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Pretrial Disparity and the Consequences of Money Bail

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PRETRIAL DISPARITY AND THE CONSEQUENCES OF MONEY BAIL

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Catalyzed by the Black Lives Matter protests in 2020, support for criminal justice reform in the United States has become a groundswell, with reformers demanding an end to racial and socioeconomic disparities in all aspects of policing, prosecution, adjudication, and incarceration. While high-profile cases of police misconduct during arrest remain in the limelight, a growing and politically diverse chorus of voices is calling for change at the first point of contact between a defendant and the court system: the bail hearing. Bail decisions are highly consequential in terms of their scale and impact on the lives of defendants, their families, and the community. Yet, to date we only have incomplete evidence of disparities in bail decisions and their consequences. Without an adequate empirical picture of how bail decisions are actually made—and how state and local regimes differ in structure and constituency—even sincere efforts at change will likely be short-lived and unsuccessful. Critically, we do not know enough about how judges evaluate whether to release a defendant prior to trial, and we have little reliable information about how the decisions made regarding pretrial release affect case outcomes and the long-term behavior of the defendants. The little data we have is either taken exclusively from or skewed heavily toward large urban areas. Without more comprehensive accounts to undergird reform, even sincere efforts at change are unlikely to yield lasting success.

This Article helps to fill this gap through an empirical analysis of more than 23,000 misdemeanor bail decisions from a mixed rural and suburban county in Arizona. Compared to the results of recent studies in large

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metropolitan cities, our findings are striking. We show that despite having virtually identical caseloads, the most "lenient" judges assign money bail in only 20% of their cases with average bail amounts of only $200, whereas the "strictest" judges assign money bail in nearly 60% of cases at an average of $2,500 per bail assignment (three and thirteen times higher, respectively). This variability has a racial component as well, with some judges as much as 12 percentage points more likely to assign money bail to Black defendants in comparison to their white counterparts. And the impacts of these disparities are not limited to the outcome of bail hearings: Downstream analysis shows that these judicial disparities have effects on the future behavior of defendants. Taken together, our findings suggest that for bail reform to be something more than merely cosmetic, reformers must take aim at high levels of judicial disparity. At the same time, reformers should be wary of one-size-fits-all policy prescriptions that purport to provide unilateral remedies to all of the country's multifarious bail problems, instead recognizing that the specific relationships between courts and institutions, criminal defendants, and the broader community likely depend on the underlying characteristics of those systems and communities.

INTRODUCTION

The pretrial criminal justice system in the United States is broken. The most recent statistics by the U.S. Department of Justice show that two out of every three inmates held in county and city jails have not yet been tried for their purported crimes.[1] By these same measures, the United States is detaining around half a million pretrial defendants,[2] which, *taken alone*, would rank the U.S. prison population as fourth in the world (behind China, Russia, and the United States' post-adjudication detainees).[3] And recent data show that the vast majority of these pretrial detainees would be released but for their inability to meet money bail; for example, a 2013 report found that nearly 90% of defendants in New York's pretrial system were unable to afford a bail of just $1,000.[4]

These statistics are compelling enough on their own, but when combined with the advent of the coronavirus ("COVID-19") pandemic and the groundswell of support for the Black Lives Matter ("BLM") movement, the appetite for large-scale bail and pretrial detention reform has reached a fever pitch. After finding that the COVID-19 infection rate in some jails was

1. ZHEN ZENG, BUREAU OF JUST. STAT., U.S. DEP'T OF JUST., NCJ 251774, JAIL INMATES IN 2017, at 1, 5 (2019) [hereinafter JAIL INMATES IN 2017]; *see also* TODD D. MINTON & ZHEN ZENG, BUREAU OF JUST. STAT., DEP'T OF JUST., NCJ 248629, JAIL INMATES AT MIDYEAR 2014, at 4 (2015) (for data on jail inmates in the United States from 2000 to 2014).

2. JAIL INMATES IN 2017, *supra* note 1, at 1, 5.

3. *World Prison Populations*, BBC NEWS, http://news.bbc.co.uk/2/shared/spl/hi/uk/06/prisons/html/nn2page1.stm (last visited Oct. 11, 2021).

4. *See* HUM. RTS. WATCH, THE PRICE OF FREEDOM: BAIL AND PRETRIAL DETENTION OF LOW INCOME NONFELONY DEFENDANTS IN NEW YORK CITY 2 (2010) (reporting that "[i]n 87 percent of the cases (16,649) in which the defendants arrested in 2008 had bail set at $1,000 or less, the defendants were not able to post bail at their arraignment and were incarcerated pending trial").

eighty-seven times higher than the national average, health professionals called for the release of any pretrial detainees held on misdemeanor charges, parole, or probation violations[5]—a recommendation that many jails have been compelled to follow due to already underfunded and overcrowded facilities in which social distancing is virtually impossible.[6] And although the focus of the BLM movement has centered on the interactions between the police and arrestees, a growing number of voices are turning their attention to bail hearings,[7] recognizing that the first meaningful interaction between defendants and the court system is a potential lynchpin for racial and socioeconomic disparities both in the judge's bail determination[8] and in disproportionate downstream effects.[9]

To highlight how problematic the current system truly is, consider the following scenario from the data used in this Article.[10] John and James are alike in just about every way: both are in their thirties and are Hispanic/Latinx males, neither have ever been convicted of a crime, both live in Tucson, Arizona, and both were recently arrested for shoplifting. Despite these

5. Letter from Drug Pol'y All. to the Ctrs. for Disease Control and Prevention (Apr. 9, 2020), https://drugpolicy.org/sites/default/files/cdc-letter-decarceration_0.pdf; *see also* LEGAL AID SOC'Y, ANALYSIS OF COVID-19 INFECTION RATE IN NYC JAILS (2020); Letter from ACLU to State and Local Officials (Mar. 16, 2020), https://www.aclu.org/sites/default/files/field_document/aclu_coronavirus_criminal_justice_-_states.pdf; Marcella Alsan & Crystal S. Yang, Nat'l Comm'n on Corr. Health Care, *NCCHC-HU COVID-19 Survey of Correctional Facilities: Weekly Report* (June 9, 2020), https://www.ncchc.org/filebin/COVID/COVID_NCCHC-HU_WeeklySummary_6.9.20_002.pdf.

6. A prominent example is the emergency amendment that the California Judicial Council adopted in June of 2020, which reduced bail to $0 for misdemeanors and low-level felonies (the amendment has since been repealed at the state level, but many California counties have elected to keep the emergency provisions in place. CAL. R. CT. EMERGENCY RULE 4 EMERGENCY BAIL SCHEDULE (2020) (repealed June 20, 2020); *see also The Most Significant Criminal Justice Policy Changes from the COVID-19 Pandemic*, PRISON POL'Y INITIATIVE (Dec. 23, 2021), https://www.prisonpolicy.org/virus/virusresponse.html [https://web.archive.org/web/20211224004530/https://www.prisonpolicy.org/virus/virusresponse.html] (for updated tracking on pretrial release policies related to COVID-19).

7. Much of the recent energy here has been focused on either promoting bail funds (which pay the bail for criminal defendants) or the abolishment of bail entirely. *See, e.g.*, Jia Tolentino, *Where Bail Funds Go from Here*, NEW YORKER (June 23, 2020), https://www.newyorker.com/news/annals-of-activism/where-bail-funds-go-from-here (for a recent discussion of bail funds); Shane Goldmacher, *Racial Justice Groups Flooded with Millions in Donations in Wake of Floyd Death*, N.Y. TIMES (June 16, 2020), https://www.nytimes.com/2020/06/14/us/politics/black-lives-matter-racism-donations.html; *End to Pretrial Detention and Money Bail*, MOVEMENT FOR BLACK LIVES, https://m4bl.org/policy-platforms/end-pretrial-and-money-bail/ (last visited Oct. 11, 2021).

8. See *infra* note 111 for examples of empirical studies exploring the considerations that judges make in setting bail.

9. See *infra* note 193 for examples of existing empirical studies that identify the downstream effect of bail on crucial issues such as access to counsel, recidivism, employment, and housing prospects.

10. The defendant and judge names have been changed to maintain anonymity.

similarities, John was released from jail and is able to prepare for his trial at home with his family, while James was assigned a bail of $2,000 and will likely remain in jail leading up to his trial date. Why the difference? John was arrested on a Sunday and was assigned to Judge Smith, whereas James was arrested four days later on Wednesday and was assigned to Judge Miller, who is more than twice as likely to assign money bail than Judge Smith. It could have been even worse—had he been arrested on that Tuesday, James's pretrial hearing would have been assigned to Judge Perry, who is three times as punitive as Judge Smith. And if James were Black, these discrepancies could have been even more severe.[11] Same court, same crime, same demographics, different judges.

Further, the consequence of these disparities might not be limited to whether or not John and James are detained before trial. Does the assignment (or not) of money bail affect the likelihood that a defendant will appear for his court hearings? Will defendants detained before trial be more likely to plead guilty or more likely to even be found guilty at trial? And might that one difference between John and James—the judge assigned for their bail hearing—even affect the likelihood that one or the other will commit additional crimes in the future? What are the implications for legal reform that will reduce these disparities and improve long-term outcomes for defendants and society?

Even before the events of 2020 magnified the importance of addressing these questions, scholars and policymakers recognized that the United States was coming into what many have called the third great wave of American bail reform.[12] Bail reform has become a buzzword at the lips of everyone from Democratic Party presidential hopefuls in the 2020 election[13] to conservative think tanks.[14] A critical mass of the legal community understands the need for reform and, just as importantly, policymakers appear to be motivated enough to actually enact change: In 2018 alone,

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11. For a visual representation of these disparities, see *infra* Part III, Figures 1, 2, 3, & 4.

12. *See, e.g.*, Sandra G. Mayson, *Dangerous Defendants*, 127 YALE L.J. 490 (2018); Alexa Van Brunt & Locke E. Bowman, *Toward a Just Model of Pretrial Release: A History of Bail Reform and a Prescription for What's Next*, 108 J. CRIM. L. & CRIMINOLOGY 701, 701 (2019) ("The criminal justice system is in the midst of the 'third wave' of bail reform in the United States."); Jeffrey J. Clayton, *The Third Generation of Bail Reform in America: Is Third Time the Charm?*, @LAW NALS MAG. FOR LEGAL PROS. Winter 2018, at 20, 20–21 (referring to the recent reforms as the "third generation").

13. Both leading candidates for the 2020 Democratic presidential nomination, Joe Biden and Bernie Sanders, even made the abolition of money bail one of the pillars of their criminal justice reform platforms. *See infra* note 90 and accompanying text.

14. *See, e.g.*, RAFAEL A. MANGUAL, MANHATTAN INST., REFORMING NEW YORK'S BAIL REFORM: A PUBLIC SAFETY-MINDED PROPOSAL (2020); *see also* Charles Fain Lehman, *Think Tank Floats Fix for New York's Disastrous Bail Reform*, WASH. FREE BEACON (Mar. 5, 2020, 4:59 AM), https://freebeacon.com/issues/think-tank-floats-fix-for-new-yorks-disastrous-bail-reform/ (reporting on the reforms proposed by the Manhattan Institute).

twenty-four states passed legislation on pretrial justice reform.[15] A number of states have implemented new court rules or policies that explicitly allow judges to account for the financial circumstances of the defendants in setting money bail.[16] Others have suggested that judicial discretion should be minimized using bail "schedules"[17] or algorithms to determine release.[18] And, spurned in large part by the tragic impact of COVID-19 on already overcrowded jails, a growing number of states have even pursued policies that would abolish money bail and pretrial detention altogether.[19] This collective momentum is undeniably exciting and encouraging.

But all of this happened before. Twice. Galvanized by the growing concern that too many defendants were being detained before trial, the U.S. federal criminal justice system began to enact large-scale bail reforms in the middle of the twentieth century, hallmarked by the Bail Reform Act of 1966.[20] Under this "new" system, judges were to use their discretion to

15. PRETRIAL JUST. INST., WHERE PRETRIAL IMPROVEMENTS ARE HAPPENING 1 (2019), https://www.prisonpolicy.org/scans/pji/where_pretrial_improvements_are_happening_jan2019.pdf ("In 2018, 24 states passed legislation relating to pretrial justice, including a major reform bill (SB 10) in the nation's most populous state, California.").

16. *See, e.g.*, MD. R. 4-216.1(e)(1) (effective July 1, 2017) ("A judicial officer may not impose a special condition of release with financial terms in form or amount that results in the pretrial detention of the defendant solely because the defendant is financially incapable of meeting that condition."); Memorandum from N.J. Att'y Gen. Christopher S. Porrino to Dir., Div. of Crim. Justice et. al 54–55 (May 24, 2017), https://www.state.nj.us/lps/dcj/agguide/directives/ag-directive-2016-6_v2-0.pdf (citing the New Jersey 2014 Criminal Justice Reform Act, which stipulates a presumption against money bail unless "the defendant is reasonably believed to have financial assets that will allow him or her to post monetary bail in the amount requested by the prosecutor without having to purchase a bond from a surety company or to obtain a loan").

17. For a discussion of the most prominent of these tools, the Laura and John Arnold Foundation's Public Safety Assessment tool, see *About the Public Safety Assessment*, ADVANCING PRETRIAL POL'Y & RSCH., https://advancingpretrial.org/psa/about/ (last visited Oct. 12, 2021) (see the webpage's "PSA Sites" tab, documenting that, as of April 2020, jurisdictions in 19 states use the Arnold Foundation PSA—including Pima County, the venue for this Article's empirical study).

18. For a discussion of the role of machine learning and artificial intelligence in determining release, see Shara Tonn, *Can AI Help Judges Make the Bail System Fairer and Safer?*, STAN. ENG'G (Mar. 19, 2019), https://engineering.stanford.edu/magazine/article/can-ai-help-judges-make-bail-system-fairer-and-safer.

19. *See, e.g.*, S.B. 10, 2017–2018 Reg. Sess. (Cal. 2018) (California's recent legislation banning money bail); *see also* James C. McKinley, Jr., *Cuomo, in Bid to Help Poor, Proposes Ending Cash Bail for Minor Crimes*, N.Y. TIMES (Jan. 2, 2018), https://www.nytimes.com/2018/01/02/nyregion/cuomo-ending-cash-bail-state-of-the-state.html (Governor Cuomo's bid to eliminate money bail for certain crimes in New York). Additionally, Alaska, Illinois, New Jersey, and New Mexico all ended money bail in recent years. Morgan Baskin, *Alaska Ends Cash Bail System*, PAC. STANDARD (Jan. 2, 2018), https://psmag.com/news/alaska-ends-cash-bail; Maria Cramer, *Illinois Becomes First State to Eliminate Cash Bail*, N.Y. TIMES (Feb. 23, 2021), https://www.nytimes.com/2021/02/23/us/illinois-cash-bail-pritzker.html; Anita Hassan, *New Mexico Eliminated Cash Bail – But Now One County Locks Up More People Without Bond Before Trial*, NBC NEWS (Dec. 8, 2020, 5:01 AM), https://www.nbcnews.com/news/us-news/new-mexico-eliminated-cash-bail-now-one-county-locks-more-n1250257.

20. Bail Reform Act of 1966, Pub. L. No. 89-465, 80 Stat. 214.

ensure that all but the most flight-prone of defendants were provided an opportunity for pretrial release. However, within just a few decades, judges—assisted by a pair of U.S. Supreme Court cases[21]—used that discretion to detain defendants at higher rates than ever.[22] Cue the Bail Reform Act of 1984 and its wave of copycat state acts that responded to an escalating fear of violent crime in America.[23] This was the second—and arguably more informed and permanent—crack at "fixing" the pretrial detention system, but increased judicial discretion combined with a preoccupation with fighting crime has led to unprecedented growth in pretrial detainment and post-conviction incarceration.

As a result, the United States is hungry for criminal justice reform—possibly more than ever before—but at the same time, poised to make the sort of change that will only necessitate another movement twenty years down the road.[24] How do we make sure this third crack at bail reform sticks? The problem is not a lack of motivation; we simply do not have a systematic understanding of how the pretrial process works.[25] In particular, we do not know enough about how judicial discretion affects the decision to release a defendant before trial, and we do not adequately understand how the decisions made at the pretrial stage affect the outcomes of criminal cases and the long-term behavior of defendants.

This Article attempts to fill these information gaps by providing data critical for enacting informed and lasting bail reform. We do not purport to have discovered the national antidote for this country's pretrial woes—in fact, one of the themes we stress in this Article is that there is no one-size-fits-all approach that can be unilaterally applied to every pretrial regime in the United States. We do, however, provide novel empirical data on two key,

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21. Bell v. Wolfish, 441 U.S. 520 (1979) (ruling that certain conditions of pretrial detention such as double-bunking and restricting reading materials do not constitute punishment under the Fifth Amendment); Schall v. Martin, 467 U.S. 253 (1984) (ruling that pretrial detention of juvenile criminal defendants was constitutionally permissible because it was consistent with New York's interest in protecting the community from future crime).

22. SHIMA BARADARAN BAUGHMAN, THE BAIL BOOK: A COMPREHENSIVE LOOK AT BAIL IN AMERICA'S CRIMINAL JUSTICE SYSTEM 23 (2018) (observing that the first wave of bail reforms "[led] to more denials of bail, not less").

23. Joint Resolution Making Continuing Appropriations for the Fiscal Year 1985, and for Other Purposes, Pub. L. No. 98-473, 98 Stat. 1837 (1984).

24. Much of the frustration fueling the BLM movement appears to be the feeling that reform has become an empty promise because of how ineffectual previous attempts at change have been, especially in terms of core issues such as racial disparities in policing, prosecution, and judging.

25. *See* PRETRIAL JUST. INST., *supra* note 15 ("Passing legislation is just one step, of course; careful implementation is also needed to realize meaningful and long-lasting pretrial practices that honor fairness, justice, and public safety."); Brandon Buskey, *Wrestling With Risk: The Questions Beyond Money Bail*, 98 N.C. L. REV. 379, 379 (2020) ("[T]here is no consensus on *how* to reform systems that routinely detain people who cannot afford money bail. This is the discussion we must begin.").

outstanding questions in bail reform: (1) To what extent do judges vary in their pretrial decision-making within a given pretrial system; and (2) what are the downstream effects that money bail assignments have on the future behavior of defendants and the outcomes of their criminal cases? We answer these questions by using cutting-edge empirical techniques and a novel dataset with more than 23,000 misdemeanor cases from Pima County, Arizona (where Tucson is located). More specifically, we exploit the random assignment of bail judges in Pima County to misdemeanor pretrial hearings, which creates a "natural experiment" that can credibly measure the differences in how individual judges approach and rule on bail decisions. We then use those inter-judge disparities to tease out the true *causal* impacts of both the existence and amount of money bail on critical downstream outcomes such as the disposition of the broader criminal case and even the likelihood that the defendant will commit more crimes in the future.

We find that pretrial judges in Pima County vary remarkably in both the frequency with which they grant money bail and the average amount of bail assigned. Despite having statistically identical caseloads, the most "lenient" judges assign money bail in only 20% of their cases with average bail amounts of only \$200, whereas the most "strict" judges assign money bail in nearly 60% of their cases at an average of \$2,500 per bail assignment (three and thirteen times higher, respectively).[26] And while our study design does not allow us to identify intentional discrimination across racial or ethnic groups, our data show that some judges are systematically more or less lenient with Hispanic/Latinx and Black defendants.[27]

Furthermore, defendants assigned money bail are slightly more likely to plead guilty for at least one charge, a finding largely in line with common belief and prior empirical evidence. However, contrary to the results of recent studies in metropolitan areas, we find, at best, an unclear impact of money bail on the likelihood of failing to appear and guilty judgments.[28] We also see a statistically distinguishable *decrease* in recidivism (9.2 percentage points) in the six months immediately following the initial appearance of defendants who are assigned money bail.[29] The differences between our findings and those in similar studies suggest that the downstream effect of money bail varies across jurisdictions and that bail reformers should therefore be wary of one-size-fits-all policy prescriptions.[30]

One other noteworthy distinguishing factor about our study is that we focus specifically on misdemeanors. Whereas felony defendants might be

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26. *See infra* Section III.A.
27. *See infra* Section III.B.
28. *See infra* Section IV.B.
29. *See infra* Section IV.B.
30. *See infra* Part V.

held on bail for crimes as serious as rape and murder, at least three of every four misdemeanor defendants are charged with either non-violent offenses or nothing more than minor public-order crimes such as traffic violations and tax evasion.[31] This might lead one to conclude that a higher proportion of misdemeanor defendants would likely be released prior to trial—either with no stipulations or with manageably lower bail amounts—but evidence suggests that this is not true and that misdemeanor defendants are more likely subject to due process violations during the pretrial process than felony defendants.[32] The necessity of understanding the misdemeanor bail system is made all the more clear given the fact that misdemeanor criminal charges in the United States outnumber felony charges five to one.[33]

We begin in Part I by outlining the basic mechanics and history of pretrial release and bail in the United States before discussing the current "third wave" of bail reform and highlighting how we are still informationally ill-equipped to enact lasting change. In Part II, we describe the design of our empirical study and introduce our dataset, demonstrating the value of utilizing the random assignment of judges to bail hearings and emphasizing the unique nature of and value in studying a venue such as Pima County, Arizona. We then present our empirical results in Part III and Part IV, which cover our findings on judicial discrepancy in bail decisions and the downstream causal effects of money bail, respectively. We discuss the implications of our study in Part V by outlining some possible prescriptive remedies that the empirical findings engender, and then briefly conclude.

I. BAIL AND PRETRIAL DETENTION IN FEDERAL, STATE, AND LOCAL COURTS

To appreciate the current state of bail reform in the United States—and, consequentially to identify the questions we should be asking when enacting new policy—we must first understand the surprisingly rich legal and empirical history of pretrial detainment. In this Part, we begin with a brief description of the basic mechanics of bail and pretrial detainment, followed

31. *See* DORIS J. JAMES, BUREAU OF JUST. STAT., DEP'T. OF JUST., PROFILE OF JAIL INMATES, 2002, at 93 (2004), https://www.bjs.gov/content/pub/pdf/pji02.pdf; Shima Baradaran Baughman, *The History of Misdemeanor Bail*, 98 B.U. L. REV. 837, 841 (2018) ("The vast majority of these misdemeanor defendants—up to eighty-five percent—are locked up for nonviolent and minor offenses.").

32. *See* Alisa Smith & Sean Maddan, *Misdemeanor Courts, Due Process, and Case Outcomes*, 31 CRIM. JUST. POL'Y REV. 1312, 1320 (2020).

33. *See* Baradaran Baughman, *supra* note 31, at 844; Alexandra Natapoff, *Misdemeanor Decriminalization*, 68 VAND. L. REV. 1055, 1063 (2015) (estimating that about five misdemeanor cases are filed in the United States each year for each felony case); *see also* Jenny Roberts, *Why Misdemeanors Matter: Defining Effective Advocacy in the Lower Criminal Courts*, 45 U.C. DAVIS L. REV. 277, 294 (2011).

by an outline of three distinct stages in U.S. pretrial jurisprudence and practice: early English and American practice; the first modern bail reforms of the 1960s; and the second bail reforms in the 1980s. We then provide a picture of how federal and state courts currently approach bail setting and release, paying particular attention to the existing empirical data that highlights flaws in the pretrial system. With this backdrop, we highlight two empirical puzzles that remain largely unanswered but must be addressed before the United States moves successfully into its third wave of large-scale bail reform: accurately measuring disparity in bail decisions across bail judges within a single jurisdiction and credibly identifying the causal impact that those bail decisions have on case outcomes and pre- and post-trial defendant behavior.

A. The Mechanics of Bail and Pretrial Detainment

Pretrial criminal systems in the United States are generally designed to answer three questions: (1) Who are the defendants in a case; (2) what crimes have they been charged with; and (3) will they be held or released before trial? While the first two determinations are often addressed *pro forma*,[34] answering the question of whether and in what form an individual will go free pending his criminal trial has always been a fraught endeavor and has recently become one of the key issues driving modern criminal justice reform. While the factors considered by courts and judges in making the pretrial release determination naturally vary across jurisdictions,[35] pretrial systems in the United States follow the same basic process.

The process usually starts with an arrest and booking of the defendant, followed by an initial appearance ("IA") within six to twenty-four hours,[36] where the defendant appears before a magistrate or judge for the first time. The IA judge or magistrate usually has a host of options available when setting pretrial release conditions. One possibility, if the defendant shows minimal risk of flight or recidivism, is to release the individual contingent on a promise to appear for all subsequent proceedings. This option is commonly referred to as a release on recognizance ("ROR"). A second option is to release the defendant subject to specific non-monetary conditions. These include a wide range of possibilities such as electronic monitoring, drug

34. We recognize that the determination of what criminal charges are brought can (and often is) quite complex, although at the pretrial stage this complexity is confined primarily to the parties in the case and is not within the purview of the court.

35. See *infra* Sections I.B and I.C for a discussion of these factors and how they have varied across time and jurisdiction.

36. Emblematic of the variation across systems, some jurisdictions explicitly call this a bail hearing, reserving the title of initial appearance for a later, more substantive hearing (or not using the term at all).

treatment, a temporary restraining order, and firearms restrictions. The measures are typically designed to increase public safety, prevent harm to victims, reduce flight risk, and encourage rehabilitation.[37]

A third possibility is that the IA judge requires the defendant to post a bail payment if she deems the individual a flight risk or public safety threat. The bail payment is typically 10% of the bail amount set by the court, which the defendant pays to obtain release. Provided there are no release violations or missed court proceedings, the defendant will receive most of the bail payment back; if there are violations or missed proceedings, the payment is forfeited to the court.[38] For those who cannot make the payment with their own funds or the funds of others, they can try to borrow the money from a bail bondsman. Commercial bail bondsmen are typically willing to receive collateral such as jewelry, cars, and real estate, and they charge a non-refundable amount for their services. Typically, the amount is the 10% bail payment that the defendant must pay to obtain release.[39] If the defendant does not comply with the release conditions or fails to appear in court for a proceeding (commonly referred to as an "FTA"), the defendant or the bail bondsman, who typically acts as a surety, is responsible for the remaining bail amount. In the event of non-compliance with release conditions or absence from a court proceeding, the bail bondsman will frequently hire someone to find the defendant and bring the individual back into custody. For the most serious crimes, like murder, judges often deny bail to the defendants, leaving no options for release.

At the end of the IA, the defendant is given an arraignment date, where the prosecutor's charges are formally filed, the judge determines whether there is probable cause, and the defendant enters a formal plea in response to the charges. If the defendant enters a not guilty plea, the case proceeds to trial. Throughout the process, the initial release conditions can be reevaluated by the IA judge, and plea negotiations between the prosecutor and defendant take place.

The precise timing, the actors involved, and the range of options available to judges at the IA can vary by jurisdiction. In Connecticut, for

37. Although most courts have a stated goal of only making pretrial decisions based on probability of flight and public safety risk, in practice, rehabilitation is also a goal based on the conditions a judge can impose.

38. Paul Heaton, Sandra Mayson & Megan Stevenson, *The Downstream Consequences of Misdemeanor Pretrial Detention*, 69 STAN. L. REV. 711, 718–19 (2017).

39. Will Dobbie, Jacob Goldin & Crystal S. Yang, *The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges*, 108 AM. ECON. REV. 201, 206 (2018).

instance, police (rather than magistrates) set bail for warrantless crimes.[40] The time between the IA and arraignment can vary based on the jurisdiction. Importantly, the decision criteria that magistrates are supposed to take into consideration also vary by jurisdiction. While most jurisdictions allow judges to factor in the flight risk and public safety risk of the defendant, New York prohibits public safety being taken into consideration.[41] Taken together, although there are patterns in the pretrial process that are similar in most jurisdictions, there is considerable variation in how each jurisdiction conducts the pretrial process.

B. A Brief History of Bail and Bail Reform in the United States

Legal systems have long recognized the importance of ensuring the appearance of defendants at criminal trials, primarily because having a defendant present at the trial makes post-conviction detention a much simpler process. So, while criminal justice reform advocates often have a negative association with the concept of criminal bail, the foundational purpose of bail is to provide an avenue for the defendant's pretrial release. Bail was built upon the presumption of innocence, asserting that all else equal, the court wants to ensure that a defendant appears for his criminal trial without detaining someone who has not yet been found guilty of a crime.[42]

Early Colonial American conceptions of bail had their roots, unsurprisingly, in revolutionary-period English law. Given the logistical difficulty in tracking criminal defendants in the seventeenth and eighteenth centuries,[43] one might reasonably assume that the rates of pretrial detainment were substantially higher than they are in the age of computers and digital tracking. However, the English practice of bail was much more liberty-

40. CONN. SENT'G COMM'N, REPORT TO THE GOVERNOR AND GENERAL ASSEMBLY ON PRETRIAL RELEASE AND DETENTION IN CONNECTICUT 23–24 (2017), https://www.ct.gov/ctsc/lib/ctsc/Pretrial_Release_and_Detention_in_CT_2.14.2017.pdf.

41. N.Y. CRIM. PROC. LAW § 510.30 (McKinney 2020) (formally restricting bail determinations to considerations of flight risk and reversal of pending appeals of previous guilty verdicts).

42. BARADARAN BAUGHMAN, *supra* note 22, at 18 ("Traditionally, bail is some form of property (such as money) deposited or pledged to a court to persuade it to release the accused on the understanding that he will return for trial or forfeit the bail. The principle of bail grounds itself in the presumption of innocence and the principles of due process.").

43. Modern technology, particularly the use of electronic monitoring, has made it much easier to track individuals with pending criminal trials, thereby making the pretrial release of defendants more palatable to courts and law enforcement. For a comprehensive review of pretrial monitoring, including electronic monitoring, see Samuel R. Wiseman, *Pretrial Detention and the Right to be Monitored*, 123 YALE L.J. 1344 (2014); *see also* Timothy P. Cadigan, *Electronic Monitoring in Federal Pretrial Release*, 55 FED. PROB. 26 (1991) (an early empirical analysis of the efficacy of electronic monitoring); WILLIAM BALES ET AL., A QUANTITATIVE AND QUALITATIVE ASSESSMENT OF ELECTRONIC MONITORING (2010) (a more recent empirical analysis of electronic monitoring, which critiques Cadigan's approach).

oriented, and courts put a substantial premium on avoiding "excessive" bail and assigned bail only when clearly necessary to ensure appearance at trial.[44] Some Medieval English law actually banned all forms of pretrial detention, but a series of seventeenth-century reforms created an enumerated category of criminal offenses which granted the court the power to keep a criminal defendant in custody leading up to his trial. This was often the case with particularly serious crimes such as capital offenses, where the incentives to flee trial were naturally high.[45]

Early examples of this approach in colonial America can be found in the 1641 Massachusetts Body of Liberties—the first legal code established in the New England Colonies—which limited pretrial detainment, "unlesse it be in Crimes Capital, and Contempts in open Court, and in such cases where some expresse act of Court doth allow it."[46] These sensibilities carried over into the advent of American independence, where the first U.S. Congress echoed the limited and focused nature of pretrial detainment in the Body of Liberties by extending bail to all but capital offenses.[47] As Baradaran chronicled in her comprehensive volume on bail, "[d]enying bail in noncapital cases was largely seen as a violation of the presumption of innocence."[48]

Over the next two centuries, the primacy of liberty at the pretrial stage remained unchanged and even made its way into the U.S. Supreme Court's jurisprudence. In an 1835 habeas case, *Ex Parte Milburn*,[49] the Court emphasized that the purpose of bail was to "secure the due attendance of the party accused, to answer the indictment, and to submit to a trial."[50] Another sixty years later, and in response to a growing belief that pretrial detainment should also be used to prevent high-risk defendants from committing further crimes, the Court acknowledged a trial judge's discretion in granting bail, but reemphasized that "[t]he statutes of the United States have been framed upon the theory that a [defendant] shall not, until he has been finally adjudged guilty . . . be absolutely compelled to undergo imprisonment or punishment."[51] A defendant's constitutional right to due process included a

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44. *See* Hunt v. Roth, 648 F.2d 1148, 1156 (8th Cir. 1981), *vacated as moot sub. nom.* Murphy v. Hunt, 455 U.S. 478 (1982) (depicting the development of American bail policies from medieval English law).

45. BARADARAN BAUGHMAN, *supra* note 22, at 19 (highlighting the intuitive rationale "that in capital cases the death penalty may be imposed and a defendant would have a serious incentive to flee before trial").

46. THE MASSACHUSETTS BODY OF LIBERTIES para. 18 (1641).

47. The Judiciary Act of 1789, Pub. L. No. 1-20, 1 Stat. 73 (1789).

48. BARADARAN BAUGHMAN, *supra* note 22, at 20.

49. 34 U.S. 704 (1835).

50. *Id.* at 710.

51. Hudson v. Parker, 156 U.S. 277, 285 (1895).

liberty stake in being provided bail except when the risk of flight was at its highest.[52]

After a half century without any major developments in the country's approach to bail, Congress passed the Sumners Courts Act in 1940, granting the U.S. Supreme Court the authority to establish unified rules of criminal procedure at the federal level, resulting in the first Federal Rules of Criminal Procedure.[53] Rule 46, which governed bail and pretrial detention, marked a substantial shift in the purpose and application of pretrial detention, allowing courts to consider much more than the seriousness of the crime accused and the likelihood of appearance at trial in determining bail. Additional factors included "the character of the defendant" and "the weight of the evidence against him" at the pretrial stage.[54] Discretion widened even further in immigration courts, where immigration authorities (not always judges) were explicitly allowed to account for the potential harm that the defendant may impose on the community if released.[55]

As state courts began to adopt these expanded conceptions of what can and should be considered when determining whether release should be provided at the pretrial stage, bail amounts began to rise rapidly, and the number of defendants who were detained prior to trial followed suit.[56] This issue hit the national stage in a pair of cases in which the U.S. Supreme Court reinforced the Eighth Amendment's prohibition on excessive bail[57] but declined to extend that reasoning to include a constitutional right to bail itself.[58] So while courts were prevented from assigning disproportionately high bail amounts, the centuries-long presupposition that defendants should be detained before trial in only the most serious of circumstances eroded.

52. BARADARAN BAUGHMAN, *supra* note 22, at 21.

53. Sumners Courts Act, Pub. L. No. 76-675, 54 Stat. 688 (1940) (codified as amended at 18 U.S.C. § 3771(f)). Up until this point, federal criminal procedure was dictated according to the Judiciary Act of 1789, under which federal courts applied the criminal procedure rules (including those governing bail and pretrial detention) of the state in which the court sat. The Judiciary Act of 1789, Pub. L. No. 1-20, 1 Stat. 73, 91–92 (1789).

54. FED. R. CRIM. P. 46(c) (1952) (amended 2002).

55. United States *ex rel.* Potash v. Dist. Dir. of Immigr. & Naturalization, 169 F.2d 747, 751 (2d Cir. 1948); *see also* 8 U.S.C. § 156 (1946) (repealed 1952).

56. BARADARAN BAUGHMAN, *supra* note 22, at 22–23.

57. Stack v. Boyle, 342 U.S. 1, 6 (1951) (ruling it a violation of the Eighth Amendment to set an unusually high bail simply "from the fact of indictment alone" because it "is an arbitrary act . . . [that] would inject into our own system of government . . . principles of totalitarianism"), *superseded by statute*, Bail Reform Act of 1966, Pub. L. No. 89-465, 80 Stat. 214.

58. Carlson v. Landon, 342 U.S. 524, 545–46 (1952) ("The Eighth Amendment has not prevented Congress from defining the classes of cases in which bail shall be allowed in this country. Thus in criminal cases bail is not compulsory where the punishment may be death. Indeed, the very language of the Amendment fails to say all arrests must be bailable. We think, clearly, here that the Eighth Amendment does not require that bail be allowed under the circumstances of these cases.").

Even without the comprehensive data collection and empirical analysis of our day, policymakers and scholars recognized that the liberal use of money bail would lead to the disproportionate detainment of poor defendants. In 1966, President Lyndon Johnson famously opined that:

> The defendant with means can afford to pay bail. He can afford to buy his freedom. But the poorer defendant cannot pay the price. He languishes in jail weeks, months, and perhaps even years before trial. He does not stay in jail because he is guilty. He does not stay in jail because any sentence has been passed. He does not stay in jail because he is any more likely to flee before trial. He stays in jail for one reason only—he stays in jail because he is poor.[59]

This sentiment spurned the genesis of what scholars have called the first wave of American bail reform,[60] the foundation of which was the Bail Reform Act of 1966 ("1966 Act").[61] On its face, the 1966 Act seemed to revert back to the earlier focus on only detaining defendants—whether through high bail or non-release—in order to "reasonably assure the appearance of the [defendant] as required."[62] And while this reemphasis on liberty appeared to stick at first, provisions in the Act ultimately gave pretrial decision-makers even more non-flight-related factors to consider, including a defendant's family ties, previous criminal record, and ability to pay bail, along with the weight of pretrial evidence.[63] With this added authority, courts again began to deny bail at higher rates.

The second wave of American bail reform came amidst a spike in both the perception of and actual rate of violent crime in America.[64] While this reform—and its own Bail Reform Act ("1984 Act")[65]—still emphasized that the true purpose of bail was to allow courts to release all defendants who could reasonably be expected to appear at trial, it simultaneously reflected the growing public reticence of releasing individuals who might pose a danger to the community. The 1984 Act explicitly endorsed what the Supreme Court had recently deemed constitutionally permissible[66] by

59. Lyndon B. Johnson, *Remarks at the Signing of the Bail Reform Act of 1966*, AM. PRESIDENCY PROJECT, https://www.presidency.ucsb.edu/node/238665 (last visited Sept. 15, 2021).

60. BARADARAN BAUGHMAN, *supra* note 22, at 23.

61. Bail Reform Act of 1966, Pub. L. No. 89-465, 80 Stat. 214, 214–17.

62. *Id.* at 214.

63. *Id.*

64. For a particularly thorough analysis of crime data during the second reform (the 1980s), see Scott Boggess & John Bound, *Did Criminal Activity Increase During the 1980s? Comparisons Across Data Sources*, 78 SOC. SCI. Q. 725, 736 (1997) (identifying a rise in violent crime starting in the mid-1980s, which the authors attribute to the crack cocaine epidemic).

65. Bail Reform Act of 1984, Pub. L. No. 98-473, §§ 3141–50, 98 Stat. 1837, 1976–85.

66. *See* Schall v. Martin, 467 U.S. 253, 264 (1984) (while assessing the legality of a pretrial detention policy, the Court stated that "[t]he 'legitimate and compelling state interest' in protecting

allowing judges to assess "the nature and seriousness of the danger to any person or the community that would be posed by the [defendant's] release."[67] Additionally, and importantly, the 1984 Act also mandated a presumption of detention (subject to defendant rebuttal) for a much wider body of crimes than just capital offenses. Defendants who had been charged with any crimes of violence, drug offenses with maximum terms of ten years or more, or non-violent felonies involving the use of a firearm—among a number of other conditions—were presumed to be a danger to the community and were denied bail.[68]

C. *In the Midst of a Third Reform*

In spite of the second-wave reformers' motivations to maintain a system in which pretrial defendants are kept out of jail when possible, increased decision-making power in assigning money bail, combined with the temptation to lock up any individual who was perceived to pose even a slight danger to the community, proved too great for America's judges.[69] In the year following the 1984 Act, just over 256,000 inmates were being held in local jails.[70] That total nearly doubled to 507,000 by 1995,[71] nearly tripled to 747,000 by 2005,[72] and has remained as astronomically high since.[73] Nearly half a million of those inmates have yet to go on trial,[74] largely because the rate at which judges predicated release on paying bail tripled over that same time period.[75] While some of these individuals will only end up spending a short time in jail—a circumstance that empirical evidence has indicated may

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the community from crime cannot be doubted") (quoting De Veau v. Braisted, 363 U.S. 144, 155 (1960)).

67. Bail Reform Act of 1984, § 3142(g)(4), 98 Stat. at 1980.

68. *Id.* at § 3142(e)–(f), 98 Stat. at 1978–80.

69. *See* Van Brunt & Bowman, *supra* note 12, at 704–05 ("The ideals of liberty and presumptive innocence part company with the reality of judicial decision-making in local criminal courts. Judicial officers overseeing bail hearings—driven by concern for community safety and, even in some cases, by a desire to preemptively punish—have consistently paid less heed to state and constitutional law than to their own intuition about who is deserving of pretrial release.").

70. DARRELL K. GILLIARD & ALLEN J. BECK, BUREAU OF JUST. STAT., DEP'T OF JUST., NCJ 167247, PRISON AND JAIL INMATES AT MIDYEAR 1997, at 2 (1998).

71. *Id.*

72. JAIL INMATES IN 2017, *supra* note 1, at 2.

73. *Id.*

74. *Id.* at 5.

75. THOMAS H. COHEN & BRIAN A. REAVES, BUREAU OF JUST. STAT., DEP'T OF JUST., NCJ 214994, PRETRIAL RELEASE OF FELONY DEFENDANTS IN STATE COURTS 2 (2007) (recording that judges assigned money bail to 23% of defendants in 1990); *see also* BRIAN A. REAVES, BUREAU OF JUST. STAT., DEP'T OF JUST., NCJ 243777, FELONY DEFENDANTS IN LARGE URBAN COUNTIES, 2009 - STATISTICAL TABLES 15 (2013) (finding that judges assigned money bail in 61% of cases in large U.S. counties) [hereinafter FELONY DEFENDANTS IN LARGE URBAN COUNTIES].

still impose substantial harm on defendants[76]—some studies show at least 38% of felony defendants will remain in pretrial detainment until their criminal trials.[77] Many of these individuals are only being held on minimal bail ($1,000 or less) but are unable to pay.[78]

So, despite the best intentions of the second-wave reformers—and in part because of them—the American criminal justice system again finds itself engulfed in a debate about how to best assess and treat pretrial defendants. This "third wave" of bail reform began in earnest in the mid-2010s,[79] with early, statewide policy shifts in states like Colorado (2013)[80] and New Jersey (2014)[81] leading the way for more recent reforms in states such as Indiana (2016),[82] Arizona (2017),[83] Maryland (2017),[84] and New Mexico (2017),[85] all of which limited—sometimes severely—the ability for magistrates and judges to assign money bail amounts beyond what the defendant could reasonably pay.

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76. *See* Dobbie et al., *supra* note 39 (measuring the causal impact of being detained at least three days before trial); Megan T. Stevenson, *Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes*, 34 J.L. ECON. & ORG. 511 (2018) (measuring the causal impact of being detained at least two days before trial); Emily Leslie & Nolan G. Pope, *The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from New York City Arraignments*, 60 J.L. & ECON. 529 (2017) (measuring the causal impact of being detained at least two days before trial).

77. *See, e.g.*, FELONY DEFENDANTS IN LARGE URBAN COUNTIES, *supra* note 75, at 15.

78. *See* HUM. RTS. WATCH, *supra* note 4 (reporting that in New York City, less than 13% of defendants can afford a bail of $1,000).

79. *See, e.g.*, KRISTIN BECHTEL ET AL., PRETRIAL JUST. INST., DISPELLING THE MYTHS: WHAT POLICY MAKERS NEED TO KNOW ABOUT PRETRIAL RESEARCH 2 (2012); Timothy R. Schnacke, Claire M.B. Brooker & Michael R. Jones, *The Third Generation of Bail Reform*, DENV. L. REV. ONLINE (Mar. 14, 2011), https://www.denverlawreview.org/dlr-online-article/2011/3/14/the-third-generation-of-bail-reform.html (referring to this movement as the third generation of bail reform).

80. H.B. 13-1236, 69th Gen. Assemb., Reg. Sess. (Colo. 2013) (redefining bail as "a security, which may include a bond with or without monetary conditions" from the previous definition as "the amount of money set by the court which is required to be obligated by a bond for the release of a person in custody").

81. N.J. STAT. ANN. § 2A:162-16(b)(1) (Supp. 2018) (revising the money bail system to focus release determinations more squarely on calculations of defendant risk).

82. Order Amending Criminal Rule 26, Cause No. 94S00-1701-MS-5 (Ind. Sup. Ct. Sept. 5, 2017) (limiting the assignment of money bail to defendants who pose a threat to themselves or the community, are charged with murder, are already on pretrial release, or are on probation or parole).

83. ARIZ. R. CRIM. P. 7.3(c)(2)(A) ("The court . . . must not impose a monetary condition that results in unnecessary pretrial incarceration solely because the defendant is unable to pay the imposed monetary condition.").

84. MD. R. 4-216.1(e)(1) (limiting the assignment of money bail if the defendant is financially incapable of meeting the amount and that will result in the defendant being detained solely because of that financial incapability) (amended 2021).

85. N.M. R. 5-401(E)(1)(b)–(c) ("The court shall set secured bond at the lowest amount necessary to reasonably ensure the defendant's appearance and with regard to the defendant's financial ability to secure a bond. The court shall not set a secured bond that a defendant cannot afford for the purpose of detaining a defendant who is otherwise eligible for pretrial release.") (amended 2020).

If the underlying impetus behind the second-wave reforms was one of crime mitigation, then the pendulum appears to have swung back to the side of liberty.[86] This shift is no more evident than the recent bail policy reforms in New York, the capstone of which was the Bail Elimination Act of 2019.[87] While evident from its name, the explicit purpose of the bill did not even mention safety considerations, focusing specifically on "end[ing] the use of monetary bail, reduc[ing] unnecessary pretrial incarceration and improv[ing] equity and fairness in the criminal justice system."[88] New Jersey, just three years after its 2014 reform, took a similar approach after a ballot initiative amended its constitution to mostly eliminate money bail for pretrial defendants.[89] And with President Biden supporting the elimination of money bail, the federal pretrial system seems poised for major reform of this sort.[90]

But there is an increasingly vocal contingent pushing against these new, more liberty-focused policies. The primary concern on this side of the debate is, unsurprisingly, the increased danger that the pretrial release of criminal defendants—particularly those accused of violent crimes—poses for local

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86. *See* Van Brunt & Bowman, *supra* note 12, at 757 ("Strikingly, unlike the push for 'public safety' detention in the 1980s, which was spurred in large part by crime control advocates, this new wave of reform has sometimes been propelled by advocacy groups and policy organizations committed to removing financial conditions from the pretrial release decision." (citing Mayson, *supra* note 12, at 492–93)).

87. S.B. S2101-A, 2019–2020 Legis. Sess. (N.Y. 2019).

88. *Senate Bill S2101A*, N.Y. STATE SENATE (Jan. 22, 2019), https://www.nysenate.gov/legislation/bills/2019/s2101.

89. N.J. CONST. art. I, § 11 (Supp. 2018) ("All persons shall, before conviction, be eligible for pretrial release. Pretrial release may be denied to a person if the court finds that no amount of monetary bail, non-monetary conditions of pretrial release, or combination of monetary bail and non-monetary conditions would reasonably assure the person's appearance in court when required, or protect the safety of any other person or the community, or prevent the person from obstructing or attempting to obstruct the criminal justice process.").

90. *The Biden Plan for Strengthening America's Commitment to Justice*, BIDEN HARRIS, https://joebiden.com/justice/ (last visited Jan. 1, 2022) ("Cash bail is the modern-day debtors' prison. The cash bail system incarcerates people who are presumed innocent. And, it disproportionately harms low-income individuals."); *see also Issues: Justice and Safety for All*, BERNIE SANDERS, https://berniesanders.com/issues/criminal-justice-reform/ (last visited Jan. 1, 2022) ("Right now, hundreds of thousands of people without a criminal conviction are in jail simply because they could not afford bail. Young people can spend hundreds of days in jail, only to be acquitted—yet the severe damage to their lives cannot be undone. This is why Bernie introduced the *No Money Bail Act of 2018* to end cash bail and to end the criminalization of poverty in America."). And while President Donald Trump's "First Step Act" did not address pretrial release specifically, even conservative policies are shifting towards a more liberty-oriented approach—while still emphasizing the importance of crime reduction. *White House Fact Sheets: President Donald J. Trump Is Committed to Building on the Successes of the First Step Act*, WHITE HOUSE ARCHIVES (Apr. 1, 2019), https://trumpwhitehouse.archives.gov/briefings-statements/president-donald-j-trump-committed-building-successes-first-step-act/ (where President Trump is quoted as saying, "Americans from across the political spectrum can unite around prison reform legislation that will reduce crime while giving our fellow citizens a chance at redemption").

communities.[91] New York's Bail Elimination Act, for example, has already received pushback, and state legislators just recently began circulating step-back provisions that would grant more traditional levels of judicial discrepancy in setting money bail, presumably to increase pretrial detention rates.[92] The COVID-19 pandemic appears to have "helped" in this regard, as Andrew Cuomo, the former governor of New York, temporarily scaled back money bail reform in order to quell concerns about releasing pretrial defendants from New York's jails, which have some of the highest COVID-19 infection rates in the world.[93]

This debate is not new: In fact, it closely mirrors much of the argument that animated the second-wave reform, and the liberty-first motivations of the third-wave reformers are very much in line with those in the first wave. The difference, however, is the unified adoption and recognition of the importance of empirical data.[94] While legal empiricists played a role in developing pretrial policies in the 1980s, reformers and policymakers in this third wave have an appreciable hunger for evidence-based policies.[95] And the wave of empirical articles over just the last five years suggests that social scientists are happy to oblige.

The importance of this work is difficult to overstate, but not all data analysis is created equal. Indeed, the (honorable) devotion to data-driven change that characterizes many of the policymakers and reformers can become a crutch. The current debate regarding Chicago's 2017 reforms is emblematic of potential tensions between the desire for reform and the need for watertight empirical evidence. In 2019, the Office of the Chief Judge in Cook County, Illinois, published an empirical analysis of the reforms, lauding higher release rates with no associated increase in violent crime.[96] In 2020, however, two empirical scholars posted a working paper that questions these findings, asserting that the original study miscalculated crime rates,

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91. *See, e.g.*, MANGUAL, *supra* note 14, at 8; *see also* Lehman, *supra* note 14.

92. *See* Jesse McKinley, *The Bail Reform Backlash That Has Democrats at War*, N.Y. TIMES (Feb. 16, 2020), https://www.nytimes.com/2020/02/14/nyregion/new-york-bail-reform.html.

93. Melissa Gira Grant, *The Shock Doctrine Came for Bail Reform*, NEW REPUBLIC (Apr. 7, 2020), https://newrepublic.com/article/157205/shock-doctrine-came-bail-reform; *see also* LEGAL AID SOC'Y, *supra* note 5.

94. *See* Van Brunt & Bowman, *supra* note 12, at 755–56 ("Unlike in Professor Foote's time [the 1960s], when risk prediction was in its infancy, many jurisdictions today are relying on sophisticated predictive risk assessments in rendering release decisions. . . . Recent state efforts are novel, however, in that they seek to expand the use of preventive detention by relying heavily on empirical assessments that have pervaded the pretrial 'market,' in order to ascertain defendant 'risk.'").

95. *See* LAURA & JOHN ARNOLD FOUND., DEVELOPING A NATIONAL MODEL FOR PRETRIAL RISK ASSESSMENT 1, 5 (2013) ("Our goal is that every judge in America will use a data-driven, objective risk assessment within the next five years.").

96. STATE OF ILL. CIR. CT. OF COOK CNTY., BAIL REFORM IN COOK COUNTY: AN EXAMINATION OF GENERAL ORDER 18.8A AND BAIL IN FELONY CASES 36 (2019).

missing a substantial increase in crimes committed by pretrial defendants who were released under the new system.[97] Caution must be practiced when analyzing these new policies and systems. Additionally, as we argue below,[98] only certain (generally newer) empirical strategies are able to provide credible answers to some of the most pressing questions related to bail reform.

D. Outstanding Questions

Despite the fervor and speed with which legislators, policymakers, and scholars started reevaluating America's bail practices,[99] there are foundational empirical questions that have yet to be adequately addressed. In the following pages, we focus on the two issues that we believe are among the most pressing and, importantly, can be addressed using sophisticated but tractable statistical techniques.[100]

1. Inter-Judge Decision-Making Disparity

When discussing disparities in bail outcomes, the vast majority of the literature focuses on the differences between the various pretrial systems as described in the preceding Section. This sort of cross-system variation should not be surprising, as different rules, laws, and decision-makers will naturally produce differing pretrial release determinations even for the same crimes or similar defendants.

What is sometimes ignored, however, is the potential for substantively identical defendants to be treated differently *within* a single governing jurisdiction. Under a traditional view of judging, this sort of inter-judge disparity should be largely non-existent. Judges are often perceived to be well-trained legal automatons—able to take the facts and circumstances of a given case and accurately apply the appropriate law to produce "the" proper outcome.[101] But naturally, even the most ardent supporters of mechanical

97. Paul G. Cassell & Richard Fowles, *Does Bail Reform Increase Crime? An Empirical Assessment of the Public Safety Implications of Bail Reform in Cook County, Illinois*, 55 WAKE FOREST L. REV. 933, 937–38 (2020).

98. *See infra* Section IV.A.

99. It should be noted (and will be throughout this Article) that the authors find this emphasis on reform both necessary and refreshing.

100. We acknowledge these are not the only questions that remain, but we feel that these are among the most critical. Other important issues involve what the role of algorithms should be in the bail setting process and how they should be regulated, the appropriate measures for specific types of crime, and the measures that should be taken to mitigate socioeconomic disparities in pretrial decision-making.

101. *See, e.g.*, Li Zhou, *Kavanaugh's Hearing Is a Partisan Fight, But He Says He's a "Neutral Arbiter" in Opening Remarks*, VOX (Sept. 4, 2018, 4:49 PM), https://www.vox.com/2018/9/4/17818708/supreme-court-nominee-brett-kavanaugh-merrick-garland.

jurisprudence understand that the judiciary's humanity will dictate some small discrepancy in the ways in which judges view, process, and utilize the information presented to them.[102]

Scholars have long sought to identify and quantify legal disparities in arenas other than pretrial decision-making through various methodological approaches. Racial disparities, for example, are pervasive throughout the criminal justice system. In comparison to similarly situated whites, African Americans are more likely to be stopped and arrested by police,[103] searched by law enforcement,[104] experience police force,[105] charged more harshly by prosecutors,[106] convicted by judges and jurors, and sentenced to longer and more harsh terms.[107] As Bushway and Piehl point out, disparity comes in two forms: warranted and unwarranted disparity.[108] The former involves disparities that result from legally relevant factors such as the defendant's criminal history, crime severity, and crime type.[109] The latter relates to legally irrelevant factors such as the defendant's race, gender, and socioeconomic attributes, especially after all legally relevant factors are considered.[110]

Figuring out what constitutes warranted and unwarranted disparity is challenging, but empirical scholars have come up with clever ways to measure these forms of disparity. A number of studies rely on "traditional" multiple regression analysis to quantify disparity in judicial decisions. These studies specify a model where they attempt to control for all factors that a judge takes into consideration when making a sentencing decision. Examples include the work of Albonetti; Frazier, Bock and Henrietta; Turner, Secret

102. Originally coined by Roscoe Pound, mechanical jurisprudence refers to the process of judges stringently applying precedent to the facts of cases without considering the practical or ethical consequences of doing so. *See* Roscoe Pound, *Mechanical Jurisprudence*, 8 COLUM. L. REV. 605, 605 (1908).

103. Andrew Gelman, Jeffrey Fagan & Alex Kiss, *An Analysis of the New York City Police Department's "Stop-and-Frisk" Policy in the Context of Claims of Racial Bias*, 102 J. AM. STAT. ASS'N 813, 816, 820–21 (2007).

104. Kate Antonovics & Brian G. Knight, *A New Look at Racial Profiling: Evidence from the Boston Police Department*, 91 REV. ECON. & STAT. 163, 164 (2009).

105. Roland G. Fryer, Jr., *An Empirical Analysis of Racial Differences in Police Use of Force*, 127 J. POL. ECON. 1210, 1231 (2019).

106. M. Marit Rehavi & Sonja B. Starr, *Racial Disparity in Federal Criminal Sentences*, 122 J. POL. ECON. 1320, 1350 (2014).

107. David S. Abrams, Marianne Bertrand & Sendhil Mullainathan, *Do Judges Vary in Their Treatment of Race?*, 41 J. LEGAL STUD. 347, 356 (2012); Dobbie et al., *supra* note 39.

108. Shawn D. Bushway & Anne Morrison Piehl, *Judging Judicial Discretion: Legal Factors and Racial Discrimination in Sentencing*, 35 LAW & SOC'Y REV. 733, 734 (2001).

109. *Id.*

110. *Id.*

and Johnson; and Lee and Ruiz.[111] These studies were important in setting the stage for future work, especially because the literature in criminal justice during this time overwhelmingly focused on sentencing disparities. One major limitation of studies of this sort is their attempt to control for every factor that drives disparity. The results of these models are thus dependent on how the model is specified, and in all likelihood, are sensitive to variables not included in the model.

A second wave of studies that rely on quasi-experimental methods overcome the limitations of this initial wave of studies. Ayres and Waldfogel offer a market-based test of unwarranted disparity using data from the bail bond market in New Haven, Connecticut, to demonstrate that judges systematically hold more Black and Hispanic/Latinx defendants in pretrial detention than similarly situated whites.[112] A related literature examines unwarranted disparity in police searches. Knowles, Persico, and Todd and Anwar and Fang, for instance, develop empirical tests that distinguish between statistical discrimination—the use of observable characteristics like race to develop accurate beliefs about the unobservable characteristics of individuals—and racist stereotyping when police conduct motor vehicle searches.[113]

In a similar vein to the police search studies (and of particular relevance to this Article), Arnold, Dobbie, and Yang use data from Miami and Philadelphia to attempt to disentangle statistical discrimination from racial stereotyping in bail setting.[114] They find that Black defendants are 3.6 percentage points more likely to be assigned monetary bail than similarly situated white defendants, and when assigned bail, Black defendants receive bail amounts that are, on average, $9,923 higher.[115] Strikingly, they find white defendants are 22.2 to 23.1 percentage points more likely to be rearrested in comparison to similarly situated Black defendants, and that

111. Celesta A. Albonetti, *Bail and Judicial Discretion in the District of Columbia*, 74 SOCIO. & SOC'Y RSCH. 40 (1989); Charles E. Frazier, E. Wilbur Bock & John C. Henretta, *Pretrial Release and Bail Decisions: The Effects of Legal, Community, and Personal Variables*, 18 CRIMINOLOGY 162 (1980); K.B. Turner, Philip E. Secret & James B. Johnson, *Race as a Factor in the Judicial Decision of Bail Amount in a Midwestern Jurisdiction*, J. ETHNICITY CRIM. JUST., 2003, at 21; James M. Ruiz & Joongyeup Lee, *Revisiting Louisiana Drug Interdiction: Drug Profiling in the Louisiana Justice System*, 11 INT'L J. POLICE SCI. & MGMT. 236 (2009).

112. Ian Ayres & Joel Waldfogel, *A Market Test for Race Discrimination in Bail Setting*, 46 STAN. L. REV. 987 (1994).

113. John Knowles, Nicola Persico & Petra Todd, *Racial Bias in Motor Vehicle Searches: Theory and Evidence*, 109 J. POL. ECON. 203 (2001); Shamena Anwar & Hanming Fang, *An Alternative Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence*, 96 AM. ECON. REV. 127 (2006).

114. David Arnold, Will Dobbie & Crystal S. Yang, *Racial Bias in Bail Decisions*, 133 Q.J. ECON. 1885 (2018).

115. *Id.* at 1886.

much of the discrimination taking place is based on inaccurate racial stereotypes held by judges.[116] Their work establishes an important benchmark for our study in terms of racial disparity in bail setting.

2. Correctly Identifying the Effects and Costs of Pretrial Detainment

Even if policymakers and judges fully understood the inter-judge disparities described above, informed and efficacious policy-making will likely only occur when the full set of costs and benefits are also understood. Policymakers and legal academics have long understood the importance of this approach, and empirical and economic analyses of the impacts of assigning bail and detaining defendants before trial have a long history in law and economics. In some cases, identifying these costs and benefits is a simple endeavor. In other cases, however, the downstream effects of bail and detainment are either empirically difficult to measure or are so non-intuitive that they have been ignored by both researchers and policymakers.

One particularly obvious cost of pretrial detainment is the financial burden it imposes on defendants. Because defendants detained before trial are unable to show up for work, many naturally lose their jobs,[117] even if the detainment was short-lived.[118] Empirical studies also show that detained defendants, particularly those who become unemployed, are thereafter likely to have reduced hourly wages.[119] Additionally, many defendants become more at risk of becoming victims of larceny while in detention, and nearly all individuals sustain a loss in social standing and reputation.[120] These fiscal and social costs are often imposed on the defendants' families.[121]

Pretrial detainment also comes with a fiscal cost to local, state, and federal governments and, as a consequence, to taxpayers. With every additional detained defendant comes an increased need for prison staff, meals, facility upkeep, and a bevy of additional monetary costs. California,

116. *Id.* at 1888–90.

117. *See* Arthur R. Angel et al., *Preventive Detention: An Empirical Analysis*, 6 HARV. CIV. RTS.-CIV. LIBERTIES L. REV. 300, 353 (1971); BARADARAN BAUGHMAN, *supra* note 22, at 82.

118. *See supra* note 77 and accompanying text.

119. *See* JAMES, *supra* note 31, at 9 (finding that "71% of jail inmates in 2002 reported that they were employed in the month before their arrest"); PEW CHARITABLE TRS., COLLATERAL COSTS: INCARCERATION'S EFFECT ON ECONOMIC MOBILITY 11–12 (2010).

120. Mark Pogrebin, Mary Dodge & Paul Katsampes, *The Collateral Costs of Short-Term Jail Incarceration: The Long-Term Social and Economic Disruptions*, CORR. MGMT. Q., Fall 2001, at 64, 64–65.

121. Kristin Turney, *Stress Proliferation across Generations? Examining the Relationship Between Parental Incarceration and Childhood Health,* 55 J. HEALTH & SOC. BEHAV. 302, 314 (2014) (finding that "paternal incarceration is more consequential than other types of father absence" such as death and divorce).

which houses a substantial portion of the U.S. prison population,[122] reported that it spends over $70,000 per inmate,[123] constituting 11% of its annual budget.[124] New York City spends a staggering $337,524 to incarcerate one person for one year.[125] While the average cost of imprisonment in other states is not quite as stunning, the Vera Institute estimated that the average yearly cost per inmate was $33,000 in 2017.[126] The figures are put into sharp focus in relation to pretrial detainment when, as we have previously noted, pretrial detainees make up more than half of the jail inmates in the United States.[127]

Another historically significant consideration made by policymakers—indeed, the factor that played the largest role in the previous two bail reforms—is the impact of a defendant's release on both the likelihood of appearing for trial and on the preponderance of crime (particularly violent crime) in the community at the hands of the defendant during the pretrial period. Recent empirical studies show, for example, that defendants who are held before trial for at least three days are simultaneously less likely to be charged with crimes leading up to trial[128] and less likely to fail to appear for that trial.[129]

Historically, pretrial and bail regimes in the United States were designed to balance these more straightforward costs and considerations. Recently, however, interested parties have become more aware of additional downstream outcomes that were either previously ignored or deemed unlikely. Despite the high stakes of such detainment, it is unclear whether

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122. DANIELLE KAEBLE & MARY COWHIG, BUREAU OF JUST. STAT., DEP'T. OF JUST., CORRECTIONAL POPULATIONS IN THE UNITED STATES, 2016, at 11–12 (2018) (showing that California had the second most inmates in the United States (behind Texas) with just over 200,000 state prison and local jail inmates).

123. Associated Press, *At $75,560, Housing a Prisoner in California Now Costs More Than a Year at Harvard*, L.A. TIMES (June 4, 2017), https://www.latimes.com/local/lanow/la-me-prison-costs-20170604-htmlstory.html.

124. Gov. Arnold Schwarzenegger, *California State of the State Address*, C-SPAN at 18:41 (Jan. 6, 2010), https://www.c-span.org/video/?291103-1/california-state-state-address (stating, "[t]oday, almost 11 percent [of the general fund] goes to prisons and only 7.5 percent goes to higher education.").

125. Ben Chapman, *Cost of Incarceration Reaches Record High*, WALL ST. J., Dec. 9, 2019, at A12A.

126. CHRIS MAI & RAM SUBRAMANIAN, VERA INST. OF JUST., THE PRICE OF PRISONS: EXAMINING STATE SPENDING TRENDS, 2010–2015, at 7 (2017).

127. Shima Baradaran & Frank L. McIntyre, *Predicting Violence*, 90 TEX. L. REV. 497, 502 (2012). *See* WILLIAM J. SABOL & TODD D. MINTON, BUREAU OF JUST. STAT., DEP'T. OF JUST., JAIL INMATES AT MIDYEAR 2007, at 5 (2008) ("At midyear 2007, 62% of inmates had not been convicted or were awaiting trial, up from 56% in 2000."); ALLEN J. BECK & JENNIFER C. KARBERG, BUREAU OF JUST. STAT., DEP'T. OF JUST., PRISON AND JAIL INMATES AT MIDYEAR 2000, at 7 (2001) ("On June 30, 2000, an estimated 56% of the Nation's adult jail inmates were awaiting court action on their current charge.").

128. Dobbie et al., *supra* note 39, at 211–12.

129. *Id.* at 214.

judges and policymakers who determine bail amounts have an accurate understanding of the costs that pretrial detainment imposes on these individuals beyond the mere time they spend in jail. While previous studies have explored such "hidden costs," they are overwhelmingly observational studies and are unable to credibly identify unbiased causal effects. Obtaining an accurate estimate of the causal effect of bail on downstream outcomes is incredibly challenging.[130] Additionally, these hidden costs almost certainly vary across jurisdictions. As a result, bail policy analyses may be underestimating or missing these hidden costs, likely resulting in overly strict bail policies and unnecessary harm to pretrial defendants.

One downstream outcome of assigning bail that researchers have largely ignored until recently is the disposition of a defendant's criminal trial. While it may not be obvious, lower bail and pretrial release may actually reduce a defendant's ultimate probability of conviction. Given the potential costs of remaining in custody, detained individuals may have a stronger incentive to agree to a plea in exchange for release. Detention also hinders the defendant's ability to access counsel and—resulting from limited access to legal resources, communication, and witnesses—prepare a strong defense. In addition, a judge or jury may be biased in cases where the detention of the defendant is known or the defendant is required to wear a prison jumpsuit, either by activating the stigma that some have of incarceration,[131] or by reducing sympathy.[132]

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130. See *infra* Section IV.A for further discussion on why this is difficult.

131. Bright offers a particularly vivid description of the process:

> In many jurisdictions, when the court calls criminal cases for arraignments, it looks like a slave ship has docked outside the courthouse. African American men in orange jumpsuits, handcuffed together, are paraded into the courtroom. In some places this is followed by a process known as "meet 'em and plead 'em." A haggard court-appointed lawyer meets each defendant, talks to him or her for five or ten minutes—in some instances with other men handcuffed on either side of the client—and then announces that a guilty plea will be entered pursuant to a deal with the prosecution. The judge races through plea colloquies like an auctioneer, eliciting waivers of constitutional rights from people who often look like they do not quite understand what is happening. The judge accepts the pleas and pronounces sentences.

Stephen B. Bright, *The Failure to Achieve Fairness: Race and Poverty Continue to Influence Who Dies*, 11 U. PENN. J. CONST. L. 23, 27 (2008).

132. *See* Kateri Schafer, The Effect of Defendant's Courtroom Attire on Jurors' Verdicts (2013) (unpublished manuscript), http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.542.7322&rep=rep1&type=pdf (describing *Estelle v. Williams*, 425 U.S. 501 (1976), a U.S. Supreme Court case where the majority held that it was unconstitutional to compel a defendant to wear prison clothing before a jury, while also stating that defendants voluntarily wore the attire in order to win over their sympathy); *see also* Michael Mueller-Smith, The Criminal and Labor Market Impacts of Incarceration (Aug. 18, 2015) (unpublished manuscript), https://sites.lsa.umich.edu/mgms/wp-content/uploads/sites/283/2015/09/incar.pdf.

More recently, empiricists have also suggested that spending time behind bars before trial—even as little as five days—can have a substantial impact on the likelihood that a defendant recommits a crime while awaiting trial and even may have a substantial impact on the chances of recidivating after trial. While a long line of work examines this inquiry, a literature review on the relationship between incarceration and recidivism by Nagin, Cullen, and Jonson characterized the state of the research in the following manner: "Remarkably little is known about the effects of imprisonment on reoffending. The existing research is limited in size, in quality, [and] in its insights into why a prison term might be criminogenic or preventative."[133] Understanding the conditions under which incarceration is rehabilitative or criminogenic is challenging for a number of reasons. Perhaps the biggest challenge is that there are many unobserved factors that drive recidivism that are unrelated to an individual's incarceration. These could include the individual's family situation, peer group outside of prison, employment situation, and numerous other factors. These factors, which might be highly correlated with one's being imprisoned (and often with the likelihood of being assigned a manageable bail amount), could independently be driving recidivism, rather than the individual's incarceration itself. The resulting situation presents challenges for those trying to isolate the effect of incarceration on recidivism.[134]

Empirical work on the effect of incarceration on recidivism has been mixed.[135] Using the random assignment of judges to cases and comparing judges who have similar caseloads but vary in their leniency with defendants, Green and Winik examined the effect of juvenile incarceration on recidivism in Washington, D.C., and found no statistically distinguishable effects.[136] Loeffler had similar results when examining the effect of adult incarceration on recidivism in Cook County, Illinois.[137] By contrast, Aizer and Doyle (studying Cook County, Illinois) and Mueller-Smith (studying Harris County, Texas) examined the effects of juvenile and adult incarceration,

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133. Daniel S. Nagin, Francis T. Cullen & Cheryl Lero Jonson, *Imprisonment and Reoffending*, 38 CRIME & JUST. 115, 115 (2009).

134. A second more practical concern is the availability of data that is required to investigate the question, since obtaining a data set with a sufficiently long time series that has individual-level data and links criminal data with employment data can be challenging to obtain. *See* Manudeep Bhuller et al., *Incarceration, Recidivism, and Employment*, 128 J. POL. ECON. 1269, 1270 (2020).

135. A long line of research explores this question. For a review of this literature, see Nagin et al., *supra* note 133; and Aaron Chalfin & Justin McCrary, *Criminal Deterrence: A Review of the Literature*, 55 J. ECON. LIT. 5 (2017).

136. Donald P. Green & Daniel Winik, *Using Random Judge Assignments to Estimate the Effects of Incarceration and Probation on Recidivism Among Drug Offenders*, 48 CRIMINOLOGY 357 (2010).

137. Charles E. Loeffler, *Does Imprisonment Alter the Life Course? Evidence on Crime and Employment from a Natural Experiment*, 51 CRIMINOLOGY 137 (2013).

finding that incarceration increased recidivism.[138] These studies also use a similar methodology of comparing randomly assigned judges. Finally, Bhuller et al., examining the effect of incarceration on recidivism in Norway, found that incarceration reduces the probability of recidivism.[139]

A host of reasons could explain why the results differ across these settings.[140] In all likelihood, context-specific factors that could be structural or contingent account for differences across the studies. One possibility is that incarceration conditions could vary markedly by location. The extent to which a penal system focuses on punishment and incapacitation versus rehabilitation and reentry can vary greatly by jurisdiction. In addition, the reentry support, labor market, community dynamics, and criminal markets all differ, and are just some of the important determinants of recidivism.

Regarding the effect of bail, two recent papers with experimental research designs obtained different results when they examined the effect of bail and pretrial release on recidivism. Gupta, Hansman, and Frenchman found that higher money bail increased rearrest probability by 0.7 percentage points (9%) when they pooled data from Philadelphia and Pittsburgh.[141] Dobbie, Goldin and Yang found that pretrial release has no statistically distinguishable impact on recidivism, but they did find that pretrial release increases rearrest probability by 7.6 percentage points (37.6%) during the pretrial period.[142] Taken together, the work shows modest or no statistically distinguishable effects of rearrest recidivism after the pretrial period.

II. STUDY OBJECTIVES, VENUE, AND DATA

As we have argued above, effective and long-lasting bail reform can only be accomplished through informed and systematic analyses of our current pretrial systems, acknowledging the conditions where they succeed and where they fail. To contribute to this effort and illustrate the importance of conducting these policy analyses in a variety of geographical and

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138. Anna Aizer & Joseph J. Doyle, Jr., *Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges*, 130 Q.J. ECON. 759 (2015); Mueller-Smith, *supra* note 132.

139. *See* Bhuller et al., *supra* note 134.

140. It is worth noting that there are also a number of statistical issues that are important to consider. Bhuller et al., *supra* note 134, at 1274, point out that Green and Winik, *supra* note 136, have a small sample size and weak instrument, and they also mention a point made by Mueller-Smith, *supra* note 132, at 2, that instrumental variable estimates could be biased because they violate two important assumptions of instrumental variables ("IV") analysis: (1) exclusion and (2) monotonicity. The IV methodology is discussed in greater depth in *infra*, Section IV.A.

141. Arpit Gupta, Christopher Hansman & Ethan Frenchman, *The Heavy Costs of High Bail: Evidence from Judge Randomization*, 45 J. LEGAL STUD. 471, 471–73 (2016). The authors do not have results on the pretrial period exclusively.

142. Dobbie et al., *supra* note 39.

institutional contexts, we spend much of the remainder of this Article presenting original results from an experimental empirical study of the pretrial justice system in Pima County, Arizona. This Part commences the analysis by introducing the Pima County pretrial process. We explain why Pima County is a particularly important source of data both for the purposes of our specific empirical approach, at least compared to the venues featured in similar empirical studies and as a venue that differs from those in previous studies in location and demographic composition. We also explore some illuminative descriptive data that provide the context for more technical statistical analyses covered later in Parts III and IV.

A. Why Pima County?

The empirical strategies we employ in this Article place a substantial premium on making unbiased causal claims. Those strategies, however, also impose various requirements regarding the types of data we can use and the institutional and procedural structures of the courts from which those data come. As a result, the first substantial barrier we encountered in this project was finding a suitable study venue. After contacting more than fifty criminal trial court jurisdictions throughout the country, we converged on the Consolidated Justice Court ("CJC") in Pima County, Arizona, where Tucson is located.[143]

First and foremost, Pima County provided us with access to reliable data on the IA process (including judicial assignment procedures), the outcomes of those IAs (including bail assignments), and the criminal history and recidivism rates of the individual defendants involved in those IAs.[144] While this may seem to be a negligible concern, as nearly all U.S. courts have data on the outcomes of criminal cases, many do not have reliable data on the IA process. Additionally, as we discuss in detail below, Pima County's criminal justice system featured an ideal set of institutional and procedural characteristics relating to who makes pretrial release determinations and with how much judicial discretion. Any jurisdictions (of which there are many) that require judges to adhere to non-discretionary bail schedules were not viable candidates, because an IA judge's relative leniency (our instrument) would ostensibly have no impact on the amount of bail that an individual is assigned. For this same reason, a jurisdiction had to have more than one

143. To the extent that empiricists are interested in the information we gathered on the many jurisdictions that are not featured in this analysis, please contact the authors. We note, however that these jurisdictions were not featured because they lacked the institutional requirements outlined above.

144. Related to the data availability, a study of this nature also requires a capable and cooperative court administrative body. We are grateful to the Pima County Courts for their willingness to be a part of this project.

judge involved in pretrial release hearings at a given time period. We found most jurisdictions either have permanent judges or magistrates whose sole responsibility is handling IAs, or, just as problematic, assign just one or two general judges to cover the IA docket for long periods of time (a month or longer). In the former jurisdictions, there would be no variation in IA judge assignment. In the latter, there is technically variation in judicial assignment, but the variation would occur over such large periods of time that the subsequent variation in bail assignments across judges would almost certainly be correlated with the pre-treatment characteristics of the cases within those periods. Finally, our design required a setting where the assignment of the judge or magistrate at the IA was either random or quasi-random, a requirement we discuss at length in Section II.D, below.

In addition to its adherence with the previously discussed design-driven requirements, the Pima County CJC is wonderfully suited for statistical analysis because of how different it is from the locales used in the vast majority of bail studies up to this point. As a review of the empirical literature shows, most of the studies take place in large, densely populated cities and focus on the role of money bail in felony cases. This is particularly true of the four studies that we have previously identified as having done a good job at avoiding the methodological pitfalls described in Section I.D.2, above—all four of these studies focus on a combination of felony and misdemeanor cases[145] in some of the largest population centers in the country. Dobbie et al., for example, analyzed data from Philadelphia and Miami-Dade counties, the seventh and eighth largest metropolitan areas in the United States, and Leslie and Pope looked at criminal cases in New York City, the largest city in the country.[146] Stevenson and Gupta et al. also conducted their studies in Philadelphia, with Gupta et al. adding Pittsburgh, which is the second largest city in Pennsylvania, with more than 2.3 million residents.[147]

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145. There are, of course, many cases that feature both misdemeanor and felony charges. These mixed-level cases are almost always treated as felony cases by empirical researchers and policymakers because of the dominance that the highest-level offense plays in criminal defense strategy, plea bargaining, trial, and sentencing.

146. Dobbie et al., *supra* note 39; Leslie & Pope, *supra* note 76. For the population data and rankings for cities and metropolitan statistical areas, see *Table: Annual Estimates of the Resident Population for Incorporated Places of 50,000 or More, Ranked by July 1, 2019 Population: April 1, 2010 to July 1, 2019*, U.S. CENSUS BUREAU, https://www.census.gov/data/tables/time-series/demo/popest/2010s-total-cities-and-towns.html#tables (last updated Oct. 8, 2021) [hereinafter *Table: Annual Estimates 2019*].

147. Stevenson, *supra* note 76; and Gupta et al., *supra* note 141. For the population data and rankings for cities and metropolitan statistical areas, see *Table: Annual Estimates 2019*, *supra* note 146.

The CJC, on the other hand, is exclusively tasked with adjudicating misdemeanor trials outside of Tucson's city limits.[148] Both of these features—crime type and the jurisdiction's population characteristics—are worthy of independent examination. Previous research has firmly established that the life of a misdemeanor criminal case is substantially different than the life of a felony,[149] a distinction that appears just as substantial at the pretrial stage, although the literature on misdemeanor bail is admittedly less robust.[150] Whereas felony defendants might be held on bail for crimes as serious as rape and murder, recent data suggest that at least three of every four misdemeanor defendants are charged with either non-violent offenses or nothing more than minor public-order offenses such as traffic violations and tax evasion.[151] This would lead one to conclude that more misdemeanor defendants would likely be released previous to trial—either through ROR or manageably lower bail amounts. But evidence suggests this is not true, and that misdemeanor defendants may even be more subject to due process violations during the pretrial process than felony defendants.[152] This is made even more relevant given the fact that "[the number of] misdemeanor charges vastly outpace[s] felony charges."[153]

We also argue that the realities of pretrial practice—including the types of defendants and the perception of and interaction with defendants by pretrial judges—are dependent on the population density of the communities in which the pretrial practice occurs. This positions our Pima County dataset

148. *Limited Jurisdiction Courts*, ARIZ. JUD. BRANCH, https://www.azcourts.gov/guidetoazcourts/Limited-Jurisdiction-Courts (last visited Jan. 2, 2022). Note that the CJC courthouse is in Tucson proper, and while there are two satellite courthouses in Ajo and Green Valley, those courthouses only handle civil matters.

149. For particularly notable views into the misdemeanor justice system more broadly, see Issa Kohler-Hausmann, *Misdemeanor Justice: Control Without Conviction*, 119 AM. J. SOC. 351, 353 (2013); ISSA KOHLER-HAUSMANN, MISDEMEANORLAND: CRIMINAL COURTS AND SOCIAL CONTROL IN AN AGE OF BROKEN WINDOWS POLICING (2018); and THE LOWER CRIMINAL COURTS (Alisa Smith & Sean Maddan eds., 2019).

150. Heaton et al., *supra* note 38, at 732 ("While the Bureau of Justice Statistics has collected extensive information about more serious crimes, there are no nationally representative data available on the numbers of misdemeanor arrests and convictions, let alone data about pretrial detention rates, bail, or sentencing.").

151. *See* JAMES, *supra* note 31, at 3; and Baradaran Baughman, *supra* note 31, at 841 ("The vast majority of these misdemeanor defendants—up to eighty-five percent—are locked up for nonviolent and minor offenses.").

152. *See* Smith & Maddan, *supra* note 32, at 1320–22. *See also* Erica J. Hashimoto, *The Problem with Misdemeanor Representation*, 49 WM. & MARY L. REV. 461 (2013) (chronicling the failure to appoint counsel in misdemeanor criminal cases); K. Babe Howell, *Broken Lives From Broken Windows: The Hidden Costs of Aggressive Order-Maintenance Policing*, 33 N.Y.U. REV. L. & SOC. CHANGE 271 (2009) (attributing due process violations to overburdened court systems and an emphasis on quantity over quality when it comes to misdemeanor prosecutions).

153. Baradaran Baughman, *supra* note 31, at 844; *see* Natapoff, *supra* note 33, at 1063 (estimating that about five misdemeanor cases are filed in the United States each year for each felony case); *see also* Roberts, *supra* note 33, at 294.

and analysis as uniquely important in understanding the avenues for bail reform in America more broadly. In their recent and comprehensive review of the pretrial release systems in seventy-five U.S. counties, Hood and Schneider find that "[s]pecific counties . . . vary considerably, both in the rates at which they use money bail to determine release and in the amounts at which that bail is set," and that such variation is better predicted by demographic and political factors than the individual case characteristics.[154] Worden and Clark highlight the particularly unique nature of rural courts, which differ in "community characteristics, as well as court structure and courthouse culture, produc[ing] different expectations for justice, and different patterns of case outcomes, than might be found in urban or even suburban courts operating under the same statutory and procedural law."[155] And much like how misdemeanor crime is understudied relative to felony crime, studies of rural and suburban courts take a back seat to studies of metropolitan jurisdictions.[156]

The non-Tucson parts of Pima County (again, the jurisdiction covered by the CJC) vary considerably in population density. The Tucson suburbs—e.g., Maran, Oro Valley, Sahuarita, and South Tucson—are demographically similar to Tucson proper, if not slightly less dense, which is a moderately-sized urban area with a population around half-a-million residents.[157] However, Pima County also includes satellite towns such as Green Valley (population of about 25,000), a smattering of smaller towns such as Sells and Ajo (about 3,000 residents each), and the Pascua Yaqui, San Xavier, and Tohon O'odham Native American tribes. This is important because it includes a wider range of rural and suburban segments of the United States population,[158] thereby highlighting the importance of studying bail and pretrial regimes previously ignored in regions like Pima County.

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154. Katherine Hood & Daniel Schneider, *Bail and Pretrial Detention: Contours and Causes of Temporal and County Variation*, RSF: RUSSELL SAGE FOUND. J. SOC. SCIS., Feb. 2019, at 126, 127. Note that although our discussion here focuses on the authors' data on current county-level variation in pretrial regimes, the authors also track the temporal shifts in policy across these counties, beginning in 1990.

155. Alissa Pollitz Worden & Alyssa M. Clark, *Misdemeanor Justice in Rural Courts*, *in* THE LOWER CRIMINAL COURTS, *supra* note 149, at 55, 56.

156. *See id.*; Susan S. Silbey, *Making Sense of the Lower Courts*, 6 JUST. SYS. J. 13, 13 (noting that "limited jurisdiction courts are described as invisible, neglected by the bar, scholars and citizenry, and at the same time as the only judicial experience for most who enter the court system").

157. While Tucson's pretrial data are not featured in our study, the city still plays a substantial role in how we should approach and understand the pretrial system in the Pima County CJC. As of a 2019 estimate by the United States Census Bureau, Tucson was the thirty-third largest city in America at a population of almost 550,000, coming in just before Fresno, Sacramento, and Atlanta. For the population data and rankings for cities and metropolitan statistical areas, see *Table: Annual Estimates 2019*, *supra* note 146.

158. *United States by Density 2020*, WORLD POPULATION REV., https://worldpopulationreview.com/states/state-densities/ (last visited Jan. 2, 2022).

B. The Pretrial System in Pima County

Criminal caseloads in Pima County are handled by three courts: the Tucson City Court ("TCC"), which handles all misdemeanors within Tucson city limits; the CJC, which handles all misdemeanors outside of Tucson; and the Pima County Superior Court ("PCSC"), which handles all felonies within the county. However, all pretrial hearings in Pima County—regardless of the level of crime or location of arrest—are initially handled by the judges and magistrates in the TCC. After arrest or appearance on a warrant, a criminal defendant's arrest report and criminal history are compiled. For all misdemeanor trials that will be handled by the CJC and PCSC, the Pima County Pretrial Services division also creates a pretrial assessment report. For those with at least one felony charge, prior to July 2016, the report included a risk assessment tool developed with University of Arizona faculty where an individual receives a score between -5 and 70 points based on demographic characteristics and criminal history.[159] In July 2016, Pretrial Services started using a risk instrument tool from the Laura & John Arnold Foundation (now Arnold Ventures) for all felony and CJC cases that relies on nine factors and a risk assessment score ranging from 0 to 27 points.[160]

Based on the score cutoffs, each defendant is given an assessment of minimum, low, moderate, elevated, or maximum risk, each of which are suggestive of certain pretrial determinations.[161] Those who receive a minimum score are recommended for ROR, which is among the most common pretrial release outcomes in our dataset. In contrast to all other potential outcomes at pretrial, defendants who are given ROR are simply allowed to leave after the pretrial hearing with the promise (on threat of additional criminal charges) of appearing for a future court date. Those with a low, moderate, or elevated score are recommended for release to Pretrial Services under a plethora of potential conditions, including release on bail, electronic monitoring, drug testing, and other forms of court ordered supervision. Finally, indefinite pretrial detention is recommended for those who are assessed at the maximum risk level. This later condition is very uncommon for the misdemeanor cases featured in the CJC dataset—so uncommon, in fact, that we simply drop such cases from our analysis. In that regard, the vast majority of misdemeanor defendants in the CJC are given at

159. Interview with Michelle Moore, Manager, Pretrial Servs. Div., Pima Cnty. Pretrial Servs. (May 15, 2017).

160. For an in-depth description of the Arnold Foundation risk assessment instrument, see *What is the PSA?*, ADVANCING PRETRIAL POL'Y & RSCH., https://advancingpretrial.org/psa/about/ (last visited Oct. 19, 2021). This shift to the Arnold Ventures risk instrument tool comes into play later in our analysis of judicial discrepancy in Part III (*see infra* Figure 3) regarding the role that such tools might play in mediating the discrepancies in judicial pretrial decision-making.

161. Telephone Interview with Pima Cnty. Superior Ct. Pretrial Servs. (May 15, 2017).

least the theoretical potential for pretrial release, although the high bail assigned to many of them practically prohibits that option.

As we outlined above, one of the important attributes of the Pima County CJC dataset was the possibility of variation among judges in their pretrial determinations. In this regard, the suggestions made by Pretrial Services could pose a problem as they may consolidate judicial tendencies. Although the court administration told us that judges and magistrates follow the Pretrial Services recommendation about 80% of the time,[162] discussions with non-administrative personnel involved in the criminal process suggested that deviation from those recommendations is much more prevalent.[163] Our data analysis below demonstrates substantial inter-judge variation in punitiveness in bail assignment,[164] further suggesting that the recommendations do not impose any serious restrictions on judicial discretion.

Following the Pretrial Services intake, the defendant is brought in for the IA. At the proceeding, the judge or magistrate confirms the defendant's name, reads the charge(s), and sets the release conditions. The judge or magistrate may appoint counsel, and in certain circumstances, may hear from the alleged victims. IAs often last less than three minutes, and in many cases, the defendants do not have lawyers present. Defendants may contest their release conditions in a Motion to Modify Conditions of Release Hearing. If a victim is involved in the crime, the hearing must take place at least five days after the IA in order to give the victim sufficient notice. For victimless crimes, the hearing typically takes place within three to seven days. Whether or not a Motion to Modify Conditions of Release Hearing is held, the charging attorney then evaluates the evidence to determine whether to file formal charges in the case. If the case is not dismissed, most defendants then either have a preliminary hearing or a grand jury hearing, followed by an arraignment that must take place within ten days of the IA if the defendant is in custody or within twenty days if the defendant is not in custody.

C. THE DATASET

The dataset we use in this study covers all Pima County misdemeanor cases resulting from arrests outside the Tucson city limits from 2014 to 2017. The data itself comes from two sources in the Pima County court system. As we discussed above, all pretrial hearings are conducted by judges in the Pima County TCC. Consequently, all the data on the defendant, charges, timing, presiding judge, and outcomes of the pretrial hearings—including the amount

162. *Id.*

163. Upon request, we are keeping the identity of this individual anonymous.

164. *See infra* Part III.

of money bail assigned, if any—is collected in IA forms (see Appendix A: Figure A1 for an example). The TCC provided us with all PDFs of IAs during this period, which we then hand-coded to create a working dataset of pretrial information.[165]

We then merged this pretrial PDF data with the administrative court data held by the CJC.[166] The CJC court data included defendant demographic data, criminal history, and case data. Demographic data included the defendant's name, gender, race, ethnicity, date of birth, address, and zip code, among other information. The case data included charges, court proceedings (including bail modification hearings), failures to appear, court transactions, and dispositions. After merging the two data sets, we were left with 29,771 IAs over the time period.[167] Table 1 (below) shows the descriptive statistics for our sample. We report means for the full sample and also for the sub-groups of defendants who were and were not assigned monetary bail. The first set of statistics report the demographic characteristics of the defendants in the sample. The sample is 74.4% male, 47.3% white, and 36.8% Hispanic/Latinx. Black and Native American defendants are roughly in equal proportion in the sample, respectively at 6.5% and 6.2%. Importantly, the racial make-up of our sample differs markedly from the other locations where similar studies have been done, with a higher percentage of Hispanic/Latinx and Native American defendants and a lower percentage of Black defendants, which might be a contextual factor that could help explain

165. We hired a firm to hand-record these IA forms into a dataset, employing a rigorous audit process to ensure the quality of the coding process.

166. The use of this dataset comes with three important limitations. First, the criminal history data only relate to the defendant's past in the CJC, so our measurements of recidivism (both charge/arrest and guilty judgments) are almost certainly understating the average defendant's propensity for committing crime in the future. Second, because the PDF data did not include outcomes for the charges that were brought against the defendant at the IA, we had to rely on the merged case data, which could only be matched with the IA data to the extent that the demographic data allowed. Some of the PDFs did not successfully merge with the court data, so those PDFs were excluded from this study. Similarly, our third limitation derives from the fact that we had to determine which charges in the case data were IA charges but only had "offense date" data at the charge level. Because some charges originate weeks if not months before a defendant appears in court, we had to determine whether charges that had offense dates before the IA date would be defined as criminal history or IA charges. In the dataset we use in this Article, any charges with offense dates that occurred before or within two days of the IA date but did not have a disposition date that occurred before the IA date were defined as IA charges. We also ran our analysis on datasets in which (1) only charges with offense dates within 100 days of the IA date and disposition dates after the IA date are defined as IA charges and (2) only charges with offense dates within five days of the IA date and disposition dates after the IA date are defined as IA charges. The results of the analysis using these alternative datasets—including balance tests, first stage results, and second stage results—did not yield any appreciably different outcomes. These results can be requested by contacting the authors.

167. For the analyses in Parts III and IV we omit the 6,092 IAs that did not have judge data or were assigned to judges who had fewer than 200 IA cases over the course of the study. This resulted in a working dataset of 23,679 IAs. *See infra* note 179 and accompanying text for more details.

different results. The average age in our sample is thirty-five years, and approximately half (50.1%) are Tucson residents. Roughly one-quarter of the sample has at least one previous guilty charge, and the mean number of past guilty charges for the sample is 0.55. Based on demographics, the means are relatively close for most of the attributes when comparing those who were assigned money bail with those who were not, with four important exceptions. The proportion of males, Tucson residents, and those with a criminal history—based both in terms of whether the individual had at least one past guilty charge and the number of previous guilty charges—is higher for defendants assigned money bail versus those who were not.

Roughly half (50.3%) of the sample was assigned non-monetary release conditions. For the full sample, individuals had an average of 2.72 charges either pending or brought at the IA. 34.5% had assault charges, 35.3% had drug charges, 13.3% had DUI charges, 30.2% had FTA charges, and 27.6% had theft charges at the IA. Individuals who had those charges were more likely to be assigned monetary bail. Defendants assigned money bail overall had 3.23 charges, compared to 2.40 charges for those who were not assigned money bail. The only category where a higher percentage of defendants were not assigned money bail was DUI, where 13.6% of those charged were not assigned money bail, compared to 12.8% of defendants charged with a DUI who were assigned money bail.

Turning to our outcomes of interest—with the caveat that we cannot confidently compare the two groups until we engage in the more sophisticated empirical techniques featured in Part IV—we see marked differences in guilty pleas and FTA rates between the two groups. Just over 51% of defendants assigned money bail pleaded guilty, while 40% did the same among those who were not assigned money bail. The FTA rate was 10.5% for those assigned money bail versus 12.9% for those who were not. The probability of a guilty judgment was nearly the same for the full sample and the two groups: just around 3%. With respect to future crime, defendants assigned money bail are less likely to be rearrested across all time windows and are either less or roughly equally likely to receive a conviction after a rearrest charge. We discuss whether these discrepancies persist after controlling for the relevant characteristics of the individuals and cases in these groups in Part IV.

Table 1: Descriptive Statistics

	Mean of Full Sample	Mean of Defendants Assigned Monetary Bail	Mean of Defendants Not Assigned Money Bail
Defendant Characteristics			
Sex/Gender (Male)	0.744	0.780	0.721
Race/Ethnicity (Black)	0.065	0.068	0.063
Race/Ethnicity (Hispanic/Latinx)	0.368	0.366	0.369
Race/Ethnicity (Native American)	0.062	0.068	0.059
Race/Ethnicity (White)	0.473	0.466	0.478
Age at Time of IA (Years)	34.920	34.933	34.911
Tucson Resident	0.501	0.562	0.461
Criminal History (Past Guilty Charge)	0.238	0.282	0.208
Criminal History (Number of Past Guilty Charges)	0.548	0.667	0.469
Bail Decision			
Nonmonetary Bail	0.503	0.383	0.582
Charge Characteristics			
Number of New and Pending Charges at IA	2.727	3.229	2.396
Any Assault Charges	0.345	0.388	0.316
Any Drug Charges	0.353	0.411	0.315
Any DUI Charges	0.133	0.128	0.136
Any FTA Charges	0.302	0.380	0.251
Any Theft Charges	0.276	0.386	0.203
Dispositions of New and Pending Charges at IA			
Guilty Plea	0.445	0.514	0.400
Guilty Judgment	0.030	0.030	0.031
Failure to Appear (FTA)	0.120	0.105	0.129
Post-IA Recidivism Measures			
New Charge Made in 0-6 Months	0.214	0.201	0.223
New Charge Made in 0-12 Months	0.278	0.266	0.286
New Charge Made in 0-18 Months	0.311	0.301	0.317
New Charge Made in 0-24 Months	0.331	0.321	0.337
Conviction on Arrest Charge Made in 0-6 Months	0.152	0.147	0.156
Conviction on Arrest Charge Made in 0-12 Months	0.203	0.201	0.205
Conviction on Arrest Charge Made in 0-18 Months	0.229	0.229	0.230
Conviction on Arrest Charge Made in 0-24 Months	0.247	0.247	0.247
Number of Observations			
Full Sample	23,679	9,422	14,257
6-month Censored Dataset	20,481	8,070	12,411
12-month Censored Dataset	17,588	6,931	10,657
18-month Censored Dataset	14,405	5,777	8,628
24-month Censored Dataset	11,688	4,779	6,909

D. CONFIRMING QUASI-RANDOM IA JUDGE ASSIGNMENT IN PIMA COUNTY

The identities of the pretrial decision-makers in Pima County and the way in which they are assigned to pretrial hearings is of particular importance to our empirical study. More specifically, it is critical that the TCC judges who make the bail determinations in Pima County are assigned to IA hearings in a manner that does not account for or even reflect the characteristics of the defendant or the defendant's criminal case.[168] This is because independent judicial assignment ("quasi-random" assignment in the case of Pima County) allows us to compare credibly the inter-judge disparities in bail assignments[169] and then reliably isolate and identify the causal impact that being assigned to a less "lenient" IA judge has on the various downstream outcomes of interest to bail scholars and policymakers.[170] As a result, we spend considerable time in this Article discussing the assignment procedures and the outcomes of the balance tests (even relative to the previous articles using similar methods), while keeping the more technical descriptions of our approach in footnotes and appendices.

For most crimes in Pima County, after a law enforcement agency arrests and books suspects, they are scheduled for an IA within twenty-four hours. IAs are held at 9:00 a.m. and 8:00 p.m. seven days a week at a minimum detention facility. Defendants, and often their attorneys, participate by videoconference from the Pima County Jail; the magistrate and prosecutors are present at the proceeding. There are nine permanent judges and a group of ten pro-tempore ("pro-tem") judges from the TCC serving at any given time. The judge assignment calendar for IAs and other assignments is compiled monthly in a loose rotation accounting for other assignments they may have and some scheduling preferences of the judges. Judges can swap shifts after the initial assignments are distributed, but both in-person interviews and a comparison of the scheduled versus actual assignment reveal diversion from the assigned shifts is uncommon. One important worry is that judges could have sufficient control over their schedules so that they would not have similar caseloads. If that was the case, the balance results would show specific judges had a greater share of certain types of cases or defendants. The rotation system coupled with our statistical balance tests outlined below demonstrate that the caseloads of the judges are balanced over the course of the time period we are studying in terms of defendant characteristics, charges, and a number of other important variables.

168. For a technical overview of the sorts of downstream experiments that we employ in this Article's analysis, see ALAN S. GERBER & DONALD P. GREEN, FIELD EXPERIMENTS: DESIGN, ANALYSIS, AND INTERPRETATION 196–204 (Ann Shin et al. eds., 2012).

169. *See infra* Part III.

170. *See infra* Part IV.

Importantly, defendants also have limited control over the judge to which they are assigned. After the defendant is arrested and booked, the individual is automatically assigned to the next IA time. Defendants could, in theory, strategically time their arrest or exploit timing close to a cutoff time for a hearing, but the likelihood of this strategic behavior occurring is small.

A number of features of the judge assignment system result in balanced caseloads across judges, resulting in what is often called "quasi-random" assignment. Based on the institutional structure we have described above, the system used to assign TCC judges to the IAs for criminal cases appears to be conducted in a way where the judges' individual caseloads were assigned in a way that was not influenced or correlated with any of the outcomes of interest and other factors (covariates) that might affect those outcomes. Nonetheless, the assignment process in Pima County is not strictly random because IA judges have some control over their assignments.[171] As a result, it is possible this "quasi-random"[172] assignment process may introduce systematic biases into the instrument of our model—the assignment of judges—that would prevent us from identifying the unbiased causal effect of our treatment. Such biases might be due to selection effects (e.g., certain judges prefer to take shifts having a higher likelihood of

171. For a technical definition of random judicial assignment, see Dane Thorley, *Randomness Pre-Considered: Recognizing and Accounting for "De-Randomizing" Events When Utilizing Random Judicial Assignment*, 17 J. EMPIRICAL LEGAL STUD. 342, 354 (2020) ("Statistically random judicial assignment can be defined as an assignment procedure in which cases are allocated independently of any value, characteristic, or variable other than an exogenous assignment mechanism. The specific assignment mechanism used by a court can be any process under which the probability of assignment is known by the court (or the researcher) and is greater than 0 and less than 1 (meaning any given case has at least some chance of being assigned to each treatment category). Such assignment mechanisms are most commonly computer-generated random numbers but may be as rudimentary as sequentially drawing judges' names from an envelope (the method for assigning homicide cases in the Nassau County Criminal Court) or a hat.").

172. *See id.* ("As-if random assignment (also known as quasi-random assignment) occurs when a case's treatment category is based on one or more of that case's pretreatment characteristics (i.e., the assignment is not random) but, importantly, pretreatment characteristics that are unrelated to that case's potential outcomes."). Three of the four articles that use judge assignment as an instrument for causal claims (see our broader discussion of these works in *supra*, Section I.D.ii, and *infra*, Section V.B) feature empirically verified, quasi-random judicial assignment, even if some of the language (and titles) suggest truly random assignment processes. *See* Dobbie et al., *supra* note 39, at 201 ("This paper uses the detention tendencies of quasi-randomly assigned bail judges to estimate the causal effects of pretrial detention on subsequent defendant outcomes."); Stevenson, *supra* note 76, at 513 ("There is one centralized bail hearing room for the entire city, and magistrates work a rotating schedule that creates random variation in which magistrate is on duty. Over time, each magistrate will work an equal number of night shifts, weekend shifts, etc."); Gupta et al., *supra* note 141, at 477 ("The centralized location, large case load, constant process, and rotating magistrate calendar result in the effectively random assignment of defendants to magistrates (an assumption we test)."). The study by Leslie and Pope features a mixture of conditionally random and quasi-random assignments. *See* Leslie & Pope, *supra* note 76, at 530–46 ("We present evidence that assignment to an arraignment judge is conditionally random for felony cases," noting there were "randomization issues in the misdemeanor subsample").

featuring certain defendants) or simply due to incidental relationships between the assignments and the potential outcome of interest that would, in expectation, be absent under a truly random process.

In order to verify that the judge assignment process does not feature such biases and can therefore be used as an appropriate instrument in the first stage of our project's empirical design, we conducted a series of balance tests on the CJC data. As we have previously argued in other work,[173] it may not be enough to simply run basic covariate balance tests, as those tests are only valid to the extent they include the relevant pre-treatment covariates (the full set of which is impossible to verifiably know)—just as with a researcher-driven randomized experiment (e.g., a randomized controlled trial), the assignment process itself has to be explored and then incorporated into the empirical balance tests.[174] We also ran a second battery of tests using analysis of variance ("ANOVA") multivariate logit regressions, which test how strongly those pre-treatment covariates are correlated with judicial assignment. We focus on the balance tests here and discuss the ANOVA tests in the Appendix.[175] Nonetheless, the results of both approaches indicate the independence assumption required for a valid instrument is almost certainly met.[176]

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173. *See* Thorley, *supra* note 171.

174. The balance tests we employ measure the distribution of various case and defendant characteristics of the set of IAs over which each judge presided against the distributions we would expect to see if the judicial assignment process was truly random (the latter of which we create computationally). Our balance tests are made up of two data components: the actual observed distribution of IAs across judges and a reference distribution of all the possible distributions of IAs that were created through Randomization Inference ("RI"). The observed distribution is simply the distribution of IA assignments in the dataset that we procured from the CJC. The reference distribution is created by re-assigning the complete sample of IAs to the judges using the same probabilities of assignment that existed in the observed distribution. To replicate the current assignment procedure as closely as possible (and to account for time-fixed effects), we randomize assignments in day-level clusters and month-level blocks. Using a sampling of 20,000 permutations, we re-assign each of the IAs to the same set of judges using the same set of probabilities repeatedly to "create" 20,000 datasets that mirror our observed data, except that the process of assignment is actually random. Combining these randomized datasets creates judge-level distributions of each of the covariates we test. We then compute the 95% confidence intervals for each of these distributions simply by finding the distribution values at the 2.5 and 97.5 percentiles. If the actual value we observe in the court data is above or below these confidence intervals, then we are unable to reject the null hypothesis of actual randomization for that particular judge-covariate pair, strong evidence that the system of judicial assignment is functionally equivalent to a truly random process. Note that an approximate Monte Carlo simulation is necessary here due to the astronomically large number of permutations that our sample sets of cases would produce. While still technically approximate, a simulation of 20,000 permutations will be more than enough to produce test statistics that asymptotically approach the true test statistics under the full set of permutations.

175. *See infra* Appendix B.

176. In both the balance tests and the ANOVA regressions, we use randomization inference, a credible, non-parametric variation of Fisher's exact test that allows us to compare the observed distribution of IA assignments against the full set of hypothetical distributions of IAs in a system

We ran these balance tests for the ten IA judges who were assigned 200 or more IA hearings from 2014 to December 2017 and a combined pool of the judges and magistrates who were assigned less than 200 cases on the following pre-treatment covariates: the sex of the defendant, whether the defendant is Hispanic/Latinx (a proxy for race/ethnicity), the number of charges brought against the defendant in their IA hearing, various binary indicators for the types of charges brought against the defendant in the IA, the number of previous charges of which the defendant was found guilty, the age of the defendant at the time of the IA, and whether the defendant is a Tucson resident.

The results of the balance tests are presented in Table 2 (below). Column 1 of Table 2 specifies the judge,[177] and the remaining columns report the average or percent for each of the pre-treatment covariates we test, with the p-value in parentheses. Because Table 2 includes the balance results for 141 independent tests (11 judges multiplied by 13 pre-treatment covariates), the results that failed the balance tests after accounting for the multiple comparison problem[178] are set in bold.

In total, 13 of the 141 tests indicate a statistically significant imbalance of a given case or defendant characteristic across judges. However, all but three of these balance "failures" occur for covariates in the combined pool of judges and magistrates, strongly indicating the assignment of IAs to these particular judges is not random at all. This is not particularly surprising, given the supplementary role of magistrates. They are probably more likely to regularly take certain, unpopular shifts and are the first in line to fill in for

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that is truly and perfectly random. *See* GERBER & GREEN, *supra* note 168, at 115. Using RI to construct our p-values and standard errors is preferable to the standard approach taken in the literature for two reasons: RI does not require modeling assumptions, and RI is intuitive and simple.

177. To protect the identities of the judges, we have anonymized all of the judge-level data presented in this Article. Some may argue that knowing which individual judges are associated with what data is even more important than knowing about the general disparities themselves, either due to the general importance of transparency or due to the "shaming" effect that public disclosure may induce on the judges' subsequent behavior. While we appreciate both of these perspectives (although we argue the effect of data transparency on judicial behavior is far from well-identified in the literature), our purpose in conducting this study was not to publicly expose or critique the Pima County judges. Additionally, our access to the data was contingent upon an agreement that all judge-level data would stay anonymous.

178. While p-values below 0.025 and above 0.975 suggest a statistically significant difference between the observed mean and the binomial distribution before accounting for multiple comparisons, it is important to note that because of how many tests are included in our balance analysis we should expect to see a number of natural "failed" tests (i.e., those with p-values low enough to suggest that we can reject the null hypothesis of as-if-random assignment) due simply to chance, even when the IA assignment process is truly random. We can account for this problem, often called the multiple comparison problem, by applying an alpha-correction, such as the Bonferroni-Holm procedure which accounts for the number of tests and distribution of p-values to re-calculate the level at which rejection should be made. *See* Sture Holm, *A Simple Sequentially Rejective Multiple Test Procedure*, 6 SCANDINAVIAN J. STAT. 65, 66–68 (1979).

others who are unable to abide by the initial shift assignments. In any case, the imbalances are substantial enough that we drop the IAs assigned to this combined pool in our instrumental variables ("IV") analysis below.[179]

With regard to the remaining judges, the results of these balance tests provide strong evidence the IA assignment process used in Pima County is as-if-random, resulting in equivalent caseloads across those judges. When comparing the judge-level values of our pre-treatment covariates against the full distribution of those covariates under a judicial assignment scheme that is truly random, only three of the remaining 130 balance tests failed (none of which "survive" our multiple comparisons correction, so they are "true" failures): Judges 4 and 8 have a very high proportion of Tucson residents and Judge 9 has a very high proportion of IAs with a drug charge relative to what we would expect. However, because these failures are rare and are not clustered on one particular judge or pre-treatment covariate, the overall results of the balance tests comport with the conclusion that those deviations are most likely the result of conducting so many tests.

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179. We also drop the IAs that do not have an IA judge in the data. As a result, the sample size of IAs drops from 29,771 to 23,679.

Table 2: Balance of Pre-Treatment Covariates Across IA Judges

IA Judge	% Female	% Hispanic	# of IA Charges	# of Previous Charges	Age at IA (Years)	% Tucson Resident	% DUI at IA	% Drug at IA	% Disorderly at IA	% FTA at IA	% Theft at IA	% Trespass at IA	% Assault at IA
Judge 1	0.757 (0.856)	0.366 (0.238)	0.578 (0.978)	2.711 (0.815)	35.043 (0.835)	0.512 (0.980)	0.040 (0.987)	0.094 (0.876)	0.078 (0.908)	0.084 (0.822)	0.074 (0.791)	0.071 (0.912)	0.073 (0.932)
Judge 2	0.743 (0.378)	0.361 (0.202)	0.623 (0.999)	2.702 (0.741)	35.408 (0.966)	0.494 (0.601)	0.033 (0.765)	0.094 (0.952)	0.069 (0.661)	0.086 (0.947)	0.080 (0.974)	0.072 (0.950)	0.066 (0.806)
Judge 3	0.739 (0.264)	0.366 (0.271)	0.474 (0.655)	2.676 (0.693)	34.679 (0.411)	0.505 (0.834)	0.033 (0.834)	0.070 (0.078)	0.082 (0.990)	0.077 (0.779)	0.067 (0.714)	0.048 (0.074)	0.068 (0.858)
Judge 4	0.743 (0.322)	0.392 (0.856)	0.660 (0.988)	2.832 (0.969)	34.850 (0.643)	**0.533*** **(1.000)**	0.031 (0.545)	0.100 (0.739)	0.081 (0.773)	0.083 (0.445)	0.077 (0.644)	0.077 (0.889)	0.073 (0.701)
Judge 5	0.744 (0.472)	0.374 (0.476)	0.512 (0.752)	2.674 (0.557)	34.94 (0.565)	0.548 (0.993)	0.030 (0.663)	0.088 (0.653)	0.096 (0.982)	0.061 (0.202)	0.079 (0.859)	0.063 (0.608)	0.087 (0.964)
Judge 6	0.723 (0.255)	0.351 (0.338)	0.172 (0.794)	2.896 (0.871)	34.826 (0.446)	0.506 (0.013)	0.027 (0.786)	0.029 (0.918)	0.015 (0.172)	0.007 (0.218)	0.029 (0.785)	0.039 (0.954)	0.017 (0.362)
Judge 7	0.736 (0.236)	0.365 (0.198)	0.574 (0.978)	2.793 (0.952)	35.169 (0.868)	0.505 (0.833)	0.034 (0.865)	0.087 (0.657)	0.095 (0.999)	0.079 (0.643)	0.079 (0.927)	0.082 (0.997)	0.075 (0.934)
Judge 8	0.735 (0.09)	0.368 (0.222)	0.520 (0.862)	2.705 (0.807)	34.623 (0.245)	**0.513*** **(1.000)**	0.030 (0.668)	0.095 (0.947)	0.077 (0.913)	0.079 (0.676)	0.077 (0.957)	0.077 (0.996)	0.078 (0.991)
Judge 9	0.744 (0.408)	0.348 (0.015)	0.525 (0.867)	2.782 (0.921)	35.099 (0.828)	0.511 (0.963)	0.034 (0.881)	**0.119*** **(1.000)**	0.076 (0.816)	0.095 (0.994)	0.079 (0.946)	0.059 (0.382)	0.078 (0.980)
Judge 10	0.765 (0.961)	0.380 (0.706)	0.544 (0.895)	2.659 (0.597)	34.8 (0.605)	0.513 (0.968)	0.028 (0.367)	0.094 (0.850)	0.077 (0.847)	0.084 (0.815)	0.070 (0.597)	0.063 (0.610)	0.068 (0.630)
Other Judges**	0.751 (0.793)	**0.401*** **(1.000)**	**0.230*** **(0.000)**	**2.227*** **(0.000)**	**34.029*** **(0.000)**	**0.479*** **(0.0000)**	0.019 (0.008)	**0.052*** **(0.000)**	**0.034*** **(0.000)**	0.056 (0.001)	**0.040*** **(0.000)**	**0.037*** **(0.000)**	**0.038*** **(0.000)**

Notes: P-values for each covariate-judge observation are presented in parentheses and were calculated using 20,000-permutation randomization inference where judicial assignments were randomly reassigned in month-level blocks. The IAs for which there was no judge-assignment data were also included in these reassignments.

*Observations in bold and designated by * are statistically significant at the .05 level after adjusting for multiple comparisons using the Bonferroni-Holm correction.

**All IAs assigned to a judge other than the ten primary IA judges in our sample were combined into one group. The vast majority of these judges are magistrate judges.

III. IDENTIFYING INTER-JUDGE DISPARITIES IN IA DECISION-MAKING

Now that we have established that the assignment of judges to IA hearings in Pima County is done as-if-randomly and have provided a descriptive introduction of our working dataset, we present two primary empirical analyses. We begin here in Part III by measuring the inter-judge disparities in IA outcomes, both across the entire dataset and across various sub-groups of IA hearings. In Part IV, we then explain and conduct the instrumental variable analysis that allows us to measure the downstream causal impact that money bail has on defendant behavior and case outcomes.

A. Disparities in Assigning Money Bail

With one exception—which we explore below—all of the TCC judges in Pima County were governed by the same set of rules and restrictions when making IA release decisions over the entire time period that our data covers. Additionally, as we described previously, the Pretrial Services Division provided all IA judges with a report, which includes a release recommendation. In interviews with administrative personnel before we collected the data, we were told that IA judges follow these recommendations for about 80% of IA hearings.[180] One might reasonably expect, then, that any inter-judge disparities in either the money bail assignment frequency or the average amount of money bail assigned would be fairly minimal.

Our data do not support these conclusions, even with the understanding that different judges with different perspectives and philosophies will naturally produce some variance over time. Figure 1 (below) displays the percentage of IA hearings resulting in the assignment of money bail by judge, ordered from the most "lenient" judge (measured purely based on the likelihood that the judge assigns money bail) to the "strictest" judge. As the data demonstrate, there are substantial decision-making disparities across the ten IA judges that are included in our dataset: while Judge 1[181] assigns money bail in only 20% of IA hearings, Judge 10 does so in nearly 60% of IA hearings. Standard statistical tests[182] demonstrate that this difference is statistically significant at the $p < .05$ level, suggesting that the differences between the judges are almost certainly not due to chance or statistical noise. Indeed, other than the differences between the rates of assigning money bail

180. Telephone Interview with Pima Cnty. Superior Ct. Pretrial Servs. (May 15, 2017).

181. As detailed earlier (*see supra* note 177), we have anonymized all judges featured in this Article. The judge numbers in Figures 1–2 do not correlate with the judge numbers in Table 2.

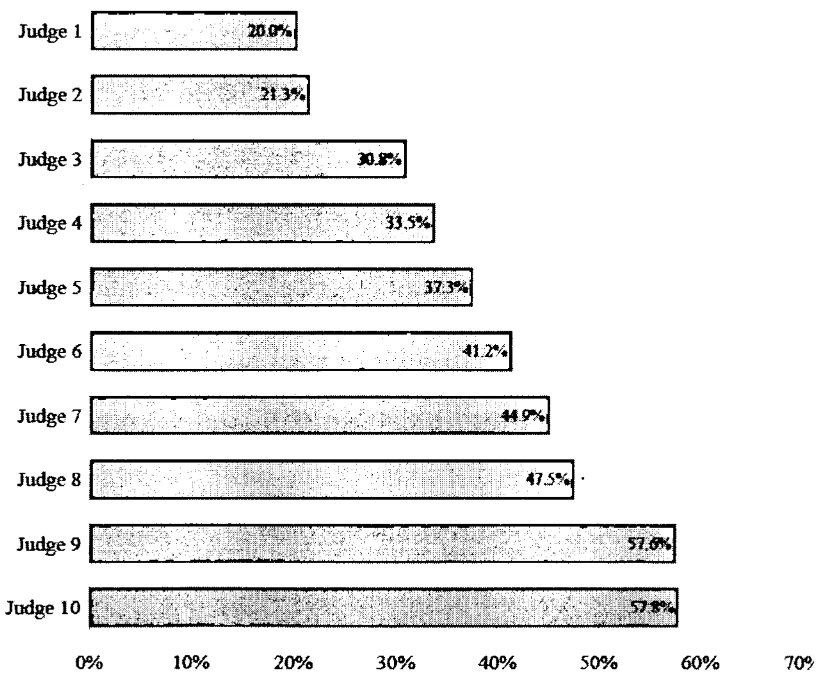
182. All of the figures in this Section run standard t-tests to compare the differences across judges.

among just four judge pairs,[183] the rates of the other forty-one judge pairs are also statistically distinguishable at $p < .05$.

These discrepancies are difficult to overstate. They indicate defendants assigned to Judge 10 are nearly 40 percentage points more likely to be assigned money bail as those assigned to Judge 1. Put more crudely, being charged with a crime on a Friday instead of the following Monday increases the likelihood of having to pay money bail to be released *three times* simply because of the judges that happened to be assigned on those days.

Importantly, as we already established through statistical tests, all ten of these judges were as-if-randomly assigned to the IA hearings in our dataset and have a nearly identical corpus of IA hearings across measurable and non-measurable case and defendant characteristics.[184] Consequently, the differences in IA release decisions are not attributable to variations in the types of defendants appearing in front of those judges or differences in their

Figure 1
Judge Variation in Likelihood of Assigning Money Bail



183. These pairs are Judges 1 and 2 (a 0.9 percentage point difference), Judges 3 and 4 (2.7 percentage points), Judges 7 and 8 (1.7 percentage points), and Judges 9 and 10 (1.3 percentage points).

184. While random assignment only guarantees that the distribution of hearings across judges will be equal in expectation, the statistical tests discussed in Section II.D demonstrate that the case assignments are equal in practice as well.

cases. Additionally, because we are only focusing on judges with a substantial number of IA hearings during the time period of the dataset, each judge-level observation reflects decisions over an average of 2,379 IA hearings.[185]

But what about the *amount* of money bail assigned by these judges? A measure of judicial severity that relies exclusively on the likelihood of assigning money bail may not accurately capture the impact that judicial behavior has on IA defendants because a judge assigning money bail in over half of his or her IA hearings (e.g., Judges 9 and 10) may nonetheless be assigning substantially lower amounts of bail per hearing, posing less of a financial hurdle for the defendant to overcome in order to be released prior to trial. Alternatively, the judges who assign money bail in the fewest IAs (e.g., Judges 1 and 2) may reserve such action only for the defendants they deem to be the highest flight risk, danger to the community, or deserving of detention. In other words, the amount of bail assigned may be just as important as the existence of money bail itself.

To account for this phenomenon, Figure 2 (below) presents the average bail amount (in dollars) assigned across the same ten judges (in the same order as presented in Figure 1). We present bail amount in three ways to account for some nuances in the data. The first column in each judge-level group in Figure 2 is the raw average of bail amount assigned, where IAs in which no amount was assigned are given a value of $0. The second column, by contrast, omits data from all IAs where money bail was not assigned, so the associated values reflect the average of only IAs with positive bail amounts. The third column is a slight variation of the second, where extreme outliers are excluded, so that the associated values reflect the average of only IAs with positive bail amounts below $32,215. Because bail above that amount is exceedingly rare in a dataset consisting solely of defendants charged with misdemeanors, only fifty-three extreme outlier observations were dropped.[186]

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185. The ten judges averaged 2,368 IA hearings. The judge who presided over the fewest hearings had only 413, and the judge presiding over the most hearings had 5,097 (the standard deviation was 1,273).

186. As is often the case with outlier data, it is unclear whether these outliers result from some recording error on the part of the Pima County TCC or a truly high bail assignment. The outliers in this dataset, as defined by any amount above three standard deviations of the sample (which was limited to only positive bail amounts for the purposes of these calculations), spanned from $40,000 to $750,000, with two particularly large groupings at $50,000 (26 observations) and $75,000 (12 observations). While these amounts are quite high for defendants only charged with a misdemeanor (at least at the present IA) and might therefore be explained by an inadvertent extra 0 during coding, 27 of the 53 outliers come from only two of the IA judges (Judges 2 and 6), who are disposed to assigning the highest average bail amounts even when outliers are accounted for, so it is not unreasonable to assume that these amounts are genuine.

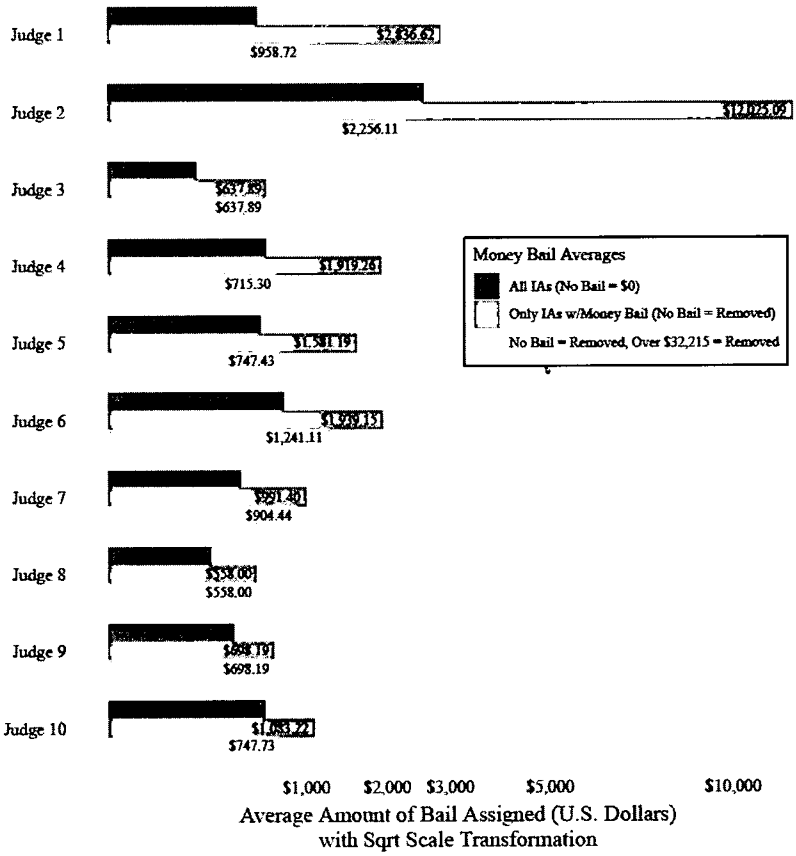
The results in Figure 2 are similarly striking as those in Figure 1, both in terms of the base-level disparities in bail amounts assigned across judges and in the difficulty in finding a pattern in those disparities in relation to the likelihood of assigning bail. Looking first at the raw averages in the first set of columns for each judge (where a non-monetary release is coded as $0), we see Judge 3, who was the third least likely to assign money bail, has the lowest average bail assignment at $196 per IA. Perplexingly, Judge 2 has the highest average at $2,548 per defendant, even though he or she is only slightly more likely to assign money bail than Judge 1. In fact, there appears to be no direct relationship between the likelihood of assigning bail and the raw average amount assigned—the increasing order here being Judges 3, 8, 9, 7, 1, 5, 10, 4, 6, and 2.

This mixed relationship remains more or less the same when non-monetary bail assignments are excluded from averages (the second column in each judge-triad in Figure 2). Judge 2 is still the harshest in terms of the average bail amount assigned at a staggering $12,000 per defendant, with Judge 1 at a distant second at $2,800. Removing the fifty-three exceedingly high bail amounts from the calculation (the third set of columns in Figure 2) does not do much to change the overall ordering of judges, but it does lower the averages of the strictest judges, suggesting that some (but not all) of the difference was driven by outliers.[187]

Regardless of which calculation is the most accurate reflection of judicial behavior, it is clear inter-judge disparities in the amount of bail assigned are just as pronounced—if not more so—than whether or not bail is assigned at all.

187. Combined, Judges 2 and 6 assigned a majority of the 53 outlier bail amounts (14 and 13, respectively).

Figure 2
Judicial Disparities in the Amount of Money Bail Assigned



B. Disparities Within Subgroups of Defendants

To shed light on how and why these disparities occur, we also present data on inter-judge disparities across various sub-groupings of the data. We begin with a comparison in the likelihood of assigning money bail before and after the Pima County courts began providing IA judges with the Arnold Foundation Risk Instrument scores for misdemeanor defendants in July 2016. Recall that prior to July 2016, judges making pretrial release determinations in Pima County were provided only with a report by the Pretrial Services Division. After July 2016, however, judges were also given a risk assessment score that accounted for nine factors, including age, prior criminality, prior failures to appear, and the seriousness of the current charges.[188] While neither the pre-July 2016 reports nor the Risk Instrument scores provided the judges with any mandatory restrictions on their discretion at the pretrial stage, the Risk Instrument was intended to function as a less-biased and more-uniform basis for release determinations.

Figure 3
Judicial Disparities in Likelihood of Assigning Money Bail (Risk Instrument Comparison)

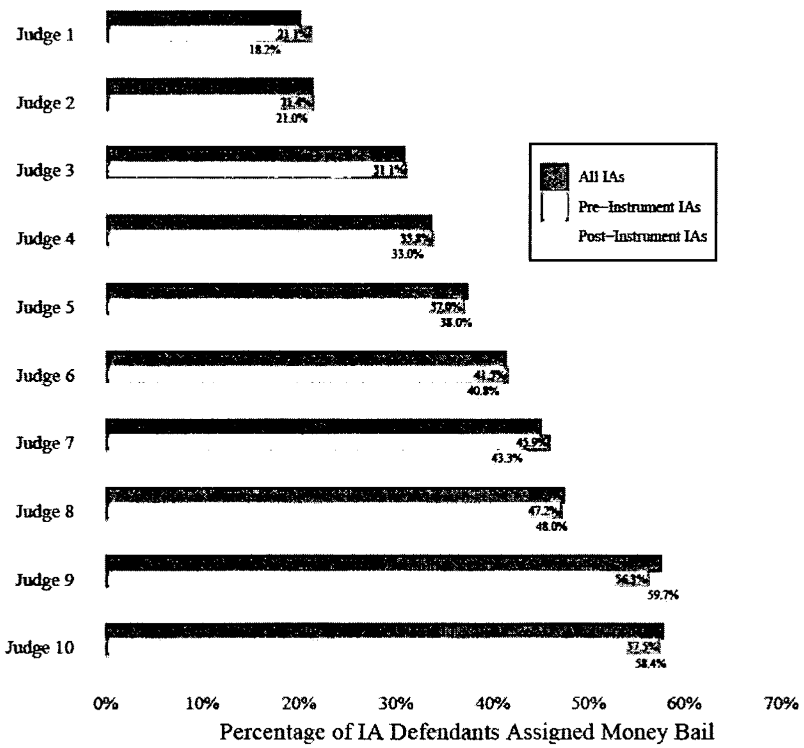


Figure 3 (above) displays the likelihood of assigning money bail across the two time periods in the data. The first column in each judge-level triad is the likelihood of money bail across the entire dataset (i.e., the same data presented in Figure 1, above). The second and third columns are those likelihoods before and after the Risk Instrument was implemented. While

188. See *supra* note 160 for a discussion of the Arnold Foundation PSA.

five of the judges become more lenient in the post-instrument period,[189] only two vary in a statistically significant way (Judges 1 and 9), and those differences are substantively small (roughly 3 percentage points each). As a consequence, the striking inter-judge disparities observed in the full sample of data are just as pronounced after the implementation of the Risk Instrument as they were before. We highlight the implications of these findings further when we discuss potential solutions to judicial disparity in Part V, below.

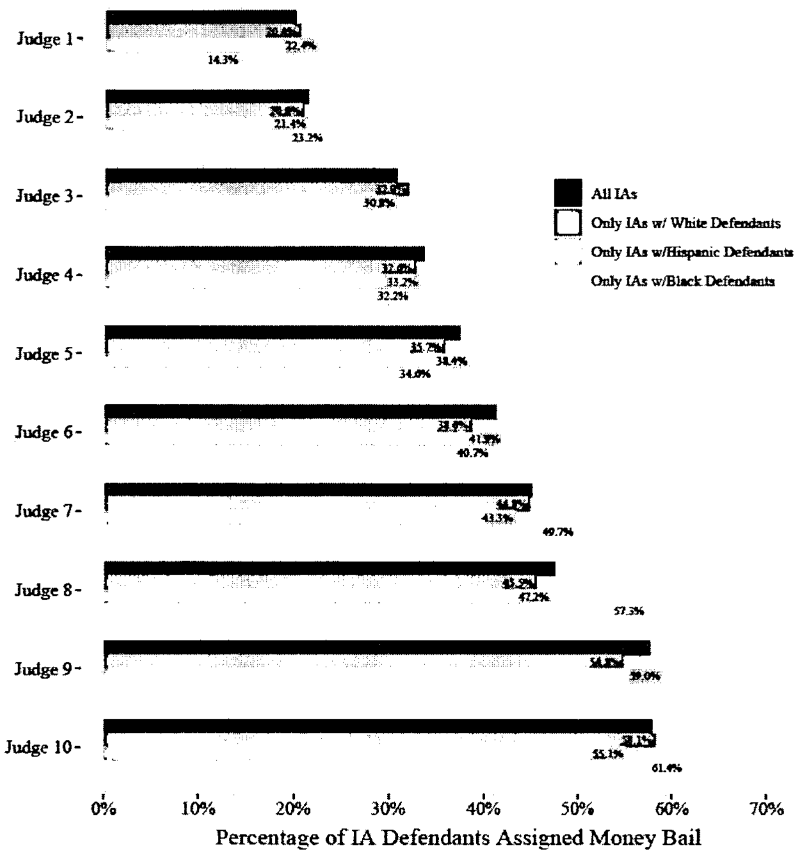
We also calculate the probability in our sample of each judge assigning money bail across race/ethnicity and gender. These data are presented in Figures 4 (below) and A2 (included in Appendix A: Figure A2). Unlike the absence of variation in judges' behavior before and after the implementation of the Risk Instrument, there is variation in disparity across race/ethnicity, particularly with respect to Black defendants. Specifically, some judges assign money bail at different rates for Black defendants than they do white and Hispanic/Latinx defendants. Judge 7, for example, is almost 5 percentage points more likely to assign money bail to Black defendants than to white defendants (an 11% increase), and Judge 8 does the same at a much higher 12 percentage points (a 26% increase). Conversely, Judge 1 is 6 percentage points less likely to assign money bail to Black defendants. Overall, when considering all judges, the varying outcomes for Black defendants range from a 6% lower likelihood of being assigned money bail to a 13-percentage point increase in the chances of receiving money bail, underscoring how the draw of the judge can have a consequential and particularly varied impact on pretrial outcomes depending on a defendant's race/ethnicity.

Not unexpectedly, the assignment patterns vary across defendant characteristics for the entire sample. The perfectly consistent lower rates of bail for female defendants reflected in Figure A2 (see Appendix A), for example, are likely explainable by the different types of charges that are brought against female defendants. Inconsistent inter-judge disparities *across* judges, however, are reflective of substantial amounts of judicial autonomy mixed with differing perceptions and treatment of various types of defendants. Some empirical caution is warranted here, as the number of Black defendants in our dataset is low, and therefore statistical inference becomes more tenuous, and in a few cases impractical because of low sample sizes (see the empty bars for data on Black defendants for Judges 3 and 9 in Figure 4). But our finding of inter-judge disparities in the way that individual judges treat certain racial and ethnic categories of defendants further

189. Judges 9 and 10 increase the rate of bail assignment after the instrument is implemented, and nearly all of Judge 3's IAs occurred before the implementation, so a pre-post comparison is not warranted or statistically feasible.

highlights the importance of understanding how and why judges make the pretrial decisions that they do.

Figure 4
Judicial Disparities in Likelihood of Assigning Money Bail (Race/Ethnicity Comparison)



IV. IDENTIFYING THE CAUSAL EFFECTS OF MONEY BAIL

Concerns about the disparities in the decision-making of IA judges demonstrated above and the exploding rates at which courts across the country assign money bail and detain defendants prior to trial come to life when pretrial release determinations have an appreciable effect on a downstream outcome with economic, policy, or rights-related relevance.

When IAs are randomly assigned to judges, some causal effects are apparent without the need for sophisticated empirical techniques. We know, for example, that a defendant assigned to a judge who assigns money bail at relatively high frequency and high average amount (Judge 6, for example, in

Figures 1 and 2, above) will inherently need to pay more cash to be released than an individual who is assigned to a judge who assigns money bail at low rates and low amounts (Judge 3, for example).[190]

As we emphasized in Section II.D, however, accurately identifying the causal impact that money bail and pretrial detainment has on many of the outcomes scholars and policymakers are interested in requires more than just a simple comparison between those who are assigned money bail and those who are not. Because those two groups are, as an inherent function of a pretrial system that explicitly discriminates on criminal history and charge severity, going to differ in ways that are almost certainly determinative of the various outcomes of interest, a more sophisticated methodology is required. While many studies have nonetheless attempted to make such comparisons by carefully attempting to control for those confounding characteristics, we argue that there are a limited number of empirical approaches that avoid the problem of controlling for all observed and unobserved factors in a standard regression model (also known as omitted variable bias) that has plagued bail studies for decades. One such approach is instrumental variable analysis.

We begin this Part with a basic explanation of this methodology, along with a discussion of what assumptions are necessary and why our dataset meets them (we include a detailed technical explanation of the theory behind the instrumental variable approach in footnotes and in Appendix C). We then explore the primary results of the analysis on the full sample of IA hearings in Pima County. To provide additional context as to why we see these patterns, we also conduct the instrumental variable analysis on various sub-groups of defendants and explain what the results indicate.

A. Instrumental Variable Design, Assumptions, and Outcomes

The cleanest and most powerful empirical methodology that can identify the causal effects of money bail would be a randomized field experiment.[191] In this hypothetical experiment, the release conditions (ROR, release on bail, or detainment) for individuals who are charged with crimes and brought before an IA judge would be randomly assigned to defendants completely independent of risk factors, personal characteristics, or case characteristics. ROR would be the control condition while release on bail or pretrial

190. See *supra* notes 117–121 and accompanying text for how this might affect defendants.

191. Also referred to as randomized controlled trials (RCTs), field experiments are generally considered by scientists as the gold standard for identifying the causal effects of real policies and laws. For a comprehensive review of what field experiments are and how they have been used in the social sciences, see GERBER & GREEN, *supra* note 168. For a review of how field experiments have specifically been used by legal scholars to study law and policy, see also Donald P. Green & Dane R. Thorley, *Field Experimentation and the Study of Law and Policy*, 10 ANN. REV. L. & SOC. SCI. 53 (2014).

detainment would be the treatment groups. These various "treatment" groups could then be compared on whatever individual-, case-, or system-level outcomes that one might be interested in (in our case, we focus on FTA, case outcome, and rearrest recidivism), and because the groups were determined by random assignment, any differences in the outcomes could reliably be attributable to the pretrial release determination. In other words, we would expect all three groups to have a statistically equivalent percentage of individuals who are females, whites, and first-time offenders. We also would expect that their unobserved characteristics are also statistically equivalent (or "balanced"). Such a methodological approach would naturally come with a bevy of ethical, practical, and popular dilemmas, so while the authors of this Article are aware of some scholars and organizations considering bail-related field experiments, to the best of our knowledge, no such studies have been conducted at this time.[192]

The methodological approach we take in this Article—the instrumental variable design—serves as a next-best empirical solution to the causal entanglement problem that has also been utilized by the four empirically-driven articles by Leslie and Pope; Gupta, Hansman, and Frenchman; Dobbie, Goldin and Yang; and Stevenson.[193] While a detailed explanation of why instrumental variable design is better suited for the appendix (see Appendix C), a basic understanding of the approach is important for understanding the results it produces. Unlike a field experiment, where the "treatment" is randomly assigned to individuals, instrumental variable design takes advantage of some factor or event that is antecedent to the non-random treatment variable but is, itself, randomly assigned. In other words, we use an "instrument" that is randomly assigned and has an appreciable impact on the variable that would ideally be randomized.[194] In the context of a study on the effect that the existence of money bail has on case outcomes or recidivism, we know that the bail decision (the treatment) is not random, but if the judge who makes that decision (and their associated relative leniency—

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192. A primitive form of such an experiment occurred in the 1980s in Philadelphia, although primitive methodological approaches and data problems limit the extent to which it can be used. For an example of how the resulting data can be used, see David Abrams & Chris Rohlfs, *Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment*, 49 ECON. INQUIRY 750 (2011). We are also aware of a number of pending field experiments conducted by the Access to Justice Lab in which the procedures used to determine pretrial release are randomly assigned. *See Current Projects*, ACCESS TO JUSTICE LAB, https://a2jlab.org/current-projects/ (last visited Nov. 17, 2021).

193. *See* Dobbie et al., *supra* note 39; Stevenson, *supra* note 76; Leslie & Pope, *supra* note 76; Gupta, et al., *supra* note 141. These four articles are featured heavily in this Article because they utilize similar methodological approaches to identify the causal effect of bail and pretrial detainment, and because they produce (in our opinion) the most reliable causal estimates.

194. *See* Guido W. Imbens & Joshua D. Angrist, *Identification and Estimation of Local Average Treatment Effects*, 62 ECONOMETRICA 467 (1994).

the instrument in our study) *is* randomly assigned and the assignment to a given judge is then determinative of the bail assignment, then we can harness that instrument to draw out the causal effect of the treatment on our outcomes of interest as if it was randomized itself.[195] In our instrumental variable design, the instrument for a given individual defendant is their IA judge's likelihood of assigning money bail or not (calculated using an average of that judge's IA decisions over the course of the study, excluding the defendant's IA outcome).[196]

Although instrumental variable analysis is a reputable tool for making reliable causal claims, proper use of the methodology requires some additional empirical assumptions, and our study is no exception. Again, we provide a brief but necessary exploration of these assumptions here but include the technical discussions of the results of the empirical tests in the appendix (see Appendix C, Tables C1–C4).

The first assumption required in an instrumental variable design is that the assignment of judges to the IA hearings is truly independent of defendant and case characteristics. As we explored this assumption quite thoroughly in Section II.D, we do not add any additional details here except to note that our balance tests provided strong evidence that the "quasi-random" judicial assignment system for IA hearings in Pima County appears to function as a truly random process.

Second, the instrument we use in the instrumental variable design (judicial leniency measures) must satisfy what is called the exclusion restriction, which in this case means that the IA judge only affects the defendant's outcomes. Any other causal pathways between the instrument and outcomes would constitute a violation of the exclusion restriction assumption. In the context of our study, such a violation would occur if the judge assigned to the IA affects case outcomes or recidivism through anything other than whether the IA judge assigns monetary bail. As Dobbie et al. emphasize in their analysis, if the defendant's decision is affected through other channels, then the estimates would incorporate additional effects of these channels that are correlated with judge assignment.[197]

The assumption that Pima County judges are affecting defendant outcomes through the pretrial decision is ultimately empirically untestable,

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195. One technical clarification that is nonetheless critical to keep in mind is that the estimand produced by this design is inherently the local average treatment effect ("LATE") of bail, or the average treatment effect for only the individuals who are on the margins of being assigned money bail prior to trial, not the average treatment effect of detainment on all pretrial defendants. *See, e.g.*, Dobbie et al. *supra* note 39, at 225.

196. This approach is commonly called a "leave-out mean." We also calculate causal effects using an alternative instrument that calculates the leave-out mean of the amount of money bail assigned (in dollars). *See* Appendix C: Tables C2 and C4.

197. Dobbie et al., *supra* note 39, at 220.

but we argue that, because of the institutional setting, the exclusion assumption is reasonable. First, the judge assigned to the IA is not the same judge assigned to any of the successive hearings in the trial and sentencing phases of the judicial process (the two sets of judges come from different courts altogether). When combined with the fact that the only substantive decision that IA judges make is the assignment of bail conditions, it is unlikely that the judges have any unobserved impact on case outcomes. Second, the other actors in the process, including prosecutors, public defenders, and private criminal defense lawyers are assigned through a separate and independent process than the assignment of IA judges, making it unlikely that judge assignment is systematically related to the assignment of other actors in the criminal justice system.

Third is the monotonicity assumption, which stipulates that any given individual not assigned monetary bail by a strict judge would also not be assigned bail by a more lenient judge, and, conversely, that individuals assigned bail by a lenient judge would also be assigned bail by a stricter judge.[198] As with the exclusion restriction, it is not possible to empirically verify monotonicity, but a series of tests has been shown to provide a strong indication that these concerns should be minimal. To test the assumption, we examine whether our judge severity measure for monetary bail assignment (our leave-out mean) is positively predictive of the bail assigned across different subsets of defendants. Table C1 (see Appendix C) shows the extent to which this assumption plays out based on the defendant's criminal history (i.e., whether or not the defendant has a criminal history), the defendant's race and ethnicity (i.e., whether or not the defendant is white, or whether or not the defendant is Hispanic/Latinx), and whether the defendant is a Tucson resident or not. The estimated coefficients in each of these models are large, positively signed, statistically significant (even after multiple comparison corrections), and roughly consistent across subgroups.[199] Taken together, the results support an assumption of monotonicity.

Finally, the instrumental variable design requires that the instrument has a potent effect on the "treatment" variable (the assignment of money bail). Part of verifying this assumption requires a showing of inter-judge variation in the propensity for granting money bail. Otherwise, the "treatment" of being assigned to one judge as opposed to the other judges would be wholly unrelated to the outcomes of interest. The data and analysis in Part III, above, verify this is the case in Pima County. Similarly, we must also show that the judge an individual is assigned should have a significant relationship with the actual existence, or lack thereof, of money bail in the individual cases. A

198. Imbens & Angrist, *supra* note 194, at 269.

199. Note that we obtain similar results whether we use our alternative leave-out measure—logged bail amount or not. See Table C2 in Appendix C.

standard least squares regression is sufficient here, and we show the results of ordinary least squares ("OLS") models in Table C3 (see Appendix C) that estimate the effect of the binary bail assignment leave-out mean on the assignment of bail. All model variations show a similarly strong relationship between the leave-out measure and bail assignment, supporting the assertion of a potent instrument in our data.

Using the instrumental variable approach, our analysis seeks to identify the causal effect of money bail on three categories of outcomes: failures to appear, charge dispositions, and defendant recidivism. Consistent with prior literature, we measure case outcomes on two fronts: whether the defendant pleads guilty of at least one of the charges brought against him at the IA stage and whether the defendant is found guilty of any of those charges through final adjudication by the court. We measure recidivism in two ways: rearrest/recharge and reconviction.[200] What we are defining as "reconviction" technically is a conviction that takes place after a subsequent rearrest in a given time period. Prior quasi-experimental literature in this area almost exclusively measures rearrest recidivism. We believe measuring recidivism in this manner will result in more nuanced empirical findings about the timing and probability of recidivism events that will contribute to the academic literature and to policy reform. We examine both types of recidivism in six-month intervals (up to twenty-four months) beginning at the date of the IA.

B. Analysis of the Full Pima County Sample

Table 3 presents the main two-stage least squares results for the three case outcomes we measure: (1) whether the defendant entered a guilty plea for at least one charge; (2) whether the defendant was found guilty of at least one charge through a court judgment; and (3) whether the defendant failed to appear at any point during the case.[201] Because both the predictive, or dependent, variable and the outcome in our models are binary, the

200. Note that our pre-analysis plan called for additional measures of recidivism, including recidivism after pretrial release but before case disposition and recidivism after post-conviction release. However, because we did not procure detention data, we do not include these variations.

A pre-analysis plan specifies the research, hypotheses, and data analysis that will be done before the analysis is done on a project. The plan is submitted to a registry and is time stamped. The process, which is common with medical studies, is becoming more common in law and social science. It prevents researchers from "p hacking" and other forms of researcher manipulation. One common issue is that researchers can run lots of regressions, but only report the results that achieve statistical significance, resulting in the reporting of results with false positives and publication bias. Our pre-analysis plan can be found at https://osf.io/jznd3/.

201. As we discuss in Section III.C (*see supra* note 167 and accompanying text), the nature of the datasets we used in this analysis required us to determine the charges that were brought at the IA using only a charge offense date and a charge disposition date. As a result, some charges included as IA charges may include charges that were simply pending at the time of the IA.

coefficients in Table 3 are best understood as the effect of being assigned money bail (relative to not being assigned any money bail) on the likelihood of whether or not the particular case outcome occurs.[202] Columns 1 and 2 in both tables report the unadjusted average outcomes for those not assigned and assigned money bail, respectively. The most informative results are found in Column 6 and were estimated using a model that fully accounts for available control variables, including: time-specific variables that control for the year, month, and week of the IA case assignment; case controls for the existence of various charge-types brought at the IA (DUI, drug, disorderly conduct, failure to appear, theft, trespass, and violent crime); and pre-treatment controls for the defendant's demographics and criminal history (age, sex, town of residence, the existence of any previous charges coded as the types previously mentioned, whether any guilty charges exist, and the number of previous charges).[203]

Looking first at the estimated causal effect of being assigned money bail on case outcomes in Table 3 (below), we see there is an increase in the likelihood of a defendant pleading guilty to at least one charge for those who were assigned money bail across all our models, with the models in Columns 3 and 4 producing statistically significant estimates of 9 and 7.5 percentage points, respectively. However, the most sophisticated model (Column 6) suggests that defendants assigned money bail at their IA are only 2.1 percentage points more likely to plead guilty and that estimate is not statistically significant at conventional levels, so we cannot rule out the possibility that the effect is not present.

We see even less evidence that money bail influences the likelihood of a guilty judgment or a failure to appear. Our model predicts a mere 0.3 percentage point difference in guilty judgments between those who are assigned money bail and those who are not and an only marginally larger difference in FTAs. Substantively, these estimates are of much smaller magnitude than what we expected coming into this study, as detailed in our pre-analysis plan.[204]

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202. Note again that the estimand produced by our empirical models is the local average treatment effect (LATE) of bail, or the average treatment effect for only the individuals who are on the margins of being assigned money bail previous to trial.

203. The two-stage least squares results for models that exclude one or more of these controls are presented in Table 3, columns 3–5. All models use robust standard errors clustered by judge and year, which are then used to calculate the p-values.

204. For a discussion of our pre-analysis plan, see *supra* note 200.

Table 3: Results for Charge Dispositions (Whether or Not Money Bail Was Assigned)

	Mean w/o Monetary Bail (1)	Mean w/ Monetary Bail (2)	2SLS Results (3)	2SLS Results (4)	2SLS Results (5)	2SLS Results (6)
Guilty Plea	0.400	0.514	0.092** (0.009, 0.176)	0.075** (0.016, 0.133)	0.012 (-0.050, 0.075)	0.021 (-0.047, 0.089)
Guilty Judgment	0.031	0.030	0.005 (-0.010, 0.021)	-0.001 (-0.019, 0.017)	-0.003 (-0.021, 0.014)	-0.003 (-0.022, 0.016)
Failure to Appear	0.129	0.105	-0.031 (-0.127, 0.065)	0.017 (-0.050, 0.084)	-0.006 (-0.063, 0.051)	-0.004 (-0.064, 0.055)
Time Fixed Effects	-	-	No	Yes	Yes	Yes
Case Fixed Effects	-	-	No	No	Yes	Yes
Pre-treatment Controls	-	-	No	No	No	Yes
Observations	-	-	23,679	23,679	23,679	20,614

Notes: Each second stage model estimates the effect of being assigned money bail (using the binary bail assignment leave-out mean as the instrument) on the disposition of charges that existed at the time of a defendant's initial appearance (both pending and new charges) using two-stage least squares regressions. Time fixed effects account for year, month, and week of IA case assignment. Case-type controls include a count of new and pending charges at the IA and the existence of the following charge types: Drug, Drug, FTA, Theft, and Violent Crime (dummy variables for disorderly conduct and trespass were excluded due to multicoliniarity). Pre-treatment controls include: a count of prior convictions, the existence of any previous convictions of the types outlined in the case type controls, age at the time of IA (years), sex/gender, race/ethnicity (white) and residence (whether the defendant resides in Tucson). Differences in the number of observations for each model is reflective of either excluding observations that have missing pre-treatment control variables or the need to censor the data for recidivism measures, as not all the IAs in the data occurred early enough to include outcome data at 12, 18, and 24 months. The 95% confidence intervals are presented in parentheses and were created using standard errors clustered by judge and year.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; (coefficients in bold survive a Bonferroni-Holm correction for multiple comparisons as $p < 0.1$).

Moving to non-case outcomes, Table 4 (below) shows the impact of money bail on rearrest and reconviction recidivism. Specifically, we measure rearrest within six, twelve, eighteen, and twenty-four months after the IA; and conviction of at least one charge brought against the defendant within six, twelve, eighteen, and twenty-four months after the IA. To the best of our knowledge, these are the first causal estimates of the effect of money bail on reconviction recidivism. Table 4 is constructed in the same way as Table 3, with the exception that the extended time-horizons for recidivism outcomes dictate that the sample sizes decrease as measurement-times increase (e.g., the sample includes 20,418 defendants who are observed for six months, but the sample size drops to 11,688 defendants for the two-year period).

The base rate for rearrest recidivism for the six-month window is 20.1% for those assigned bail, compared to 22.3% for those who were not assigned money bail. The annual rate climbs to 28.6% after one year from arrest and 26.6% after two years from arrest, respectively. The instrumental variable estimates for rearrest recidivism in Column 6 (our preferred model) indicate that the causal effect of money bail on recidivism is, indeed, *negative* and decreases over time: The marginal individuals who are assigned money bail are 9.2 percentage points less likely to be rearrested within six months, 6.5 percentage points less likely to be rearrested within twelve months, and 5.3 and 3.5 percentage points less likely to be arrested within eighteen and twenty-four months, respectively. Of these estimates, however, only the effect on rearrest at six months is statistically significant ($p < .01$) at a level that survives multiple comparison correction.

The reconviction recidivism estimates largely mirror the rearrest recidivism results. In our preferred specification (Column 6), we estimate a decline of 5.0 percentage points in reconviction for a charge made at six months for the marginal defendant assigned bail. Estimations of the reduction of reconviction recidivism in the twelve-, eighteen-, and twenty-four-month periods drop to 3.7, 1.1, and .02 percentage points, respectively. As with the rearrest outcomes, however, these longer-horizon measures are not statistically significant at a reliable level, making the results suggestive of a short-lived rearrest recidivism reduction from the assignment of money bail.

Many of these results contrast with the conventional wisdom and extant quasi-experimental literature we have discussed above.[205] All three of the studies that measure plea and conviction rates find a positive and statistically significant effect of money bail or pretrial detention, with Dobbie et al. and Leslie and Pope identifying causal effects of 10 percentage points.[206] Likewise, these papers find a substantive impact on guilty verdicts (between 4.7 and 13 percentage points[207]), whereas our estimate is effectively zero.

The recidivism results are particularly surprising and contrast starkly with the work of Gupta et al., Leslie et al., and Dobbie et al., all of whom find *increases* in rearrest recidivism as a consequence of higher money bail or pretrial detention.[208] The results also cut against a more criminogenic channel at work, where being exposed to the criminal justice system increases the probability of recidivism over time, as shown in the work of Aizer and Doyle[209] and Mueller-Smith.[210] Our results instead point to the possibility of

205. *See supra* note 193 and accompanying text.

206. Dobbie et al., *supra* note 39, at 204–05, 277; Leslie & Pope, *supra* note 76, at 543.

207. Stevenson, *supra* note 76, at 532; Leslie & Pope, *supra* note 76, at 530.

208. *See supra* note 193 and accompanying text. One difference worth noting in the studies is the instrument used. Dobbie et al., *supra* note 39; Leslie & Pope, *supra* note 76, and Stevenson, *supra* note 76, all use pretrial release (or pretrial detention) leave-out means across the judges. Our study, like Gupta et al., *supra* note 141, relies on a leave-out mean of the judge's probability of assigning money bail. We think the ideal instrument would be pretrial release or detention, but because we were unable to obtain detention data, we relied on the assignment of money bail as our instrument.

209. Aizer & Doyle, *supra* note 138.

210. Mueller-Smith, *supra* note 132.

being "scared straight" once one is assigned high money bail, although the effects appear to be short-lived, lasting somewhere between six and twelve months. As we highlight below in Section V.B, we suspect these deviations are reflective of the variation in our venue's institutions, judges, and defendant population, as opposed to meaningful differences in methodological approaches.

Table 4: Results for Recidivism (Whether or Not Money Bail Was Assigned)

	Mean w/o Money Bail (1)	Mean w/ Money Bail (2)	2SLS Results (3)	2SLS Results (4)	2SLS Results (5)	2SLS Results (6)
Rearrest in 6 Months	0.223	0.201	**-0.1004***** (-0.166, -0.035)	**-0.089***** (-0.156, -0.023)	**-0.094***** (-0.155, -0.034)	**-0.092***** (-0.155, -0.029)
Rearrest in 12 Months	0.286	0.266	-0.078** (-0.142, -0.014)	-0.065* (-0.137, 0.006)	-0.072** (-0.143, -0.000)	-0.065* (-0.135, 0.005)
Rearrest in 18 Months	0.317	0.301	-0.064* (-0.130, 0.003)	-0.056 (-0.147, 0.036)	-0.058 (-0.141, 0.025)	-0.053 (-0.144, 0.038)
Rearrest in 24 Months	0.337	0.321	-0.046 (-0.115, 0.022)	-0.042 (-0.135, 0.051)	-0.045 (-0.133, 0.042)	-0.035 (-0.124, 0.054)
Conviction of 6-Month Rearrest Charge	0.156	0.147	-0.065** (-0.122, -0.008)	-0.049* (-0.101, 0.003)	-0.053** (-0.100, -0.007)	-0.050* (-0.101, 0.002)
Conviction of 12-Month Rearrest Charge	0.205	0.201	**-0.061***** (-0.103, -0.019)	-0.041 (-0.098, 0.016)	-0.048** (-0.096, -0.001)	-0.037 (-0.100, 0.027)
Conviction of 18-Month Rearrest Charge	0.230	0.229	-0.031 (-0.096, 0.034)	-0.013 (-0.083, 0.057)	-0.016 (-0.086, 0.054)	-0.011 (-0.086, 0.064)
Conviction of 24-Month Rearrest Charge	0.247	0.247	-0.023 (-0.093, 0.047)	-0.013 (-0.089, 0.064)	-0.017 (-0.092, 0.059)	-0.002 (-0.077, 0.074)
Time Fixed Effects	-	-	No	Yes	Yes	Yes
Case Fixed Effects	-	-	No	No	Yes	Yes
Pre-treatment Controls	-	-	No	No	No	Yes
Observations: 6 Month	-	-	20,481	20,481	20,481	17,901
Observations: 12 Month	-	-	17,588	17,588	17,588	15,473
Observations: 18 Month	-	-	14,405	14,405	14,405	12,749
Observations: 24 Month	-	-	11,688	11,688	11,688	10,363

Notes: Each second stage model estimates the effect of being assigned money bail (using the binary bail assignment leave-out mean as the instrument) on arrest/charge and conviction recidivism using two-stage least squares regressions. Time fixed effects account for year, month, and week of IA case assignment. Case-type controls include a count of new and pending charges at the IA and the existence of the following charge types: Drug, Drug, FTA, Theft, and Violent Crime (dummy variables for disorderly conduct and trespass were excluded due to multicoliniarity). Pre-treatment controls include: a count of prior convictions, the existence of any previous convictions of the types outlined in the case type controls, age at the time of IA (years), sex/gender, race/ethnicity (white) and residence (whether the defendant resides in Tucson). Differences in the number of observations for each model is reflective of either excluding observations that have missing pre-treatment control variables or the need to censor the data for recidivism measures, as not all the IAs in the data occurred early enough to include outcome data at 12, 18, and 24 months. The 95% confidence intervals are presented in parentheses and were created using standard errors clustered by judge and year.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; (coefficients in bold survive a Bonferroni-Holm correction for multiple comparisons as $p < 0.1$).

C. Analysis of Defendant Subgroups

In addition to the main causal effects of pretrial detention that we have discussed above, Table 5 (below) presents results for subgroups of the full Pima County sample. We compare three pairs of subgroups based on defendant residency (Tucson County resident or not—Columns 1 and 2); defendant criminal history[211] (whether the defendant has a previous criminal conviction in the CJC or not—Columns 3 and 4); and defendant race/ethnicity (Hispanic/Latinx and white—Columns 5 and 6). Each column of coefficients here reflects estimates using the empirical model that includes all control variables (the equivalent of Column 6 results in Tables 3 and 4, above). While comparing across subgroups does not allow us to make any credible causal claims—for example, we cannot say that differences between Hispanic/Latinx and white defendants occurred *because* of race/ethnicity—they do give us valuable context for the primary results we discussed above and help explain why the causal impact of money bail in Pima County appears to be more moderate than it is in venues like Philadelphia and Miami. The results in Table 5 (below) suggest that Hispanic/Latinx defendants are driving recidivism reductions. If there is a meaningful percentage of undocumented individuals in this Hispanic/Latinx group, these findings raise two possibilities. First, the cost of recidivating may be higher for those individuals than for the rest of the sample (e.g., deportation), meaning that the Hispanic/Latinx sub-group may be more responsive to sanctions. Second, our recidivism results rely on being able to link together various criminal records across time, and undocumented individuals may be more incentivized and able to use different names, resulting in an undercounting of subsequent offenses for those individuals.

For case outcomes, we find that whether an individual resides in Tucson seems to be driving the small effects on guilty pleas that we see in the broader dataset. Of the defendants assigned to money bail, Tucson residents are 5.6 percentage points more likely to plead guilty.[212]

Treatment group heterogeneity results are more consistent when we examine recidivism outcomes. In contrast with much of the literature, we find that recidivism reductions for those assigned monetary bail are being driven by individuals who have a prior conviction. The results are striking: defendants assigned monetary bail with a criminal history were 17.8 percentage points less likely to be rearrested within six months, 26.9

211. Criminal history is of particular theoretical interest as it measures the individual's "closeness" to criminal activity.

212. With the exception of the failure to appear result for non-Tucson residents, none of the other subgroup results for case outcomes withstand multiple comparison testing done by the Benjamini-Hochberg correction. Consequently, those results should be taken with some degree of caution since there is a possibility of Type 1 error (false positives).

percentage points less likely to be rearrested in twelve months, 24.3 percentage points less likely to be rearrested in eighteen months, and 26.8 percentage points less likely to be rearrested in twenty-four months. The results should be taken with some degree of caution because we did not have a main effect for eighteen- and twenty-four-month rearrest, but we did specify a subgroup effect in our pre-analysis plan and the results withstand multiple comparison testing. The results for conviction on the rearrest charge are similar based on the individual's criminal history. Those with a prior conviction that were assigned monetary bail were 16.0 percentage points less likely to be convicted on a rearrest charge that was made within six months of the IA. The estimate increases to a 19.1 percentage point reduction in the probability of conviction when the window for the rearrest charge is extended to twelve months. Both results are statistically significant at conventional levels and withstand the correction for multiple comparisons as $p < 0.05$. As we discuss in our limitations section (*see supra* Section IV.D), two non-mutually exclusive explanations offer possible explanations for the result. First, the result might be explained by the greater legal acumen through previous exposure to the criminal justice system, where defendants who have already gone through the criminal justice system are more savvy in avoiding a subsequent arrest. Second, marginal deterrence is also a potential explanation where defendants have a stronger desire to avoid a conviction because of potentially harsher sentencing.[213] Finally, the desire to avoid detention, having already been exposed to incarceration or other criminal sanctions, could also play a role.

The results are particularly surprising in light of the study by Dobbie et al., which found little evidence of treatment effect heterogeneity for rearrest recidivism based on an individual's criminal history.[214] Though their study in terms of analyzing this subgroup result is closest to our own, it is worth noting that their analysis included felonies, and their instrument was pretrial release, rather than the assignment of money bail. While other similar studies have not examined treatment effect heterogeneity based on criminal history for recidivism, to the extent that any effects are present, they would contribute to driving increases in recidivism as a consequence of monetary bail or pretrial detention, rather than the recidivism reducing effects that we find.

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213. Stevenson, *supra* note 76, at 537–38.

214. Dobbie et al., *supra* note 39, at 230–32.

Table 5: Demographic Subgroup Results (Whether or Not Money Bail Was Assigned)

	2SLS Results [Tucson Res.] (1)	2SLS Results [Non-Tucson Res.] (2)	2SLS Results [Crim. History] (3)	2SLS Results [No Crim. History] (4)	2SLS Results [Hispanic/Latinx Def.] (5)	2SLS Results [White Def.] (6)
Guilty Plea	0.056*	-0.029	-0.061	0.047	0.041	-0.018
	(-0.012, 0.124)	(-0.129, 0.070)	(-0.177, 0.056)	(-0.019, 0.113)	(-0.054, 0.136)	(-0.110, 0.074)
Guilty Judgment	-0.015	0.012	-0.033	0.007	-0.022	0.016
	(-0.043, 0.012)	(-0.028, 0.050)	(-0.088, 0.022)	(-0.023, 0.037)	(-0.061, 0.017)	(-0.018, 0.051)
Failure to Appear	0.010	-0.027	-0.043	-0.014	0.050	-0.014
	(-0.044, 0.061)	(-0.107, -0.025)	(-0.141, 0.055)	(-0.055, 0.082)	(-0.065, 0.165)	(-0.081, 0.053)
Rearrest in 6 Months	**-0.117*** **	-0.051	**-0.178*** **	-0.062	-0.117***	-0.106**
	(-0.190, -0.043)	(-0.147, 0.045)	(-0.266, -0.090)	(-0.158, 0.035)	(-0.210, -0.024)	(-0.180, 0.032)
Rearrest in 12 Months	-0.087**	-0.023	**-0.243*** **	-0.007	**-0.155*** **	-0.050
	(-0.168, 0.007)	(-0.147, 0.101)	(-0.370, -0.115)	(-0.083, 0.068)	(-0.281, -0.029)	(-0.129, 0.029)
Rearrest in 18 Months	-0.078*	-0.004	-0.199**	-0.021	-0.144**	-0.052
	(-0.157, 0.001)	(-0.131, 0.122)	(-0.410, -0.052)	(-0.140, 0.100)	(-0.285, 0.003)	(-0.169, 0.065)
Rearrest in 24 Months	-0.063	0.045	**-0.268*** **	0.013	-0.146*	-0.006**
	(-0.152, 0.025)	(-0.136, 0.226)	(-0.441, -0.094)	(-0.088, 0.115)	(-0.305, 0.132)	(-0.143, 0.131)
Conviction of 6-Month Rearrest Charge	**-0.078*** **	-0.003	**-0.160*** **	-0.008	-0.063	**-0.060** **
	(-0.138, -0.019)	(-0.088, 0.083)	(-0.257, -0.063)	(-0.058, 0.042)	(-0.151, -0.026)	(-0.115, -0.006)
Conviction of 12-Month Rearrest Charge	-0.060	0.011	**-0.191*** **	0.016	-0.083*	-0.031
	(-0.147, 0.027)	(-0.091, 0.113)	(-0.322, -0.060)	(-0.058, 0.068)	(-0.174, 0.008)	(-0.115, -0.053)
Conviction of 18-Month Rearrest Charge	-0.031	0.031	-0.101	0.009	-0.020	-0.042
	(-0.120, 0.057)	(-0.066, 0.128)	(-0.244, -0.042)	(-0.088, 0.107)	(-0.136, 0.097)	(-0.160, 0.077)
Conviction of 24-Month Rearrest Charge	-0.027	0.068	-0.216**	0.046	-0.064	0.019
	(-0.118, 0.063)	(-0.045, 0.184)	(-0.37, -0.057)	(-0.039, 0.130)	(-0.220, 0.092)	(-0.086, 0.124)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Case Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations: Full Sample	10,245	10,369	4,784	15,830	7,670	9,703
Observations: 6 Month	9,280	8,621	3,866	14,035	6,580	8,482
Observations: 12 Month	8,249	7,224	3,123	12,350	5,660	7,329
Observations: 18 Month	6,919	5,830	2,315	10,434	4,528	6,075
Observations: 24 Month	5,694	4,669	1,688	8,675	3,645	4,556

Notes: Each second stage model estimates the effect of being assigned money bail (using the binary bail assignment leave-out mean as the instrument) on the disposition of charges that existed at the time of a defendant's initial appearance (both pending and new charges) and arrest/charge and conviction recidivism using two-stage least squares regressions. Time fixed effects account for year, month, and week of IA case assignment. Case-type controls include a count of new and pending charges at the IA and the existence of the following charge types: Drug, Drug, FTA, Theft, and Violent Crime (dummy variables for disorderly conduct and trespass were exduded due to multicoliniarity). Pre-treatment controls include: a count of prior convictions, the existence of any previous convictions of the types outlined in the case type controls, age at the time of IA (years), sex/gender, race/ethnicity (white) and residence (whether the defendant resides in Tucson), with the relevant sub-group control removed. Differences in the number of observations for each model is reflective of either excluding observations that have missing pre-treatment control variables or the need to censor the data for recidivism measures, as not all the IAs in the data occurred early enough to include outcome data at 12, 18, and 24 months. The 95% confidence intervals are presented in parentheses and were created using standard errors clustered by judge and year.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; (coefficients in bold survive a Bonferroni-Holm correction for multiple comparisons as $p < 0.1$).

D. Limiting Factors and Other Considerations

Although our empirical tests suggest that the underlying assumptions required to conduct the instrumental variable analyses are met, these results must still be understood with some limitations and additional considerations in mind.

Foremost among these potential limitations is the lack of incarceration data in our analysis, which prevents us from verifying the mechanisms driving the negative treatment effects we see for recidivism in the results above.[215] Criminologists, sociologists, and economists, among others, have long shown that reductions in recidivism rates are largely the result of two factors: incapacitation and deterrence.[216] Criminal deterrence occurs when an individual's prior experiences with the criminal justice system or their fear of future interactions disincentivizes criminal activity that would have occurred but for the intervention of interest (the bail hearing and judge's pretrial determinations, in the case of our study). Incapacitation effects, on the other hand, are simply the reduction in criminal behavior that results from an individual being detained and therefore naturally less likely and able to commit crimes. Without the data showing if and when individuals who were assigned money bail were released—either as the result of finding the funds to meet bail or through future changes in detention decisions by the judges—we are not able to empirically show whether the treatment effects we do (and do not) identify are due to incapacitation or deterrence.

Nonetheless, the results of our analyses combined with the descriptive data presented in previous studies give us a reasonably clear explanation of our findings. Although the Dobbie et al. study took place in large, metropolitan areas (Miami-Dade and Philadelphia counties) and did not exclusively focus on misdemeanors[217] (differences that we have argued are potentially significant), their dataset did include misdemeanor defendants, and they were able to procure the post-IA incarceration data. They find that among defendants who are released within three days of the bail hearing, nearly half (41.4%) were assigned money bail.[218] Additionally, they find that

215. Data on incarceration are not held by the Pima County Courts, and we were unable to procure it from the agencies who do possess them.

216. For a more detailed discussion of criminal deterrence, see Gary S. Becker, *Crime and Punishment: An Economic Approach*, in ESSAYS IN THE ECONOMICS OF CRIME AND PUNISHMENT 1, 33–34 (Gary S. Becker & William M. Landes eds. 1974), https://www.nber.org/system/files/chapters/c3625/c3625.pdf; Isaac Ehrlich, *On the Usefulness of Controlling Individuals: An Economic Analysis of Rehabilitation, Incapacitation and Deterrence*, 71 AM. ECON. REV. 307 (1981); Daniel S. Nagin, *Deterrence and Incapacitation*, in THE HANDBOOK OF CRIME AND PUNISHMENT 345 (Michael Tonry ed., 2000).

217. *See* Dobbie et al., *supra* note 39, at 231–32. Note that the authors did include a subgroup analysis of misdemeanors in their online appendix, Tables A15 and A16.

218. *Id.* at 212.

defendants charged only with misdemeanors were twice as likely to be released as those charged only with felonies,[219] lending further support to the conclusion that the pretrial detainment periods for misdemeanor defendants are likely very short, even when those defendants are assigned money bail. Subsequent, non-experimental research specifically on misdemeanors in Miami-Dade also shows that misdemeanor cases are resolved significantly faster (51%) than felony cases.[220] If those same characteristics are at play in our data, any incapacitation effects would be largely limited to the days just following the IA, meaning that the decreased recidivism rates that we identify in our early periods (six and twelve months) are best explained through a mix of initial, short-term incapacitation and mid-term deterrence.

Additionally, as with all pre-COVID-19 studies involving the criminal justice system and recidivism, our results have to be understood in the context of a world that looks much different than it did even two years ago. As we highlight at the beginning of this article, COVID-19 wreaked a special kind of havoc on prison and jail populations. Some jurisdictions took drastic and unprecedented measures to address the crises, including the pretrial release of all but the most dangerous defendants, regardless of whether they could pay the bail that was initially assigned to them or moratoriums on any new detainees.[221] At this moment in time, it is still very much unclear as to whether these emergency changes will impact future policies in the pretrial space, meaning that our results—and any other pre-COVID-19 findings—must be used to inform policy change only after considering the unique cultural and institutional characteristics of the jurisdiction under consideration.

V. IMPLICATIONS AND SOLUTIONS

This Article provides critical data on two outstanding empirical questions that must be answered before lasting change can occur: (1) How much do judges vary in their bail determinations, even when they are working in the same legal and institutional system; and (2) what is the impact of money bail assignments on a defendant and the defendant's case? We find that judges vary drastically and that, at least in Pima County, the downstream impacts of money bail are not as severe as many had feared.

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219. *Id.*

220. Nick Peterson, *Low-Level, but High Speed?: Assessing Pretrial Detention Effects on the Timing and Content of Misdemeanor Versus Felony Guilty Pleas*, 36 JUST. Q. 1314, 1324 (2019).

221. For a frequently-updated picture of where the post-COVID bail policies are moving, see *The Most Significant Criminal Justice Policy Changes from the COVID-19 Pandemic*, PRISON POL'Y INITIATIVE, https://www.prisonpolicy.org/virus/virusresponse.html (last updated Dec. 23, 2021).

These empirical results raise some obvious questions. If judges are using their discretion to come to wildly different legal outcomes, should their discretion be curbed? How might that be accomplished? And if the causal impact of money bail is not as detrimental in Pima County as other empirical analyses in cities like Philadelphia and Miami, what policy prescriptions make the most sense? Similarly, are the disparities in empirical outcomes across similarly designed (and equally valid) empirical studies suggestive of a flaw in the way we are approaching the task of evaluating bail reform more broadly?

In this Part we address these questions and—to the extent we feel it is prudent—provide some potential solutions. We begin with the issue of judicial disparities and then address the temptation to use the results of studies like ours to create one-size-fits-all reforms.

A. Curbing Judicial Disparity

Our evaluation of inter-judge disparities in Part III provided striking results. We verified, both through surveying the Pima County courts regarding their judicial assignment protocols and through a battery of statistical tests, that IA judges in Pima County have nearly identical caseloads. Nonetheless, the "strictest" IA judges assign money bail three times as often (60% of cases) as their more lenient counterparts (20%). Furthermore, the strict judges assign bail amounts that are more than twelve times higher ($2,500) than the most lenient judges ($200). These disparities persist even after Pima County adopted a popular system for curbing the discretion of judges in favor of a data-driven pretrial process. Similarly, the varied outcomes across judges for Black defendants where the chance of receiving money bail—ranging from a reduced likelihood of 5 percentage points to an increased chance of receiving money bail as high as 12 percentage points—highlights how the draw of the judge can have a significant impact on the pretrial decision. Although the Black Lives Matter movement has primarily focused on policing, racial disparities in the criminal justice system are buttressed by this pattern of judicial behavior.

In light of these results, the more difficult question about what to do emerges. There are a number of options that jurisdictions across the country have experimented with to reduce disparity. These range from "shoves" to "nudges" in terms of the extent to which they directly restrict judicial discretion.

Shoves. One option is to constrain judges by imposing mandatory guidelines on their pretrial decisions. This possibility is very unlikely to have any traction; in all likelihood, it would be unconstitutional, and there would be strong resistance to what would be perceived as heavy-handed curbing of judicial discretion. More common are the use of bail schedules, where police and judges or magistrates are given bail amounts based on the charge and other factors. Although judges and magistrates have discretion to depart from bail schedules—oftentimes more discretion than police—departures tend to be the exception, and jurisdictions vary in the bail ranges they set for various charges. One problematic aspect of bail schedules is that they often do not take account of the individual's income situation, and they likely result in excessive bail setting for more economically disadvantaged defendants.

Nudges. More palatable options reside in the world of nudges, where judges would have mandates limit their discretion. One possibility, which is commonplace in most jurisdictions, is to have guidelines and bail schedules that are voluntary. Federal circuit courts are split on the constitutionality of bail schedules. Some courts apply a rational basis standard, finding that money bail reasonably ensures a defendant's return for a court appearance.[222] Other jurisdictions have found that detaining defendants in the pretrial system without considering the defendant's ability to pay may constitute an Eighth Amendment violation because the act of doing so constitutes excessive bail.[223] The U.S. Supreme Court denied certiorari in a case that raised the issue as recently as April of 2019.[224] Although bail fee schedules are in place in a number of jurisdictions, disparity is still prominent, as was the case in Pima County. The likely reason for the persistent disparity in most cases is a combination of judges departing from the bail fee schedule and the wide ranges for bail setting within existing bail schedules. While seemingly attractive, a reduction in the recommended range of bail for a given crime is likely to be viewed as undermining judicial discretion, even though bail schedules are generally not binding. Consequently, achieving reductions in disparity through this type of reform would likely elicit pushback from judges and is therefore not politically feasible.

Risk assessment scores and algorithms also offer the prospect of reducing disparity, but the evidence is still at its early stages in terms of its prospects for reducing disparity in pretrial decisions. Some of the most recent and best evidence available shows that algorithmic risk assessment does little to reduce disparity, and in certain instances, can increase

222. *See, e.g.*, Katona v. City of Cheyenne, 686 F.Supp. 287, 293 (D. Wyo. 1988) (finding $35 bond to be rationally and reasonably related to assuring defendant's trial appearance); Vasquez v. Cooper, 862 F.2d 250, 253–54 (10th Cir. 1988) (distinguishing case from those where defendants' sentences exceeded the statutory maximum term).

223. *See, e.g.*, Pierce v. Velda City, No. 4:15-cv-570–HEA, 2015 U.S. Dist. WL 10013006 at *1, *2–3 (E.D. Mo. June 3, 2015) (where the district court issued a declaratory judgment stating, "[n]o person may, consistent with the Equal Protection Clause of the Fourteenth Amendment to the United States Constitution, be held in custody after an arrest because the person is too poor to post a monetary bond," and where the parties ultimately reached a settlement altering the jurisdiction's bail setting system).

224. Walker v. City of Calhoun, 901 F.3d 1245, 1268 (11th Cir. 2018), *cert. denied*, 139 S. Ct. 1446 (2019).

disparity.[225] Our work descriptively shows that disparity does not change much when changes are made to a risk assessment algorithm. While this initial evidence does not preclude the possibility of algorithms reducing behavior, our best evidence to date is that their use is unlikely to reduce disparity.

One alternative we propose to reduce disparity is to inform judges of their behavior relative to their colleagues. We believe this would very likely reduce disparity. Although judges have a rough sense of which of their colleagues are more versus less lenient, our experience through field interviews and more informal conversations with judges is that they are unlikely to be aware of the magnitude of the disparities. We also think presenting the disparities gives judges information on which they can act, while also not being violative of their discretion, which might be important in individual cases and is likely to be more politically feasible since it does not threaten judicial autonomy.

Although this idea has never been rigorously tested with judges, there are some forms of similar information that have been disseminated to adjudicators, and information that has been rigorously tested in other settings. One close example involves immigration judges being informed of their asylum grant rates.[226] Because the intervention was not staged in a systematic manner, the effects on disparity are unknown. A randomized informational intervention or a staged rollout of such information for judges would allow one to know the effects of such information on disparity. Randomized peer effects studies in other areas have revealed welfare-maximizing behavior. Studies informing individuals of peer behaviors in areas ranging from

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225. *See* Megan T. Stevenson & Jennifer L. Doleac, *Algorithmic Risk Assessment in the Hands of Humans* (IZA Inst. Lab. Econ., Working Paper IZA DP No. 12853, 2019) (examining the impact of algorithmic risk assessment used for sentencing in Virginia and finding no evidence that risk assessment affected racial disparities statewide but finding "a relative increase in sentences for black defendants in courts that appeared to use risk assessment most." The authors further state that disparities increased for defendants under the age of 23.); Megan Stevenson, *Assessing Risk Assessment in Action*, 103 MINN. L. REV. 303, 309 (2018) (showing a law requiring the use of risk assessment algorithms in Kentucky benefited white pretrial defendants more than Blacks, not because of racially biased risk assessment scoring, but instead because "[j]udges from predominantly white rural counties liberalized their bail setting practices more than judges from more racially mixed urban areas, but *within* the same county, white and black defendants saw similar increases in release"); Bryce Covert, *A Bail Reform Tool Intended to Curb Mass Incarceration Has Only Replicated Biases in the Criminal Justice System*, INTERCEPT (July 12, 2020, 8:00 AM), https://theintercept.com/2020/07/12/risk-assessment-tools-bail-reform/.

226. One noteworthy effort made judge-specific asylum grant rates available in an accessible format. The Transactional Records Access Clearinghouse (TRAC) at Syracuse University collects data and publishes it in an accessible format. TRACIMMIGRATION, https://trac.syr.edu/immigration (last visited Feb. 23, 2022).

voting[227] to household power usage[228] have consistently found conforming effects.

B. Avoiding One-Size-Fits-All Reform

The inter-judge disparities we identify only address one of the unanswered empirical questions we argue are preventing efficacious bail reform. We also need to know how those disparities—specifically a given judge's or system's propensity for conditioning pretrial release on a defendant's ability to pay money bail—affect the defendant and his criminal case. The instrumental variable design we present in Part IV does just that, and the empirical results were surprising.

Specifically, defendants not assigned money bail are significantly more likely (5.1 percentage points) to fail to appear at their criminal trial. This purports with the intuitive and traditional understanding that one of the primary purposes of pretrial detainment is to ensure that defendants do not abscond. However, we find, at best, an unclear impact of money bail on the likelihood of guilty pleas and judgments and see a statistically distinguishable *increase* in short-term recidivism (11.4 percentage points) for those assigned money bail. In total, this suggests that assigning the marginal defendant money bail will help ensure that defendants (and their potential victims) have their time in court without any of the potential injury to the defendant or the community.

Many of these results contrast with the conventional wisdom and extant quasi-experimental literature we have discussed and endorsed above.[229] All three studies that measure plea and conviction rates find a positive and statistically significant effect from money bail or pretrial detention, with Dobbie et al. and Leslie and Pope identifying causal effects of 10 percentage points.[230] While the magnitude of our estimate here (5.4 percentage points) is similar to Stevenson's findings (4.7 percentage points),[231] our result is not statistically significant. Likewise, these papers find a substantive impact on guilty verdicts (4.7 up to 13 percentage points), whereas our estimate is effectively zero.

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227. *See, e.g.*, Alan S. Gerber, Donald P. Green & Christopher W. Larimer, *Social Pressure and Voter Turnout: Evidence from a Large-scale Field Experiment*, 102 AM. POL. SCI. REV. 33, 42 (2008).

228. *See e.g.*, Ian Ayres, Sophie Raseman & Alice Shih, *Evidence from Two Large Field Experiments that Peer Comparison Can Reduce Residential Energy Usage*, 29 J.L., ECON., & ORG. 992, 1015 (2013).

229. *See supra* note 193 and accompanying text.

230. *See supra* note 206 and accompanying text.

231. Stevenson, *supra* note 76.

The recidivism results are particularly surprising and contrast starkly with the work of Gupta et al., Leslie and Pope, and Dobbie et al., all of whom find *increases* in rearrest recidivism as a consequence of higher money bail or pretrial detention.[232] The results also cut against a more criminogenic channel at work, where being exposed to the criminal justice system increases the probability of recidivism over time shown in the work of Aizer and Doyle[233] and Mueller-Smith.[234] Our results instead point to the possibility of being "scared straight" once one is assigned high money bail, although the effects appear to be short-lived, lasting somewhere between six and twelve months.

Some will see these deviations and conclude that they are the results of some meaningful differences in methodological approaches or, more seriously, a flawed or substandard analysis in one or more of the analyses (including our own). While errors are possible, we argue that such an outlook reflects a well-meaning but ultimately short-sighted desire for one set of well-identified results to provide a unified salve for the woes we are currently experiencing in bail reform. Instead, we suspect these deviations are reflective of the variation in our venue's institutions, judges, and defendant population. As we highlighted above in Section II.B, the Pima County data we used in our analysis exclusively features misdemeanor defendants arrested and charged in a population—non-Tucson Pima County—that is a mix of suburban and rural communities.

These deviations consequentially call into question a one-size-fits-all approach to pretrial justice reform. Rigorous policy evaluation work has only begun in this area, and the bulk of the studies have overwhelmingly focused on large urban centers in the Eastern United States. Our study shows that results likely differ across geography, and even within geographic areas of the country, varying in rural, suburban, and urban areas.

We think this variation motivates a multi-pronged approach to adopting reforms of the pretrial system involving (1) rigorous experimental or quasi-experimental policy evaluation on a regular basis; (2) replication across a variety of settings; (3) piloting and testing of interventions; and (4) reporting of results across jurisdictions in order to encourage policy testing and diffusion. Randomized or quasi-randomized experiments allow for legal and policy interventions to be isolated from other factors, which are an important ingredient for determining whether reforms work. This multi-step approach combines rigorous policy evaluation, while also taking context-specific factors into account. The reporting of the results across jurisdictions also allows for the testing and implementation of appropriate reforms in similar

232. *See supra* note 193 and accompanying text.

233. Aizer & Doyle, *supra* note 138.

234. Mueller-Smith, *supra* note 132.

settings, resulting in data-driven reform that will be suitable for the jurisdiction in which the reform is adopted. We think short-term goals include the implementation of similar experiments in a variety of settings, but longer term, there is a need for foundations and administrative agencies—in this case, judicial agencies like administrative offices of the courts—to coordinate programs that would promote the long-term viability of this policy evaluation and reform model. Work of this sort will naturally curb the temptation to generalize the results of one or just a few rigorously conducted studies in a given setting to the broader criminal justice landscape and will set the stage for appropriate policies to be implemented for a given jurisdiction. In other words, it will promote a world where nuanced and suitable reforms are implemented in the right settings.

CONCLUSION

Although the United States was already primed for a third wave of bail reform coming into 2020, the one-two punch of recent BLM protests and the COVID-19-induced health crisis in jails has made widescale change in the country's criminal pretrial system seem even more likely. Against this backdrop, the empirical results presented in this Article highlight the need for the desire for change to be coupled with an informed perspective of what is actually going on in the pretrial system. We have demonstrated that even in a system (Pima County, Arizona, in the case of our study) where the institutions, laws, procedures, and defendant demographics are consistent, the individual dispositions and propensities of bail judges can have an uncomfortably monumental impact on whether or not a given defendant is required to pay bail before being released from jail. Furthermore, we see that these disparities in pretrial detention have consequences for case outcomes and recidivism. Specifically, we show that being assigned money bail—versus being released with or without conditions—potentially increases the chances of a guilty plea, although these effects were not statistically significant. We also find that those assigned money bail are less likely to reoffend (whether measured by rearrest or reconviction), but the effects are short-lived—lasting only six to twelve months, after which the effect disappears. For sub-groups of the population, particularly those with a criminal history, we find that the effects of money bail are longer lasting—with a lower likelihood of recidivism lasting until at least twenty-four months after the individual is released.

We explore several possible solutions to the problem of pretrial disparity and argue that non-legal interventions such as simply informing judges of their decision-making are both low-cost and effective. At the same time, the results of this study clearly highlight the danger in adopting a one-size-fits-all approach to reshaping the pretrial system. The dynamics in rural and suburban courts are likely different than those in urban courts, misdemeanor criminal charges are distinct from felony charges, and the specific institutional characteristics of a given legal regime will almost certainly interact differently to reforms than another regime. While this sort of message is often perceived as the killjoy of progress, we believe that it is only with an informed and individualized approach to bail reform that the current climate can produce lasting and meaningful change.

APPENDIX

Appendix A: Supplemental Figures

Figure A1: Sample Pima County Initial Appearance Form

APPEARANCE ORDER WITH RELEASE CONDITIONS – COURTS OF PIMA COUNTY, ARIZONA

State of Arizona vs. [redacted] | CASE NO. [redacted]

Citation #s and or Docket #s:	Charges: (Cargos) Bond: (Fianza)	Agency #s:
1. ____ 2. ____ 3. ____ 4. ____ 5. ____	3-1 SHOPLIFTING	BN# 170528025 SO 170528045
YOUR NEXT DATE IS: SU PROXIMA CITA EN EL TRIBUNAL 6/1/17 AT: 2:30 ☐ A.M. ☒ P.M. Interpreter Needed: ____	**YOUR COURT IS:** Pima County Justice Court 240 N Stone Ave Tucson AZ 85701 520-724-3171	**YOUR NEXT EVENT IS:** TRANSPORT: ☐ YES VIDEO: ☒ YES ☐ Arraignment ☐ Preliminary Hearing ☒ Pretrial/CM Conference ☐ Trial ☐ Sentencing/Disposition Hearing ☐ Review Hearing ☐ Violation Hearing ☐ No Bond/Prop ____ ☐

ORDER: ☐ Quash Warrant ☐ Lift Suspension ☐ Consolidate Civils ☐ Set Aside Defaults

☒ Assign to Judge: PESQUIERA ☐ Stay Fines

YOUR ORDERS INCLUDE THE STANDARD CONDITIONS OF RELEASE (see reverse side) AND THOSE CHECKED BELOW:

☐ RELEASED ON SIGNATURE (ROR) ☐ HOLD WITHOUT BOND

☐ RELEASED TO P.T.S. or ☐ 3RD PARTY by: Print Name ____ /S/ ____

☒ BOND TOTAL $ 500 ☐ Cash ☐ Secured ☐ Already Posted ☐ P.T.S if bond posted

☐ The demanding jurisdiction has 24 hours to make arrangements to take custody of the defendant. If the demanding jurisdiction does not take custody of the defendant by ____ (if blank, 72 hours), the release conditions are modified to ROR, and the defendant is ordered to appear on the date and at the location listed above.

☐ Do not drive without valid license (No conducir auto sin licencia vigente)

☐ Drink no alcoholic beverages (No tomar bebidas embriagantes)

☐ Commit no acts of Domestic Violence (No cometer actos de Violencia Domestica)

☐ Possess no firearms (No tener armas de fuego en su poder)

☐ Complete drug monitoring/treatment as directed by P.T.S. (Sometorse a la vigilancia/tratamiento por abuso de drogas si lo exige la agencia de Servicios Pre-procesales.

☐ Go to the Minimum Security Facility @ 1801 S. Mission Rd, Ph 351-8311, on any Tuesday from 10 AM to 2 PM or any Friday from 5 PM to 9 PM for ☐ fingerprints and photo ☐ a DNA sample by ____ and bring this form. (Vaya a las instalaciones de seguridad minima (carcel), cualquier dia Martes entre las 10 a.m. y las 2 p.m. o cualquier dia Viernes entre las 5 p.m. y las 9 p.m. ☐ para que le tomen las huellas digitales y fotografia. ☐ para la prueba de ADN, antes del ____ y traiga este formulario con usted.)

☐ No contact of any kind with (No tener comunicacion ni contacto de ningun tipo con): ____

☐ Do not go to (no acudira): ____

☐ Counsel appointed ☐ PD 724-6800 ☐ LD 740-5775 ☐ City PD 791-4857 ☐ Contract Attorney: ____ Ph # ____

☐ Contact the OCAC at 243-4465 for a financial review of the cost of counsel.

☐

I WILL COMPLY WITH ALL MY RELEASE CONDITIONS. I KNOW IF I VOLUNTARILY FAIL TO APPEAR, EVEN IF DEPORTED, THE TRIAL AND OTHER PROCEEDINGS MAY CONTINUE, ANY BOND MAY BE FORFEITED AND A WARRANT MAY ISSUE FOR MY ARREST. IF I AM DEPORTED, I KNOW IT MAY BE POSSIBLE TO GET PAPERS TO TEMPORARILY RE-ENTER THE U.S. I KNOW IT IS MY OBLIGATION TO MAINTAIN CONTACT WITH MY LAWYER IN THIS CASE FROM OUTSIDE THE U.S. IT IS MY RESPONSIBILITY TO ATTEMPT TO ARRANGE RE-ENTRY TO BE PRESENT AT MY TRIAL. I KNOW IT IS NOT AN OBLIGATION OF THE STATE OF ARIZONA TO GET THE PAPERS AND THE PAPERS ARE ISSUED AT THE DISCRETION OF THE U.S. FEDERAL GOVERNMENT. (CUMPLIRE CON TODAS MIS CONDICIONES DE LIBERTAD. SE QUE SI NO COMPAREZCO VOLUNTARIAMENTE, AUNQUE SE ME HAYA DEPORTADO, EL JUICIO Y OTRAS DILIGENCIAS SE PUEDEN LLEVAR A CABO EN MI AUSENCIA, QUE SE PUEDE DECOMISAR CUALQUIER FIANZA PAGADA Y QUE SE PUEDE EMITIR UNA ORDEN PARA MI ARRESTO. SI SOY DEPORTADO, SE QUE TAL VEZ SEA POSIBLE RECIBIR DOCUMENTACION QUE ME PERMITA REGRESAR PROVISIONALMENTE A LOS ESTADOS UNIDOS. ACEPTO QUE SI ESTOY FUERA DEL PAIS QUE ES OBLIGACION MIA MANTENER CONTACTO CON MI ABOGADO EN CUANTO A MI CASO. RECONOZCO QUE ES MI OBLIGACION HACER LOS ARREGLOS PARA MI REINGRESO PARA ESTAR PRESENTE EN MI JUICIO. YO SE QUE NO ES UNA OBLIGACION DEL ESTADO DE ARIZONA DE DARME ESTA DOCUMENTACION PROVISIONAL Y QUE LA DECISION SI OTORGAR DICHA DOCUMENTACION ES AL CRITERIO ABSOLUTO DEL GOBIERNO FEDERAL.)

Defendant's Signature (Firma) Address (Direccion) Zip Code (Zona Postal) Phone# (Telefono)

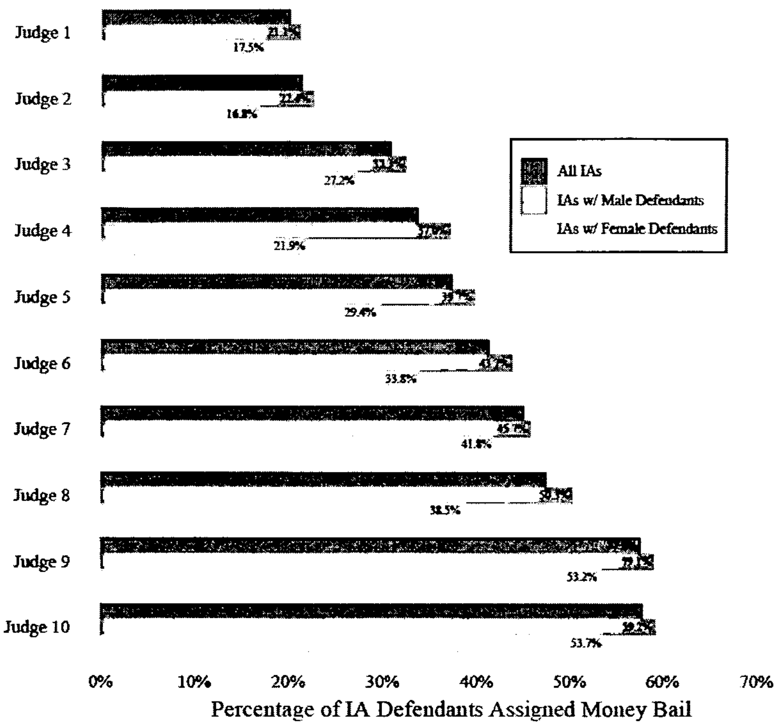
Judge/Releasing Authority

Form 40(?)

5/28/2017 PM

Date

Figure A2
Judicial Disparities in Likelihood of Assigning Money Bail
(Sex Comparison)



Appendix B: ANOVA Regression Analysis Supplemental Explanation and Table

Although our balance tests suggest that the IA assignment process yields caseloads that are balanced across all covariates, it is also prudent to test whether the small imbalances that do exist are not correlated with judge assignment. In order to do this, we conducted a series of time-fixed (month of IA hearing) multinomial logit regressions of judge assignment using the following covariates: sex, race/ethnicity (Hispanic/Latinx), age (years at IA), residency (Tucson), criminal history (number of previous charges), and IA charge type dummy variables (DUI, drug, disorderly conduct, theft, and violent crime).[235] In order to measure the individual predictiveness and significance of these covariates, we use ANOVA tests to compare the residual deviance (a measure of model "fit") of this base model against the residual deviance of a series of comparison models that each left out one of the aforementioned covariates. By running an ANOVA test on each leave-out model, we can know how strongly each covariate is related to judge assignment. Each of these comparison tests produced two measures: a differenced residual, or the extent to which the leave-out variable was predictive of judge assignment, and an F-test p-value.

The results of the ANOVA tests comparing the fit of the multinomial logit regressions are presented in Table B1. Column 1 specifies the leave-out model against which the full model is compared, where the "Sex" model is a regression of judicial IA assignment on all covariates except for the binary variable for a defendant's sex, the "Age" model is a regression on all the covariates except for age, etc. Column 2 reports the differenced residuals of the base model against the given leave-out model. While the exclusion of each variable from the full multinomial logit model does result in a positive difference in residuals—suggesting that there is a positive relationship between each variable and the assignment of IA judge, only the relationship with the binary indicator for whether the IA included a drug charge is statistically significant at the 0.05 level once a multiple-comparison correction is applied (highlighted in bold in Table 3).[236] None of the other ten differenced residuals approach a level that would suggest reliably strong relationships. Just as with the three failures in the balance tests, the drug charge failure is not expected with a truly random IA assignment process, but alone it is not indicative of any systematic threats to our ability to utilize the system in Tucson County to draw causal inferences. Additionally, to account for any potential bias, we include the binary indicator for drug charges in the

235. We defined each of these variables for the logit regressions in the same way that we defined them for the balance tests in Section II.D.

236. See *supra* note 178 for a discussion of how we treat multiple comparison testing.

IA—along with the other covariates featured in the balance and ANOVA tests—as a control variable in the first and second stages of the instrumental variable design.

Table B1: ANOVA Regression Residuals

Excluded Pre-treatmnet Covariates	Residual Difference
Full Model**	97,608.827
	-
Sex	6.504
	(0.069)
Hispanic	4.595
	(0.868)
Tucson Resident	5.271
	(0.810)
# of Previous Charges	11.552
	(0.240)
DUI at IA	1.149
	(0.266)
Drug at IA	**34.165***
	(0.000)
Disorderly Conduct at IA	20.518
	(0.015)
Theft at IA	5.039
	(0.831)
Assault/Violent at IA	9.230
	(0.410)

Notes: P-values for each regression are presented in parentheses and were calculated using 2,000-permutation randomization inference where judicial assignments were randomly reassigned in month-level blocks.
*Observations in bold and designated by * are statistically significant at the $p < .05$ level after adjusting for multiple comparisons using the Bonferroni-Holm correction.
**The full ANOVA model included controls for sex, race/ethnicity (Hispanic), age (years at IA), residency (Tucson), criminal history (number of previous charges), and IA charge type dummy variables (DUI, drug, disoderly conduct, theft, and violent crime).

Appendix C: Instrumental Variable Analysis Supplemental Explanation and Tables

Our research design in this Article draws on the instrumental variable approaches that have been used in similar studies that have used quasi-random assignment of judges to measure the impact of bail amounts on similar outcomes.[237]

Identifying the unbiased causal effect of pretrial detainment decisions on case outcomes and recidivism is deceptively difficult. As Gupta et al.[238] and Dobbie et al.[239] point out, one could simply estimate a linear probability model of the following form:

$$Y_{ict} = \alpha + \beta MoneyBail_{ict} + \gamma X_{ict} + \epsilon_{ict},$$

where Y_{ict} is the outcome of interest—whether a case outcome, recidivism, or employment—for individual i, in case c, in year t. α is the constant, β is whether money bail assigned to an individual at the conclusion of the IA, γX_{ict} is a matrix of covariates with defendant and case-level control variables, and ϵ_{ict} is an error term.

While this equation serves as a simple model, it likely does not provide unbiased causal estimates of the impact that bail has on the outcomes of interest. Higher bail amounts may induce higher convictions and recidivism, but the relationship may also result from other factors such as race, gender, and criminal history. Judges, for example, are probably more likely to assign money bail to defendants who are at the highest risk of offending, so any positive correlation between the assignment of money bail and the propensity for an individual to recidivate could actually be reflective of the causal relationship between defendant characteristics and recidivism. If that is the case, recidivism estimates will be biased toward finding that pretrial release reduces future crime.

This causal entanglement is not new to empirical researchers interested in studying pretrial detainment. As we outlined previously, some recent work has homed in on the potential of utilizing random judicial assignment to measure the causal effects of bail assignment and pretrial detention. Following this recent methodological trend, our analysis relies on two variations of a basic instrumental variables model to predict the impact of pretrial release on potential outcomes. Our primary model uses the propensity for a given judge to assign money bail in an IA as the instrument, and the secondary model uses the average amount of bail assigned by a judge as reported in Tables C2, C4, and C5, below. Our pre-analysis plan called for the primary model to use pretrial release as the treatment (along with a

237. *See supra* note 193 and accompanying text.

238. Gupta et al., *supra* note 141, at 479–80.

239. Dobbie et al., *supra* note 39, at 214–18.

number of variations on release), but we were not able to access the release data.

Both of these estimators are multivariate generalizations of the instrumental variable models presented by Imbens and Angrist,[240] where the outcome of interest is modeled as a linear function of the treatment (the assignment of bail or the existence of pretrial release), a set of "prognostic" pre-treatment covariates, and unobserved error.[241]

Formally,

$$Y_{ict} = \beta_0 + \beta_1 MoneyBail_{ict} + X_{ict}\delta + \epsilon_{ict},$$

where Y_{ict} is the outcome of interest for individual i in court c, at time t. β_0 is the constant, $\beta_1 MoneyBail_{ict}$ is the effect of an individual's bail assignment, $X_{ict}\delta$ is a matrix of covariates with ϵ_{ict} as the error term, and the subscript c denotes court-level specific effects (this is necessary because random assignment to judges is done at the court level). Similarly, an individual's bail amount is modeled as a linear function of the instrument (assignment of judge), a set of pre-treatment covariates, and unobserved error.

We follow the instrumental variable strategy developed by Dahl et al.,[242] Dobbie and Song,[243] and Dobbie et al.,[244] where a residualized leave-out mean is estimated for each defendant. Because we know that the judges and magistrates who make bail determinations are not truly randomly assigned in Pima County—for instance, judges can (and do) trade shifts and do not have the same number of days of the week over the course of a rotation—a simple leave-out mean could bias estimates. To take care of these time-related fixed effects, we use fixed effects for the day of week, month, and year. These fixed effects constrain the universe of defendants to those who would be assigned to the same set of judges.

Our instrument thus takes the following form:

$$MoneyBail^*_{it} = MoneyBail_i - \gamma X_{it} = Z_{jt} + \epsilon_{it},$$

where X_{it} includes the time-related fixed effects, $MoneyBail^*_{it}$ includes the judge propensity measure, Z_{jt}, and the error term, ϵ_{it}, captures defendant-specific characteristics.

More formally, for each case, we calculate a leave-out mean in the following manner:

240. *See supra* note 194.

241. We note here, as we did in the body of this Article, that the estimand produced by this design is inherently the local average treatment effect (LATE) of bail, or the average treatment effect for only the individuals who are on the margins of being assigned money bail previous to trial, not the average treatment effect of detainment on all pretrial defendants.

242. Gordon B. Dahl, Andreas Ravndal Kostøl & Magne Mogstad, *Family Welfare Cultures*, 129 Q.J. ECON. 1711, 1720–21 (2014).

243. Will Dobbie & Jae Song, *Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection*, 105 AM. ECON. REV. 1272, 1284–85 (2015).

244. Dobbie et al. *supra* note 39; *see also supra* note 193 and accompanying text.

$$Z_{jt} = \frac{1}{n_{jt} - 1}\left[\sum_{k=1}^{n_{jt}}(MoneyBail_k) - MoneyBail_i\right] - \frac{1}{n_t - 1}\left[\sum_{k=1}^{n_{ct}}(MoneyBail_k) - MoneyBail_i\right]$$

For each subject in the dataset, the individual instrumental value is the assigned IA judge's overall likelihood of assigning money bail in a given time period (month-level periods) absent the bail determination in that subject's personal IA hearing. In other words, the instrument is the assigned judge's mean bail propensity excluding the result in the case associated with the datapoint in question. This produces an instrumental value that captures the variation across judges in the propensity for assigning money bail, which is, importantly, a function of quasi-random assignment and therefore allows for accurate causal identification. Results from the primary leave-out measure (binary bail assignment) are presented in the body of this Article.[245] Results from the secondary leave-out measure (logged bail amount) are presented below.[246]

245. *See infra* Tables 3–5

246. *See infra* Table C5.

Table C1: Monotonicity Tests (Whether or Not Money Bail Was Assigned)

Sub-sample Regression (1a)	Coefficient (Standard Error) (1b)	Sub-sample Regression (2a)	Coefficient (Standard Error) (2b)
Defendants with Criminal History	1.220* (0.056)	Defendants with No Criminal History	1.024* (0.029)
Hispanic/Latinx Defendants	0.988* (0.043)	White Defendants	1.077* (0.037)
Defendants Residing in Tucson	1.235* (0.040)	Defendants Residing Outside of Tucson	0.878* (0.036)

Notes: Each model in this table estimates first stage regressions of the binary bail assignment leave out mean on judge assignment across sub-samples, includes time-fixed effects (year-month-day), and controls for IA case characteristics (IA charge count and type), defendant characteristics (sex, ethnicity/race (white), age at IA (years) [Tuscon residency was excluded due to excessive missing data]), and criminal history (prior convictions count and type). Columns (1a/1b) and (2a/2b) present inverse sub-samples. Each sub-sample regression excludes the pre-treatment covariates according to which the sub-samples were divided (e.g. the criminal history sub-samples exclude criminal history covariates).
*All coefficients are statically significant at $p < 0.001$ after adjusting for multiple comparisons using the Bonferroni-Holm correction.

Table C2: Monotonicity Tests (Logged Bail Amount)

Sub-sample Regression (1a)	Coefficient (Standard Error) (1b)	Sub-sample Regression (2a)	Coefficient (Standard Error) (2b)
Defendants with Criminal History	0.996* (0.079)	Defendants with No Criminal History	0.844* (0.029)
Hispanic/Latinx Defendants	0.814* (0.044)	White Defendants	0.882* (0.037)
Defendants Residing in Tucson	1.000* (0.041)	Defendants Residing Outside of Tucson	0.729* (0.036)

Notes: Each model in this table estimates first stage regressions of the logged bail amount leave out mean on judge assignment across sub-samples, includes time-fixed effects (year-month-day), and controls for IA case characteristics (IA charge count and type), defendant characteristics (sex, ethnicity/race (white), age at IA (years) [Tuscon residency was excluded due to excessive missing data]), and criminal history (prior convictions count and type). Columns (1a/1b) and (2a/2b) present inverse sub-samples. Each sub-sample regression excludes the pre-treatment covariates according to which the sub-samples were divided (e.g. the criminal history sub-samples exclude criminal history covariates).
*All coefficients are statically significant at $p < 0.001$ after adjusting for multiple comparisons using the Bonferroni-Holm correction.

Table C3: First-stage Regression (Whether or Not Money Bail Was Assigned)

	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Full Sample	1.082*	1.071*	1.070*	1.061*
	(0.024)	(0.025)	(0.025)	(0.027)
6-Month Censored Dataset	1.080*	1.070*	1.079*	1.065*
	(0.025)	(0.027)	(0.027)	(0.029)
12-Month Censored Dataset	1.074*	1.058*	1.064*	1.052*
	(0.027)	(0.029)	(0.029)	(0.031)
18-Month Censored Dataset	1.080*	1.059*	1.066*	1.055*
	(0.030)	(0.032)	(0.032)	(0.034)
24-Month Censored Dataset	1.083*	1.056*	1.060*	1.052*
	(0.033)	(0.035)	(0.035)	(0.037)
Time Fixed Effects	No	Yes	Yes	Yes
Case-type Controls	No	No	Yes	Yes
Pre-treatment Controls	No	No	No	Yes

Notes: Each first stage model in this table estimates the effect of the binary bail assignment leave out mean on whether bail is assigned using ordinary least squares regressions. Time fixed effects account for year, month, and week of IA case assignment. Case-type controls include a count of new and pending charges at the IA and the existence of the following charge types: Drug, Drug, FTA, Theft, and Violent Crime (dummy variables for disorderly conduct and trespass were excluded due to multicoliniarity). Pre-treatment controls include: a count of prior convictions, the existence of any previous convictions of the types outlined in the case type controls, age at IA (years), sex, race/ethnicity (white) and residence (whether the defendant resides in Tucson).

*All coefficients are statically significant at $p < 0.001$ after adjusting for multiple comparisons using the Bonferroni-Holm correction.

Table C4: First-Stage Regression (Logged Bail Amount)

	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Full Sample	1.000*	0.987*	0.964*	0.941*
	(0.021)	(0.022)	(0.022)	(0.023)
6-Month Censored Dataset	1.002*	0.991*	0.978*	0.949*
	(0.022)	(0.024)	(0.023)	(0.025)
12-Month Censored Dataset	1.001*	0.984*	0.971*	0.945*
	(0.024)	(0.026)	(0.026)	(0.027)
18-Month Censored Dataset	1.000*	0.976*	0.963*	0.939*
	(0.026)	(0.028)	(0.028)	(0.029)
24-Month Censored Dataset	1.000*	0.970*	0.953*	0.934*
	(0.028)	(0.030)	(0.030)	(0.031)
Time Fixed Effects	No	Yes	Yes	Yes
Case-type Controls	No	No	Yes	Yes
Pre-treatment Controls	No	No	No	Yes

Notes: Each first stage model in this table estimates the effect of the logged bail amount assignment leave out mean on whether bail is assigned using ordinary least squares regressions. Time fixed effects account for year, month, and week of IA case assignment. Case-type controls include a count of new and pending charges at the IA and the existence of the following charge types: Drug, Drug, FTA, Theft, and Violent Crime (dummy variables for disorderly conduct and trespass were excluded due to multi-coliniarity). Pre-treatment controls include: a count of prior convictions, the existence of any previous convictions of the types outlined in the case type controls, age at IA (years), sex, race/ethnicity (white) and residence (whether the defendant resides in Tucson).

*All coefficients are statically significant at $p < 0.001$ after adjusting for multiple comparisons using the Bonferroni-Holm correction.

Table C5: Results for Case Outcomes and Recidivism (Logged Bail Amount)

	2SLS Results (1)	2SLS Results (2)	2SLS Results (3)	2SLS Results (4)
Guilty Plea	0.083*	0.063	0.000	0.010
	(-0.012, 0.177)	(-0.141, 0.140)	(-0.085, 0.086)	(-0.082, 0.102)
Guilty Judgment	0.007	0.001	0.002	0.002
	(-0.012, 0.025)	(-0.017, 0.019)	(-0.020, 0.016)	(-0.024, 0.022)
Failure to Appear	-0.026	0.023	-0.001	-0.001
	(-0.123, 0.070)	(-0.045, 0.091)	(-0.058, 0.056)	(-0.057, 0.058)
Rearrest in 6 Months	-0.096**	**-0.088*****	**-0.094*****	-0.091**
	(-0.171, -0.021)	(-0.155, -0.021)	(-0.159, -0.028)	(-0.161, -0.021)
Rearrest in 12 Months	-0.079***	-0.067*	-0.074**	-0.067*
	(-0.145, -0.012)	(-0.144, 0.009)	(-0.144, -0.004)	(-0.137, 0.004)
Rearrest in 18 Months	-0.067**	-0.061	-0.064	-0.061
	(-0.133, -0.001)	(-0.145, 0.024)	(-0.148, 0.021)	(-0.129, 0.008)
Rearrest in 24 Months	-0.051	-0.052	-0.057	-0.049
	(-0.129, 0.027)	(-0.129, 0.025)	(-0.142, 0.029)	(-0.158, 0.061)
Conviction of 6-Month	-0.065*	-0.051*	-0.055**	-0.049*
Rearrest Charge	(-0.130, 0.001)	(-0.108, 0.007)	(-0.110, 0.000)	(-0.115, 0.017)
Conviction of 12-Month	-0.066***	-0.047	-0.054	-0.041
Rearrest Charge	(-0.109, -0.022)	(-0.109, 0.015)	(-0.112, 0.004)	(-0.105, 0.023)
Conviction of 18-Month	-0.037	-0.021	-0.024	-0.020
Rearrest Charge	(-0.101, 0.026)	(-0.094, 0.053)	(-0.089, 0.041)	(-0.099, 0.060)
Conviction of 24-Month	-0.027	-0.023	-0.028	-0.014
Rearrest Charge	(-0.099, 0.044)	(-0.121, 0.075)	(-0.118, 0.063)	(-0.086, 0.059)
Time Fixed Effects	No	Yes	Yes	Yes
Case Fixed Effects	No	No	Yes	Yes
Pre-treatment Controls	No	No	No	Yes
Observations: Full Sample	23,679	23,679	23,679	20,614
Observations: 6 Month	20,481	20,481	20,481	17,901
Observations: 12 Month	17,588	17,588	17,588	15,473
Observations: 18 Month	14,405	14,405	14,405	12,749
Observations: 24 Month	11,688	11,688	11,688	10,363

Notes: Each second stage model estimates the effect of being assigned money bail (using the the logged continuous bail amount leave-out mean as the instrument) on judge assignment using two-stage least squares regressions. Time fixed effects account for year, month, and week of IA case assignment. Case-type controls include a count of new and pending charges at the IA and the existence of the following charge types: Drug, Drug, FTA, Theft, and Violent Crime (dummy variables for disorderly conduct and trespass were excluded due to multicoliniarity). Pre-treatment controls include: a count of prior convictions, the existence of any previous convictions of the types outlined in the case type controls, age at the time of IA (years), sex/gender, race/ethnicity (white) and residence (whether the defendant resides in Tucson), with the relevant sub-group control removed. Differences in the number of observations for each model is reflective of either excluding observations that have missing pre-treatment control variables or the need to censor the data for recidivism measures, as not all the IAs in the data occurred early enough to include outcome data at 12, 18, and 24 months. The 95% confidence intervals are presented in parentheses and were created using standard errors clustered by judge and year.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; (coefficients in bold survive a Bonferroni-Holm correction for multiple comparisons as $p < 0.1$).