The Case for Clemency: Differential Impacts of Pretrial Detention on Case and Crime Outcomes

George Botros Zarif Rateb

Professor Bocar Ba, Faculty Advisor
Professor Michelle Connolly, Faculty Advisor

Honors Thesis submitted in partial fulfillment of the requirements for Graduation with Distinction in Economics in Trinity College of Duke University.

Duke University
Durham, North Carolina
2022

Contents

Acknowledgements

First and foremost, I would like to extend my gratitude to Dr. Bocar Ba. His constant support and mentorship throughout the duration of this year has been instrumental to both the completion of this thesis and my continued desire to study economics to analyze questions that are important to me. I would not be where I am without his consistent presence in my thesis building process, from answering my late night slacks to spending hours assisting me in Vondy. I would also like to extend my gratitude to Roman Rivera, on whom I could always count for great advice on data analytics and R, and even better banter.

I would like to thank Dr. Michelle Conolly for her guidance and willingness to assist me hone in on a specific topic, and always being there to ground me whenever I was overwhelmed with indecision. Of course, I would also like to thank my classmates for their feedback and ideas through my many presentations gone long.

Finally, I would like to thank my friends and family for their endless care and willingness to listen to me ramble about data, instrumental variables, and bond court proceedings far more than anyone should ever have to. I am eternally grateful for their love and support.

Abstract

About half-million of individuals in US jails are detained pretrial while legally presumed innocent. Using data on quasi-randomly assigned bail judges in the third-largest court system in the U.S., we study the impact of pretrial detention on defendants' court and crime outcomes between 2008 and 2012. We supplement our primary analysis to document patterns on bail amounts and how they differentially impact Black defendants relative to their white and Hispanic counterparts. Instrumental variable estimates suggest that pretrial detention increases the likelihood of being found guilty, mainly driven by the uptake of guilty pleas, especially for minorities. By linking court and jail data, we provide mechanistic evidence that jail time is positively correlated with the uptake of these guilty pleas. To the best of our knowledge, these findings have not been empirically documented due to a lack of previous data availability.

JEL classification: C26; J15; K14

Keywords: Instrumental Variables; Economics of Minorities; Criminal Law

1 Introduction

With 639 incarcerated individuals per 100,000 (The Sentencing Project 2021) and a total of 2,094,000 prisoners (Szmigiera 2021), the United States holds both the highest incarcerated population and highest incarceration rate in the entire world. These alarming statistics have raised red flags and political commentary from both sides of the political spectrum, gathering bipartisan support that over-incarceration is an issue in the U.S. (Equal Justice Initiative 2019). An often overlooked aspect of this mass incarceration is pretrial detention, in which defendants are held in jails leading up to their court dates. Dobbie, Goldin, and Yang (2018) indicate that over 11 million people worldwide are imprisoned prior to their court date each year. In an ideal world, we would not want detention to cause an increase in guilty outcomes, as conviction should be based purely on whether or not a defendant committed a crime. Thus, our primary goal is to understand the impacts of pretrial release on future court and criminological outcomes for individual defendants to see if it does have the capacity to influence guilty verdicts.

Pretrial detention is a feature of U.S. court systems intended at preventing dangerous criminals from causing more harm to society before their court date and ensuring that defendants with a high likelihood of fleeing their court date are present at their hearing (18 USCS § 3142). Pretrial detention is determined by bail judges in bond court soon after a defendant is arrested, which happen to be quasi-randomly assigned judges within Chicago. An important aspect of pretrial detention, however, is that it is ultimately an outcome of whether a defendant makes bail as opposed to the direct decision a bail judge determines. Bail judges only determine what type of bond to give defendants (if any) and the bail amount associated with that bond, hence, influencing a defendant's likelihood of detention through a primarily monetary mechanism. This has incited a debate headlined by the notion that pretrial detention is oftentimes more determined by an individual's ability to post bail as opposed to their risk to the community. Bail judges can also place other types of conditions on a bond, such as drug counseling or travel bans. Here, we do not however explicitly study these other conditions, and for our final outcomes, we take for granted a defendant's final state of being detained or released, regardless of how they arrived there.

Previous literature has examined the impacts of pretrial release on court outcomes and found that release significantly decreases the probability of being found guilty for defendants. This literature hypothesizes that, holding all else equal, the inability to make bail leads to an uptake in unfavorable plea bargains by defendants that are detained, due to being in a position of either objectively or perceived worse bargaining power. Our primary goal is to confirm these results and show that the impacts of pretrial detention are widespread, and to extend their generalizability.

We explore this idea further by examining the differential determinants and impacts of pretrial detention with race. Mass incarceration is largely a racial issue, and black people in the U.S. have historically been given more prison sentences and longer prison sentences than their hispanic and white counterparts (Rehavi and Starr 2014). There is a rich literature documenting racial bias in the judicial system, including specifically judge bias (Kang et. al. 2011). However, despite the disparity in racial outcomes in the legal system, previous literature within the context of pretrial detention has done little more than acknowledge these racial disparities and moved along. This leaves a paucity of research examining how the intersection of pretrial outcomes and race can differentially impact a defendant's outcomes.

We seek to fill this gap in the literature by first understanding if the positive impacts associated with pretrial release vary in treatment size or significance for different races. Dobbie et. al. (2018) hypothesize that bargaining power in plea deals is one of the main drivers of guilty outcomes for detained individuals. Since the literature observes racial disparities in many economic and legal outcomes, it would not be unreasonable to assume that there is also a racial disparity in a collective race's initial state of bargaining power. We empirically test this assumption by calculating the treatment size for different racial groups to find that there are in fact racially differential impacts of release on the uptake of guilty pleas.

In addition, previous literature has done little to understand the extent to which Constitutional rights are upheld throughout the criminal justice system. We make it a priority to at least document the ways in which amendments such as "no excessive bail," "speedy trial," and "equal treatment" impact detention rates and court outcomes. Specifically, we include an examination of monetary bail amounts and detention rates for defendants. We enhance this analysis by observing differential defendant detention rates by race conditional on bail amount. Additionally, we include a descriptive analysis documenting the relationship between jail time and the uptake of guilty pleas. The positive association documented supports the hypothesis that bargaining power and detention are related, and suggests another mechanism of decreasing defendant resilience as a correlated unobservable with jail time.

Other papers that create judge leniency measures as instruments within court data do little to explore the explicit preferences of individual judges. This makes sense, as other papers in the field have dealt with a large number of bail judges. However, within our context of Chicago, we deal exclusively with the 12 most common judges, so we are able to add an analysis of judge preference over various defendant characteristics, similarly to Stevenson (2018). Individual judge bias would be expected to have compounding impacts on a defendant, and we document that judge bias over defendant race, gender, and criminology in the last year do in fact exist. This provides a violation of the monotonicity requirement for instruments that stands as a limitation to both our paper and other papers that use similar judge leniency measures.

Although previous literature has taken the ultimate binarized outcome of detention or release for granted in the construction of our instrument in order to aid in the interpretability and policy relevance of our outcomes, Andresen and Huber (2021) show how such simplifications can violate the exclusion restriction. That is, since the decision bail judges make is actually multifaceted in nature, its reduction to a binarized outcome might have consequences on our instrument's validity if the specific avenue by which a bail judge determines release or detention could have an impact on a defendant's future outcomes.

The rest of the paper is organized as follows. First, we conduct an extensive literature review on incarceration, race, and pretrial detention. Second, we provide a deep dive into the legal and geographic context of detention within the Chicago court system. Third, awend conduct a descriptive analysis of the data. Fourth, we provide and justify our empirical strategy. Fifth, we showcase our primary findings. We then provide a discussion of these results and future work directions, and then offer a short conclusion.

2 Literature Review

There are many factors that contribute to incarceration rates beyond crime itself. Legal and state characteristics such as republican legislature and voting cycles (Smith 2004), political lobbying (Piller 2020), laws that enforce nonoptimal sanctions, like mandatory minimums (Andreoni 1991), higher state revenue (Grenberg & West 2001), weaker welfare system (Beckett & Western 2001), and local wealth inequality and poverty rates (Petach & Pena 2021) can all increase incarceration rates. There are also laws that ban activities associated with homelessness, such as laws against begging, loitering, sleeping in a vehicle, and sleeping in public that perpetuate systems that criminalize poverty (Vallas & Dietrich 2014). There is also a rich literature detailing how factors such as judicial discrimination (Anwar, Bayer, and Hjalmarsson 2012; Abrams, Bertrand, & Mullainathan 2012; Rehavi & Starr 2014; Arnold, Dobbie, & Yang 2018) and varying law enforcement efforts & mandatory minimum sentencing (Ba et. al. 2021; Owens & Ba 2021; Sykes & Maroto 2016) increase racial disparities in incarceration rates. Galinato & Rohla (2020) also find that the incidence of private prisons & corruption increase incarceration rates. We contribute to this literature on policy and incarceration by examining how the policies surrounding bail in the United States can affect pretrial incarceration which in turn can impact guilty verdicts.

Mass incarceration is also largely a racial issue, with black men being six times more likely to be incarcerated than white men (The Sentencing Project 2021). Lyons and Pettit (2011) find that one third of all black men will face some amount of time in prison. There is strong evidence that on the extensive and intensive margin that black people do not face similar judicial outcomes as their white peers, with more incarcerations per capita and longer sentences per incarceration. Rehavi and Starr (2014) show that black people face longer prison sentences for the same crimes as their white peers after controlling for case and defendant characteristics. Anwar, Bayer, and Hjalmarsson (2012) find that juries statistically significantly convict black defendants more than white ones, but if there is a single black person in the jury, conviction rates become nearly identical. We contribute to the literature on racial determinants of incarceration by showing how for identical bail amounts, black defendants are incarcerated at significantly higher rates.

In addition to incarceration being racially biased in its causes, it’s also racially biased in effect. It’s well documented that criminal justice contact, namely incarceration, has negative impacts on individuals' future earnings, employment, and recidivism. There is growing literature that shows that even amongst these negative effects, the effects are more pronounced for black people (Apel & Powell 2019). Lyons and Pettit (2011) find that wage growth is 21% slower for black ex-inmates than for white ones, controlling for case characteristics. This raises concerns for our context within the negative impacts of pretrial-detention: how much of these detriments disproportionately affect black people and people of color? This is another gap in the literature we fill.

There is a rich literature studying the economic impacts of pretrial detention. As we mentioned, Dobbie et. al. (2018) contend that pretrial detention reduces a defendant's bargaining power in plea deals. They hypothesize that, first, defendants may plead guilty to be freed from jail. Second, it might be harder for defendants to gather evidence to support their case if they are detained. Third, release might allow defendants and prosecutors to strategically delay the resolution of a case. Finally, jurors and judges might be implicitly biased when observing a defendant in jail uniforms and shackles. We mainly provide evidence for their first hypothesis by documenting the relationship between jail time and guilty pleas. Stevenson (2018) specifically studies how inability to pay bonds distorts justice for defendants. We seek to contribute to her research in showing how inability to pay bonds is differential in race.

Finally, we would be remiss not to mention the issues that arise in criminal justice work relating to endogeneity and selection bias. Imbens et. al. (1994) developed the instrumental variable technique which we use in order to avoid potential sources of endogeneity so we can identify the causal impact we are interested in.Studies show that oftentimes, individuals fare poorly in the labor market *before* they enter the criminal justice system (Looney & Turner 2018; Harding & Siegel 2017). The effects of criminal justice contact are difficult to identify as there are unobservable personal and environmental characteristics that simultaneously impact criminal behavior and economic outcomes, like access to credit (Czafit & Köllő 2015). Apel & Powell (2019) discuss how since criminal justice contact is more concentrated for those on the lowest rungs of the social ladder, selection mechanisms are implicated, as individuals may have faced poor outcomes regardless of the contact. Following the literature (Aneja & Avenancio-León 2019; Bhuller et. al. 2018; Jordan, Karger, & Neal 2021), we use an instrumental variable approach using quasi-randomly assigned judge leniency to instrument the causal impact of detention on the outcomes of interest for defendants on the margin of being released.

3 Background

3.1 Legal Context

The 8th amendment of the United States Constitution dictates that excessive bail "shall not be required" of citizens. In 2006, the average bail amount in the United States was $55,500 (Justice Policy Initiative 2022) while the average annual income was $58,029 (Johnston 2008). In turn, this has led the Department of Justice to find multiple bail systems around the country guilty of violating the U.S. Constitution and by direct quote, "bad public policy."

The 6th amendment of the United States Constitution grants defendants the right to a speedy trial. According to recent studies, however, the overloaded and at capacity carceral system often detaining defendants for anywhere between 50-200 days before their trial. This has sparked debate about the ethicality of the bail system which has only slowed down as the coronavirus pandemic has slowed and halted courts around the country, leaving many defendants stuck detained for undetermined amounts of time.

Finally, the 11th and 14th amendments of the Constitution guarantees defendants the often cited "right to be presumed innocent until proven guilty" and "equal protection of the laws." It is therefore important to note that defendants are being exposed to constitutionally-determined excessively high bails and detained for unconstitutionally-long amounts of time, all the while *not even being found guilty*. Further, these infractions often unconstitutionally occur along identity lines, such as the over policing of minorities and the tendencies of juries to be less lenient towards black defendants.

Beyond the Constitution, there is a complex legal system that informs and influences how the lifetime of a case evolves. Given that a large part of our analysis assesses defendants' choices to plead guilty after spending time in jail, we want to address the adverse legal incentives at play that might contribute to a defendant's choice to plead guilty. The 1984 Crime Bill is famous for kickstarting a movement to implement so-called "mandatory minimum" sentences for defendants, especially relating to drug crimes. These laws essentially worked by placing a state-imposed minimum sentence of incarceration for defendants convicted of a certain number of drug crimes. In order to avoid the gamble that is trial and the high incarceration time associated with mandatory minimums, many defendants are keen to avoid the courtroom, providing an adverse incentive for prosecutors to use mandatory minimums as a credible threat in plea bargains (Heumann 1979).

3.2 Geographic Context

Chicago has had a constantly growing and evolving relationship with its own bail system. On September 18, 2017, Chief Justice of the Cook County Circuit Court, Timothy C. Evans, issued General Order 18.8A which, in essence, encouraged judges to use non-monetary bail as often as possible, and when it was absolutely necessary, the court should "consider the defendant's social and economic circumstances." A following report issued by Cook County itself demonstrated that the ensuing increase in released defendants did *not* lead to an increased public safety threat in Chicago, and rather actually coincided with an 8% decline in violent crime (General Order 18.8A).

Additionally, Chicago has a unique history relating to race, segregation and crime, being the place where the term "redlining" was coined as well as being a source of national controversy with its historically racially biased police department. All this leads us to the conclusion that Chicago has a unique history from other large metropolitan areas in the United States. While we cannot use the results of this case study to speak to how bail decisions are made and impact defendants in other cities, the rich data can inform us about the fundamental dynamics that defendants are exposed to in courthouses all around the country.

3.3 The Defendant's Journey

In order to understand the impacts of the bail system on subsequent court outcomes, we first account for how defendants end up in the courtroom in the first place. Chicago laws dictate that within 48 hours of being arrested for a crime, a defendant must be seen by a bail judge, where they are allowed a lawyer (but oftentimes don't have time to counsel with them), are informed of their rights, and are subsequently informed of the bail judge's decision. The Chicago Central Bond Court (henceforth referred to as "Branch 1") handles all felony cases that are not murders and violent sex offenses. Generally, misdemeanor cases are seen in various bond courts across Chicago depending on where the arrest took place or the arresting police division. However, on weekends and holidays, *all* cases are seen in Branch 1, and Branch 1 judges always begin seeing cases at 1:30 pm, so there is no heterogeneity in start time.

Judge rotations in Branch 1 are seemingly sporadic, with judge assignment not being correlated with day of the week or time of month. The vast majority of cases within our time frame are seen by 12 judges, the remainder of which being seen by regular trial judges who come in on the weekend to assist with the increased flow into Branch 1. We filter out cases seen by these trial judges, as they do not generally see enough cases to have a developed and identifiable leniency measure, relative to the judges that specialize in bond decisions. Since judge assignment is based on day of week and generally sporadic, defendant's cannot choose their judge and furthermore, can't commit crimes in anticipation of receiving one judge over another. Hence, it is likely that judge assignment is quasi-random for defendants, which previous literature in the field has exploited to construct judge leniency instruments.

The bail judges weigh the public safety risk posed by the defendant and assign them one of several bond types: no bond, I-bond, C-bond, EM-bond, D-bond, or D-EM bond. The usual case for being assigned no bond is that a crime was too severe and the judge views any form of release as being a hazard, and therefore the defendant is detained. An I-bond, also known as "Release on Recognizance" is assigned to a defendant when a bail judge determines they can be released. These bonds are assigned with a monetary amount that the defendant is responsible for paying if they choose not to appear for their court date or violate the conditions of their bond, i.e. committing another crime prior to their hearing, however so long as they do not break the conditions of their bond, they never have to pay any amount on the I-bond. C-bonds are a vastly shrinking form of bond assignment, in which a monetary bail is assigned to a defendant and the defendant is responsible for paying 100% of the amount up front to be released. Due to the incredibly small share of the bond population they occupy (<1%) and the severity of crime associated with them, we choose not to include them in our analysis.

By far, the most common form of bond is a D-bond, where there is once again a monetary amount posted with the bond, but the defendant is only responsible for paying 10% of the amount. However, if the defendants violated bond conditions (fails to appear or commits another crime pretrial), they will be responsible for the full amount. If the defendant abides by the conditions, however, they receive 90% of their deposit back, the remaining 10% being left as a court fee. Finally, a growing type of bond alternative is EM, electronic monitoring, which serves as a medium between release and detention, in which defendants are released but are monitored until their court date to ensure they do not flee. This can often be paired with a D-bond as we find within the data and construct it as its own type of bond. We can see how bond distribution changes over time in Figure 1. Additionally, we plot the release rate to see if it varies with bond distribution over time, but we find that beyond short-term fluctuation, there is no meaningful long-term trend. The rise of EM is an interesting phenomenon and merits its own studies to examine its effects on court, crime, and economic outcomes.

By the nature of the court system, following Branch 1, all potential felony charges are taken to a hearing where the court determines which ones can be transferred to a criminal division, and which ones are dropped for misdemeanor charges. If the felony review results in the transfer to a criminal division, then a new case is opened up for the same defendant and the same incident. Using lifetime tracking of defendants within the court system, we can assess if

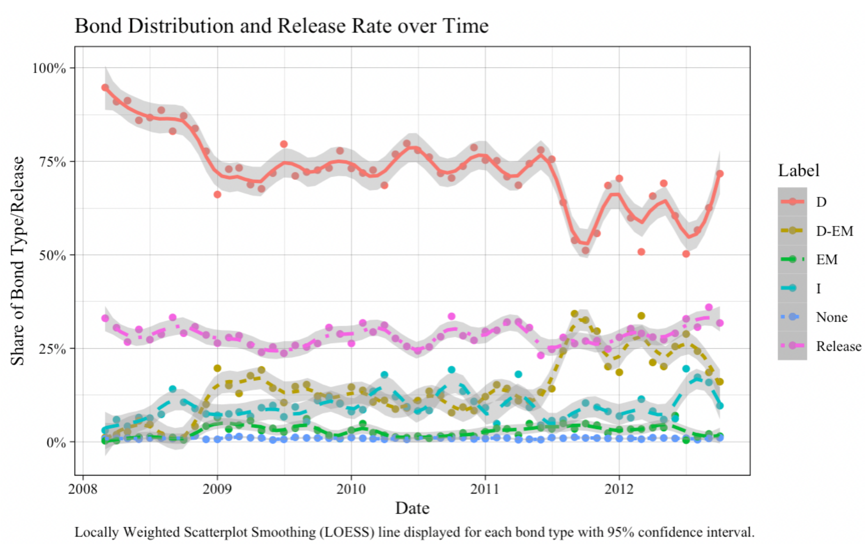

Figure 1: Share of Bonds over Time

pretrial detention is impacted by or affects lifetime criminological behavior. We find that at any given incident-case, 17.6% of defendants come in with no prior criminal history.

If a defendant receives an I-bond or EM-bond, they are free to be released on the spot. If the defendant receives a monetary bond, they can choose to pay the sum up front and be released or they are sent to jail. They are henceforth detained until their court date, until they come up with the money, or until they petition their bond. If a defendant is not granted bail, they must also be detained in jail or petition their bond.

3.4 Qualitative Research

Since Branch 1 of the Cook County Court system live streams all their hearings, we sat in on 20 such hearings in order to better understand the standard proceedings of bond court in Chicago. Of course, we are attending bond court a decade after the date of our study, so we cannot necessarily generalize the findings to our data. However, general court standards were still useful to observe. Defendants are seen by the bail judge on average for two to three minutes in order for a decision to be made. The judge begins by addressing the defendant and informing them of their right to a public defender. The judge then lists the current charges against the defendant. At this point, the state provides a brief summary of the police report of the incident, the defendant's criminal and court history, and the defendant's previous history upholding bonds, if applicable. Following this, the public defender provides a short description of the defendant, including their age, family, work status, education, and housing status. The public defender then declares how much money the defendant has indicated they are capable of dedicating towards bail. At this stage, the judge determines what type of bond to provide the defendant along with the amount.

By attending these bond court hearings, we learned that in recent years (after our sample end date), Branch 1 judges in Chicago have begun using an algorithmic assessment called the

"Public Safety Assessment" (PSA) to assign quantitative risk measures to defendants in order to aid with the bail setting process. Since the PSA was not employed at the time of our sample, we do not use it as an explicit control as it is unlikely that judges were internally constructing and abiding by the direct computation for all defendants, and in addition, it is still just a supplement to the judge's decision making process. However, we use the PSA to inform our choice of controls, explicitly including each component of its respective formulas[1] as a direct control in all our regressions to the best of our ability. We do this operating under the assumption that public safety risk factors for defendants likely have not changed drastically in the last decade.

We additionally interviewed a Chicago public defender, allowing us to compile various disposition codes that occur throughout the incident-case timelines. The attorney was able to guide us through what different difficult to interpret codes referenced in the courtroom, as well standard practices that occur over a case's lifetime. This provides us immense detail on which charges are found guilty and which are dropped, along with actions defendants and the court takes, such as demanding trial, pleading guilty, posting bail, etc., as well as when these actions take place. The public defender also pointed us to the FBI's Uniform Crime Reporting Handbook which we used to convert highly specific charges and crimes into more meaningful, broader categories of crimes. Finally, the public defender helped us understand common strategies that are taken both on the prosecutor and defendant side, such as defendants demanding trial and prosecutors opening new cases in felony review to pressure defendants. This was useful in our compilation of cases down to the incident-case level and in our understanding of the relationship between jail time and lack of trial, bargaining power, and pleading guilty.

---

[1] For more information on how specifically the PSA score is calculated, refer to https://advancingpretrial.org/psa/factors/. There are three separate formulas for Failure To Appear (FTA), New Criminal Arrest (NCA), and New Violent Criminal Arrest (NVCA) scores, respectively. The components included in the formulas are: pending charge at time of arrest, prior conviction (misdemeanor or felony), past FTAs in the last 2 years, past FTA over 2 years ago, age at arrest, prior violent conviction, previous sentencing to incarceration, current violent offense, and current violent offense and under 20 years old.

4 Data

4.1 Data Source

We are able to access a unique dataset providing detailed data following individuals through their journey with the criminal justice system. This data provides us an account of all actions taken in the Cook County courts. Specifically, we have case, defendant, arrest, jail, and charge level data. These different levels provide us with the following information for cases in Chicago up until 2019:

- Cases seen by the courts, presiding judges, various crime codes invoked, their outcomes, and finalized bail decisions
- Detailed defendant information, including race, age, and sex
- Arrest information, including arrest location and crime information for the initial charge
- Docket data, including every motion that occurs, essentially providing us a unique play-by-play for each case and each charge's path in the courtroom
- Cook County Department of Corrections Jail data, allowing us to see when an individual is booked, released, and the reasons for both

Of course, the caveat with using such heavily specialized data in Cook County, as mentioned previously, is that we lose geographic generalizability to other cities.

4.2 Sample Construction

We construct common defendant and incident identifiers[2] using fuzzy matching[3] and network theory[4] in order to link across all cases opened for a defendant for the same incident and compile them on that level (henceforth referred to as an "incident-case"). This allows us to examine the entire criminal history and future court outcomes for a defendant, as well as *all* their court outcomes. In addition, the defendant identifier allows us to link to Chicago Police Department Arrest and Crime data, as well as Cook County Jail data in order to get a detailed look at arrest information and details with respect to their detention. We drop all defendants that are not matched to the jail data and are not released (do not receive I-bonds or EM bonds) (12% of the original sample) and defendants for which we calculate negative jail time (8 observations).

After linking across all relevant defendant information, we selectively filter for several key features. Primarily, we only want defendants that pass through Branch 1 within the first 48 hours days of their arrest. Additionally, we selectively filter after 2008 due to a lack of data quality in years preceding 2008 and filter for cases that begin before November 2012 since the use of EM bonds becomes much more prevalent. Due to the controls we use, demographic variables are necessary for any defendants in the final sample. Even though these descriptors are missing for several incident-cases, by aggregating at the defendant level over all time, we are able to recover the mode race, date of birth, and sex of the vast majority of defendants. We drop

[2] Amongst all the datasets we use, there is no single common identifier. Arrest data documents arrests by Central Booking (CB) numbers. Crime data documents crimes by Records Division (RD) numbers. Raw docket events are documented by Case Numbers (CN). Jail data documents individuals by Individual Record (IR) numbers. Cases themselves include CB, RD, CN, and IR numbers to provide a basis for linkage. However, due to the nature of felony review, oftentimes multiple CN's are opened for the same incident. Additionally, data documentation often results in one or more of these identifiers missing for a case, or one or more identifiers not being unique. Hence, we seek to create identifiers that can track individuals throughout all the data as well as tracking all cases related to specific incident-cases.

[3] We use the Levenshtein distance, calculated as the number of characters that need to be added, deleted, or swapped to turn one string into another as our metric for how similar strings are when matching. We use different thresholds for different types of matching, but generally allow 2 to 3 characters to vary when matching, conditional on other factors being similar.

[4] We create a unique defendant identifier (DID) using IR number when possible, and fuzzy matching with names when IR number is missing. We ensure that defendant characteristics like date of birth, race, height, etc. match up when constructing this measure. We then define our incident-case identifier as "Fake" Central Booking (FCB) number. To do this, we group by RDxDID and generate the minimum CB (min_CB) number for that intersection to link all RDxDID nodes together. We then group by CBxDID and find the minimum "min_CB" number which we dub our FCB for that intersection to link all CBxDID nodes together (at this point transitively grouping RDxCBxDID into one incident-case identifier, FCB). We can now aggregate all CN's for each FCB as a single incident-case.

all those that can't be recovered by this methodology as well as defendants that are under 18 at the time of their bail hearing (3.1% of the original sample).

Detailed docket data allows us to aggregate whether certain events, like pleading guilty or failing to appear (FTA) in court occur at any point during an incident-case. This docket data also allows us to see which judges defendants interacted with during branch 1 and all the actions associated with bail-setting. We are also then able to filter selectively for full time Branch 1 judges, instead of judges that appear for only a few cases. We are left with 153,524 observations of incident cases that were initiated in Branch 1 between 2008 and November 2012 in Chicago (84.9% of the original sample).

4.3 Summary Statistics

We begin by examining different characteristics at a surface level for defendants who are released vs defendants who are detained. We define release to be true if a defendant satisfies the following conditions:

- Receives an I-bond & is unmatched to jail data
- Receives an I-bond
- Receives an EM-bond & is unmatched to jail data
- Spends less than 3 days in jail (effectively pays off monetary bond in 3 days)
- Receives an EM-bond

Otherwise, if the defendant spends more than 3 days in jail, we consider them to be detained. If the jail release date is less than the bail hearing date or there is no match to the jail data, we consider release to be missing. We test the robustness of this assumption in Appendix A.2, ultimately showing that our results are consistent across other matching specifications. Our threshold for release is spending less than 3 days in jail, following the previous literature which cites that the adverse effects of pretrial detention need only 3 days to begin imposing negative impacts on a defendant's life. It's also the margin of time in which a bail judge's decision is most likely to affect the outcome. That being said, we note that 19% of detained defendants are released within two weeks. In a robustness check in Appendix A.1, we show that past 2 days, all detention thresholds yield very similar results.

In Table 1, we can observe how bail decisions influence whether a defendant was ultimately detained or released. Note that the pretrial release rate in Chicago is lower than in other contexts studied in Dobbie et. al. (2018) giving us context into how more stringent bail systems might function. We see that amongst those that are detained, 98% are detained because they have received a monetary bond. This consists of 81% being D bonds and 17% being D bonds with electronic monitoring.

Table 1: Release Outcome by Bail Decision

	(1) Detained	(2) Released
Monetary Bond	0.98	0.53
	(0.140)	(0.499)
D Bond	0.81	0.45
	(0.395)	(0.498)
D Bond with EM	0.17	0.08
	(0.379)	(0.268)
Release on Recognizance	0.01	0.36
	(0.0919)	(0.480)
Electronic Monitoring	0.00	0.11
	(0)	(0.311)
No Bond	0.01	0.00
	(0.106)	(0.0288)
Bail amount ($ thousands)	54.22	13.64
	(108.3)	(56.78)
Observations	108991	44533

mean coefficients; sd in parentheses

We also see that the average bond amount for those that are detained is $54,022, which is extremely high, and as we discover, is heavily influenced by a few abnormally high bail amounts. The median bail amount for those that are detained is actually $30,000. The characteristics for those that are released vary greatly, with only 53% of those released having a monetary bond, and 36% being released on recognizance (I-bond). We also see 11% of released defendants are released on electronic monitoring. Released defendants face an average bond amount of $13,640, which is less extremely influenced by outliers with a median bond amount of $10,000.

In Table 2, we observe defendant characteristics and how those vary with release status. From this, we can see that the characteristics of the released population tend to be less black, having a hearing on the weekend, and generally having a less prior criminal justice contact, with a particularly low composition of previously convicted felons. On average, both detained and released defendants tend to be in their early thirties and majority male. After calculating the same defendant characteristics by detained defendants that are given a bond versus those that are not, we find no significant difference across characteristics between the two subsamples.

Table 2: Defendant Characteristics

	(1) Detained	(2) Released
Male	0.88	0.82
	(0.320)	(0.381)
Black	0.77	0.63
	(0.418)	(0.483)
Hispanic	0.14	0.20
	(0.344)	(0.401)
White	0.09	0.16
	(0.281)	(0.370)
Age	34.38	32.69
	(11.63)	(12.12)
Weekend	0.39	0.46
	(0.487)	(0.499)
Case in Last Year	0.53	0.32
	(0.499)	(0.467)
Any Past Failures to Appear	0.60	0.34
	(0.489)	(0.473)
Any Previous Case	0.89	0.69
	(0.310)	(0.462)
Any Past Felony	0.72	0.40
	(0.451)	(0.489)
Any Past Guilty Cases	0.41	0.12
	(0.491)	(0.329)
Any Past Guilty Felony	0.31	0.06
	(0.462)	(0.245)
Any Past Violent Felony Case	0.11	0.04
	(0.310)	(0.198)
Any Past Guilty Violent Felony	0.04	0.01
	(0.205)	(0.0855)
Observations	108991	44533

mean coefficients; sd in parentheses

In Table 3 we can see the charge characteristics for detained and released defendants. We can see that on average, both detained and released defendants have a fairly similar number of charges against them near 2 per incident-case. Detained individuals tend to have a higher proportion of felonies, at 77%, and only 18% are exclusively dealing with a misdemeanor. In

Table 3: Charge Characteristics

	(1) Detained	(2) Released
Number of Charges	2.00	1.90
	(1.779)	(1.569)
Any Felony Charges	0.77	0.63
	(0.419)	(0.482)
Any Misdemeanor Charges	0.37	0.45
	(0.482)	(0.497)
Only Misdemeanor Charges	0.18	0.30
	(0.382)	(0.457)
Any Property Crime	0.14	0.05
	(0.342)	(0.221)
Any Violent Crime	0.19	0.13
	(0.396)	(0.333)
Any Drug Crime	0.48	0.52
	(0.499)	(0.500)
Any Other Crime	0.49	0.54
	(0.500)	(0.498)
Observations	108991	44533

mean coefficients; sd in parentheses

general, around 50% of both detained and released individuals are dealing with a drug charge and around 50% of both groups are dealing with some other type of charge. We defined other crimes to include traffic, weapons, and court-determined "other" crimes. Violent crimes include murder, sex offenses, domestic violence charges, and other violent crimes. Drug crimes include both possession and delivery. Within released individuals, 30% are dealing with only a misdemeanor, and 63% have a felony charge. The general statistics indicate what we might expect from bail judges and their reliance on severity of crime as a metric for determining the conditions of an individual's release.

Finally, in Table 4, we can get an overview of heterogeneity in court outcomes conditional on pretrial release. First, and very notably, we see that 54% of detained individuals incur a guilty verdict, as compared to 28% of released individuals. Of course, this can be due to many different factors, most obviously, if bail judges are assessing the criminality of an individual and sorting to some degree of accuracy, it would make some sense that a higher share of detained individuals would be guilty. However, we are interested more in what the impact of pretrial detention is, holding constant crime severity, individual characteristics, and guilty status.

All else equal, does just the state of being detained pretrial somehow impact the probability of a defendant being found guilty? We also find that for both detained and released individuals, the vast majority of guilty verdicts are driven by guilty pleas. Detained defendants, in general, fail to appear (FTA) in court and commit crimes before the final disposition less than their released counterparts. This mechanistically makes sense, as it is much more difficult to dodge court and commit crimes when detained than released, which is the main argument against having more lenient bail systems. The crimes and FTA's for "detained" individuals are driven by defendants that we label as detained by our 3 days threshold but eventually are released from jail.

The net impact relationship with recidivism however is that in general a smaller share of the released population commits crime in the future. Of course, however, this could once again be selection bias on behalf of judges releasing less criminologically involved defendants. As far as sentencing goes, 32% of the detained individuals see some prison time, while only 5% of released individuals go to prison.

Finally, we see that on average, detained individuals have 86.85 days in jail[5], however, this is driven mostly by a few extremely high outliers, with a median of 24 days spent in jail for all detained defendants. That being said, 25% of detained defendants spend over 92 days in jail, and even as much as 40% of detained defendants spend over 40 days in jail. Of these 40% that

[5] A quirk in our data is that defendants on EM bonds are technically still "booked" in the jail data, so somebody could be released on EM but their time released is counted as jail time. We handle this by explicitly coding EM bonds with over 3 days in jail as still being released and by not counting cases that have EM bonds to our calculations of days in jail.

Table 4: Court Outcomes

	(1) Detained	(2) Released
Any Guilty Verdict	0.54 (0.499)	0.28 (0.449)
Guilty Plea	0.52 (0.500)	0.27 (0.443)
Any Guilty Felony	0.45 (0.498)	0.21 (0.405)
Any Guilty Misdemeanor	0.09 (0.281)	0.07 (0.262)
Defendant Demanded Trial	0.98 (0.148)	0.91 (0.291)
Days in Jail	86.85 (162.0)	0.76 (0.732)
Failure to Appear in Court	0.04 (0.185)	0.11 (0.316)
Any Future Case before Final Disposition	0.12 (0.320)	0.15 (0.355)
Any Future Case 0-2 Years after Final Disposition	0.42 (0.493)	0.31 (0.464)
Any Future Cases 0-4 Years Later	0.67 (0.469)	0.55 (0.497)
Total Cases 0-4 Years Later	2.45 (3.154)	1.81 (2.782)
Any Prison	0.32 (0.467)	0.05 (0.222)
Any Jail	0.09 (0.292)	0.05 (0.223)
Any Probation	0.11 (0.312)	0.12 (0.329)
Any Other Sentence	0.55 (0.498)	0.28 (0.449)
Observations	108991	44533

mean coefficients; sd in parentheses

spend over 40 days in jail, 80.5% end in a guilty plea as opposed to being seen in court. For reference, this is 93.4% of *all* guilty cases that spend over 40 days in jail and do not involve EM bonds. Note that the guilty plea rate is 44.9% for all cases, but conditional on being found guilty eventually, the guilty plea rate is 94.2%. Hence, we see that even though defendants are given the "right to a speedy trial," trial is not something most defendants end up seeing, and when it is, it is by no means speedy.

As we mentioned earlier, another possibility is that defendants might be choosing to stay in jail to avoid going to trial and be faced with potential mandatory minimum sentences. However, we find that 98% of all detained defendants demand a trial before their jailstay is over. These detained defendants spend a median of 20 days waiting between demanding trial and being eventually released from jail (typically the end of the case). A shocking 25% of detained defendants spend over 66 days in jail even after demanding a trial.

5 Empirical Strategy

5.1 Ordinary Least Squares

Fundamentally, we want to examine the impacts of pretrial release for defendant *i* and incident-case *c* on date *t* as:

$$Y_{ict} = \beta_1 R_{ict} + \beta_2 X_{ict} + \mu_t + \epsilon_{ict}$$

Where *Y* represents our outcome of interest, *R* is a binary variable defined as we've constructed release, and *X* is a vector of controls. Our main outcomes of interest will be: any guilty verdict, any guilty plea, any failure to appear, any crime before the final disposition date, any crime 2 years beyond the final disposition date, and any incarceration. *X* contains a vector of controls relating to defendant characteristics including race, age, sex, number of charges, the nature and severity of those charges, and past criminal history. We include μ_t as time fixed effects defined as the sum of a month-year interaction indicator and a day of week indicator to account for various judge schedules and any variation in defendant characteristics over time..

Unfortunately, as discussed in the literature, the impacts of pretrial release β_1 various outcomes are likely endogenous, as the choice to release a defendant might be correlated with the choice to find that defendant guilty resulting in selection. We've also empirically seen in our summary statistics that the characteristics of released defendants are different from the characteristics of detained defendants. If released defendants are selected by bail judges on the basis that they are less likely to commit further crime, then our OLS estimates might be biased to find that release reduces future crime. To account for this and other biases with our OLS, we would require an instrument in order to identify the true impact of release on different outcomes.

5.2 Instrumental Variable

5.2.1 Jackknife Instrumental Variable Estimator (JIVE) Construction

In order to identify the causal impact of pretrial release on different outcomes, we want to find an instrument that is relevant, exogenous, and exclusionary (Imbens & Angris 1994). To interpret our results as the Local Average Treatment Effect (LATE) we additionally need to show that our treatment, release, is monotonically increasing in our instrument (Imbens, Angrist, & Rudin 1996). Following the literature, we use a measure of quasi-randomly assigned judge leniency as our instrument. This identification strategy allows us to interpret differences in outcomes for defendants on the margin of release (as in defendants that are released by the leniency of one judge but might have been detained by another or vice versa) as the causal effect of the change in probability of release associated with judge assignment. Assuming our instrument satisfies the necessary conditions, we will be able to calculate our desired reduced form regression with the addition of the following first stage regression:

$$R_{ictj} = \gamma_1 Z_{ctj} + \gamma_2 X_{ict} + \mu_t + \phi_{ict}$$

Where Z_{ctj} is our constructed measure of leniency for incident-case *c*, court date *t*, and judge *j*, and κ_t is the same time fixed effect of the month, year, and day of week.

As mentioned before, we want our instrument to capture judge leniency, which we estimate using a residualized, leave-out judge leniency jackknife measure following Dobbie et. al. (2018) and Dahl et. al. (2014), to account for case selection. This leniency measure is allowed to vary over time to account for a judge becoming more or less stringent. Since judge assignment is only quasirandom, if certain judges are more likely to take on some shifts over others, then a simple leave-out mean would be biased. Hence, we use the residualized measure of leniency, where we estimate the residualized pretrial release decision after removing time fixed effects as:

$$R_{ict}* \;=\; R_{ic} - \mu_t$$

We then use this measure of residualized release to construct the leave-out mean release decision of the assigned judge within a respective bail year to construct our Jackknife Instrumental Variable Estimator (JIVE) calculated as:

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

where n_{tj} is the number of cases of judge *j* in year *t*, and n_{itj} is the number of cases seen by defendant *i* in year *t* by judge *j*. Note that since there are only 12 judges in our sample and there is a high rate of recidivism, it is not impossible for an individual to encounter the same judge multiple times in a year.

We caution that the contexts in which this JIVE is used in the literature are usually dependent on an increasing number of judges with defendants. Since our case in Chicago deals exclusively with 12 bail judges, this specific jackknife construction may not be as relevant. On the converse, however, since there are such few judges and defendants are more likely to reencounter judges, the jackknife might still be helpful. As a robustness check in Appendix A.3, we evaluate our same court outcomes using judge-year fixed effects as our instrument. We find that our results are robust with respect to the specific construction of our judge leniency measure, with the treatment size and significance of release not changing by a maximum of 2.6 percentage points for any outcome.

5.2.2 Exogeneity

In order to interpret our two stage least squares regression estimates as the Local Average Treatment Effect (LATE), we must ensure that our instrument is exogenous. In other words, we want our instrument (judge leniency) to be randomly assigned and thus have no correlation with defendant characteristics. Even though we know that judges are quasi-randomly assigned to defendants, and we can't directly test exogeneity, we can still test to see if the leniency measures we have constructed are relatively independent of defendant characteristics.

Table 5 shows us that our assignment of cases to bail judges is random after factoring in time fixed effects. The first column of Table 5 uses a Linear Probability Model (LPM) to regress all defendant characteristics against an indicator for pretrial release, using heteroskedasticity resistant standard errors clustered at the individual-judge level. We find that male defendants are 8.3 percentage points less likely to be released than their female counterparts of otherwise equal characteristics, a 28.6% decrease from the mean pretrial release rate of 29%. Black defendants are 11 percentage points less likely to be released than white defendants, a 37.9% decrease from the mean. Only having a misdemeanor charge is one of the best indicators of pretrial release at a 14.2 p.p. increase in the probability of release, or a 49% increase from the mean release rate. Intuitively, this makes sense from the bail judge's assessment of crime severity.

We bin age, past cases, number of charges, and past failures to appear in court by their quantiles in order to account for any nonlinearities in the probability of release, and indeed, we find that there are heterogenous impacts for the variously constructed bins. Age bins are measured relative to a 25 to 30 year old, number of charges relative to 1 charge, and number of FTAs to having none historically. Finally, one can observe the impacts on various controls for crime type and severity, the majority of which being significant. As we can see, the collection of defendant characteristics is jointly significant with an F-statistics of 1627, meaning that we can reject the idea that defendant characteristics do not predict pretrial release.

Using an identical specification, we try to predict our constructed judge leniency measure using the defendant characteristics. As we would hope and expect for an exogenous instrument, almost all the covariates are insignificant in predicting judge leniency, and from the few that are, the magnitude of their impact is incredibly low. Although an F-test of joint significance would find our controls with time fixed effects jointly significant at the 1% level, the F-statistic is very low at 2.745 compared to our LPM for release. Additionally, the R^2 at .073 for our instrument tells us we are only predicting 7.3% of judge leniency with our characteristics, compared to the 17.5% for the release LPM. There are 4 variables that are individually significant, however since each individually has a small magnitude of impact, we follow Aizer and Doyle (2015) in proceeding with the assumption of the exogeneity of our instrument.

5.2.3 Relevance

In order for our judge leniency measure to be a valid instrument, it should be a good predictor of our endogenous treatment, in this case, pretrial release. Table 6 shows the results of this first stage regression, where all standard errors are clustered at the judge-defendant level. Column (1) displays a standard OLS estimate, column (2) adds defendant controls, and column (3) adds in time fixed effects. As we can see, judge leniency is an extremely significant predictor of pretrial release. Adding in controls from the OLS model increases our F statistic from 609.8 to 1655.8, and even though adding in fixed effects in model (3) reduces the F statistic slightly, a one unit increase in judge leniency is still indicative of a 94.1 percentage point increase in the probability of release. Controlling for time fixed effects, the judge leniency measure itself varies from from -.0639 to .0613, so moving from the least to the most lenient judge corresponds with a 11.8

Table 5: Test of Randomization

	(1) Pretrial Release	(2) Judge Leniency
Male	-0.0829***	-0.0000969
	(0.00333)	(0.000206)
Black	-0.110***	-0.000386
	(0.00379)	(0.000233)
Hispanic	-0.0496***	-0.000191
	(0.00460)	(0.000278)
[18,24) Years Old	0.0261***	0.000523*
	(0.00315)	(0.000205)
[31,43) Years Old	-0.0783***	-0.000318
	(0.00304)	(0.000200)
43+ Years Old	-0.0737***	-0.000112
	(0.00311)	(0.000205)
Case in the Last Year	-0.0926***	0.000274
	(0.00231)	(0.000156)
Over 2 Charges	-0.0570***	-0.000113
	(0.00301)	(0.000202)
1 to 2 Past FTAs	-0.110***	-0.000347*
	(0.00266)	(0.000173)
Over 2 Past FTAs	-0.166***	-0.000146
	(0.00277)	(0.000201)
Only Misdemeanor Charges	0.142***	0.0000618
	(0.00574)	(0.000377)
Any Past Guilty Felonies	-0.169***	-0.000173
	(0.00209)	(0.000176)
Any Felony Property Charge	-0.0779***	0.0000776
	(0.00465)	(0.000353)
Any Violent Felony Charge	-0.150***	-0.000219
	(0.00491)	(0.000431)
Any Drug Felony	0.0477***	0.0000833
	(0.00429)	(0.000288)
Any Felony Murder/Sex Charges	-0.208***	0.000908
	(0.0119)	(0.00131)
Any Felony Weapon Charges	-0.0839***	0.000201
	(0.00520)	(0.000391)
Any Other Felony Charge	-0.00759	-0.000579
	(0.00472)	(0.000317)
Any Misdemeanor Property Charge	-0.0610***	0.00102*
	(0.00629)	(0.000449)
Any Misdemeanor Drug Possession Charge	0.0772***	0.000471
	(0.00420)	(0.000274)
Any Misdemeanor Weapon Charges	-0.00686	-0.000900
	(0.00736)	(0.000506)
Any Misdemeanor Domestic Violence Charges	-0.103***	0.000560
	(0.00592)	(0.000374)
Any Violent Misdemeanor Charge	-0.0333***	0.000336
	(0.00628)	(0.000438)
Any Other Misdemeanor Charge	0.0340***	-0.000230
	(0.00355)	(0.000242)
Any Traffic Charge	0.0909***	0.000549*
	(0.00431)	(0.000270)
Observations	153524	153524
Adjusted R^2	0.175	0.073
F	1627.0	2.745

se in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

percentage point increase in the probability of release. In Figure 2, we can observe the distribution of our instrument as well as the first stage regression, which we can see is approximately linear, as we would hope.

Table 6: First Stage of Judge Leniency on Pretrial Release

	(1) OLS	(2) OLS Controls	(3) OLS Controls FE
Judge Leniency	1.029***	0.965***	0.941***
	(0.0417)	(0.0378)	(0.0393)
Controls	No	Yes	Yes
Observations	153524	153524	153524
R^2	0.004	0.176	0.179
Adjusted R^2	0.004	0.176	0.179
F	609.8	1655.8	1590.8

se in parentheses
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

5.2.4 Monotonicity

As Angrist, Imbens, and Rubin (1996) show, if a LATE instrument is not monotonic, then the 2SLS results would be biased as an increasing function in the number of observations for which

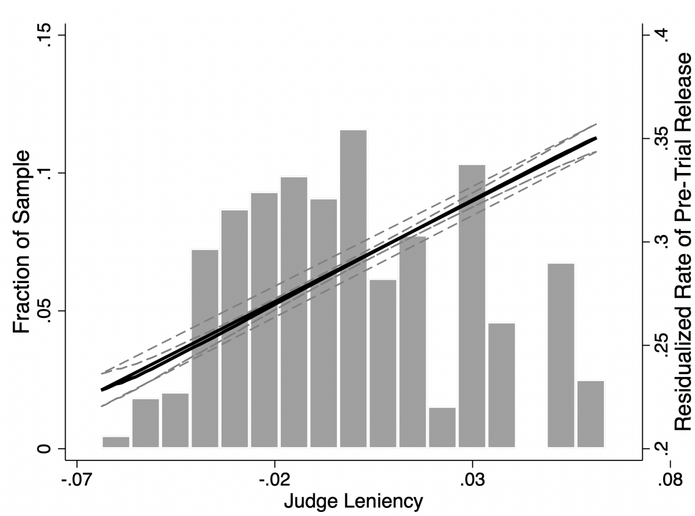

Figure 2: Distribution of Judge Leniency and First Stage Regression

monotonicity fails to hold. In our case, monotonicity in judge leniency intuitively means that if a stringent judge releases a defendant, then a more lenient judge should release a defendant with the same characteristics. Similarly, if a lenient judge detains a defendant, we should hope a more stringent judge also detains that defendant. As we can see in Figure 2, the residualized rate of pretrial release appears to be monotonically increasing in judge leniency, and is nearly linear, as we would desire from the relationship. This provides us with good evidence that our instrument is generally monotonic. However, as we explore later in the results of judge preferences, we find that monotonicity is not held for all defendants due to the presence of judges with biases towards certain races, genders, and prior criminal statuses.

5.2.5 Exclusion

Finally, to have a valid instrument, we require that it does not influence outcomes through any other avenue than pretrial release. By the nature of the Chicago courts, this is likely, as bail judges in general do not reappear later as case judges, and there is a different form of selection. Within the data, we find that only 0.2% of defendants encounter their bail judge later in the trial. As mentioned earlier, 12% of observations eventually reencounter a bail judge in a later case due to high rates of recidivism and fixed number of judges, but this is unlikely to influence court outcome by any other avenues than release. It is also unlikely that the assignment of the bail judge is related to other court related entities, such as public defenders and prosecutors, which are assigned with different processes. Additionally, since bail judges need only decide bonds for defendants and make no other choices for them, this institutional contrasting makes it unlikely that there are other avenues by which the bail judge influences the defendant than via release. Hence, it makes it unlikely that the exclusion assumption is violated for our instrumental variable. However, as we cannot test the assumption directly, we proceed with this potential caveat in mind.

6 Results

6.1 Case and Crime Outcomes

In this section, we examine the result of our 2SLS model, examining the Local Average Treatment Effect (LATE) for the marginal defendants of pretrial release on case outcomes. We examine the various outcomes in Table 7. We list the different outcomes of interest in the leftmost column, and column (1) contains the mean of that outcome variable for detained individuals that are not released within the first 3 days. For each of regressions (2) through (5), the coefficient listed is that of our treatment variable, release. Regressions (2) and (3) display regular OLS results, both with fixed effects, but (3) includes controls while (2) does not. Similarly, regressions (4) and (5) display our LATE estimators from our instrumented release measure with both using fixed effects, and (5) adding controls in (4).

Table 7: Impact of Pretrial Release on Outcomes

Outcome of Interest	(1) Detained Outcome Mean	(2) OLS FE Release	(3) OLS Controls FE Release	(4) IV FE Release	(5) IV Controls FE Release
Any Guilty Verdict	0.538	-0.257***	-0.0982***	-0.129**	-0.104**
	(0.499)	(0.00262)	(0.00242)	(0.0462)	(0.0368)
Any Guilty Plea	0.522	-0.251***	-0.108***	-0.144**	-0.125**
	(0.500)	(0.00260)	(0.00246)	(0.0459)	(0.0384)
Any FTA	0.0353	0.0773***	0.0746***	0.182***	0.181***
	(0.185)	(0.00160)	(0.00180)	(0.0223)	(0.0234)
Any New Case Before	0.116	0.0338***	0.0610***	0.140***	0.129***
Final Disposition	(0.320)	(0.00195)	(0.00212)	(0.0316)	(0.0323)
Any New Case Post	0.418	-0.112***	-0.0697***	0.0462	0.0327
Final Disposition (0-2 yrs)	(0.493)	(0.00266)	(0.00269)	(0.0462)	(0.0446)
Any Incarceration	0.407	-0.303***	-0.125***	-0.0988*	-0.0495
	(0.491)	(0.00208)	(0.00204)	(0.0430)	(0.0355)
Controls	No	No	Yes	No	Yes
Observations	153524	153524	153524	153524	153524
Adjusted R^2	0.012	0.059	0.457	0.041	0.454
F	546.8	9584.2	12260.1	7.848	4842.4

All regressions use heteroskedasticity resistant standard errors clustered at the individual-judge level reported in parenthesis. All coefficients reported are for pretrial release. We display the detained mean for each outcome to calculate the difference release creates from detained defendants

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

OLS estimates show that initially released defendants are less likely to have a guilty verdict, plead guilty, commit crime 2 years out from their final disposition, or face any kind of incarceration, but are likely to fail to appear to court or commit crime before their final disposition. However, the OLS estimates for most of the outcomes are incredibly sensitive to the inclusion of controls, suggesting that the controls are in fact useful in addressing potential omitted variable bias. For instance, with a regular OLS estimate, being released decreases the likelihood of pleading guilty by 25.7 percentage points, a 47.8% decrease from the detained mean. Once you add controls, however, the decrease shrinks to a magnitude of 9.8 percentage points, a 18.2% decrease from the detained mean.

The IV estimates reported in columns in (4) and (5) improve upon these OLS results by exploiting exogenous variation in pretrial release from the quasi random assignment of defendants to bail judges. In general, these IV estimators verify that pretrial release does improve court outcomes, however, the magnitudes of that impact vary from the OLS estimators. With the full set of controls, we find that being released pretrial decreases the likelihood of being found guilty by 10.4 percentage points, a 19.3% decrease from the detained mean, significant at the 1% level. We can also see that this difference is primarily driven by a decreased uptake in guilty pleas, finding that for the marginal defendant, release decreases their likelihood of taking up a guilty plea by 12.5 percentage points, a 23.9% decrease from the detained mean, significant at the 1% level. These results affirm the previous literature's theory that detention has a coercive effect and likely decreases a defendant's bargaining position in plea negotiations[6]. Finally, with court outcomes, we find that conversely to what OLS estimates show, the relationship between pretrial release and an eventual sentencing to incarceration is insignificant and marginally

[6] As mentioned in Dobbie et. al. (2018) this can occur as detained defendants (1) might be desperate to be released from jail, (2) might be negatively judged by jurors or judges' biases against incarceration, (3) might not be able to gather exculpatory evidence to their case, and/or (4) might be less likely to strategically extend their cases due to their stay in jail.

negative. In some respects, the finding of this insignificant result is affirming that our LATE is only capturing the effect of the bail judge's binary release outcome and that our instrument does not violate the exclusion restriction. Since sentencing is determined by an entirely different judge and process, even though we might expect it to be related to the underlying characteristics that might determine release, we would not expect nor want this outcome to be caused by release.

We additionally examine outcomes relating to failure to appear and recidivism. Our IV controls regression shows that released defendants are 18.1 percentage points more likely to fail to appear in court than their detained counterparts, a 513% increase at the 0.1% significance level. We also see that released defendants are 12.9 percentage points more likely to have a new case opened before their final disposition, a 111% increase at the 0.1% significance level. However, we document that of the 10,926 new cases that occur before a final disposition date for released defendants, only 9.8% have a violent charge associated with them, a smaller 4.4% are actually found guilty of a violent charge, and as little as 2.1% are found guilty of a violent felony charge. Hence, even though there is evidence that release significantly causes new cases, these new cases in general do not pose a large public safety risk and tend to not be guilty of violent charges. Finally, we find that the relationship between pretrial release and recidivism within 2 years of their case outcome is statistically not significant. Once again, recidivism after the end of a defendant's case is likely determined by a variety of factors that go beyond just a bail judge's decision, so it makes sense that we do not observe a significant result here.

6.2 Heterogeneity by Defendant Race

An important margin, as explored by the literature, is the differential impacts of the criminal justice system on black people specifically, and marginalized populations in general. As mentioned previously, black defendants are in general found guilty for similar crimes to their white peers and are more likely to be victims of police stops and violence.Since we've shown that release does have significant and relevant impacts on court outcomes, the next question we seek to answer is if these effects behave differently with respect to race.

Since we identify the primary driver of guilty verdicts as guilty pleas, we focus our analysis on the differential impacts of pretrial release on the likelihood of taking up a guilty plea by defendant race. To analyze this question, we create three different subsamples of the data for black, white, and hispanic defendants. We justify our use of samples in examining heterogeneity in different defendant populations by following Dobbie et. al. (2018) who do the same in studying defendants with prior and no prior criminal history. We run the IV regressions with the full set of controls and fixed effects on these subsamples and display the treatment effect size by race in Figure 3. Indeed, as can be seen by the confidence intervals around the coefficients, released black defendants are 11.5 percentage points less likely to plead guilty than their detained counterparts, a 22.2% decrease from their detained counterparts with a p-value of 0.006. We cannot reject the null hypothesis that release has no impact on the likelihood of pleading guilty

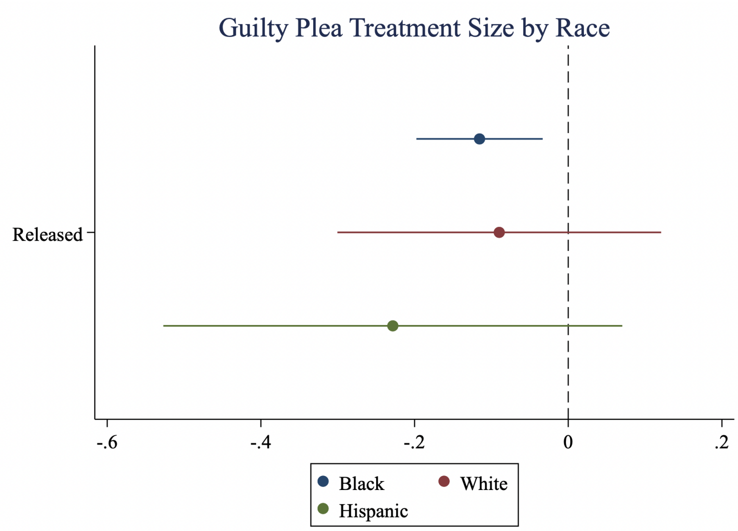

Figure 3: Guilty Plea Coefficient Confidence Intervals by Defendant Race

for white defendants or Hispanic defendants. We are cautious about our results with Hispanic defendants, however, as they are accompanied by a large standard error of 15.2 percentage points. There are over 7,000 more Hispanic defendants than there are white ones, which have a smaller standard error at 10.7 percentage points, so the increased variability in the mean for Hispanic defendants is not due to fewer observations. A potential explanation motivated by Ba (2021) is that the descriptor "Hispanic" encompasses a wide diversity of cultures and national origins and that Chicago court data is not granular enough to disaggregate but might correspond to observed heterogeneity in behavior.

6.3 Adherence to Constitutional Rights

6.3.1 Bail Amounts and Detention

To further understand the general relationship between bond amount and release, we bin bond amounts rounding up to the nearest $5,000, and for each bin, calculate the share of defendants within it who are detained. The results are shown in Figure 4, where we cut off the x-axis at $300,000 because past this amount, 99% of all defendants are detained. There are several bins beyond this, but the pattern is almost uniformly that defendants are detained when bond amounts reach those levels. Moreover, this pattern of higher bail amounts being associated with higher proportions of defendants being detained affects races differentially. In Figure 5, you can see that for the first $40,000, black defendants are significantly more likely to end up detained than their white and hispanic counterparts. Past $40,000, 94.5% of *all* defendants regardless of race are detained, so differences in ability to pay are no longer as pronounced. It is unlikely that these differential detention rates are due to crime severity or flight risk, as one might assume that bail judges independently consider those factors when assigning the scheduled bail amount. However, we leave the question of understanding how bond amount and liquidity constraints affect detention to future research. For the purposes of our paper, we take for granted whether or not a defendant was able to post bail and care only if they were released or detained.

6.3.2 Detention Length and Guilty Pleas

In Figure 6, we can examine the relationship between jail-time and pleading guilty with a little more depth. Although we cannot make any claims as to what drives the nature of this relationship, it is clear that higher jail time is positively correlated with a higher share of defendants pleading guilty (significant at the 0.1% level). We do not graph beyond 109 days (80% of the data) because there are generally very few observations per day in jail, however,

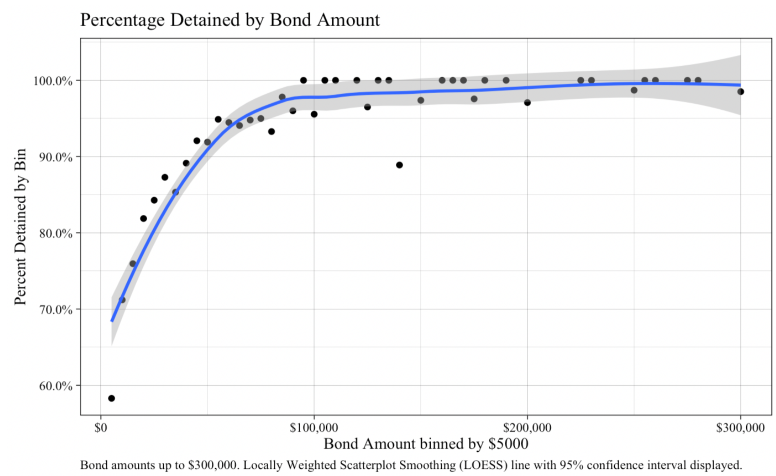

Figure 4: Percentage Detained by Bail Amount

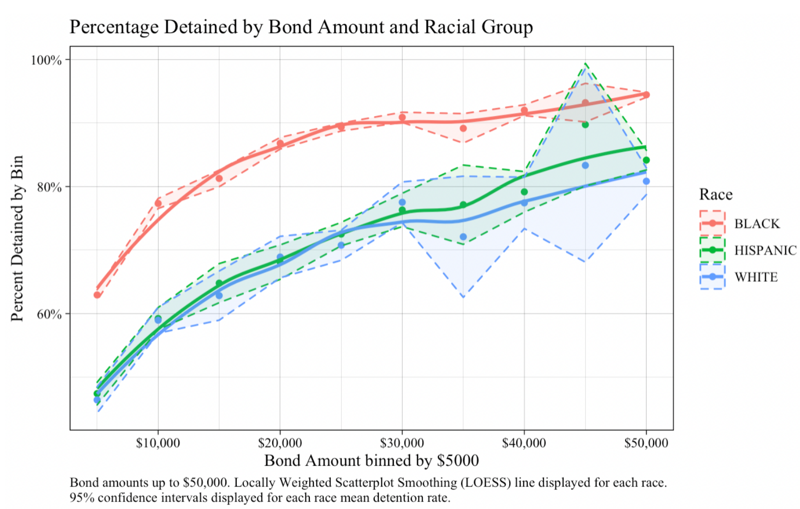

Figure 5: Percentage Detained by Bail Amount and Race

76.8% of those defendants on the average plead guilty. We find that there is no significant heterogeneity in the relationship between jail time and guilty pleas by race. Dobbie et. al. (2018) suggest that the reason pretrial detention leads to more guilty charges against defendants in Miami and Philadelphia is that detention places them in a place of worse bargaining power and they are more likely to take up an unfavorable plea bargain than their released counterparts.

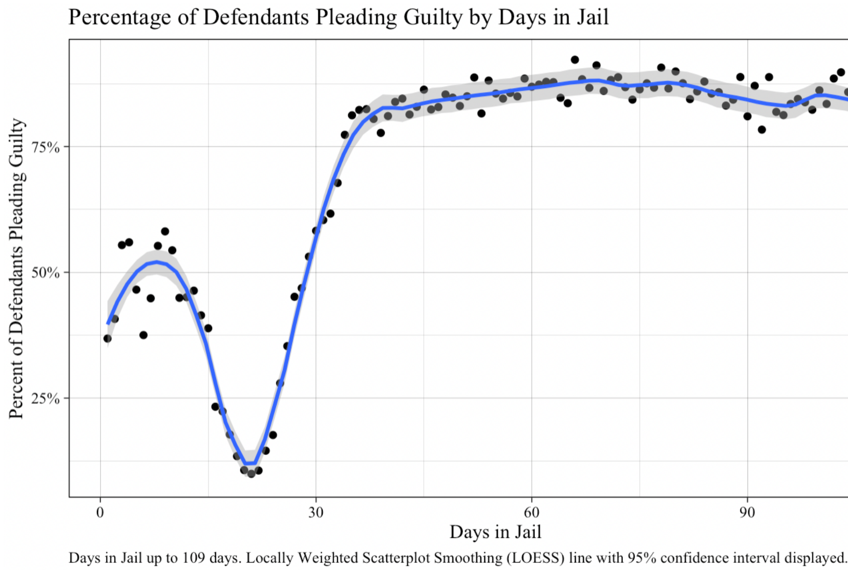

Figure 6: Share of Defendants Pleading Guilty by Days in Jail

Our identification strategy in Chicago corroborates this story. Furthermore, Figure 3 allows us to visualize the “weariness” that defendants undertake, spending more and more time in detention to eventually pleading guilty. There is also an interesting U-shaped relationship between days in jail and share pleading guilty between 10 and 30 days that is worth further exploration by future literature. Some interesting descriptive statistics about this group of defendants that only stay in jail between 10 and 30 days is that on average, only 10% have past guilty cases even though 91% have had a previous case. This means that even though this selection has a higher proportion of defendants with a past case, the share with a guilty past case is a quarter of what it is for all detained defendants. Additionally, 67% of these defendants have a drug crime as opposed to the 48% for the all detained individuals. So in general for this share of defendants that stays in jail for 15-25 days, only 14% are found guilty and these defendants tend to come from less criminological background and tend to be charged with more drug crimes than anything else.

6.3.3 Judge Preferences over Defendant Characteristics

Given the limited number of bail judges we deal with in Cook County between 2008 and 2012, we compute means for our judge leniency measure for each judge over various defendant characteristics. Specifically, we are interested in observing if there is heterogeneity in judge preferences over defendant race, gender, and prior criminal history. Ideally, we would be able to make comparisons of judge bias based on judge characteristics, however, due to a lack of data availability, we can only make raw comparisons by individual judges. We construct confidence intervals on the means of these margins and display the results in Figures 7 and 8. In Figure 7, we can see that judges 1, 2, and 12 are significantly less lenient towards black defendants than white ones, while judge 4 is more lenient to black defendants. We find the same pattern is true for Hispanic defendants.

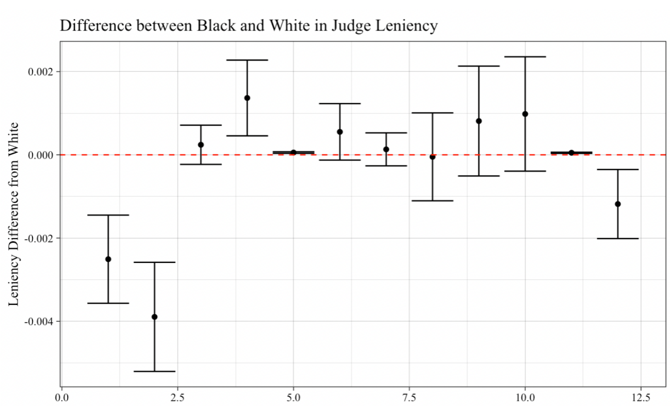

Figure 7: Difference between Black and White Defendant Leniency Means

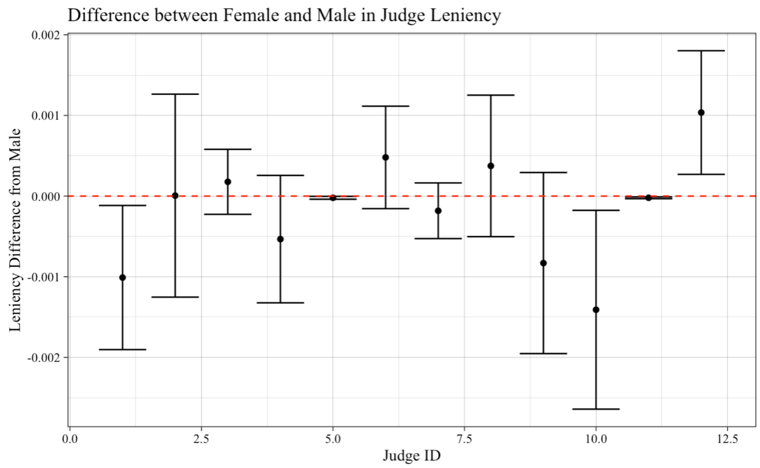

Figure 8: Difference between Female and Male Defendant Leniency Means

Figure 8 shows that judges 1 and 10 are significantly less lenient towards female defendants than male ones, while judge 12 is significantly more lenient towards female defendants. We construct identical confidence intervals for preferences over having a case in the last year. We find that judges 1, 2, and 8 tend to be more strict on defendants with a criminal history in the last year, while judges 7, 10, and 11 tend to have the opposite preference.

Of course, the existence of these recovered judge preferences is a violation of the monotonicity restriction on our instrument. If a generally more lenient judge tends to suddenly become strict in the presence of certain defendant characteristics, such as race or gender, then we cannot say judge leniency is monotonically increasing in release rates for all defendants. This is of course a limitation of our paper, as it weakens the viability of our instrument and hence the strength of our claims. However, we include this as an argument that future literature in the field should be explicit in its testing of individual judge preference, as the documented existence of similar judge biases would violate the monotonicity restriction for all judge leniency instruments.

7 Discussion

Our findings affirm that bail judges' decisions can have consequential impacts on defendants' case outcomes and future behavior. We go beyond what previous literature has established to show how for any given bail amount below $40,000, black defendants are significantly more likely to be detained due to an inability to post bail. Although we have no direct evidence, we speculate that due to systematic racism and economic disadvantage, black defendants in the criminal justice system enter it with more liquidity constraints than their white and hispanic counterparts. Hence, "punishments" that are meant to be uniform across defendant crime severity end up impacting black defendants more. It is worth note that Chicago court bond guidelines urge bail judges to factor in a defendant's financial circumstances when making bail decisions. However, given what we observe, the data is indicative of one or a combination of three things:

1) Chicago judges are systematically overestimating how much money black defendants have to spend on bail
2) Chicago judges are accurately estimating how much money black defendants have to spend on bail but are intentionally setting the bail amount too high for them
3) Black defendants assess their need or ability to pay off bail differently than their white and hispanic counterparts and are more stringent with their decision to post bail

Further research with access to defendant wealth prior to entering bail court would be instrumental in understanding how liquidity constraints and attitudes towards posting bail impact detention rates for defendants of different races.

By our estimates, when marginal defendants are unlucky enough to be assigned to a more stringent bail judge and end up being detained for more than 3 days, those defendants are more likely to be found guilty later in the case. We find specifically that this is driven by an increased uptake in guilty pleas for said defendants. This supports the hypothesis that being detained in jail places defendants in worse bargaining positions or mental states during plea negotiations. We supplement this hypothesis with our own novel jail data that allows us to not make any assumptions about defendant detainment length and directly observe the relationship between jail-time and plea bargaining. Although by no means causal, the relationship is direct and clear: there is a significant and positive relationship between jail time and share of defendants at the time taking up guilty pleas. This is might be a negative externality of our criminal justice system with direct policy implications: while the Constitution explicitly ensures defendants the right to a speedy trial, we observe that the sustained lack of delivery on such promises potentially has influences on defendants to take up guilty pleas in avoidance of spending more time incarcerated without being first found guilty.

If defendants are guaranteed a trial within a certain amount of days, we would be able to identify more clearly to what extent jail time contributes to the uptake of guilty plea bargains. Specifically, we would be able to analyze the mechanism of weariness leading to pleading guilty to be released from jail on the defendant’s end. Relying on more lenient types of bonds, such as using more I-bonds or Electronic Monitoring could help reduce these negative externalities. However, further experimentation on this relationship is necessary in order to estimate what the true impact of jail time on a defendant's bargaining positions is.

We also show that the impacts of pretrial detention vary by defendant race. Previous studies have established that there are negative impacts associated with pretrial detention, but we enhance this analysis by showing that even these impacts are differential in defendant race. Black and hispanic defendants are significantly more likely to take up guilty plea bargains due to pretrial detention, as opposed to white defendants, who do not face the same relationship.

Although we do not have a direct explanation for the mechanism by which this happens from the data, if black and hispanic defendants already view their bargaining position as being worse off due to systemic racism, they may be less hopeful that a trial would lean in their direction, so they may just want to get out of jail as soon as possible. Alternatively, it is possible that black and hispanic defendants do not have access to as good of lawyers as their white peers, and so it is rather access to effective legal counsel that drives these relationships. Further experimentation needs to be done, however, to understand the true driving mechanism that guides minority races to take up unfavorable plea bargains.

Finally, we document how individual judges can have different biases over various defendant characteristics, from uncontrollables like gender and race, to criminological ones, like criminal history. These differences in leniency towards different groups do not control for crime type, severity, or anything else. However, we argue that since defendants are quasi-randomly assigned to judges, such differences should be swept away through the random assignment, and all that should be left are individual judge preferences. What we are left with is suggestive evidence that judges' preferences impact how they interact with defendants. Of course, this has policy implications. Should a decision as pivotal as assigning a bond be left to a single individual with their own potential preferences and biases? Could judges be better vetted to make sure that they don't exhibit consequential implicit preferences over defendant traits such as race or gender? We leave the task of further dissecting individual judge bias and its impacts to future literature.

8 Conclusion

Understanding the ways in which our legal system abides by its own laws is essential in ensuring that equal justice under the law is distributed. Effective reform in a constantly changing and complex legal system requires a detailed understanding of the ways in which policies and practices affect defendant outcomes. With the racial reckoning the U.S. is facing, it is additionally important to be critical of the ways existing practices facilitate and allow systemic racism to continue.

Using a comprehensive dataset of Chicago cases between 2008 and 2012, this paper addresses these concerns by examining how judge assignment affects defendant outcomes for defendants that pass through bond court. This paper helps evaluate the ways in which policies like bail that should have no bearing on the distribution of justice can actually impact the likelihood of guilty verdicts for defendants. Additionally, it breaks down how these potentially negative externalities are more pronounced for black defendants. By examining how judge biases can affect their leniency towards defendants, we also highlight the need for research in the field of criminal justice economics to be cautious with its assumptions underlying instrument construction.

We also help shine a light on how monetary bail differentially affects racial groups, most prominently, placing black defendants into detention at significantly higher rates than their white and hispanic counterparts. Once detained, all defendants are subjugated to the negative and coercive effects of detention which we observe to be highly correlated with the uptake of guilty pleas. Further work needs to be done to understand the mental burden jail-time has on defendants as well as how it affects their bargaining power, especially with respect to other state imposed constraints, such as mandatory minimums.

9 References

Abrams, D. S., Bertrand, M., & Mullainathan, S. (2012). Do Judges Vary in Their Treatment of Race? *The Journal of Legal Studies*, *41*(2), 347–383. https://doi.org/10.1086/666006

Agarwal, Sumit, Souphala Chomsisengphet, Johannes Stroebel, and Neale Mahoney. 2017. "Do Banks Pass Through Credit Expansions to Consumers Who Want to Borrow?" Quarterly Journal of Economics Forthcoming.

Alexander, Michelle, 2012. The New Jim Crow: Mass Incarceration in the Age of Colorblindness. The New Press, New York

Andreoni, J. (1991). Reasonable Doubt and the Optimal Magnitude of Fines: Should the Penalty Fit the Crime? *The RAND Journal of Economics*, *22*(3), 385–395. https://doi.org/10.2307/2601054

Andresen, Martin E., & Huber, Martin, Instrument-based estimation with binarised treatments: issues and tests for the exclusion restriction, *The Econometrics Journal*, Volume 24, Issue 3, September 2021, Pages 536–558, https://doi.org/10.1093/ectj/utab002

Aneja, A.P., & Avenancio-León, C.F. (2019). No Credit For Time Served? Incarceration and Credit-Driven Crime Cycles∗.

Apel, R., & Powell, K. (2019). Level of Criminal Justice Contact and Early Adult Wage Inequality. *RSF: The Russell Sage Foundation Journal of the Social Sciences*, *5*(1), 198–222. https://doi.org/10.7758/rsf.2019.5.1.09

David Arnold, Will Dobbie, Crystal S Yang, Racial Bias in Bail Decisions, *The Quarterly Journal of Economics*, Volume 133, Issue 4, November 2018, Pages 1885–1932, https://doi.org/10.1093/qje/qjy012

Carolina Arteaga. The cost of bad parents: Evidence from the effects of parental incarceration on children's education. *Working paper*, 2019.

Ba, Bocar A. and Bayer, Patrick J. and Rim, Nayoung and Rivera, Roman and Sidibe, Modibo, Police Officer Assignment and Neighborhood Crime (September 2021). Available at SSRN: https://ssrn.com/abstract=3922517

Bail and bonds. Justia. (2021, October 15).

Patrick Bayer, Randi Hjalmarsson, David Pozen, Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections, *The Quarterly Journal of Economics*, Volume 124, Issue 1, February 2009, Pages 105–147, https://doi.org/10.1162/qjec.2009.124.1.105

BECKETT, K., & WESTERN, B. (2001). Governing Social Marginality: Welfare, Incarceration, and the Transformation of State Policy. Organization, 3(1), 3–7. https://doi.org/10.1177/1350508420966740

Billings, Stephen B., David J. Deming, and Stephen L. Ross. 2019. "Partners in Crime." *American Economic Journal: Applied Economics*, 11 (1): 126-50. DOI: 10.1257/app.20170249

Bipartisan support for Criminal Justice Reform still strong. Equal Justice Initiative. (2019, October 16).

Brian Bell, Anna Bindler, and Stephen Machin. Crime scars: Recessions and the making of career criminals. *The Review of Economics and Statistics*, 100(3):392–404, 2018.

Carson, E. Ann. (2019), Prisoners in 2019, Technical report, Bureau of Justice Statistics.

Chetty, Raj, Nathaniel Hendren, and Lawrence Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Project." American Economic Review 106 (4).

Rosa Minhyo Cho. The impact of maternal imprisonment on children's educational achievement: Results from children in chicago public schools. *The Journal of Human Resources*, 44(3):772–797, 2009b. ISSN 0022166X.

Manudeep Bhuller, Gordon Dahl, Katrine Loken, and Magne Mogstad. Incarceration spillovers in criminal and family networks. *Working paper*, 2018.

Czafit, B., Köllő, J. Employment and wages before and after incarceration – evidence from Hungary. *IZA J Labor Stud* 4, 21 (2015). https://doi.org/10.1186/s40174-015-0044-z

Criminal justice facts. The Sentencing Project. (2021, June 3).

Anna Piil Damm and Christian Dustmann. Does growing up in a high crime neighborhood affect youth criminal behavior? *American Economic Review*, 104(6):1806–32, June 2014.

Darity, S. (2005). Stratification economics: The role of intergroup inequality. Journal of Economics and Finance, 29 (2), 144–153.

Darity, S., Hamilton, D., & Stewart, J. (2015). A tour de force in understanding intergroup inequality: An introduction to stratification economics. Review of Black Political Economy, 42 (1–2), 1–6.

Denis Fougere, Francis Kramarz, and Julien Pouget. Youth unemployment and crime in france. *Journal of the European Economic Association*, 7(5):909–938, 2009.

Dobbie, Will, Jacob Goldin, and Crystal S. Yang. 2018. "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review*, 108 (2): 201-40.

Dobbie, W., & Yang, C. S. (2021, March 25). *The economic costs of pretrial detention*. Brookings.

Galinato, Gregmar & Rohla, Ryne, 2018. "Do Privately-Owned Prisons Increase Incarceration Rates?," Working Papers 2018-6, School of Economic Sciences, Washington State University.

Amanda Geller & Irwin Garfinkel & Bruce Western, 2006. "The Effects of Incarceration on Employment and Wages An Analysis of the Fragile Families Survey ," Working Papers 932, Princeton University, School of Public and International Affairs, Center for Research on Child Wellbeing

General order no. 18.8A - procedures for bail hearings and pretrial release. Illinois Circuit Court of Cook County. (n.d.). Retrieved April 8, 2022

Greenberg, D. F., & West, V. (2001). State Prison Populations and Their Growth, 1971-1991. *Criminology, 39(3),* 615-654.

David J Harding, Jonah A Siegel, Jeffrey D Morenoff, Custodial Parole Sanctions and Earnings after Release from Prison, *Social Forces*, Volume 96, Issue 2, December 2017, Pages 909–934, https://doi.org/10.1093/sf/sox047

Heumann, M., & Loftin, C. (1979). Mandatory sentencing and the abolition of plea bargaining: The Michigan felony firearm statute. *Law and Society Review*, 393-430.

Imbens, G. W., & Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, *62*(2), 467–475. https://doi.org/10.2307/2951620

Imbens G, Angrist J, Rubin D. Identification of Causal effects Using Instrumental Variables. Journal of Econometrics. 1996;71 (1-2) :145-160.

Jordan, Andrew and Karger, Ezra and Neal, Derek Allen, Heterogeneous Impacts of Sentencing Decisions (October 26, 2021). University of Chicago, Becker Friedman Institute for Economics Working Paper No. 2021-113, Available at SSRN: https://ssrn.com/abstract=3927995 or http://dx.doi.org/10.2139/ssrn.3927995

Kang, J., Bennett, M., Carbado, D., Casey, P., & Levinson, J. (2011). Implicit bias in the courtroom. *UCLa L. rev.*, *59*, 1124.

Lawrence F. Katz, Jeffrey R. Kling, Jeffrey B. Liebman, Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment, *The Quarterly Journal of Economics*, Volume 116, Issue 2, May 2001, Pages 607–654, https://doi.org/10.1162/00335530151144113

Jeffrey R. Kling, Jens Ludwig, Lawrence F. Katz, Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment, *The*

Quarterly Journal of Economics, Volume 120, Issue 1, February 2005, Pages 87–130, https://doi.org/10.1162/0033553053327470

Lum Kristian, Swarup Samarth, Eubank Stephen, and Hawdon James. 2014. The contagious nature of imprisonment: an agent-based model to explain racial disparities in incarceration rates*J. R. Soc. Interface.*112014040920140409. http://doi.org/10.1098/rsif.2014.0409

Lochner, Lance, and Enrico Moretti. 2004. "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports." *American Economic Review*, 94 (1): 155-189.

Looney, A., & Turner, N. (2018, March 14). *Work and opportunity before and after incarceration*. Brookings.

Jens Ludwig, Greg Duncan, and Paul Hirschfield. Urban poverty and juvenile crime: Evidence from a randomized housing-mobility experiment. *The Quarterly Journal of Economics*, 116(2): 655–679, 2001.

Lyons, C. J., & Pettit, B. (2011). Compounded Disadvantage: Race, Incarceration, and Wage Growth. *Social Problems*, *58*(2), 257–280. https://doi.org/10.1525/sp.2011.58.2.257

Maroto, M.L. The Absorbing Status of Incarceration and its Relationship with Wealth Accumulation. *J Quant Criminol* 31, 207–236 (2015). https://doi.org/10.1007/s10940-014-9231-8

Johnston, D. C. (2008, August 25). *Average U.S. income showed first rise over 2000*. The New York Times. Retrieved April 8, 2022

Joseph Murray and David P. Farrington. Parental imprisonment: effects on boys' antisocial behaviour and delinquency through the life-course. *Journal of Child Psychology and "Psychiatry*, 46 (12):1269–1278, 2005.

Justice Policy Institute. (n.d.). Retrieved April 8, 2022, from https://justicepolicy.org/

Nagin, D., & Waldfogel, J. (1998). The effect of conviction on income through the life cycle. *International Review of Law and Economics*, *18*(1), 25-40. https://doi.org/10.1016/S0144-8188(97)00055-0

Owens, Emily, and Bocar Ba. 2021. "The Economics of Policing and Public Safety." *Journal of Economic Perspectives*, 35 (4): 3-28. DOI: 10.1257/jep.35.4.3

Petach, L., & Pena, A. A. (2021). Local Labor Market Inequality in the Age of Mass Incarceration. The Review of Black Political Economy, 48(1), 7–41. https://doi.org/10.1177/0034644620966029

Piller C. Gaming the system. Science. 2020 Jan 17;367(6475):243. doi: 10.1126/science.367.6475.243. PMID: 31949064.

Release or detention of a defendant pending trial 18 U.S.C. § 3142. https://www.law.cornell.edu/uscode/text/18/3142

Schneider D, Turney K. Incarceration and Black-White inequality in Homeownership: A state-level analysis. Soc Sci Res. 2015 Sep;53:403-14. doi: 10.1016/j.ssresearch.2015.06.007. Epub 2015 Jun 10. PMID: 26188463.

Smith, K.B. (2004), The Politics of Punishment: Evaluating Political Explanations of Incarceration Rates. Journal of Politics, 66: 925-938. https://doi.org/10.1111/j.1468-2508.2004.00283.x

Starr, Sonja B. "Racial Disparity in Federal Criminal Sentences." M. M. Rehavi, co-author. J. Pol. Econ. 122, no. 6 (2014): 1320-54.

Stevenson, Megan. (2018). Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes. Journal of Law, Economics, and Organization. 34. 511-542. 10.1093/jleo/ewy019.

Bryan L. Sykes, & Michelle Maroto. (2016). A Wealth of Inequalities: Mass Incarceration, Employment, and Racial Disparities in U.S. Household Wealth, 1996 to 2011. *RSF: The Russell Sage Foundation Journal of the Social Sciences*, *2*(6), 129–152. https://doi.org/10.7758/rsf.2016.2.6.07

M. Szmigiera, & 30, J. (2021, July 30). *Countries with the most prisoners 2021*. Statista.

Vallas, R., & Dietrich, S. (2014, April 26). *One strike and you're out*. Center for American Progress.

Western B, Pettit B. Incarceration and social inequality. Daedalus. 2010;139(3):8-19. doi: 10.1162/daed_a_00019. PMID: 21032946.

Christopher Wildeman, Signe Hald Andersen, Hedwig Lee, and Kristian Bernt Karlson. Parental incarceration and child mortality in denmark. *American Journal of Public Health*, 104(3):428–433, 2014.

Christopher Wildeman. Paternal Incarceration and Children's Physically Aggressive Behaviors: Evidence from the Fragile Families and Child Wellbeing Study. *Social Forces*, 89(1):285–309, 09 2010.

What are bond conditions? Martinez Bail Bonds. (2021, May 20).

Wildeman, Christopher & Muller, Christopher. (2012). Mass Imprisonment and Inequality in Health and Family Life. Annual Review of Law and Social Science. 8. 11-30. 10.1146/annurev-lawsocsci-102510-105459.

10 Appendix

A. Robustness Checks

A.1 Impacts of Binarized Release

As we discussed earlier, the binarization of our release outcome might lead to a violation of the exclusion restriction for instruments. Additionally, beyond just violating exclusion, having a qualitatively justified but relatively arbitrary cutoff for our binary outcome could have further impacts down the line. In order to test if the 3 day cutoff used in this paper and others in the field is a valid one, we conduct a sensitivity analysis, varying the release cutoff between 1 day and 14 days. Since our case outcome results show that they are primarily driven by guilty pleas, we construct confidence intervals for various treatment effects on this outcome for each of our 14 instruments and display the results in Figure A1. As can be seen, past the first day threshold, changes in magnitude and significance are marginal, and even between the first and second day,

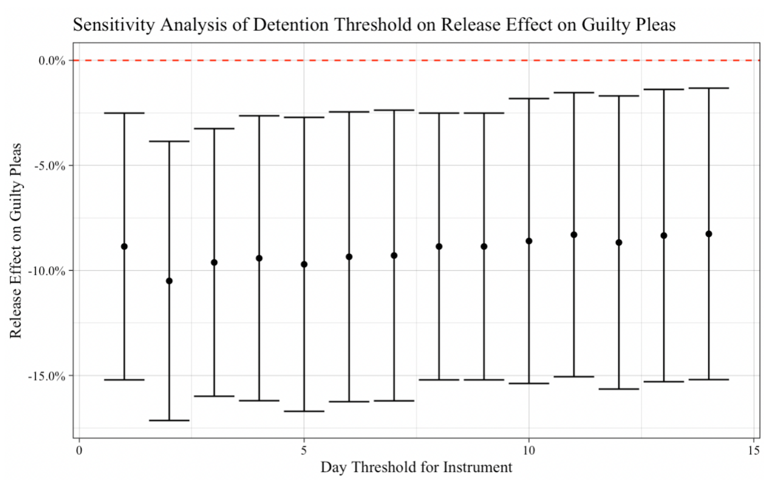

Figure A1: Sensitivity Analysis on Binarized Release

there is only a marginal difference. Hence, our sensitivity analysis at least confirms that the threshold for the binarization of our instrument has no measurable impact on the treatment of release on guilty pleas. Despite these results, we acknowledge that implicitly by the work done in Andresen and Huber (2021), our use of a binarized outcome might still contribute to the violation of the exclusion restriction on our instrument.

A.2 Impacts of Unmatched Jail Data

The other assumption we impose when constructing our measure of binarized release is that if a defendant is unmatched to jail data but has been given an I-bond or an EM-bond, then that individual is counted as a released individual. This is in large part a reasonable assumption, as these bonds indicate that a defendant would be released anyway, so even though all defendants are expected to be booked for some amount of time, perhaps if this time is small enough, it is not kept in the books. A valid critique, however, is that our decision to include only some of the unmatched defendants, even if by valid assumption, distorts our results. In response, we compute two other treatment effects of release on guilty pleas: the first of which we count all defendants missing from the jail data as NAs, the second of which we assume all defendants missing from

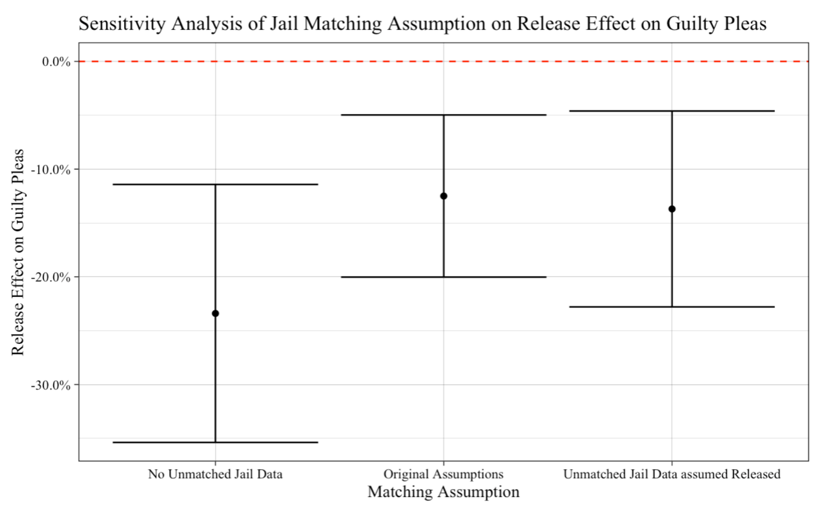

Figure A2: Sensitivity Analysis on Matching Assumption the jail data are released. Note, the second assumption is not unreasonable either, as one might expect if a defendant pays off a D-bond very quickly, they might also end up unmatched to the jail data.

We can see the results of this sensitivity analysis in Figure A2. As we can see, our original assumption yields a different treatment effect on the uptake of guilty pleas from when we drop all unmatched individuals from the jail data. Alternatively, our original assumption is indeed very similar to when we assume that all unmatched individuals from the jail data are released. However, we note that there is greater variability in this subset of the data, despite having more observations and power.

All the results of this sensitivity analysis make sense when we consider how we are changing the data. Given that we *know* receiving an I-bond or EM-bond results in the release of a defendant, if we intentionally omit these observations on the basis of (reasonably) not finding them within the jail data, we are essentially omitting instances of judges being more lenient on their defendants. Hence, we are left with more instances of harsher bail judges, and so the positive impacts of release are amplified. On the other hand, when we assume that all unmatched jail data relates to released individuals, we are generally including more instances of judges being lenient while mislabeling some unknown percentage of cases. The result is that the treatment size actually stays the same, however its variability increases despite the higher number of observations, likely due to the mislabeling.

We take this sensitivity analysis as indication that our initial assumptions are the optimal set of assumptions we could have made regarding unmatched defendants. Knowing the mechanics of how EM and I-bonds work allow us to discard the results associated with dropping their unmatched instances with jail data. Dropping the rest of the unmatched jail data, although it makes us lose power and likely drop many more instances of judges being on the more lenient end, allows us to err on the side of caution with the size of the treatment effect, effectively providing a lower bound on its impact.

A.3 Jackknife Instrument Design Choice

As we mentioned previously, the specific jackknife instrument we used, following most of the literature, might itself have an impact on the results we find. Jackknife estimators are fundamentally best used in contexts estimating the biases of large populations, which is why they have been used previously in studies where there are large numbers of bail judges relative to defendants. However, in our specific context, since we are dealing with only 12 judges, the jackknife might not be the best instrument we can use. It is still justifiable because it allows for judge variation over time and because defendants can reencounter judges later, which the leave-out mean helps as account for. However, as a robustness check, we employ judge-year fixed effects as our instrument and present our findings.

First, we verify in Table A1 that the judge-year fixed effects are randomly determined by defendant characteristics with respect to release. Second, we observe in Table A2 that this instrument is a relevant and strong predictor of pretrial release. Third, Figure A3 shows us that our release appears to be monotonically increasing in judge-year fixed effects. We maintain that exclusion should hold for this instrument for the same reasons that it might hold for the jackknife instrument.

We display the results of this new instrument in Table A3. As can be seen, no release coefficients changes in significance levels. All the magnitudes of the coefficients on release remain very similar to those calculated with the jackknife instrument. Hence, we have found that our results are robust with respect to an entirely different instrument construction, strengthening the validity of our hypothesis.

Table A1: Test of Randomization Judge-Year Fixed Effects

	(1) Pretrial Release	(2) Judge Leniency
Male	-0.0829***	-0.0000832
	(0.00333)	(0.000231)
Black	-0.110***	-0.000604*
	(0.00379)	(0.000261)
Hispanic	-0.0496***	-0.000368
	(0.00460)	(0.000312)
[18,24) Years Old	0.0261***	0.000547*
	(0.00315)	(0.000229)
[31,43) Years Old	-0.0783***	-0.000444*
	(0.00304)	(0.000223)
43+ Years Old	-0.0737***	-0.000184
	(0.00311)	(0.000229)
Case in the Last Year	-0.0926***	0.000262
	(0.00231)	(0.000174)
Over 2 Charges	-0.0570***	-0.0000989
	(0.00301)	(0.000225)
1 to 2 Past FTAs	-0.110***	-0.000396*
	(0.00266)	(0.000193)
Over 2 Past FTAs	-0.166***	-0.000282
	(0.00277)	(0.000224)
Only Misdemeanor Charges	0.142***	0.0000970
	(0.00574)	(0.000422)
Any Past Guilty Felonies	-0.169***	-0.000313
	(0.00209)	(0.000195)
Any Felony Property Charge	-0.0779***	0.000220
	(0.00465)	(0.000393)
Any Violent Felony Charge	-0.150***	-0.000283
	(0.00491)	(0.000476)
Any Drug Felony	0.0477***	0.0000973
	(0.00429)	(0.000320)
Any Felony Murder/Sex Charges	-0.208***	0.00146
	(0.0119)	(0.00149)
Any Felony Weapon Charges	-0.0839***	0.000209
	(0.00520)	(0.000435)
Any Other Felony Charge	-0.00759	-0.000429
	(0.00472)	(0.000354)
Any Misdemeanor Property Charge	-0.0610***	0.00102*
	(0.00629)	(0.000505)
Any Misdemeanor Drug Possession Charge	0.0772***	0.000629*
	(0.00420)	(0.000306)
Any Misdemeanor Weapon Charges	-0.00686	-0.000850
	(0.00736)	(0.000561)
Any Misdemeanor Domestic Violence Charges	-0.103***	0.000422
	(0.00592)	(0.000422)
Any Violent Misdemeanor Charge	-0.0333***	0.000270
	(0.00628)	(0.000490)
Any Other Misdemeanor Charge	0.0340***	-0.000205
	(0.00355)	(0.000270)
Any Traffic Charge	0.0909***	0.000694*
	(0.00431)	(0.000300)
Observations	153524	153524
Adjusted R^2	0.175	0.524
F	1627.0	2.823

se in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: First Stage Judge-Year Fixed Effects

	(1) OLS	(2) OLS Controls	(3) OLS Controls FE
Judge Leniency	0.751***	0.663***	0.936***
	(0.0269)	(0.0244)	(0.0352)
Controls	No	Yes	Yes
Observations	153524	153524	153524
R^2	0.005	0.177	0.180
Adjusted R^2	0.005	0.177	0.179
F	781.7	1661.3	1597.5

se in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

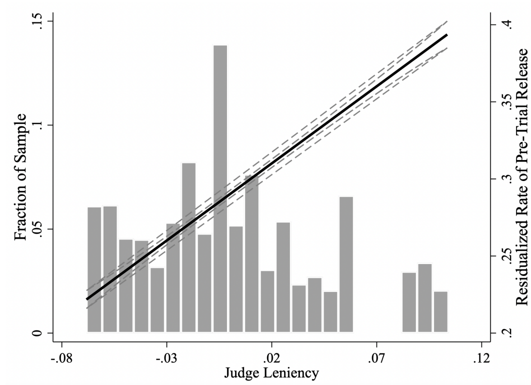

Figure A3: Judge-Year Fixed Effect Instrument First Stage and Distribution

Table A3: Outcomes Using Judge-Year Fixed Effect Instrument

Outcome of Interest	(1) Detained Outcome Mean	(2) OLS FE Release	(3) OLS Controls FE Release	(4) IV FE Release	(5) IV Controls FE Release
Any Guilty Verdict	0.538	-0.257***	-0.0982***	-0.123**	-0.0913**
	(0.499)	(0.00262)	(0.00242)	(0.0412)	(0.0332)
Any Guilty Plea	0.522	-0.251***	-0.108***	-0.125**	-0.0987**
	(0.500)	(0.00260)	(0.00246)	(0.0410)	(0.0346)
Any FTA	0.0353	0.0773***	0.0746***	0.175***	0.174***
	(0.185)	(0.00160)	(0.00180)	(0.0198)	(0.0211)
Any New Case Before Final Disposition	0.116	0.0338***	0.0610***	0.125***	0.116***
	(0.320)	(0.00195)	(0.00212)	(0.0281)	(0.0291)
Any New Case Post Final Disposition (0-2 yrs)	0.418	-0.112***	-0.0697***	0.0172	0.0206
	(0.493)	(0.00266)	(0.00269)	(0.0410)	(0.0401)
Any Incarceration	0.407	- -0.303***	-0.125***	-0.105**	-0.0460
	(0.491)	(0.00208)	(0.00204)	(0.0383)	(0.0320)
Controls	No	No	Yes	No	Yes
Observations	153524	153524	153524	153524	153524
Adjusted R^2	0.012	0.059	0.457	0.041	0.454
F	546.8	9584.2	12260.1	7.848	4842.4

All regressions use heteroskedasticity resistant standard errors clustered at the individual-judge level reported in parenthesis. All coefficients reported are for pretrial release. We display the detained mean for each outcome to calculate the difference release creates from detained defendants
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$