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

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

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

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

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

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

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

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

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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 Nick Pinto, "The Bail Trap," *New York Times*, August 13, 2015, http://www.nytimes.com/2015/08/16/magazine/the-bail-trap.html.

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

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

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

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

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

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

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

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

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

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

Our findings are related to an important literature estimating the effects of incarceration and sentence length on defendants. Kling (2006) finds no impact of sentence length on labor market outcomes using prison records from Florida and California. However, Mueller-Smith (2015) finds that post-conviction incarceration reduces employment and increases future crime using data on defendants from Harris County, Texas, and Aizer and Doyle (2015) find that juvenile incarceration reduces high school completion and increases adult incarceration using data on juveniles from Chicago. Consistent with Mueller-Smith (2015) and Aizer and Doyle (2015), we find that pretrial detention reduces employment and increases future crime through a criminogenic effect (although unlike those papers, we find that this

criminogenic effect is offset by an incapacitation effect). Importantly, however, our paper is the first to shed light on the effects of a criminal conviction and the effects of pre-conviction detention, as opposed to incarceration per se.[6]

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

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

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

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

I. The Bail System in the United States

A. *Overview*

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

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

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

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

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

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

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

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

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

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

Miami-Dade County.—The Miami-Dade bail system follows a similar procedure, with one important exception. As opposed to Philadelphia where all

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

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

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

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

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

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

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

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

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

II. Data

A. *Data Sources and Sample Construction*

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

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

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

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

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

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

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

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

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

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

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

B. *Descriptive Statistics*

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

TABLE 1—DESCRIPTIVE STATISTICS

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

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

released pretrial. We measure initial pretrial release based on whether a defendant is released within the first three days of the bail hearing for two reasons. First, policy advocates have argued that the adverse effects of pretrial detention start as early

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

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

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

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

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

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

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

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

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

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

III. Research Design

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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[15] Algebraically, the leave-out mean measure is equivalent to a judge fixed effect estimated in a leave-out regression estimated in each year. Our leave-one-out procedure is essentially a reduced-form version of jackknife IV, which is recommended when the number of instruments (the judge fixed effects) is likely to increase with sample size (Stock, Wright, and Yogo 2002, Kolesár et al. 2015). Results using a full set of judge fixed effects as instruments are presented in robustness checks.
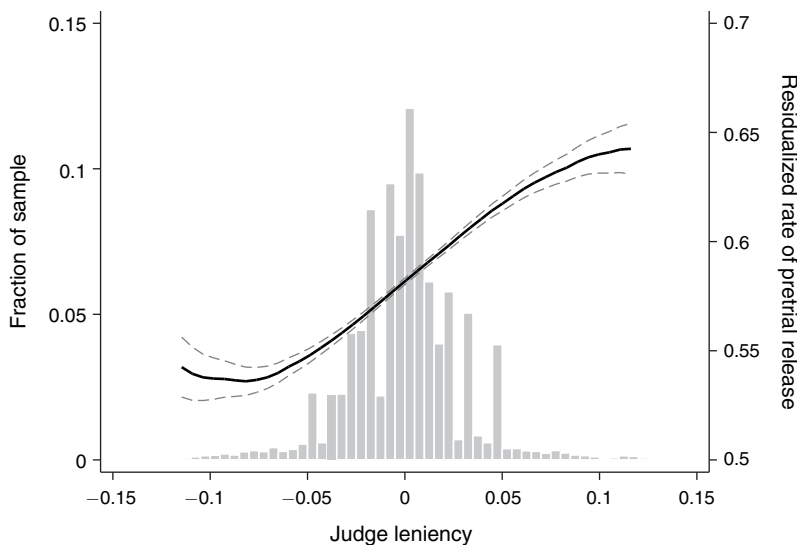
FIGURE 1. DISTRIBUTION OF JUDGE LENIENCY MEASURE AND FIRST STAGE

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

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

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

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

captures a margin of particular policy relevance. Section IVF explores the impact of other margins such as being assigned monetary bail.[16]

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

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

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

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

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

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

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

TABLE 2—JUDGE LENIENCY AND PRETRIAL RELEASE

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

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

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

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

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

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

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

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

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

TABLE 3—TEST OF RANDOMIZATION

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

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

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

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

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

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

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

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

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

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

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

IV. Results

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

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

Table 4—Pretrial Release and Criminal Outcomes

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

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

A. *Case Outcomes*

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

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

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

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

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

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

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

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

B. *Failures to Appear and Future Crime*

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

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

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

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

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

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

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

C. *Labor Market and Tax Administration Outcomes*

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

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

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

Table 5—Pretrial Release and Labor Market Outcomes

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

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

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

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

TABLE 6—PRETRIAL RELEASE AND SOCIAL BENEFITS TAKE-UP

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

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

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

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

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

D. *Additional IRS Outcomes*

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

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

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

E. *Subsample Results*

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

TABLE 7—RESULTS BY PRIOR CRIMINAL HISTORY

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

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

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

In online Appendix Tables A15 and A16, we present additional two-stage least squares subsample results by crime severity, highest crime type, and defendant characteristics. While we caution against the strong interpretation of these subsample results given concerns about multiple hypothesis testing, there is some evidence that

the results are larger for defendants charged with misdemeanor and drug offenses, although large standard errors mean that none of the differences are statistically significant. Our labor market results are also somewhat larger for individuals who were employed prior to the bail hearing, but not meaningfully different for defendants from high- and low-income zip codes. Overall, these results suggest that the social costs imposed by pretrial detention may be larger for those with more limited ties to the criminal justice system and stronger ties to the formal labor sector.

F. *Robustness Checks*

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

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

Another potential violation of the exclusion restriction is if judges affect not only the pretrial release decision, but also the length of stay in pretrial detention. Following Aizer and Doyle (2015), we explore this concern in two ways. First, we test whether our judge leniency measure is predictive of the number of days detained conditional on being detained at all before trial. Second, we test whether a separate leave-out measure based on length of stay has any additional predictive value for the number of days detained (including zero length of stays) beyond our preferred

leave-out instrument. These results are reported in online Appendix Table A19, Consistent with the exclusion restriction, we find that our preferred leave-out instrument is not predictive of the number of days detained conditional on being detained at all before trial. We also find that there is no additional explanatory value of the separate length of stay leave-out measure. In unreported results, we also find similar but imprecise results if we estimate our preferred specification separately for short versus long stays in detention, and below we show similar results for different definitions of pretrial release. Taken together, these results suggest that our main results are driven by judge variation in initial pretrial release, not judge variation in length of stay.

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

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

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

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

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

V. Discussion

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

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

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

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

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

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

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

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

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

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

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

VI. Conclusion

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

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

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

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

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

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

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

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

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

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

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