to appear in court, commits a new crime, or violates any other conditions of release, he or she forfeits the deposit and is liable for the remaining bail amount. As a result, defendants released through monetary bail have an increased incentive to comply with the release conditions.

In practice, however, there is a heated debate on whether the bail system achieves these objectives. Critics of the bail system argue that pre-trial detention is unlikely to protect the public or reduce bail jumping if release is not based on risk, but rather factors like race or financial resources. Some are particularly concerned that excessive bail and pre-trial detention disrupts defendants' lives, putting jobs at risk and increasing the pressure to accept an unfavorable plea bargain to avoid a lengthy stay in jail before trial.[1] Others claim that the bail system is operating as designed, and that releasing more defendants would increase both pre-trial crime and bail jumping. This debate is currently playing out across the country, as a number of cities and states consider reforming their bail systems.[2] Yet, despite the attention generated by the ongoing efforts to reform the bail system,

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

[2] For example, some cities are considering the use of risk-based assessment tools to more accurately predict each

there is little systematic evidence on the social costs or benefits of detaining an individual before trial.

Estimating the causal impact of pre-trial detention on criminal defendants has been complicated by two important issues. First, there are few datasets that include information on both bail hearings and long-term outcomes for a large number of defendants.[3] Second, detained defendants are likely 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 likely to commit another crime in the future.[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 social costs of pre-trial detention in terms of criminal case outcomes and foregone earnings. To shed light on the potential benefits of pre-trial detention, we also estimate the extent to which pre-trial release affects bail jumping and future criminal behavior. Finally, we investigate how pre-trial detention affects tax filing behavior and the take-up of employment-related benefits such as the Earned Income Tax Credit (EITC). By examining a wide range of important outcomes, we establish a new set of facts on both the social costs and social benefits of the current bail system.

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, 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

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defendant's flight risk and other release options such as electronic monitoring. Other cities, such as New York City, have earmarked substantial funds to supervise low-risk defendants instead of requiring them to post bail or face pre-trial detention. In May 2015, Illinois lawmakers passed a bill requiring that a nonviolent defendant be released pre-trial without bond if his or her case has not been resolved within 30 days. In addition, communities have created charitable bail organizations like the Bronx Freedom Fund and the Brooklyn Community Bail, which posts bail for individuals held on misdemeanor charges when bail is set at $2,000 or less.

[3]Data tracking defendants often contain some information on pre-trial detention and follow individuals through 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.

[4]Prior 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 mixed evidence on whether bail amounts are correlated with the probability of jumping bail (Landes 1973, Clarke, Freeman, and Koch 1976, Myers 1981).

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 versus incarceration in Argentina.[5]

We begin by estimating the impact of pre-trial release on case outcomes. Pre-trial release may affect case outcomes by reducing a defendant's incentive to plead guilty to obtain a faster release from jail, or by affecting a defendant's ability to prepare an adequate defense or negotiate a settlement with prosecutors. It is also possible that seeing detained defendants in jail uniforms and shackles may bias trial judges or jurors. Our two-stage least squares results suggest statistically and economically significant effects for most case outcomes. Pre-trial release decreases the probability of being found guilty by 15.6 percentage points, a 27.3 percent change from the mean for detained defendants. The probability of pleading guilty also decreases by 12.0 percentage points, a 27.5 percent change. Both effects are larger for drug and property defendants, defendants charged with misdemeanors, and defendants with no prior offenses in the past year. The effect of pre-trial release on incarceration in the full sample is small and not precisely estimated, but large and statistically significant for defendants charged with felonies and drug offenses (i.e. cases where the baseline rates of incarceration are highest).

Next, we explore the impact of pre-trial release on court appearances and future crime. We find that pre-trial release increases the probability of failing to appear in court by 15.0 percentage points, a 124.0 percent increase from the detained defendant mean. Pre-trial release also increases the likelihood of rearrest prior to case disposition by 7.6 percentage points, a 37.6 percent change. These results suggest that while pre-trial detention has a negative impact on case outcomes, it also reduces failures to appear in court and pre-trial crime, two of the purported benefits of the bail system. Conversely, we find no detectable effects of pre-trial release on measures of future crime up to four years later. These results suggest that pre-trial detention has a short-run mechanical incapacitation effect on defendants who are detained, but minimal effects on crime once we include arrests following case disposition.

Finally, we examine the effects of pre-trial release on formal sector employment, tax filing behavior, and social benefits receipt. Apart from direct employment effects, pre-trial release may 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.[6] Similarly, pre-trial release may

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[5]Outside of the criminal justice setting, Chang and Schoar (2008), Dobbie and Song (2015), 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.

[6]In addition, because the EITC cannot be claimed on the basis of income earned while incarcerated, pre-trial detention may reduce tax benefit claiming behavior through this channel as well. More generally, helping those with criminal convictions reenter the formal employment sector is a central feature of tax policy with respect to this population. For example, the Work Opportunity Tax Credit subsidizes employers who hire individuals that have

affect participation in social welfare programs such as Unemployment Insurance (UI), which are also tied to formal sector employment. We find suggestive evidence that pre-trial release increases both formal sector employment and the receipt of employment- and tax-related government benefits. Pre-trial release increases the probability of filing a tax return three to four years after the bail hearing by 4.3 percentage points, a 14.0 percent increase from the detained defendant mean. Pre-trial release also increases the amount of UI benefits received over the same time period by $323, a 131.8 percent increase, and the amount of EITC benefits received by $239, a 66.4 percent increase. While less precisely estimated, we find that pre-trial release also increases the probability of employment in the formal labor market three to four years after the bail hearing by 10.2 percentage points, a 26.9 percent increase. The probability of having any formal sector income over this time period increases by 8.5 percentage points, a 18.3 percent increase. The results are substantially larger among individuals with no prior offenses in the past year and among individuals who were employed in the year prior to their bail decision.

We argue that these results are consistent with (i) pre-trial release significantly strengthening a defendant's bargaining position during plea negotiations, and (ii) a criminal conviction significantly lowering defendants' ties to the formal labor market. Our findings contribute to an important literature documenting the negative labor market consequences of incarceration following a guilty verdict (e.g. Pager 2003, Western 2006, Mueller-Smith 2015, Agan and Starr 2016).[7] Our results suggest that these adverse labor market effects begin at the pre-trial stage prior to any finding of guilt, while also highlighting the potential costs of weakening a defendant's negotiating position before trial. Yet, we also find that pre-trial detention provides some social benefits through the incapacitation of defendants, leading to decreases in both pre-trial crime and missed court appearances. As a result, we are unable to draw sharp welfare conclusions about the optimality of the current bail system without strong, ad-hoc assumptions. That being said, our findings underscore the potential value to defendants of alternatives like electronic monitoring that would facilitate pre-trial release while preserving many of the social benefits the current system provides.

Our results also speak to the optimal design of the tax code and other policies meant to promote economic opportunity. In particular, our findings suggest that to increase labor force attachment, it may be more cost-effective to adopt policies that prevent some of the negative effects of pre-trial detention from occurring in the first place, as opposed to focusing primarily on programs like the EITC and the Work Opportunity Tax Credit that encourage formal sector employment upon defendants' reentry into society.

In parallel work, Gupta, Hansman, and Frenchman (2016), Leslie and Pope (2016), and Stevenson (2016) use similar approaches to estimate the impact of bail decisions on case outcomes. Gupta, Hansman, and Frenchman (2016) find that the assignment of money bail causes a 6.0 percentage

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been convicted of a felony in the past year.

[7]Our results are also related to a broad literature documenting the presence of racial disparities at various stages of the criminal justice process (e.g., Ayres and Waldfogel 1994, Bushway and Gelbach 2011, McIntyre and Baradaran 2013, Rehavi and Starr 2014, Anwar, Bayer, and Hjalmarsson 2012, Abrams, Bertrand, and Mullainathan 2012, Alesina and La Ferrara 2014), and suggest that any costs of pre-trial detention are disproportionately borne by black defendants.

point rise in the likelihood of being convicted and a 0.7 percentage point yearly rise in recidivism in Philadelphia and Pittsburgh, Leslie and Pope (2016) find that pre-trial detention causes a 14.2 percentage point increase in the probability of conviction in New York City, and Stevenson (2016) finds that pre-trial detention leads to a 6.6 percentage point increase in the likelihood of being convicted in Philadelphia. We view our results on case outcomes as being broadly consistent with these papers. However, none of these papers is able to examine non-criminal outcomes such as formal sector employment or social benefits take-up.

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 both their appearance at required court proceedings and the public's safety. The federal right to non-excessive bail before trial is guaranteed by the Eighth Amendment to the U.S. Constitution,[8] with almost all state constitutions granting similar rights to defendants.[9]

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 minimal risk of flight may be released on their promise to return for all court proceedings, known broadly as release on recognizance (ROR). Second, defendants may be released subject to some 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,

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[8]The Eighth Amendment to the U.S. Constitution states that "[e]xcessive bail shall not be required." In 1966, Congress passed the Bail Reform Act, designed to allow for release of federal defendants who were too poor to post bail, the first significant reform in federal bail legislation since the Judiciary Act of 1789. Generally speaking, the 1966 Bail Reform Act provided that all defendants accused of federal crimes would be released from custody without having to post any bond unless the government could demonstrate that the defendant was likely to flee the jurisdiction to avoid prosecution. The next major reform in federal bail law was the Bail Reform Act of 1984, which allowed for defendants to be held until trial if the government could prove that they were dangerous to others in the community. In addition, the Federal Rules of Criminal Procedure specify that before conviction, a person arrested for an offense "not punishable by death shall be admitted to bail," as the "right to freedom before conviction permits the unhampered preparation of a defense, and serves to prevent the infliction of punishment prior to conviction." (U.S. Supreme Court).

[9]For instance, 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."

defendants may be required to post a bail payment to secure release if they pose an appreciable risk of flight or threat of harm to the public. Defendants are typically required to pay 10 percent of the bail amount to secure release, with most of the bail money refunded after the case if there were no failures to appear for court or other release violations.[10] Those who do not have the 10 percent deposit in cash can borrow this amount from a commercial bail bondsman, who will accept cars, houses, jewelry and other forms of collateral for their loan. Bail bondsman also charge a non-refundable fee for their services, generally 10 percent of the total bail amount.[11] If the defendant fails to appear, he 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 be 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 hearing is typically very brief – in Philadelphia and Miami-Dade counties, our setting, most hearings last less than five minutes. 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).[12] 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, 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. At the hearing, the defendant also receives a copy of the criminal complaint, is advised of his or her rights, and appointed counsel if indigent. Defendants generally have the opportunity to appeal the initial bail decision in later proceedings, which can lead to modifications of the initial bail conditions.[13]

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[10]In Philadelphia, 70 percent of the bail deposit is available for refund 31 days after the final disposition of the case. The City of Philadelphia retains the remaining 30 percent of the deposit, up to $750, even if charges get dropped or the defendant is acquitted on all charges.

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

[12]For example, under the Pennsylvania Rules of Criminal Procedure, "the bail authority shall consider all available information as that information is relevant to the defendant's appearance or nonappearance at subsequent proceedings, or compliance or noncompliance with the conditions of the bail bond," including information such as the nature of the offense, the defendant's employment status and relationships, and whether the defendant has a record of bail violations or flight. See Pa. R. Crim. P. 523. Under the Florida Rules of Criminal Procedure, judges consider similar factors such as "the nature and circumstances of the offense charged and the penalty provided by law; the weight of the evidence against the defendant;...the defendant's past and present conduct, including any record of convictions, previous flight to avoid prosecution, or failure to appear at court proceedings; the nature and probability of danger that the defendant's release poses to the community; [and] the source of funds used to post bail." See Fl. R. Crim. P. 3.131.

[13]Bail reductions will generally not be granted if a defendant has detainers or open bench warrants. In considering whether to reduce bail, the subsequent judge will take in account the severity of the crime, prior failures to appear for court, the amount of bail, and whether essential witnesses have appeared in court.

Following the bail hearing, defendants usually attend 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.[14] 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.[15] If defendants plead guilty or are found guilty, they are sentenced in a later hearing. Appendix Figure 1 provides the general timeline of the criminal justice process in a typical jurisdiction, although the precise timing of the process differs across jurisdictions.

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

*Philadelphia County:* Immediately following arrest in Philadelphia County, defendants are brought to 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 4-by-10 grid of bail guidelines (see Appendix Figure 2) 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, the bail hearing is 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 if present). 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 to the defendant, informs the defendant of his or her right to counsel, sets bail after hearing from representatives from the prosecutor's office and 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,

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[14]In Miami-Dade, misdemeanor arraignments coincide with the bail hearing, but felony arraignments generally occur several weeks after the bail hearing. In contrast, in Philadelphia, all arraignments usually happen within a month of the bail hearing.

[15]Prior work finds that approximately 95 percent of felony convictions are reached through a plea deal (see Durose and Langan 2007). Philadelphia differs from many other jurisdictions in its wide use of bench trials on felony cases and relatively low rates of both conviction and plea bargaining. In our sample from Philadelphia, 45 percent of defendants were not found guilty, 41 percent pled guilty before trial, and 15 percent were found guilty at trial.

most defendants in Miami-Dade can avoid a bail hearing by posting an amount designated by a standard bail schedule immediately following arrest and booking.[16] The Miami-Dade County 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 schedule discriminates against poor defendants by setting a fixed price on release according to the charged offense rather than taking into account a defendant's propensity for flight or crime. Approximately 30 percent of all defendants are able to secure release immediately, and the other 70 percent attend a bail hearing where their bail is determined by the assigned bail judge (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. For the 70 percent of defendants who attend the bail hearing, Miami-Dade conducts separate daily hearings for felony and misdemeanor cases. Both bail hearings are conducted by the bail judge on duty through videoconference to the central detention center. 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 standard bail amount may be lowered, raised, or remain the same depending on the case situation and the arguments made by defense counsel and the prosecutor. If a bail judge grants monetary bail, he or she often follows the amount recommended by the standard bail schedule, but the choice between monetary versus non-monetary bail conditions varies widely across judges in Miami-Dade (Goldkamp and Gottfredson 1988). Felony defendants are also screened by a Pre-Trial Services officer to identify individuals who may be eligible for pre-trial release. The information from the screening process is presented by this officer at the defendant's bail hearing.

*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, the Philadelphia Municipal Court has six arraignment court magistrates who work in the Preliminary Arraignment Court.[17] In Miami-Dade, there are multiple bail judges serving simultaneously to hear weekend bond hearings, allowing us to measure variation in bail decisions across judges for these cases. Approximately 60 different bail judges rotate through the felony and misdemeanor shift each Saturday and Sunday throughout the year.[18]

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 magistrates serve rotating

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[16]Non-bailable offenses include murder and domestic violence offenses. For a current version of the bail schedule by offense type, see http://www.brennanbailbonds.com/dade-county-bond-schedule-numerical.pdf.

[17]These judges serve four-year terms, are appointed by the Municipal Court Board of Judges, and are eligible for an unlimited number of reappointments. The bail judge positions were created by the Pennsylvania state legislature in 1984 in order to relieve the workload of Philadelphia Municipal Court judges. By law, Philadelphia bail judges are not required to be lawyers.

[18]We drop all cases heard by bail judges during the week in Miami-Dade, as only one judge typically handles these weekday hearings. The weekend bail judges are trial court judges from the misdemeanor and felony courts in Miami-Dade that assist the bail court with weekend cases.

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 different day of the week, the fact that these six magistrates 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. Similarly, in Miami-Dade, 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 serves the misdemeanor shift and another judge serves 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.[19]

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

Fourth, in both the Philadelphia and Miami-Dade systems, the bail judge is different from trial and sentencing judges, 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. Following the preliminary arraignment, cases in Philadelphia are assigned to a completely separate pool of trial judges. The bail judge in Miami-Dade County is also different from trial and sentencing judges. While the composition of the Miami-Dade trial courts is comprised of the same judges that rotate through

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[19]There are two potential complications with the judge rotation systems used in our setting. First, most defendants in our sample have the opportunity to appeal the initial bail decision in later proceedings, which can lead to modifications of the initial bail conditions. In our sample, approximately 20 percent of defendants petition for some modification of the initial bail decision. These subsequent bail decisions will be often be made by a different judge than the initial bail decision. We 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. We discuss this issue in greater detail below.

weekend bail shifts, the case is newly assigned after the bail hearing.[20]

## 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). The Data Appendix 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 levels. 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.[21] The case-level data include information on attorney type, arrest date, and the date of and judge presiding over each court appearance from arraignment to sentencing. Importantly, the case-level data also include information on bail type, bail amount when monetary bail is set, and whether bail was met. 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

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[20]The rotation schedules of the bail judges also do not align with the schedule of any other actors in the criminal justice system. In Philadelphia, non-capital attorneys handle matters within specified units, meaning that a different attorney handles each stage of the criminal proceedings such that staff is deployed on a "horizontal" basis. For instance, charging and bail are handled exclusively by Assistant District Attorneys in the Charging Unit. In the Trial Division Bureaus, a separate pool of Assistant District Attorneys handle misdemeanor trials and felony preliminary hearings. Likewise, if the defendant is represented by the Defender Association, he or she will have a different defense attorney at each stage because public defenders are assigned to courtrooms rather than to individual clients. In Miami-Dade, attorney representation is also deployed on a "horizontal" basis with different attorneys handling different stages of the criminal justice process. For instance, the Attorney General's office has a group of attorneys in the Criminal Intake Unit, which screens and files charges generally within 21-30 days following arrest. Similarly, in the Public Defender's Office, certain attorneys work in the Felony Early Representation Unit, aimed at serving clients between arrest and arraignment.

[21]In Florida, there are five distinct offense grades: F1 (first degree felony), F2 (second degree felony), F3 (third degree felony), M1 (first degree misdemeanor), and M2 (second degree misdemeanor). In Florida, misdemeanors are less serious crimes, punishable by up to one year in county jail whereas felonies are punishable by the death penalty or incarceration in a state prison. In Pennsylvania, there are 10 distinct offense grades: H (homicide), F1 (first degree felony), F2 (second degree felony), F3 (third degree felony), F (ungraded felony), M1 (first degree misdemeanor), M2 (second degree misdemeanor), M3 (third degree misdemeanor), M (ungraded misdemeanor), and S (summary offense). In Pennsylvania, summary offenses are minor breaks in the law punishable by up to 90 days in jail such as disorderly conduct, underage drinking, shoplifting (first offense), and criminal mischief. Individuals convicted of misdemeanors could be imprisoned for up to five years and individuals convicted of felonies could be sentenced to prison for more than five years.

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 Philadelpha and 97,538 cases from 66,067 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 matched 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.[22] Information on formal sector earnings and employment comes from annual W-2s issued by employers, and 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.

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, we impute AGI to equal the individual's W-2 earnings plus UI income reported by the state UI agency. 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) individual and household income from the first two full years after the bail hearing, and average from the third and fourth years after the bail hearing, as outcome measures.

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

We match the court data to administrative tax data from the IRS using first and last name, date of birth, gender, zipcode, and state of residence.[23] We were able to successfully match approximately

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[22]Undocumented immigrants without a valid SSN are not included in these data.

[23]Specifically, 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

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. 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.[24] In Section IV.E, we explore the robustness of our results to alternative measures of pre-trial release. Additional summary statistics by bail type are presented in Appendix Table 1.

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, 36.8 percent are released ROR, 21.7 percent are released on non-monetary bail, and 41.5 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 three days, 94 percent are detained on monetary bail with an average bail amount of $52,577 and median bail amount of $7,500.

Panel B presents demographic characteristics of defendants. In our sample, 38.5 percent of detained defendants are white and 60.5 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 presents offense characteristics of defendants in our sample. Detained defendants are arrested and charged for 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, 28.9 percent are charged with a violent offense and 34.6 percent are charged with a property offense. In contrast, only 19.1 percent of released defendants are charged with a violent offense and 18.5 percent are charged with a property offense. Released defendants are also much more likely to be charged with drug offenses. In general, released defendants are substantially less likely to be charged with felonies compared to detained defendants.

Panel D presents case outcomes, future crime, and labor market outcomes by detention status. In our sample, 57.1 percent of detained defendants are found guilty of at least one charge compared

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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 zipcode matched a zipcode 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.

[24]See, for example, the 3DaysCount project at the Pretrial Justice Institute.

to 48.7 percent of released defendants. Forty-three percent of detained defendants plead guilty compared to just 20.8 percent of released defendants. Detained defendants are also 15.0 percent more likely to be incarcerated compared to released defendants, and have prison sentences that are 264.6 days longer on average. Conversely, released defendants are more likely to fail to appear in court and more likely to abscond from the jurisdiction, with 17.9 percent of released defendants failing to appear compared to 12.1 percent of detained defendants. Released defendants also experience a longer time between bail and case disposition compared to detained defendants, with released defendants waiting 247.1 days between bail and disposition compared to 200.2 days for detained defendants.

In terms of crime, released defendants are more likely to be rearrested prior to case disposition compared to detained defendants, with 23.6 percent of released defendants rearrested before disposition compared to 20.2 percent of detained defendants. Released defendants, however, are also less likely to be rearrested in the several years after the bail hearing. By three to four years post-bail, 26.8 percent of released defendants are rearrested compared to 32.2 percent of detained defendants.

Finally, released defendants earn substantially more in the two years post-bail compared to detained defendants and are more likely to be employed. In our sample, 37.8 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 $5,202 in reported earnings compared to $7,897 for released defendants. Released defendants are also more likely to receive any income in the the first two years post-bail compared to detained defendants. Differences in earnings outcomes of released and detained defendants also persist three to four years post-bail. By three to four years post-bail, 37.9 percent of detained defendants are employed in the formal labor market compared to 48.2 percent of released defendants, with detained defendants make annual reported earnings of $5,861 compared to $8,363 for released defendants.

## III.  Research Design

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

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

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 $\varepsilon_{ict}$ is an error term. The key problem for inference is that OLS estimates of equation (1) are likely to be biased by the correlation between 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.[25]

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

$$Released^*_{ict} = Released_{ict} - \gamma \mathbf{X}_{ict} = Z_{ctj} + \varepsilon_{ict} \tag{2}$$

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 $\varepsilon_{ict}$.

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

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

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

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[25]Our approach is also similar to the procedure used to estimate teacher value-added accounting for baseline differences across students (e.g. Chetty, Friedman, and Rockoff 2014).

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 -0.150 to 0.179 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 32.9 percentage points, a 59.6 percent change from the mean three day release rate of 55.2 percentage points.

In practice, a judge affects whether a defendant is released pre-trial through a combination of different bail decisions (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 3 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 2. We find that judges who are more likely to use conditional non-monetary bail are also more likely to release defendants pre-trial. Additionally, judges who are more likely to use monetary bail and assign higher monetary bail amounts are less likely to release defendants pre-trial. These results suggest that defendants on the margin of pre-trial release are those for whom judges disagree about the appropriateness of non-monetary bail versus monetary bail.

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 ef-

fects, 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 an accused 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:

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

where the vector $\mathbf{X}_{ict}$ includes court-by-time fixed effects. As described previously, $Z_{ctj}$ are leave-out (jackknife) measures of judge leniency that are allowed to vary across years. We obtain similar results using a probit model, which is unsurprising given that the mean three day pre-trial release rate is 0.552 and far from zero or one. Robust standard errors are clustered at the individual 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 increasing in our leniency measure, and is close to linear. A 10 percentage point increase in the residualized judge's release rate in other cases is associated with an approximately seven percentage point increase in the probability that an individual is released before trial.

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, 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 an arrested defendant is released pre-trial, with an F-statistic for the instrument of 569.1. Including controls in columns 3 and 4 do 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 6.9 percentage points more likely to be released pre-trial. This estimate suggests that moving from the least to most lenient judge would imply an increase in the likelihood of pre-trial release by 22.7 percentage points, a 41.1 percent increase from the mean rate of pre-trial release.

The probability of pre-trial release does not increase one-for-one with our measure of judge

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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 detention outcomes in our setting.

Appendix Table 3 presents additional first stage results. We find that a defendant assigned to a bail judge that is 10 percentage points more likely to release a defendant pre-trial is 4.0 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 5.7 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 cluster standard errors at the individual level. We find that male defendants are 11.4 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 55.2 percent. Black defendants are 3.9 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 15.3 percentage points less likely to be released compared to defendants with no prior offense, a 27.7 percent decrease. Additionally, defendants arrested for felonies are 25.4 percentage points less likely to be released than those arrested for misdemeanors, a 46.0 percent decrease. Drug defendants are 11.9 percentage points more likely to be released compared to defendants in the omitted category, and DUI defendants are 10.6 percentages points more likely to be released. Violent defendants are 1.6 percentage points less likely to be released compared to defendants in the omitted category, while property defendants are 0.9 percentage points more likely to be released. 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 = 0.26), suggesting that the exclusion restriction is valid in our setting.

Nevertheless, the exclusion restriction could also be violated if bail conditions imposed by judges had an independent effect on outcomes other than through the channel of pre-trial release. For exam-

ple, conditional on a defendant posting monetary bail, a higher bail amount may have independent effects on outcomes. Note that in our setting, the exclusion restriction is more likely to be valid than in the context of using sentencing judge tendencies as an instrument for incarceration (Kling 2006, Mueller-Smith 2015). In the context of sentencing, judges impose multiple treatments such as incarceration, probation, and fines (Mueller-Smith 2015). In contrast, bail judges in our setting exclusively handle the setting of bail and a separate judge takes over the subsequent trial and sentencing processes. However, 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 4. 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, this monotonicity assumption requires that individuals released by a strict judge would also be released by a more lenient judge, and similarly that individuals detained by a lenient judge would also be detained by a stricter judge. One testable implication of the monotonicity assumption is that the first stage estimates should be non-negative for all subsamples. Panel A of Appendix Table 5 and Appendix Table 6 present these first stage results using the full sample of cases to calculate our measure of judge leniency. In all subsamples, we find that our residualized measure of judge leniency is consistently positive and sizable, in line with the monotonicity assumption.

A second implication of the monotonicity assumption is that judges who are stricter towards one group (e.g., minority defendants) are also relatively strict towards other defendants outside of this group (e.g., white defendants). Following Bhuller et al. (2016), we test this assumption by estimating first stage results for all subsamples, but recalculate our judge leniency instrument in each subsample using cases from the opposing subsample. Panel B of Appendix Table 5 and Appendix Table 6 present these results. In all subsamples, we find that our first stage estimates using this "reverse-sample instrument" are positive and statistically different from zero.

Appendix Figure 4 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 is non-negative, suggesting that judge tendencies are similar across observably different defendants and cases. We provide further evidence that the monotonicity condition is satisfied in robustness checks.

# 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 bail jumping, future crime, and labor market outcomes.

## A. Case Outcomes

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 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 clustered at the individual 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 18.6 percentage points less likely to plead guilty, a 42.7 percent decrease from the mean for detained defendants. When we add baseline crime and defendant controls (column 3), the magnitude of the estimate is more than halved, dropping to 9.2 percentage points. In contrast, adding baseline IRS controls (column 4) does not change the size of the estimate, which remains at 9.2 percentage points. These results suggest that, at least for case outcomes, crime and defendant controls are important for addressing potential omitted variable bias. Controls for baseline labor market outcomes appear relatively unimportant for these case outcomes.

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 15.6 percentage points

less likely to be found guilty, a 27.3 percent decrease from the mean, and 12.0 percentage points less likely to plead guilty, a 27.5 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 7, 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 find that the marginal released defendant is also 3.0 percentage points less likely to be incarcerated after case disposition, a 10.1 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. In Section IV.D, we find that the impact of pre-trial release on incarceration is large and statistically significant for felony and drug defendants, cases with a much higher baseline rate of incarceration. These results suggest that pre-trial release reduces post-trial incarceration primarily for defendants charged with more severe crimes, most likely through a reduction in the extensive margin of conviction rather than the intensive margin of punishment conditional on conviction.

To make the counterfactual more precise, we estimate results that differentiate between release without any conditions (ROR) and release with conditions. By separately estimating these two decision margins relative to 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.[26]

In Appendix Table 9, 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. While the OLS results suggest that pre-trial release improves case outcomes more for defendants who are released with no conditions relative to those who are released with conditions, the two-stage least squares estimates show no statistically significant differences in the effect of pre-trial release on marginal defendants with and without conditions. For example, the marginal defendant released with no conditions is 12.9 percentage points less likely to plead guilty and the marginal defendant released with conditions is 11.9 percentage points less likely to plead guilty. Our standard errors are also precise enough that we can rule out any large differences by release type. These findings suggest that pre-trial release by itself improves case outcomes.

## B. Bail Jumping 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 bail jumping and future crime.

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[26]In Appendix Table 8, we 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.

Table 5 examines the impact of pre-trial release on various procedural measures of court performance. We find that pre-trial release leads to substantial increases in failing to appear for required court appearances and fleeing from the jurisdiction.[27] The OLS estimates show that released defendants are significantly more likely to miss a court appearance and jump bail. 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 6.3 percentage points more likely to miss a court appearance, a 52.1 percent decrease from the mean for detained defendants. When we add baseline crime and defendant controls (column 3), the magnitude of the estimate drops to only 0.7 percentage points. Adding baseline IRS controls (column 4) does not significantly change the size of the estimate.

Our two-stage least squares results also suggest that released defendants are more likely to miss court appearances and jump bail. Controlling for our full set of controls (column 7), we find that the marginal released defendant is 15.0 percentage points more likely to fail to appear in court, a 124.0 percent increase from the mean. Bail jumping 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. As before, we find no statistically significant differences in the effect of pre-trial release on failures to appear for marginal defendants released with and without conditions (Appendix Table 9).

We also find that the marginal released defendant waits an extra 48.6 days between bail and case disposition, a 24.3 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.[28] In addition, marginal released defendants may wait longer between bail and case disposition because they are less likely to plead guilty.

Table 6 presents estimates of the impact of pre-trial release on future criminal behavior. We explore both short-run (mechanical) effects prior to case disposition and longer-term effects up to four years after the bail hearing. For crime within two years, our sample is limited to the 306,634 defendants who we observe for two years following the bail hearing, and for crime within four years, our sample is limited to the 202,166 defendants who we observe for four years following the bail hearing. We measure crime using rearrests, but the results follow a similar pattern if we use new convictions instead.

Both with and without baseline controls, our OLS results show that released defendants are more likely to be arrested before case disposition, but less likely to be arrested in future years. For example, with all baseline controls (column 4), we find that released defendants are 5.8 percentage

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[27]These measures are only available in our Philadelphia sample.

[28]In Pennsylvania, other than cases in which a defendant is not entitled to release on bail, a defendant generally cannot be detained for longer than 180 days from the date the complaint is filed. In Florida, formal charges are usually filed within 21 days of arrest, and may not be delayed beyond 33 days. If charges have not been filed within 33 days of arrest, any defendants who are detained will be released from custody ROR. In Florida, persons charged with felonies are entitled to be brought to trial within 175 days of having been taken into custody and persons charged with misdemeanors are entitled to be brought to trial within 90 days of having been taken into custody.

points more likely to be rearrested compared to a detained defendant, a 28.7 percent increase from the mean. These results are largely mechanical – it is impossible to be rearrested for a new crime when one is incapacitated. Nevertheless, these results suggest that released defendants are still engaged in some criminal behavior before case disposition. In contrast, released defendants are 1.8 percentage points less likely to be rearrested in the two years following the bail hearing, *including the time period prior to case disposition.* Similarly, released defendants are 4.7 percentage points less likely to be rearrested in the third to fourth years following the bail hearing. In contrast to the case outcomes results, the OLS results are relatively insensitive to the addition of controls for crime characteristics or baseline labor market outcomes.

Our two-stage least squares results follow a similar pattern for arrests prior to case disposition and in the first two years following the bail hearing. With all baseline controls (column 7), we find that the marginal released defendant is 7.6 percentage points more likely to be rearrested for a new crime prior to disposition, a 37.6 percent increase from the mean, but 5.0 percentage points less likely to be arrested in the two years following the bail hearing. The point estimate for our two year result is not statistically significant, however. In contrast to the OLS results, we find a positive but statistically insignificant point estimate on rearrest in the third to fourth years following the bail hearing. Taken together, we interpret these results as suggesting that pre-trial detention prevents new criminal activity prior to case disposition through a short-run incapacitation effect, but has little net impact on future crime following case disposition. However, we emphasize that for longer-run measures of crime, the large standard errors and smaller sample size makes definitive conclusions difficult. Finally, we again find that our results on future crime are similar for marginal defendants who are released with and without conditions (Appendix Table 9).

### C. Labor Market and Tax Administration Outcomes

Table 7 presents estimates of the impact of pre-trial release on individual-level formal sector earnings and employment. For outcomes measured as the average across the first two years post-bail hearing, our sample is limited to the 302,816 defendants matched to IRS data with cases before 2013, and for outcomes measured as the average over the third to fourth years post-bail hearing, our sample is limited to the 224,319 defendants matched to IRS data with cases before 2011.

The OLS estimates in Table 7 show that released defendants have significantly higher formal sector earnings and employment following the bail hearing. For example, with all baseline controls (column 4), released defendants are 5.1 percentage points more likely to be employed within two years of the bail hearing, a 13.5 percent increase from the mean, and 3.6 percentage points more likely to have any income, a 7.8 percent increase from the mean. However, the magnitude of these OLS estimates are extremely sensitive to the addition of baseline labor market controls. For example, with no baseline controls (column 2), the OLS estimates suggest that released defendants earn approximately $2,681 more per year than detained defendants in the two years following the bail hearing, a 51.5 percent increase from the mean. Adding baseline crime and defendant controls does little to alter the significance and magnitude of these results (column 3). However, the estimate

falls to only $406 with the addition of the baseline IRS controls. The OLS results follow a similar pattern for formal sector earnings and employment in the third to fourth years following the bail hearing. These results imply that, perhaps not surprisingly, controlling for baseline earnings and employment is critical when estimating the impact of pre-trial release on labor market outcomes.

The two-stage least squares estimates are broadly similar to the OLS estimates with baseline controls, but much less precisely estimated. With our full set of baseline controls (column 7), we find that marginal released defendants are 9.7 percentage points more likely to have any income two years after bail, a 21.0 percent increase from the mean. Estimates on other outcomes in the first two years post-bail hearing are smaller and not statistically different from zero. By three to four years post-bail hearing, released defendants are 10.2 percentage points more likely to be employed in the formal labor sector, a 26.9 percent increase from the mean. Formal sector earnings are $1,024 higher over the same time period, a 17.5 percent increase from the mean, and the probability of having any income is 8.5 percentage points higher, a 18.3 percent increase from the mean. Although neither of these estimates are statistically significant, they are broadly consistent with the more precise OLS estimates with our full set of baseline controls (column 4). When we separately identify marginal defendants who are released with and without conditions, we find no significant differences in the impact of pre-trial release by release type (Appendix Table 9).[29]

Table 8 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. For example, only 30.8 percent of detained defendants file a tax return three to four years after the bail hearing, much lower than would be typical for the average working-age adult. In our two-stage least squares results with the full set of controls (column 7), we find that released defendants are 9.5 percentage points more likely to file a tax return one to two years post-bail, a 22.4 percent increase from the mean. Pre-trial release also increases the receipt of EITC benefits by $185 over the same time period, a 55.4 percent increase. Three to four years post-bail, released defendants are 4.3 percentage points more likely to file a tax return, a 14.0 percent increase from the mean, and receive an additional $323 in UI benefits and $239 in EITC benefits, 131.8 and 66.4 percent increases from the mean, respectively. These results suggest that pre-trial release allows individuals to remain connected to the formal sector, 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 9 presents selected two-stage least squares subsample results by crime severity and highest crime type. We find that the effects on case outcomes are consistently larger for misdemeanor

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[29]A valid question is why we find a significant impact of pre-trial release on the extensive margin, 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 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. These results are available upon request from the authors.

offenses. For example, released misdemeanor defendants are 19.0 percentage points less likely to be found guilty and 18.6 percentage points less likely to plead guilty. In contrast, released felony defendants are only 10.0 percentage points less likely to be found guilty and 1.2 percentage points less likely to plead guilty. However, effects on incarceration are nearly 10 percentage points larger for felony defendants, likely because the baseline rate of incarceration is nearly three times higher for felonies compared to misdemeanors. Released felony defendants are also 9.4 percentage points more likely to miss a court appearance compared to released misdemeanor defendants, potentially because felony defendants have a higher flight risk. Effects for other outcomes are largely similar for the two groups.

By crime type, case outcomes are most precisely estimated for drug and property defendants. For example, released drug defendants are 10.8 percentage points less likely to be found guilty and 12.8 percentage points less likely to be incarcerated. Released property defendants are 14.3 percentage points less likely to be found guilty and 10.7 percentage points less likely to plead guilty. However, the point estimates for nearly all outcomes, particularly labor market outcomes, are larger for defendants charged with violent offenses, although large standard errors mean that few of the estimates are statistically significant. With that caveat, it is possible that the larger labor market effects for violent offenders are due to the greater stigma associated with a conviction for a violent offense (Holzer et al. 2003).

Table 10 presents additional subsample results by defendant race, prior criminal history, and baseline employment status. We find that for nearly all outcomes, our estimates are larger for white defendants compared to black defendants. For example, released white defendants are 25.9 percentage points less likely to be found guilty and 19.8 percentage points less likely to plead guilty, compared to only 10.1 percentage points and 12.2 percentage points less likely for released black defendants. Released white defendants are also more likely to be rearrested prior to disposition than released black defendants. Finally, we find that released white defendants are substantially more likely to have any income several years post-bail compared to released black defendants.

By criminal history, we find that the impacts of pre-trial release are largest for those without a prior offense in the past year. For individuals without a recent prior offense, released defendants are 20.2 percentage points less likely to be found guilty, 15.2 percentage points less likely to plead guilty, and 10.7 percentage points more likely to be employed three to four years post-bail. In contrast, almost all results for individuals with a recent prior offense are small and imprecisely estimated. These results suggest that the social costs imposed by pre-trial detention are relatively larger for those with more limited ties to the criminal justice system.

We also find suggestive evidence that pre-trial release improves case outcomes the most for defendants who were employed in the year prior to bail. For example, among those employed prior to the bail hearing, released defendants are 18.5 percentage points less likely to be found guilty, 10.9 percentage points less likely to plead guilty, and 7.3 percentage points less likely to be incarcerated. In contrast, among those not employed prior to the bail hearing, released defendants are 10.3 percentage points less likely to be found guilty, 10.7 percentage points less likely to plead

guilty, and 2.0 percentage points less likely to be incarcerated. While not precisely estimated, our labor market results are also larger for individuals who were employed prior to bail. Overall, these results suggest that pre-trial detention imposes relatively larger costs on individuals with stronger ties to the formal labor sector.

## E. Robustness Checks

Appendix Table 10 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. 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 11 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 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 reason 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 7). The fact that so many of our results are driven by changes at 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.

*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 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. Mueller-Smith 2015, 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. For example, we find that pre-trial release increases formal labor market employment in the three to four years post-bail, but the average sentence length in our sample is only 186 days. Among misdemeanor offenses, the average sentence length is 30 days, yet we continue to find suggestive evidence of increased labor market attachment several years post-bail.

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 12, we present estimates of the joint probability of conviction and employment in the several years post-bail. 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 has no detectable effect on future crime. Finally, we find suggestive evidence 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 and defendants employed in the year prior to bail. We argue that these results are consistent with (i) pre-trial release strengthening defendants' bargaining position during plea negotiations, and (ii) a criminal conviction lowering defendants' attachment to the formal labor market.

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 recidivism rates are substantially lower among individuals who randomly receive electronic monitoring compared to detention. Electronic monitoring is also extremely cost-effective 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 could provide many of the same benefits of pre-trial detention without the substantial costs to defendants documented in our analysis.

In addition, our results offer important lessons for tax administration and policy related to individuals who interact with the criminal justice system. As described above, we find that criminal defendants are substantially more likely to file a tax return and to claim the EITC in subsequent years if they are released pre-trial. One possible explanation is that pre-trial release raises the likelihood of formal sector employment, which is a prerequisite for claiming the EITC. In turn, eligibility for the EITC may greatly increase the incentives for individuals to file tax returns. Another explanation is that pre-trial release may contribute to tax filing and benefit take-up by improving (all else equal) defendants' attitudes towards the government. These findings suggest that bail reform and outreach to those with criminal convictions may be useful methods for increasing tax filing and the take-up of social benefits.

There are at least three 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 a harsher bail judge. 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, we are unable to draw any sharp welfare conclusions about the optimality of the current bail system using our research design. The substantial costs of the bail system documented in our analysis must be weighed against the social benefits of increasing court appearances and decreasing pre-trial crime. While beyond the scope of this paper, developing a framework to assess the welfare effects of the bail system is an important area of future work.

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Table 1
Descriptive Statistics

| | Initial Bail Decision | |
|---|---|---|
| | Detained | Released |
| *Panel A: Bail Type* | (1) | (2) |
| Release on Recognizance | 0.017 | 0.368 |
| Non-Monetary Bail | 0.042 | 0.217 |
| Monetary Bail | 0.940 | 0.415 |
| Bail Amount (in thousands) | 52.577 | 12.497 |
| | | |
| *Panel B: Characteristics* | | |
| Male | 0.875 | 0.785 |
| White | 0.385 | 0.424 |
| Black | 0.605 | 0.556 |
| Age at Bail Decision | 33.896 | 33.468 |
| Prior Offense in Past Year | 0.335 | 0.193 |
| | | |
| *Panel C: Charge Characteristics* | | |
| Number of Offenses | 3.729 | 2.508 |
| Felony Offense | 0.635 | 0.327 |
| Misdemeanor Only | 0.365 | 0.673 |
| Any Drug Offense | 0.288 | 0.420 |
| Any DUI Offense | 0.024 | 0.116 |
| Any Violent Offense | 0.289 | 0.191 |
| Any Property Offense | 0.346 | 0.185 |
| | | |
| *Panel D: Outcomes* | | |
| Any Guilty Offense | 0.571 | 0.487 |
| Guilty Plea | 0.436 | 0.208 |
| Any Incarceration | 0.296 | 0.146 |
| Failure to Appear in Court | 0.121 | 0.179 |
| Absconded | 0.002 | 0.007 |
| Days to Case Decision | 200.237 | 247.114 |
| Rearrest Prior to Disposition | 0.202 | 0.236 |
| Rearrest in 0-2 Years | 0.453 | 0.397 |
| Rearrest in 3-4 Years | 0.322 | 0.268 |
| Earnings (thousands) in 1-2 Years | 5.202 | 7.897 |
| Employed in 1-2 Years | 0.378 | 0.509 |
| Any Income in 1-2 Years | 0.462 | 0.521 |
| Earnings (thousands) in 3-4 Years | 5.861 | 8.363 |
| Employed in 3-4 Years | 0.379 | 0.482 |
| Any Income in 3-4 Years | 0.465 | 0.507 |
| | | |
| *Panel E: Data Quality* | | |
| Missing Race | 0.273 | 0.483 |
| Observations | 190,769 | 235,261 |

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

Table 2
Judge Leniency and Pre-Trial Release

|  | Sample Mean | Judge Leniency | | |
|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) |
| Pre-Trial Release | 0.552 | 0.693*** | 0.690*** | 0.687*** |
|  | (0.497) | (0.025) | (0.023) | (0.023) |
| Court x Year FE | – | Yes | Yes | Yes |
| Crime Controls | – | No | Yes | Yes |
| IRS Controls | – | No | No | Yes |
| Observations | 426,030 | 426,030 | 426,030 | 426,030 |

Notes: This table reports first stage results. The regressions are estimated on the sample as described in the notes to Table 1. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. The dependent variable is an indicator for being released before trial within three days of the bail hearing. 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 clustered at the individual level are reported in parentheses. *** = 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 | −0.11385*** | −0.00003 |
| | (0.00199) | (0.00013) |
| Black | −0.03922*** | 0.00005 |
| | (0.00194) | (0.00015) |
| Age at Bail Decision | −0.01197*** | −0.00006 |
| | (0.00065) | (0.00005) |
| Prior Offense in Past Year | −0.15286*** | 0.00010 |
| | (0.00164) | (0.00011) |
| Number of Offenses | −0.02469*** | −0.00001 |
| | (0.00032) | (0.00002) |
| Felony Offense | −0.25377*** | 0.00011 |
| | (0.00198) | (0.00010) |
| Any Drug Offense | 0.11910*** | 0.00014 |
| | (0.00192) | (0.00013) |
| Any DUI Offense | 0.10627*** | 0.00022 |
| | (0.00261) | (0.00015) |
| Any Violent Offense | −0.01563*** | 0.00013 |
| | (0.00226) | (0.00015) |
| Any Property Offense | 0.00832*** | −0.00027* |
| | (0.00201) | (0.00015) |
| Matched to IRS Data | 0.00759*** | −0.00004 |
| | (0.00208) | (0.00014) |
| Baseline Earnings | 0.00112*** | −0.00001 |
| | (0.00008) | (0.00001) |
| Baseline UI | 0.00287*** | −0.00002 |
| | (0.00042) | (0.00002) |
| Baseline EITC | 0.01187*** | 0.00002 |
| | (0.00102) | (0.00007) |
| Baseline Filed Return | 0.04826*** | −0.00018 |
| | (0.00252) | (0.00019) |
| Baseline Employed | 0.02529*** | 0.00015 |
| | (0.00205) | (0.00015) |
| Baseline Any EITC | −0.01631*** | 0.00006 |
| | (0.00347) | (0.00026) |
| Baseline Any Income | 0.00000 | 0.00000 |
| | (0.00000) | (0.00000) |
| Baseline Any UI | 0.02333*** | 0.00032 |
| | (0.00396) | (0.00026) |
| Joint F-Test | [0.00000] | [0.26686] |
| Observations | 426,030 | 426,030 |

Notes: This table reports reduced form results testing the random assignment of cases to bail judges. Judge leniency is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III. Column 1 reports estimates from an OLS regression of 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 clustered at the individual level are reported in parentheses in all specifications. *** = 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 Case Outcomes

| | Detained Mean | OLS Results | | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Any Guilty Offense | 0.571 | −0.070*** | −0.052*** | −0.050*** | −0.149*** | −0.157*** | −0.156*** |
| | (0.495) | (0.002) | (0.002) | (0.002) | (0.036) | (0.033) | (0.034) |
| Guilty Plea | 0.436 | −0.186*** | −0.092*** | −0.092*** | −0.115*** | −0.121*** | −0.120*** |
| | (0.496) | (0.002) | (0.002) | (0.002) | (0.034) | (0.032) | (0.032) |
| Any Incarceration | 0.296 | −0.158*** | −0.101*** | −0.098*** | −0.018 | −0.032 | −0.030 |
| | (0.457) | (0.001) | (0.001) | (0.001) | (0.028) | (0.027) | (0.027) |
| 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 | 190,769 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 |

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

Table 5

Pre-Trial Release and Process Outcomes

| | Detained Mean | OLS Results | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Failure to Appear in Court | 0.121 | 0.063*** | 0.007*** | 0.010*** | 0.153*** | 0.149*** | 0.150*** |
| | (0.326) | (0.001) | (0.001) | (0.001) | (0.032) | (0.031) | (0.031) |
| Absconded | 0.002 | 0.005*** | 0.002*** | 0.002*** | 0.006 | 0.006 | 0.006 |
| | (0.045) | (0.000) | (0.000) | (0.000) | (0.006) | (0.006) | (0.006) |
| Days to Case Decision | 200.237 | 13.449*** | 34.174*** | 34.117*** | 52.980*** | 48.674*** | 48.591*** |
| | (250.014) | (0.851) | (0.886) | (0.895) | (15.761) | (15.543) | (15.610) |
| 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 | 190,769 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 |

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

Table 6

Pre-Trial Release and Future Crime

| | Detained Mean | OLS Results | | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Rearrest Prior to Disposition | 0.202 | 0.042*** | 0.055*** | 0.058*** | 0.077** | 0.074** | 0.076** |
| | (0.402) | (0.001) | (0.001) | (0.001) | (0.033) | (0.033) | (0.033) |
| Rearrest in 0-2 Years | 0.453 | −0.049*** | −0.025*** | −0.018*** | −0.056 | −0.054 | −0.050 |
| | (0.498) | (0.002) | (0.002) | (0.002) | (0.049) | (0.048) | (0.047) |
| Rearrest in 3-4 Years | 0.322 | −0.060*** | −0.053*** | −0.047*** | 0.059 | 0.061 | 0.063 |
| | (0.467) | (0.002) | (0.002) | (0.002) | (0.053) | (0.054) | (0.054) |
| 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 | 190,769 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 |

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

Table 7
Pre-Trial Release and Labor Market Outcomes

| | Detained Mean | OLS Results | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Years 1-2* | | | | | | | |
| Employed | 0.378 | 0.134*** | 0.102*** | 0.051*** | 0.073 | 0.068 | 0.037 |
| | (0.485) | (0.002) | (0.002) | (0.002) | (0.049) | (0.049) | (0.043) |
| Any Income | 0.462 | 0.103*** | 0.080*** | 0.036*** | 0.131*** | 0.125*** | 0.097** |
| | (0.499) | (0.002) | (0.002) | (0.002) | (0.048) | (0.048) | (0.045) |
| Earnings (thousands) | 5.202 | 2.681*** | 2.239*** | 0.406*** | 0.461 | 0.793 | −0.462 |
| | (15.108) | (0.071) | (0.073) | (0.044) | (1.536) | (1.539) | (0.976) |
| Household Income (thousands) | 10.215 | 2.689*** | 2.191*** | 0.241*** | 2.250 | 2.770 | −0.251 |
| | (22.741) | (0.105) | (0.107) | (0.065) | (2.214) | (2.228) | (1.532) |
| | | | | | | | |
| *Panel B: Years 3-4* | | | | | | | |
| Employed | 0.379 | 0.103*** | 0.077*** | 0.032*** | 0.111* | 0.118** | 0.102* |
| | (0.485) | (0.002) | (0.003) | (0.002) | (0.057) | (0.058) | (0.053) |
| Any Income | 0.465 | 0.089*** | 0.066*** | 0.030*** | 0.112** | 0.110* | 0.085 |
| | (0.499) | (0.002) | (0.003) | (0.002) | (0.055) | (0.057) | (0.054) |
| Earnings (thousands) | 5.861 | 2.416*** | 1.995*** | 0.224*** | 0.334 | 1.142 | 1.024 |
| | (15.806) | (0.085) | (0.088) | (0.061) | (1.858) | (1.898) | (1.338) |
| Household Income (thousands) | 10.953 | 2.434*** | 1.946*** | 0.036 | 1.259 | 2.085 | −0.083 |
| | (23.879) | (0.126) | (0.130) | (0.088) | (2.664) | (2.729) | (2.022) |
| | | | | | | | |
| 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 | 147,295 | 338,793 | 338,793 | 338,793 | 338,793 | 338,793 | 338,793 |

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

Table 8

Pre-Trial Release and Social Benefits Take-Up

| | Detained Mean | OLS Results | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Years 1-2* | | | | | | | |
| Filed Return | 0.424 | 0.091*** | 0.071*** | 0.032*** | 0.126*** | 0.122*** | 0.095** |
| | (0.494) | (0.002) | (0.002) | (0.002) | (0.039) | (0.039) | (0.037) |
| UI (thousands) | 0.282 | 0.413*** | 0.375*** | 0.205*** | 0.170 | 0.178 | 0.060 |
| | (1.535) | (0.008) | (0.010) | (0.009) | (0.176) | (0.178) | (0.171) |
| EITC (thousands) | 0.334 | 0.189*** | 0.154*** | 0.093*** | 0.252** | 0.237** | 0.185* |
| | (0.954) | (0.004) | (0.005) | (0.004) | (0.110) | (0.109) | (0.099) |
| Any UI | 0.066 | 0.067*** | 0.058*** | 0.030*** | 0.058** | 0.058** | 0.036 |
| | (0.248) | (0.001) | (0.001) | (0.001) | (0.028) | (0.028) | (0.026) |
| Any EITC | 0.220 | 0.070*** | 0.055*** | 0.033*** | 0.122*** | 0.114*** | 0.100** |
| | (0.414) | (0.002) | (0.002) | (0.002) | (0.044) | (0.043) | (0.042) |
| | | | | | | | |
| *Panel B: Years 3-4* | | | | | | | |
| Filed Return | 0.308 | 0.056*** | 0.043*** | 0.019*** | 0.065** | 0.062* | 0.043 |
| | (0.462) | (0.002) | (0.002) | (0.002) | (0.033) | (0.033) | (0.032) |
| UI (thousands) | 0.245 | 0.276*** | 0.253*** | 0.155*** | 0.326* | 0.361* | 0.323 |
| | (1.333) | (0.008) | (0.009) | (0.008) | (0.195) | (0.201) | (0.198) |
| EITC (thousands) | 0.360 | 0.177*** | 0.139*** | 0.090*** | 0.345** | 0.312** | 0.239* |
| | (1.002) | (0.005) | (0.006) | (0.006) | (0.134) | (0.135) | (0.128) |
| Any UI | 0.064 | 0.055*** | 0.048*** | 0.030*** | 0.027 | 0.030 | 0.020 |
| | (0.245) | (0.001) | (0.001) | (0.001) | (0.033) | (0.034) | (0.033) |
| Any EITC | 0.234 | 0.057*** | 0.042*** | 0.026*** | 0.143*** | 0.134** | 0.113** |
| | (0.423) | (0.002) | (0.002) | (0.002) | (0.052) | (0.052) | (0.051) |
| | | | | | | | |
| 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 | 147,295 | 338,793 | 338,793 | 338,793 | 338,793 | 338,793 | 338,793 |

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

Table 9
Results by Crime Characteristics

| | Crime Severity | | Crime Type | | | |
|---|---|---|---|---|---|---|
| | Misd. | Felony | Drug | DUI | Property | Violent |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Any Guilty Offense | −0.190*** | −0.100 | −0.108* | 0.085 | −0.143*** | −0.538 |
| | (0.035) | (0.068) | (0.063) | (0.121) | (0.050) | (0.382) |
| | [0.509] | [0.543] | [0.667] | [0.528] | [0.548] | [0.291] |
| Guilty Plea | −0.186*** | −0.012 | −0.088 | −0.028 | −0.107** | −0.203 |
| | (0.032) | (0.068) | (0.061) | (0.108) | (0.049) | (0.298) |
| | [0.214] | [0.420] | [0.349] | [0.264] | [0.411] | [0.200] |
| Any Incarceration | 0.017 | −0.116* | −0.128** | 0.109 | 0.005 | −0.274 |
| | (0.019) | (0.065) | (0.055) | (0.114) | (0.043) | (0.263) |
| | [0.112] | [0.330] | [0.255] | [0.430] | [0.223] | [0.148] |
| Failure to Appear in Court | 0.116*** | 0.210*** | 0.193** | 0.095 | 0.141*** | 0.120 |
| | (0.041) | (0.049) | (0.079) | (0.102) | (0.036) | (0.246) |
| | [0.201] | [0.101] | [0.237] | [0.182] | [0.187] | [0.082] |
| Rearrest Prior to Disposition | 0.084*** | 0.064 | 0.012 | 0.045 | 0.038 | 0.385 |
| | (0.029) | (0.074) | (0.065) | (0.097) | (0.055) | (0.359) |
| | [0.192] | [0.254] | [0.271] | [0.163] | [0.251] | [0.142] |
| Employed in 1-2 Years | 0.038 | 0.033 | 0.132* | −0.060 | −0.002 | 0.475 |
| | (0.050) | (0.075) | (0.080) | (0.114) | (0.075) | (0.318) |
| | [0.481] | [0.418] | [0.424] | [0.601] | [0.397] | [0.490] |
| Any Income in 1-2 Years | 0.114** | 0.074 | 0.116 | −0.020 | 0.044 | 0.627* |
| | (0.052) | (0.078) | (0.083) | (0.121) | (0.078) | (0.345) |
| | [0.509] | [0.480] | [0.458] | [0.591] | [0.485] | [0.524] |
| Employed in 3-4 Years | 0.091 | 0.117 | 0.119 | −0.031 | −0.111 | 0.877** |
| | (0.061) | (0.091) | (0.096) | (0.228) | (0.105) | (0.378) |
| | [0.458] | [0.414] | [0.407] | [0.568] | [0.387] | [0.469] |
| Any Income in 3-4 Years | 0.058 | 0.122 | 0.028 | −0.183 | 0.057 | 0.576* |
| | (0.063) | (0.092) | (0.097) | (0.239) | (0.105) | (0.329) |
| | [0.492] | [0.486] | [0.461] | [0.556] | [0.477] | [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 | 228,056 | 197,974 | 117,666 | 20,786 | 73,057 | 34,514 |

Notes: 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. Robust standard errors clustered at the individual level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 10
Results by Defendant Characteristics

| | Black | White | No Priors | Priors | Not Emp. | Employed |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Any Guilty Offense | −0.101* | −0.259*** | −0.202*** | −0.053 | −0.103** | −0.185*** |
| | (0.053) | (0.064) | (0.042) | (0.055) | (0.050) | (0.058) |
| | [0.565] | [0.581] | [0.495] | [0.611] | [0.547] | [0.501] |
| Guilty Plea | −0.122** | −0.198*** | −0.152*** | −0.054 | −0.107** | −0.109** |
| | (0.053) | (0.063) | (0.039) | (0.055) | (0.048) | (0.055) |
| | [0.377] | [0.385] | [0.283] | [0.388] | [0.323] | [0.289] |
| Any Incarceration | −0.049 | −0.049 | −0.024 | −0.040 | −0.020 | −0.073 |
| | (0.044) | (0.051) | (0.033) | (0.046) | (0.041) | (0.045) |
| | [0.276] | [0.239] | [0.190] | [0.281] | [0.231] | [0.195] |
| Failure to Appear in Court | 0.212*** | 0.166** | 0.135*** | 0.185*** | 0.138*** | 0.111* |
| | (0.068) | (0.073) | (0.042) | (0.044) | (0.045) | (0.059) |
| | [0.201] | [0.209] | [0.149] | [0.181] | [0.171] | [0.143] |
| Rearrest Prior to Disposition | −0.057 | 0.123* | 0.078** | 0.064 | 0.045 | 0.072 |
| | (0.053) | (0.064) | (0.040) | (0.053) | (0.050) | (0.057) |
| | [0.231] | [0.213] | [0.195] | [0.294] | [0.242] | [0.210] |
| Employed in 1-2 Years | 0.063 | 0.059 | 0.047 | 0.009 | 0.016 | 0.053 |
| | (0.071) | (0.092) | (0.051) | (0.075) | (0.058) | (0.065) |
| | [0.403] | [0.444] | [0.485] | [0.358] | [0.234] | [0.689] |
| Any Income in 1-2 Years | 0.098 | 0.222** | 0.139*** | −0.004 | 0.081 | 0.119* |
| | (0.074) | (0.097) | (0.053) | (0.080) | (0.063) | (0.064) |
| | [0.481] | [0.490] | [0.511] | [0.452] | [0.357] | [0.646] |
| Employed in 3-4 Years | 0.034 | 0.038 | 0.107* | 0.075 | 0.076 | 0.133* |
| | (0.098) | (0.136) | (0.060) | (0.105) | (0.069) | (0.081) |
| | [0.389] | [0.425] | [0.464] | [0.363] | [0.246] | [0.634] |
| Any Income in 3-4 Years | −0.027 | 0.290** | 0.111* | 0.023 | 0.065 | 0.133* |
| | (0.097) | (0.143) | (0.061) | (0.107) | (0.074) | (0.078) |
| | [0.466] | [0.495] | [0.512] | [0.422] | [0.356] | [0.625] |
| 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 | 155,310 | 104,918 | 316,644 | 109,386 | 178,060 | 160,733 |

Notes: This table reports two-stage least squares results of the impact of pre-trial release by defendant type. 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. Robust standard errors clustered at the individual level are reported in parentheses. *** = 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
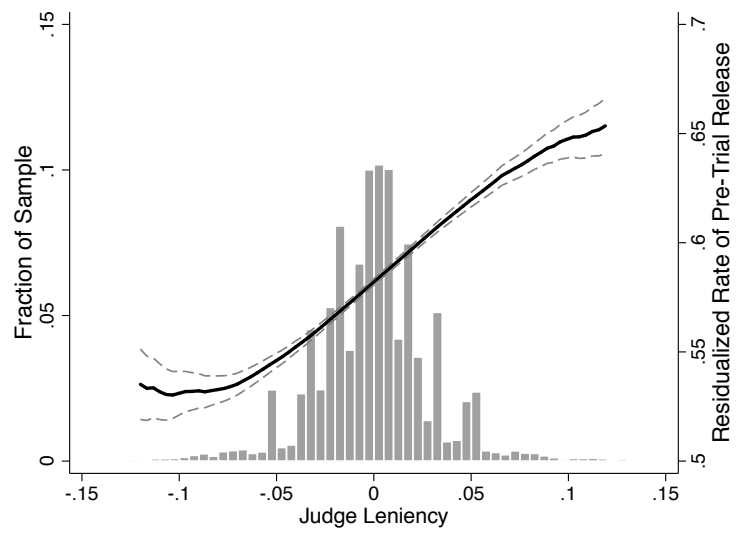


Notes: This figure reports the distribution of the judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year following the procedure described in Section III.

Appendix Table 1
Descriptive Statistics by Bail Type

| | Initial Bail Decision | | |
|---|---|---|---|
| | ROR | Non-Monetary | Monetary |
| *Panel A: Characteristics* | (1) | (2) | (3) |
| Male | 0.765 | 0.786 | 0.846 |
| White | 0.422 | 0.434 | 0.382 |
| Black | 0.555 | 0.556 | 0.604 |
| Age at Bail Decision | 34.157 | 33.894 | 33.229 |
| Prior Offense in Past Year | 0.145 | 0.277 | 0.281 |
| | | | |
| *Panel B: Charge Characteristics* | | | |
| Number of Offenses | 1.887 | 2.047 | 3.735 |
| Felony Offense | 0.134 | 0.423 | 0.633 |
| Misdemeanor Only | 0.866 | 0.577 | 0.367 |
| Any Drug Offense | 0.490 | 0.501 | 0.306 |
| Any DUI Offense | 0.190 | 0.049 | 0.044 |
| Any Violent Offense | 0.042 | 0.093 | 0.348 |
| Any Property Offense | 0.165 | 0.281 | 0.268 |
| | | | |
| *Panel C: Outcomes* | | | |
| Any Guilty Offense | 0.516 | 0.569 | 0.493 |
| Guilty Plea | 0.140 | 0.262 | 0.342 |
| Any Incarceration | 0.132 | 0.184 | 0.267 |
| Failure to Appear in Court | 0.251 | 0.263 | 0.099 |
| Absconded | 0.013 | 0.007 | 0.002 |
| Days to Case Decision | 261.294 | 232.109 | 229.731 |
| Rearrest Prior to Disposition | 0.234 | 0.314 | 0.216 |
| Rearrest in 0-2 Years | 0.373 | 0.479 | 0.414 |
| Rearrest in 3-4 Years | 0.251 | 0.316 | 0.297 |
| Earnings (thousands) in 1-2 Years | 8.015 | 5.871 | 6.460 |
| Employed in 1-2 Years | 0.514 | 0.443 | 0.438 |
| Any Income in 1-2 Years | 0.510 | 0.498 | 0.491 |
| Earnings (thousands) in 3-4 Years | 8.657 | 6.380 | 7.049 |
| Employed in 3-4 Years | 0.495 | 0.421 | 0.428 |
| Any Income in 3-4 Years | 0.504 | 0.496 | 0.485 |
| | | | |
| *Panel D: Data Quality* | | | |
| Missing Race | 0.488 | 0.359 | 0.371 |
| Observations | 89,870 | 59,134 | 271,646 |

Notes: 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.

Appendix Table 2
Predictors of Pre-Trial Release

|  | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Residualized ROR Rate | 0.020 | | | |
|  | (0.013) | | | |
| Residualized Non-Monetary Rate | | 0.146*** | | |
|  | | (0.011) | | |
| Residualized Monetary Rate | | | −0.482*** | |
|  | | | (0.019) | |
| Residualized Monetary Amount | | | | −0.002*** |
|  | | | | (0.000) |
| Observations | 426,030 | 426,030 | 426,030 | 426,030 |

Notes: This table reports the pair-wise correlation between different judge leniency measures. The correlations are calculated using 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.

Appendix Table 3
Additional First Stage Results

|  | Sample Mean | Judge Leniency | | |
|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) |
| Bail Modification Petition | 0.208 | −0.399*** | −0.398*** | −0.398*** |
|  | (0.406) | (0.033) | (0.032) | (0.032) |
| Released in 14 Days | 0.593 | 0.684*** | 0.680*** | 0.677*** |
|  | (0.491) | (0.025) | (0.024) | (0.023) |
| Released Before Trial | 0.713 | 0.576*** | 0.570*** | 0.568*** |
|  | (0.452) | (0.024) | (0.023) | (0.023) |
| Court x Year FE | − | Yes | Yes | Yes |
| Crime Controls | − | No | Yes | Yes |
| IRS Controls | − | No | No | Yes |
| Observations | 426,030 | 426,030 | 426,030 | 426,030 |

Notes: This table reports additional first stage results. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variable is listed in each row. 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 clustered at the individual level are reported in parentheses in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 4
Reduced Form Effect of Judge Leniency

|  | Detained Mean | OLS Results | | |
|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) |
| Any Guilty Offense | 0.571 | −0.103*** | −0.109*** | −0.107*** |
|  | (0.495) | (0.025) | (0.023) | (0.023) |
| Guilty Plea | 0.436 | −0.080*** | −0.083*** | −0.083*** |
|  | (0.496) | (0.024) | (0.022) | (0.022) |
| Any Incarceration | 0.296 | −0.012 | −0.022 | −0.021 |
|  | (0.457) | (0.019) | (0.019) | (0.018) |
| Failure to Appear in Court | 0.121 | 0.142*** | 0.138*** | 0.139*** |
|  | (0.326) | (0.029) | (0.029) | (0.029) |
| Rearrest Prior to Disposition | 0.202 | 0.054** | 0.051** | 0.052** |
|  | (0.402) | (0.023) | (0.022) | (0.022) |
| Employed in 1-2 Years | 0.378 | 0.049 | 0.045 | 0.025 |
|  | (0.485) | (0.034) | (0.033) | (0.029) |
| Any Income in 1-2 Years | 0.462 | 0.089*** | 0.083*** | 0.064** |
|  | (0.499) | (0.032) | (0.032) | (0.030) |
| Employed in 3-4 Years | 0.379 | 0.076* | 0.078** | 0.067* |
|  | (0.485) | (0.039) | (0.038) | (0.035) |
| Any Income in 3-4 Years | 0.465 | 0.076** | 0.073* | 0.056 |
|  | (0.499) | (0.038) | (0.037) | (0.035) |
| Court x Year FE | − | Yes | Yes | Yes |
| Crime Controls | − | No | Yes | Yes |
| IRS Controls | − | No | No | Yes |
| Observations | 190,769 | 426,030 | 426,030 | 426,030 |

Notes: 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 clustered at the individual level are reported in parentheses in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 5
First Stage Results by Case Characteristics

|  | Crime Severity | | Crime Type | | | |
|  | Misd. | Felony | Drug | DUI | Property | Violent |
| *Panel A: Full Sample* | (1) | (2) | (3) | (4) | (5) | (6) |
| Judge Leniency | 0.875*** | 0.498*** | 0.705*** | 1.269*** | 0.923*** | 0.136*** |
|  | (0.030) | (0.035) | (0.042) | (0.113) | (0.044) | (0.053) |
|  | [0.694] | [0.388] | [0.636] | [0.854] | [0.393] | [0.488] |
|  |  |  |  |  |  |  |
| *Panel B: Reverse Sample* |  |  |  |  |  |  |
| Judge Leniency | 1.126*** | 0.831*** | 1.014*** | 1.311*** | 1.195*** | 0.424*** |
|  | (0.030) | (0.035) | (0.042) | (0.113) | (0.043) | (0.053) |
|  | [0.694] | [0.388] | [0.636] | [0.854] | [0.393] | [0.488] |
| 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 | 228,056 | 197,974 | 117,666 | 20,786 | 80,690 | 73,057 |

Notes: 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. In Panel A, judge leniency is estimated using all cases assigned to a bail judge in the same year following the procedure described in Section III. In Panel B, judge leniency is estimated using data excluding own-type cases assigned to a bail judge in the same year following the procedure described in Section III. The dependent variable is an indicator for being released before trial within three days of the bail hearing. All specifications control for court-by-time fixed effects. Robust standard errors clustered at the individual 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.

Apppendix Table 6
First Stage Results by Defendant Characteristics

| | Black | White | No Prior | Prior | Emp. | Non-Emp. |
|---|---|---|---|---|---|---|
| *Panel A: Full Sample* | (1) | (2) | (3) | (4) | (5) | (6) |
| Judge Leniency | 0.668*** | 0.601*** | 0.645*** | 0.810*** | 0.649*** | 0.715*** |
| | (0.036) | (0.039) | (0.028) | (0.041) | (0.039) | (0.035) |
| | [0.451] | [0.491] | [0.600] | [0.415] | [0.619] | [0.517] |
| | | | | | | |
| *Panel B: Reverse Sample* | | | | | | |
| Judge Leniency | 1.009*** | 0.969*** | 0.964*** | 1.050*** | 0.952*** | 1.005*** |
| | (0.035) | (0.039) | (0.031) | (0.041) | (0.038) | (0.035) |
| | [0.451] | [0.491] | [0.600] | [0.415] | [0.619] | [0.517] |
| 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 | 155,310 | 104,918 | 316,644 | 109,386 | 160,733 | 178,060 |

Notes: 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. In Panel A, judge leniency is estimated using all cases assigned to a bail judge in the same year following the procedure described in Section III. In Panel B, judge leniency is estimated using data excluding own-type cases assigned to a bail judge in the same year following the procedure described in Section III. The dependent variable is an indicator for being released before trial within three days of the bail hearing. All specifications control for court-by-time fixed effects. Robust standard errors clustered at the individual 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.

Appendix Table 7
Additional Results

| | Detained Mean | OLS Results | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Number of Guilty Offenses | 0.927 | −0.241*** | −0.078*** | −0.075*** | −0.182** | −0.187*** | −0.186*** |
| | (1.745) | (0.004) | (0.004) | (0.004) | (0.074) | (0.069) | (0.069) |
| Guilty Plea to Time Served | 0.202 | −0.140*** | −0.064*** | −0.062*** | −0.141*** | −0.131*** | −0.130*** |
| | (0.401) | (0.002) | (0.002) | (0.002) | (0.037) | (0.035) | (0.035) |
| Offense Charged Down | 0.482 | 0.015*** | 0.051*** | 0.048*** | 0.082** | 0.088** | 0.086** |
| | (0.500) | (0.002) | (0.002) | (0.002) | (0.036) | (0.035) | (0.035) |
| Max Days Incarcerated | 332.244 | −293.075*** | −156.557*** | −155.805*** | −144.049 | −146.112 | −144.745 |
| | (2,617.928) | (4.197) | (4.227) | (4.554) | (210.643) | (211.005) | (211.974) |
| 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 | 190,769 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 | 426,030 |

Notes: 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 clustered at the individual level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 8
First Stage Results by Release Type

| | Sample Mean | Judge Leniency | |
| --- | --- | --- | --- |
| | | No Conditions | With Conditions |
| | (1) | (2) | (3) |
| Released with No Conditions | 0.203 | 0.938*** | −0.040*** |
| | (0.402) | (0.017) | (0.014) |
| Released with Conditions | 0.349 | −0.265*** | 0.728*** |
| | (0.477) | (0.026) | (0.024) |
| Court x Year FE | − | Yes | Yes |
| Crime Controls | − | Yes | Yes |
| IRS Controls | − | Yes | Yes |
| Observations | 426,030 | 426,030 | 426,030 |

Notes: 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 clustered at the individual level are reported in parentheses in all specifications. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

| | Detained Mean | OLS Results | | 2SLS Results | |
|---|---|---|---|---|---|
| | | No Conditions | With Conditions | No Conditions | With Conditions |
| | (1) | (2) | (3) | (4) | (5) |
| Any Guilty Offense | 0.571 | −0.075*** | −0.042*** | −0.168*** | −0.154*** |
| | (0.495) | (0.002) | (0.002) | (0.036) | (0.034) |
| Guilty Plea | 0.436 | −0.137*** | −0.079*** | −0.129*** | −0.119*** |
| | (0.496) | (0.002) | (0.002) | (0.034) | (0.032) |
| Any Incarceration | 0.296 | −0.107*** | −0.096*** | −0.015 | −0.032 |
| | (0.457) | (0.002) | (0.001) | (0.028) | (0.027) |
| Failure to Appear in Court | 0.121 | 0.060*** | −0.006*** | 0.178*** | 0.151*** |
| | (0.326) | (0.002) | (0.001) | (0.033) | (0.031) |
| Rearrest Prior to Disposition | 0.202 | 0.058*** | 0.058*** | 0.091*** | 0.074** |
| | (0.402) | (0.002) | (0.002) | (0.034) | (0.033) |
| Employed in 1-2 Years | 0.378 | 0.053*** | 0.050*** | 0.042 | 0.037 |
| | (0.485) | (0.003) | (0.002) | (0.046) | (0.043) |
| Any Income in 1-2 Years | 0.462 | 0.036*** | 0.036*** | 0.094** | 0.097** |
| | (0.499) | (0.003) | (0.002) | (0.048) | (0.045) |
| Employed in 3-4 Years | 0.379 | 0.041*** | 0.029*** | 0.085 | 0.102* |
| | (0.485) | (0.003) | (0.002) | (0.055) | (0.053) |
| Any Income in 3-4 Years | 0.465 | 0.032*** | 0.029*** | 0.045 | 0.087 |
| | (0.499) | (0.003) | (0.002) | (0.056) | (0.054) |
| Court x Year FE | − | Yes | Yes | Yes | Yes |
| Crime Controls | − | Yes | Yes | Yes | Yes |
| IRS Controls | − | Yes | Yes | Yes | Yes |
| Observations | 190,769 | 426,030 | 426,030 | 426,030 | 426,030 |

Notes: 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 clustered at the individual level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 10
Robustness of Two-Stage Least Squares Results

| | Judge Leniency | | | | | | |
|---|---|---|---|---|---|---|---|
| | Crime Severity | Crime Type | Release Ever | Non-Money Bail | Split Sample | Scheduled Judge | Judge FE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Any Guilty Offense | -0.141*** | -0.173*** | -0.212*** | -0.076*** | -0.174*** | -0.147*** | -0.192*** |
| | (0.027) | (0.027) | (0.039) | (0.021) | (0.039) | (0.036) | (0.034) |
| Guilty Plea | -0.131*** | -0.134*** | -0.203*** | -0.045** | -0.131*** | -0.129*** | -0.166*** |
| | (0.026) | (0.026) | (0.038) | (0.020) | (0.038) | (0.035) | (0.032) |
| Any Incarceration | 0.012 | -0.043** | -0.035 | -0.010 | -0.035 | -0.032 | -0.044 |
| | (0.021) | (0.021) | (0.030) | (0.017) | (0.031) | (0.029) | (0.027) |
| Failure to Appear in Court | 0.116*** | 0.148*** | 0.235*** | 0.081*** | 0.128*** | 0.117*** | 0.097*** |
| | (0.026) | (0.023) | (0.049) | (0.018) | (0.036) | (0.036) | (0.037) |
| Rearrest Prior to Disposition | 0.075*** | 0.101*** | 0.086** | 0.065*** | 0.060 | 0.068* | 0.062* |
| | (0.025) | (0.025) | (0.039) | (0.020) | (0.038) | (0.036) | (0.033) |
| Employed in 1-2 Years | 0.041 | 0.026 | 0.010 | 0.023 | 0.040 | 0.047 | 0.017 |
| | (0.036) | (0.035) | (0.065) | (0.027) | (0.049) | (0.048) | (0.039) |
| Any Income in 1-2 Years | 0.070* | 0.042 | 0.128* | -0.005 | 0.033 | 0.123** | 0.040 |
| | (0.038) | (0.036) | (0.068) | (0.028) | (0.051) | (0.050) | (0.040) |
| Employed in 3-4 Years | 0.027 | 0.069* | 0.092 | 0.073** | 0.091 | 0.091 | 0.080* |
| | (0.043) | (0.041) | (0.079) | (0.032) | (0.060) | (0.058) | (0.045) |
| Any Income in 3-4 Years | 0.050 | 0.020 | 0.162** | 0.030 | 0.074 | 0.095 | 0.029 |
| | (0.044) | (0.041) | (0.081) | (0.032) | (0.061) | (0.059) | (0.046) |
| 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 | 426,030 | 426,030 | 426,030 | 426,030 | 319,677 | 426,030 | 426,030 |

Notes: 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 clustered at the individual level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 11
Robustness of Two-Stage Least Squares Results by Sample

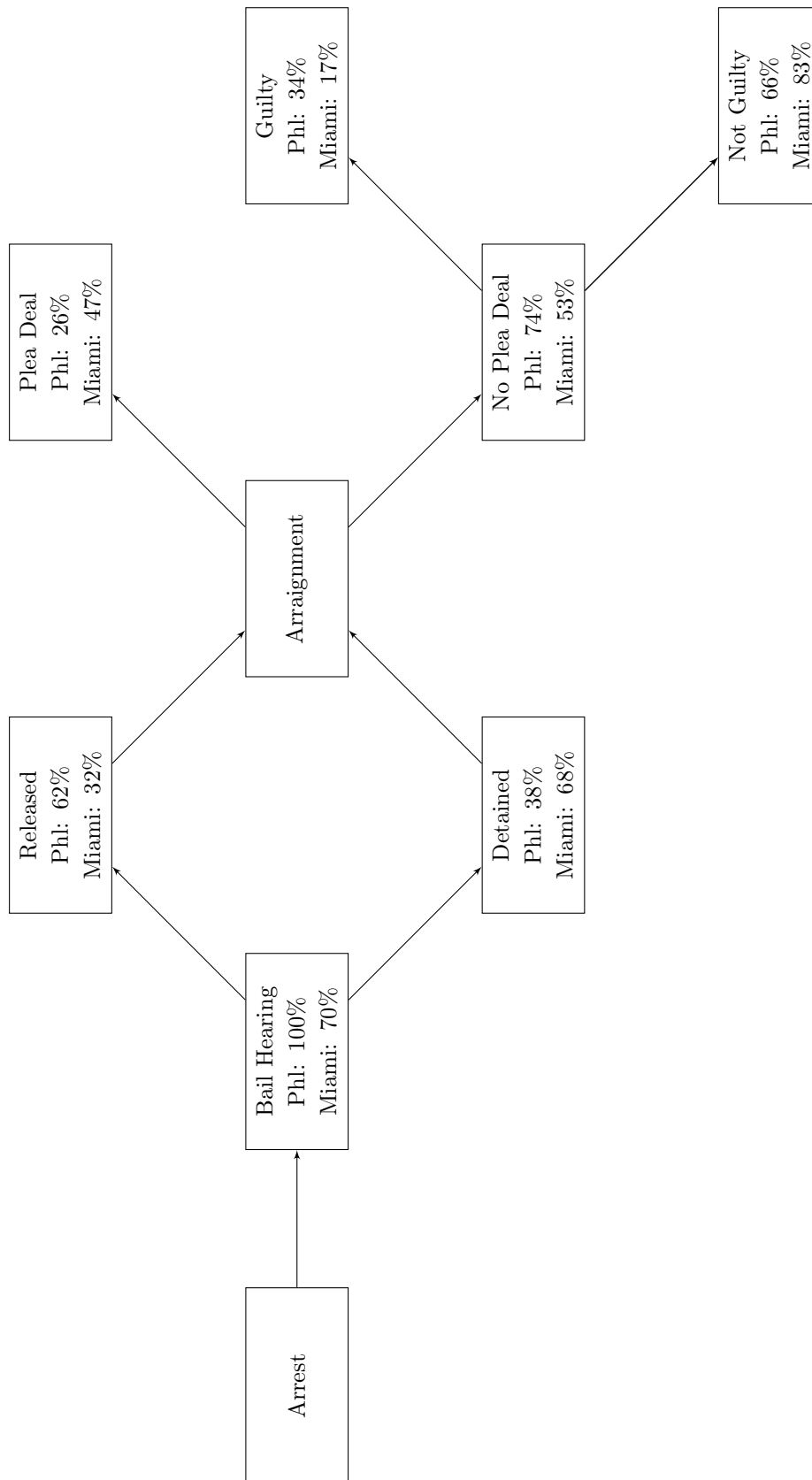| | Philadelphia Only | Miami Only | IRS Sample |
|---|---|---|---|
| | (1) | (2) | (3) |
| Any Guilty Offense | −0.126*** | −0.182*** | −0.143*** |
| | (0.041) | (0.054) | (0.038) |
| Guilty Plea | −0.041 | −0.205*** | −0.113*** |
| | (0.035) | (0.054) | (0.037) |
| Any Incarceration | −0.012 | −0.041 | −0.043 |
| | (0.032) | (0.044) | (0.030) |
| Failure to Appear in Court | 0.150*** | – | 0.125*** |
| | (0.031) | | (0.036) |
| Rearrest Prior to Disposition | 0.135*** | 0.015 | 0.058 |
| | (0.036) | (0.056) | (0.038) |
| Employed in 1-2 Years | −0.003 | 0.087 | 0.037 |
| | (0.045) | (0.086) | (0.043) |
| Any Income in 1-2 Years | −0.023 | 0.294*** | 0.097** |
| | (0.046) | (0.092) | (0.045) |
| Employed in 3-4 Years | 0.160*** | 0.003 | 0.102* |
| | (0.055) | (0.104) | (0.053) |
| Any Income in 3-4 Years | 0.061 | 0.138 | 0.085 |
| | (0.056) | (0.106) | (0.054) |
| Court x Year FE | Yes | Yes | Yes |
| Crime Controls | Yes | Yes | Yes |
| IRS Controls | Yes | Yes | Yes |
| Observations | 328,492 | 97,538 | 338,793 |

Notes: 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 clustered at the individual level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 12

Effects on Joint Probabilities of Conviction and Employment

| | Detained Mean | OLS Results | | | 2SLS Results | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Years 1-2* | | | | | | | |
| Guilty and Employed | 0.187 | 0.028*** | 0.022*** | 0.000 | −0.054* | −0.059* | −0.072** |
| | (0.390) | (0.001) | (0.002) | (0.002) | (0.033) | (0.032) | (0.031) |
| Guilty and Not Employed | 0.333 | −0.095*** | −0.070*** | −0.045*** | −0.075** | −0.075** | −0.061* |
| | (0.471) | (0.002) | (0.002) | (0.002) | (0.036) | (0.035) | (0.034) |
| Not Guilty and Employed | 0.152 | 0.090*** | 0.069*** | 0.045*** | 0.107*** | 0.109*** | 0.095*** |
| | (0.359) | (0.001) | (0.002) | (0.002) | (0.032) | (0.031) | (0.031) |
| Not Guilty and Not Employed | 0.225 | −0.024*** | −0.022*** | −0.001 | 0.022 | 0.025 | 0.037 |
| | (0.417) | (0.002) | (0.002) | (0.002) | (0.033) | (0.033) | (0.032) |
| | | | | | | | |
| *Panel B: Years 3-4* | | | | | | | |
| Guilty and Employed | 0.138 | 0.009*** | 0.009*** | −0.005*** | −0.003 | −0.005 | −0.014 |
| | (0.345) | (0.001) | (0.001) | (0.001) | (0.028) | (0.027) | (0.027) |
| Guilty and Not Employed | 0.237 | −0.053*** | −0.036*** | −0.020*** | −0.039 | −0.039 | −0.029 |
| | (0.425) | (0.001) | (0.002) | (0.001) | (0.031) | (0.031) | (0.030) |
| Not Guilty and Employed | 0.108 | 0.057*** | 0.042*** | 0.025*** | 0.064** | 0.064** | 0.055** |
| | (0.310) | (0.001) | (0.001) | (0.001) | (0.027) | (0.027) | (0.026) |
| Not Guilty and Not Employed | 0.165 | −0.013*** | −0.014*** | −0.001 | −0.022 | −0.020 | −0.012 |
| | (0.371) | (0.001) | (0.001) | (0.001) | (0.028) | (0.028) | (0.028) |
| | | | | | | | |
| 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 | 147,295 | 338,793 | 338,793 | 338,793 | 338,793 | 338,793 | 338,793 |

Notes: 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 clustered at the individual level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.
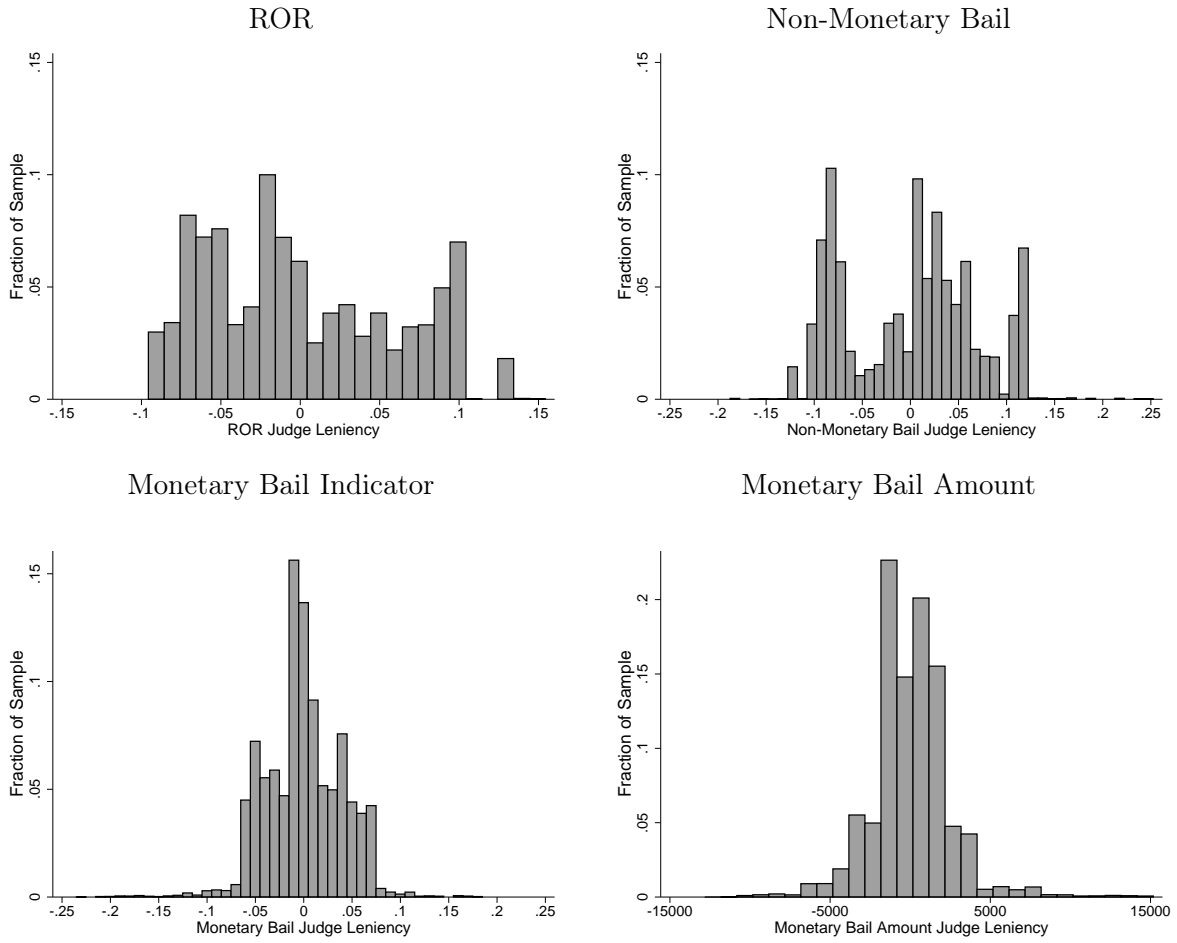
Appendix Figure 1
Criminal Case Process



```
Arrest ──→ Bail Hearing
           Phl: 100%
           Miami: 70%
```

```
                    Released
                    Phl: 62%
                    Miami: 32%
```

```
                    Detained
                    Phl: 38%
                    Miami: 68%
```

```
                              Arraignment
```

```
                    Plea Deal
                    Phl: 26%
                    Miami: 47%
```

```
                    No Plea Deal
                    Phl: 74%
                    Miami: 53%
```

```
                    Guilty
                    Phl: 34%
                    Miami: 17%
```

```
                    Not Guilty
                    Phl: 66%
                    Miami: 83%
```

Appendix Figure 2
Bail Guidelines in Philadelphia

LOW <------------------------------Charge Seriousness------------------------------> HIGH

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

LOW <--------Risk Level----------> HIGH

Notes: 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.

# Appendix Figure 3
## Distribution of Alternative Judge Leniency Measures

ROR



Non-Monetary Bail



Monetary Bail Indicator



Monetary Bail Amount



Notes: 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.

Appendix Figure 4
Judge Leniency by Defendant and Case Characteristics
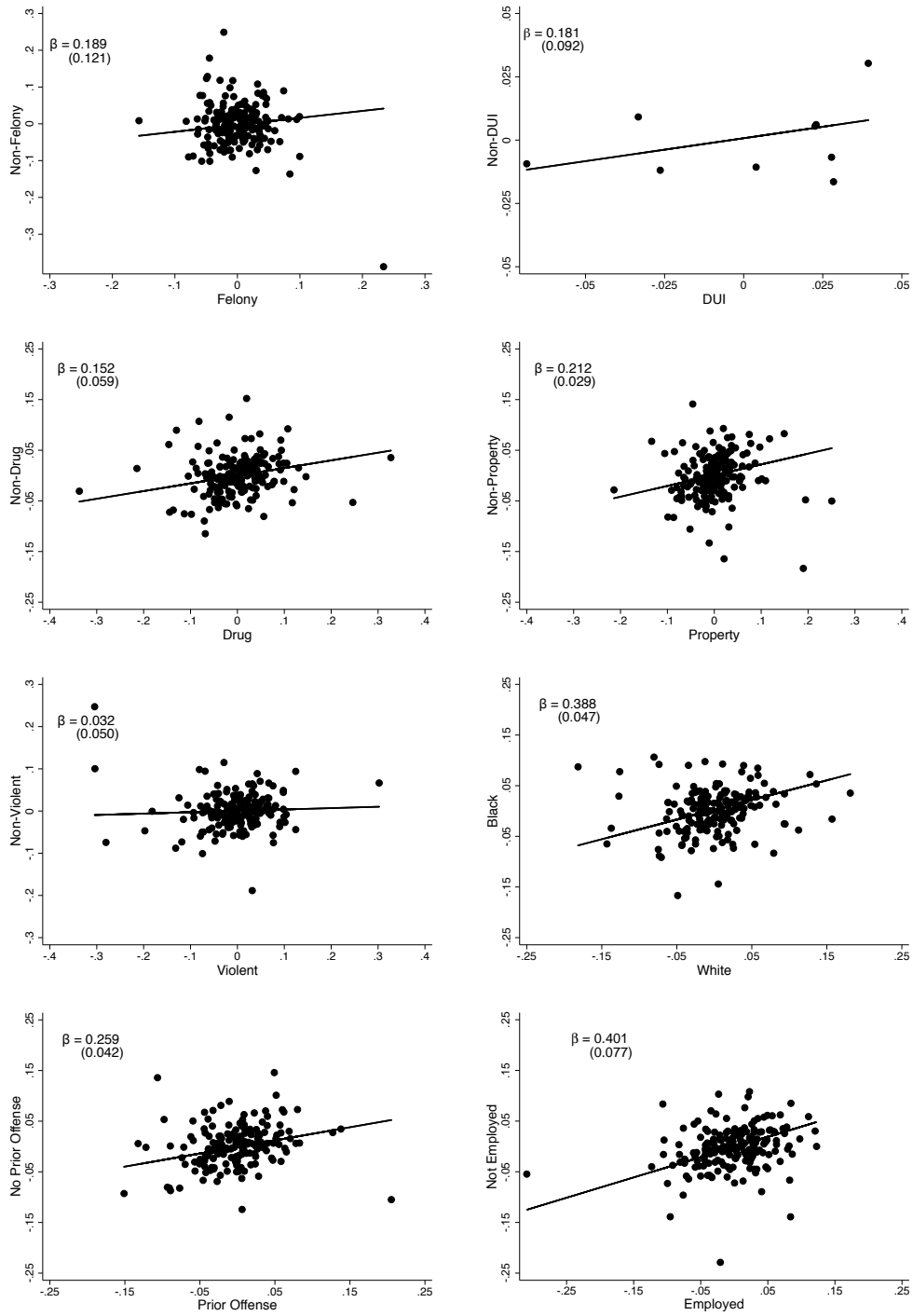


Notes: These figures show the correlation between our residualized measure of judge leniency for different groups of defendants. DUI cases are only available in Philadelphia. We take the average leniency for each group over all available years of data. The solid line shows the best linear fit estimated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