No Money Bail, No Problems?

Evidence from an Automatic Release Program

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Abstract

Are the effects of money bail on pretrial misconduct large enough to justify its costs? Money bail advocates argue that its usage is critical for averting misconduct, while skeptics counter that its effects are small and not worth the human costs of pretrial detention. I examine these tradeoffs using a program in Kentucky that eliminated financial bail for a subset of low-level cases. I use administrative data and a stacked differences-in-differences approach to estimate the program's effects on misconduct and detention. The program decreased the rate of financial bail by 50.5 p.p. Even though initial bail amounts were only $360 on average, the program increased the rate of one-day pretrial release by 13.7 p.p., indicating an inability to put up even small bail amounts initially barred release. The program increased failure to appear in court by 3.3 p.p. and had no detectable effect on pretrial rearrest, with the data ruling out even modest sized increases. The effects on pretrial misconduct are mainly driven by reduced usage of money bail, which requires pretrial payments, rather than reduced usage of unsecured bail, which threatens future financial obligations, suggesting that these threats may not effectively deter pretrial misconduct. Taken together, the results imply that one instance of pretrial misconduct needs to be at least 18 times as costly as one day in detention for money bail to justify its costs.

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1 Introduction

Are the effects of money bail on pretrial misconduct large enough to justify its costs? In the US, pretrial defendants make up 65% of the approximately 500,000 people in jail each day, typically because of an inability to post money bail (Zeng and Minton 2021; Reaves 2013). Pretrial detention contributes to the $25 billion counties spend on jails each year and harms the legal, financial, and labor market outcomes of detained individuals.[1] Concerns about pretrial detention's scale, harms, and disparate impact on disadvantaged communities have fueled an ongoing wave of bail reform policies aiming to reduce the use of money bail.[2]

Alongside bail reform comes a debate about its effects on pretrial misconduct. Money bail advocates argue money bail is necessary to reduce pretrial misconduct, which includes failure to appear in court and rearrest for criminal offenses while awaiting case disposition.[3] Meanwhile, bail reform advocates argue that money bail is not an effective means of achieving its goals – its effects on misconduct are small and not worth the consequent human costs of detention.[4]

I contribute to this debate by leveraging the design and implementation of a major bail program in Kentucky. Before the program, judges chose among three bail conditions. Judges could (1) release people without financial bail conditions, *unconditional release*, (2) release people with financial bail obligations that would be paid only in the event of pretrial misconduct, *unsecured bail*, or (3) require bail be paid before people were released, *money bail*. The Automatic Release (AR) program, which was adopted at different times across Kentucky's 120 counties between 2013-2017 automatically assigned eligible low-level cases unconditional release without judicial involvement.[5]

Bail programs or reforms that bypass judges or other criminal justice decision-makers are rare but provide an attractive opportunity for examining the effects of money bail absent any judicial decision-maker involvement. Furthermore, one feature of the AR program is particularly useful for causal inference. Within each county, the presence of both eligible cases (treated) and ineligible cases (control) allows me to estimate program effects with a differences-in-differences strategy.[6]

[1] A number of papers use judge designs to estimate the causal effects of pretrial detention on harms to defendants. They find pretrial detention increases the likelihood of conviction (through guilty pleas) (Dobbie, Goldin, and Yang 2018; Heaton, Mayson, and Stevenson 2017; Leslie and Pope 2017) and decreases formal sector employment and receipt of government benefits (Dobbie, Goldin, and Yang 2018). To put the $25 billion sum in context, 6% of county spending goes towards jails, 6% goes towards county roads, and 10% goes towards K-12 education (Horowitz, Velázquez, and Clark-Moorman 2021).

[2] An incomplete list of places that have implemented changes to bail in recent years includes New Jersey (2017), New Mexico (2017), Kentucky (2017), Connecticut (2017), New Orleans (2017), Alaska (2018), Atlanta (2018), Philadelphia (2018), and New York (2020). Illinois is slated to become the first state to completely eliminate money bail in 2023 (Cramer 2021).

[3] The stated legal objective of bail is to ensure court appearance and avert new criminal activity (American Bar Association Criminal Justice Standards Committee 2007).

[4] For example, Judge Truman Morrison of the District of Columbia Superior Court says "there is no evidence you need money to get people back to court" (Harrison 2017).

[5] This program is called "Administrative Release" in Kentucky.

[6] Since this approach would be misleading if criminal justice system actors manipulated eligibility to shift people in and out of the program, I also confirm that there is no evidence of manipulating cases in or out of eligibility.

Since counties vary in their AR start dates, I use a stacked differences-in-differences approach to address time-varying treatment. My baseline specification, therefore, provides the reduced-form effects of the AR program on bail conditions, pretrial release, and pretrial misconduct.

To estimate the effects of eliminating financial conditions (moving from money bail or unsecured bail to unconditional release), I use AR program coverage as an instrument for financial bail and outline my instrumented differences-in-differences identification strategy in a potential outcomes framework. In addition to the conventional parallel trends assumption required for differences-in-differences, two additional assumptions are required. First, AR can impact bail conditions only by inducing substitution to unconditional release (extended monotonicity). Second, AR can impact pretrial release and misconduct only through changes in bail conditions (exclusion restriction).

Moreover, because there are two types of financial bail (unsecured bail and money bail), the effects of financial bail are, in fact, a combination of two underlying effects: the effects of unsecured bail and the effects of money bail, both relative to unconditional release. Separately examining these underlying effects can shed light on the mechanisms behind the program's effects. They also address the question of external validity – the effects of switching from money or unsecured bail to unconditional release are more transportable parameters than the reduced-form effects of the Kentucky AR program itself.

To decompose the total effect of the AR program into effects driven by substitution away from money bail vs. unsecured bail, I leverage pre-program variation in bail rates by type across counties, attributable to different norms across judges. First, I estimate instrumented differences-in-differences results for two subsamples – counties that rarely used money bail and counties that rarely used unsecured bail – to focus on substitution from each alternative individually. Second, I treat unconditional release and unsecured bail as two distinct endogenous variables and instrument for them using AR program coverage interacted with county indicators. Both approaches extend Kline and Walters (2016)'s counterfactual alternatives framework to the instrumented-differences-in-differences context.

My results demonstrate that the AR program shifted bail conditions by increasing unconditional release by 50.5 percentage points (p.p.) for eligible cases (relative to a baseline of 19.8%).[7] Almost 40% of the shift to unconditional release came from substitution away from money bail, which decreased by 20.6 p.p. (relative to a baseline of 32.9%). Accordingly, the amount of money required for release also decreased by 76.9%. I find that the rate of pretrial release in one day increased by 13.7 p.p. and hours in detention decreased by 42% (relative to a baseline of 49 hours).[8] Since the mean monetary amount required for release was relatively small at baseline ($360), this result

[7] In the most related work on the direct effects of bail reform, Ouss and Stevenson (2022) find that a prosecutor-focused reform demonstrated a 11 p.p. increase in unconditional release. The fact that cases eligible for the AR program were intentionally low-level and low-risk cases demonstrates that judges err on the side of setting conditions even for the lowest level cases.

[8] Judges are required to make bail decisions within 24 hours of booking in Kentucky, so this change should be thought of as a consequence of the changes in bail conditions rather than changes in timing due to the averting judges altogether.

shows that financial difficulty with posting modest sums can constrain release. Consistent with this interpretation, release effects are larger for individuals with more binding liquidity constraints: residents of lower-income zip codes and unemployed individuals.

I provide results on two distinct types of pretrial misconduct: failure to appear in court and pretrial rearrest. I find that the program increased failure to appear rates by 3.3 p.p. (relative to a baseline of 10.7%). However, effects on pretrial rearrest are insignificant and I can rule out effects larger than 1.68 p.p. at a 5% level of confidence. Using my instrumented differences-in-differences approaches, I show that eliminating money bail is responsible for most of the program's effects. Eliminating unsecured bail has small effects that are often indistinguishable from zero, suggesting that threats of future fines (forfeiting unsecured bail) may not effectively deter pretrial misconduct.

The effects of money bail and unsecured bail could differ for two reasons. First, money bail has an incapacitation effect: people who are detained pretrial (because they cannot post bail) cannot commit pretrial misconduct. Second, money bail requires payment ex ante rather than payment ex post. When people pay ex ante, they know that pretrial misconduct will lose them money with certainty rather than with some probability. I find that the marginally released defendants would need to fail to appear more than 60% of the time for the differences to solely be due to incapacitation. Since this is a very high failure to appear rate, it is likely that the timing of payment also affects people's behavior as a certain loss of pre-paid money is a greater deterrent than the potential loss of future fines.

Taken together, my findings suggest that the complete elimination of money bail relative to always using money bail is associated with a 16.6 p.p. higher pretrial misconduct rate (mostly driven by failure to appear in court) and a 46.5 p.p. higher rate of pretrial release within 1 day. Translating release effects into detention hours saved suggests that one instance of misconduct needs to be at least 18 times as costly as a day in detention for money bail to be worth its costs. Since unconditional release is the most lenient form of bail, my money bail estimates theoretically present an upper bound on the effects of switching away from money bail for low-level offenses regardless of the exact alternative (e.g., supervision, electronic monitoring).[9]

In a final exercise, I use my estimates to explore how courts value averting misconduct in contrast to averting detention. First, I develop a theoretical framework to investigate what valuation of detention hours relative to misconduct is implied by the status quo. The key intuition is that cases that receive unconditional release due to AR are, by revealed preference, cases where the court preferred the outcomes associated with the alternate bail condition (unsecured bail or money bail). The quasi-experimental nature of the AR program provides empirical estimates of the causal effects that I can use in this framework. Adopting the same extended monotonicity and exclusion restrictions as earlier, I show that courts find the cost of 1 court non-appearance to be greater than the cost of 27 days in detention.

[9]The consideration of different alternatives is policy-relevant – many proposed reforms aim to replace money bail with alternatives that are stricter than unconditional release, such as supervision or electronic monitoring.

To provide context for this tradeoff, I explore whether it aligns with observable financial costs to courts of detention and misconduct.[10] The financial costs to courts of failure to appear are about 10 times as large as financial costs to courts of 1 day in detention, based on estimates from prior literature. These estimates do not rationalize the revealed preference valuations – the AR program saves courts money, which suggests that courts do not simply minimize their fiscal costs. This could be explained if courts value avoiding misconduct beyond its fiscal consequences, consistent with a theory from Ouss and Stevenson (2022) that judges experience asymmetric penalties for their errors – allowing misconduct is more highly penalized than setting unnecessarily harsh bail conditions.

My paper proceeds as follows. I first provide background information on the US bail system and the Kentucky AR program in Section 2. Section 3 describes the administrative court data and my identification strategy for estimating program effects. Section 4 presents results on the causal effects of the AR program. Section 5 builds a potential outcomes framework for using program-induced variation to estimate the effects of eliminating financial conditions and Section 6 homes in on the effects of eliminating money bail (vs. eliminating unsecured bail) specifically. In a final exercise, Section 7 uses my estimates to illustrate how courts theoretically value detention relative to misconduct. Section 8 concludes.

1.1 Related literature

In studying the effect of a bail reform program, my paper joins a nascent literature evaluating recent bail reform measures that intend to shift norms away from money bail. For instance, Ouss and Stevenson (2022) studies a prosecutor-driven reform, which increased unconditional release by 11 p.p. for eligible defendants. Skemer, Redcross, and Bloom (2020) shows a New York City supervision program successfully shifted cases away from receiving money bail, however, the program also resulted in less unconditional release.

The AR program I study is distinctive in its avoidance of judicial discretion. Bail reform, like many policy reforms, is often at the mercy of the discretion of criminal justice actors, meaning effects are often weaker than expected (Ouss and Stevenson 2022; Stevenson and Doleac 2019). The AR program's aversion of judicial discretion is responsible for the large 50.5 p.p. effect on unconditional release. Whether rules are binding administrative processes or simply nudges to judicial officers makes a large difference when it comes to comparing intended and realized outcomes (Stevenson 2018; Albright 2019).

My paper also joins an existing literature on the effects of bail conditions themselves. The closest related work is Abrams and Rohlfs (2011), which studies effects of money bail amounts using

[10] Financial costs to courts do not include the costs of pretrial detention to detained individuals (in terms of increased convictions or decreased employment/receipt of government benefits) (Dobbie, Goldin, and Yang 2018; Heaton, Mayson, and Stevenson 2017; Leslie and Pope 2017). Since non-court costs of detention are large, ignoring them biases the exercise in the direction of understating detention costs relative to misconduct costs.

random assignment of judges to bail guidelines. Myers Jr (1981) and Helland and Tabarrok (2004) demonstrate that less bail is associated with more failure to appear. However, these later papers do not rely on quasi-experimental variation. My paper is the first to my knowledge to derive causal effects for distinct types of bail conditions.

Bail conditions are not the only policy lever for impacting defendant behavior pretrial. There is a growing experimental literature demonstrating that court reminders have meaningful effects on court appearance (Emanuel and Ho 2020; Fishbane, Ouss, and Shah 2020). Moreover, Emanuel and Ho (2020) demonstrate that the causal effect of failing to appear varies across defendants, but may result in larger fines or fees.

A key part of the motivation of my paper is the large costs of pretrial detention. The costs to detained individuals have been demonstrated by a number of papers using quasi-experimental judge designs – pretrial detention increases the likelihood of conviction (through guilty pleas) and decreases formal sector employment and receipt of government benefits (Dobbie, Goldin, and Yang 2018; Heaton, Mayson, and Stevenson 2017; Leslie and Pope 2017). I focus on the causal effects of bail conditions themselves, which, in turn, impact detention outcomes.

Finally, this paper contributes to the literature on interactions between personal finances and the criminal justice system. In this paper, I show small monetary amounts restrict pretrial release, suggesting tight liquidity constraints on people in the criminal justice system. This result is consistent with Mello (2021)'s finding that traffic tickets increase the chance of a new default for low-income drivers. Aneja and Avenancio-León (2020) demonstrate that (post-conviction) incarceration reduces access to credit, which in turn increases recidivism in a perverse feedback loop.

2 Bail Background and Kentucky's Automatic Release Program

2.1 Background on the US Bail System

There are over 10 million arrests every year in the US (O'Toole and Neusteter 2019). After arrest, a judge or magistrate determines the conditions that govern a person's release pretrial. These are what we refer to as bail conditions. The institutional details and process of bail setting varies widely across the country.[11] However, the stated legal objective of bail is consistent: bail conditions should be set at the least restrictive levels to ensure court appearance and public safety (American Bar Association Criminal Justice Standards Committee 2007).[12] As such, bail is meant to incentivize

[11] The determination can occur anywhere between 12 and 48 hours after initial arrest. The decision can take place in person in a courtroom, over video, or over the phone. It also varies by jurisdiction if defendants are present during the process and whether prosecutors are involved.

[12] In this context, public safety usually means averting pretrial rearrest. Also, note that some places, such as New York City, are only supposed to consider failure to appear in bail decisions.

good conduct pretrial, but it can also lead to pretrial detention due to individuals' liquidity constraints.

There are four broad and mutually exclusive categories of bail. In order of least to most restrictive, they are: (1) unconditional release, (2) unsecured bail, (3) money bail, and (4) bail denial.[13]

1. Unconditional release has no financial bail penalties. People who are unconditionally released do not have to post money for release nor will they forfeit a bail amount if they commit misconduct.

2. Unsecured bail does not limit release but can impose additional financial penalties ex post. People assigned unsecured bail do not need to post money for release, but they may forfeit a set bail amount if they commit misconduct.

3. Money bail requires individuals to post some money amount for release. If individuals do not meet money bail requirements, they are detained pretrial until the bail amount is revisited or until the case is concluded.

4. Bail denial means that no amount of money can secure pretrial release. This is reserved for the most severe cases.

Figure 1a demonstrates how bail types feed into potential pretrial release, which feeds into potential pretrial misconduct. Under unconditional release, unsecured bail, and money bail if paid, individuals are released. Since they are then free pretrial, it is possible for people to commit pretrial misconduct (fail to appear in court or be rearrested pretrial). If individuals do not post the required amount when assigned money bail, they are detained and it is simply not possible for them to commit pretrial misconduct.

If someone fails to appear in court, the court may put out a warrant for their arrest, send a court notice, or charge them with contempt of court and a fine. Emanuel and Ho (2020) demonstrate that the causal effect of failing to appear varies across defendants, but may result in larger fines or fees. If someone is rearrested for a new charge while out pretrial, they face additional charges associated with the new arrest.[14]

Therefore, there is always a potential cost of pretrial misconduct to people in the criminal justice system even if there are no financial bail penalties (that is, under unconditional release). If someone is released pretrial, bail conditions should be thought of as an additional layer of sanctions on top of the baseline criminal justice system penalty to pretrial misconduct. Under money bail, individuals forfeit their already posted bail amounts. Under unsecured bail, individuals may forfeit their

[13] On top of these four main categories, there are also conditions that are not financial in nature. For instance, bail conditions can require supervision (defendants must check in with court staff during the pretrial period), or electronic monitoring (defendants must wear physical monitoring devices). Bail conditions can also disallow certain behaviors such as driving or drinking or contact with a victim. Failure to comply with conditions can mean rearrest for violation of release conditions.

[14] They are also less likely to receive lenient bail on the rearrest since they have a pending case.

predetermined bail amount.

2.2 Kentucky Bail System Background

After someone is arrested and booked in one of Kentucky's 120 counties, a pretrial officer (an employee of the statewide Pretrial Services agency) working in that county collects information about the defendant and arrest incident to help facilitate pretrial decisions.[15] Within 24 hours of booking, the pretrial officer will present this information to a judge (usually during a phone call).[16] Judges then make decisions about bail type and bail amount (if applicable) within a few minutes.[17]

In terms of the four main categories, 16.8% of Kentucky cases receive unconditional release, 27.2% receive unsecured bail, 54.3% receive money bail, and 1.8% have bail denied.[18] Since bail denial is so rare, I focus on the three remaining categories that characterize nearly 100% of cases: unconditional release, unsecured bail, and money bail.

In most states, people cannot afford money bail on their own, they can work with a bail bonds company to secure the required amount. Commercial bail bonds companies front the bail amount if paid some non-refundable fee by the defendant (usually 10% of the total).[19] In Kentucky, there is no commercial bail bonds industry. Therefore, defendants or their networks need to post the required bail amount.[20]

2.3 Kentucky Automatic Release Program

From 2013 through 2017, Kentucky phased in a program called Administrative Release, which I call Automatic Release (AR), to expedite pretrial release for defendants charged with non-violent, non-sexual misdemeanors (e.g., shoplifting, disorderly conduct, criminal driving offenses).[21] The

[15] Kentucky's Pretrial Services is a state-funded agency that serves all 120 counties in the state. Pretrial employees are housed in individual counties and include pretrial officers/supervisors as well as risk assessment specialists/coordinators.

[16] In Kentucky, initial bail decisions are not subject to prosecutorial review, as they are in most other states. Therefore, judge decisions are not conditional on prosecutor actions; judges make decisions solely based on information presented by pretrial officers.

[17] If someone remains in jail for 24 hours after receiving money bail in Kentucky, their bail can be revisited and lowered. In other words, the initial bail is not necessarily the permanent bail decision through case disposition. Revisiting bail in Kentucky can be thought of as a way to proactively avoid long periods of pretrial detention due to inability to pay.

[18] Using the same categories, national data on felony cases shows that 28% of cases receive unconditional release, 4% receive unsecured bail, 62% receive money bail, and 6% have bail denied (Cohen and Reaves 2007). (Note that 8% of the 28% unconditional release cases involve non-financial conditions.)

[19] Distinct from the commercial bail bonds industry, there are also non-profit organizations that post bail on defendants' behalf – there are more than 60 such bail funds nationally (Rahman 2020).

[20] Judges can set 10% money bail instead of full money bail, thus only requiring a 10% deposit (similar to what a commercial bail bondsman would require). However, this is rare according to the administrative data.

[21] The goal was to reserve resources for higher-risk cases by providing automatic unconditional release for a subset of defendants who would normally have bail set by a judge.

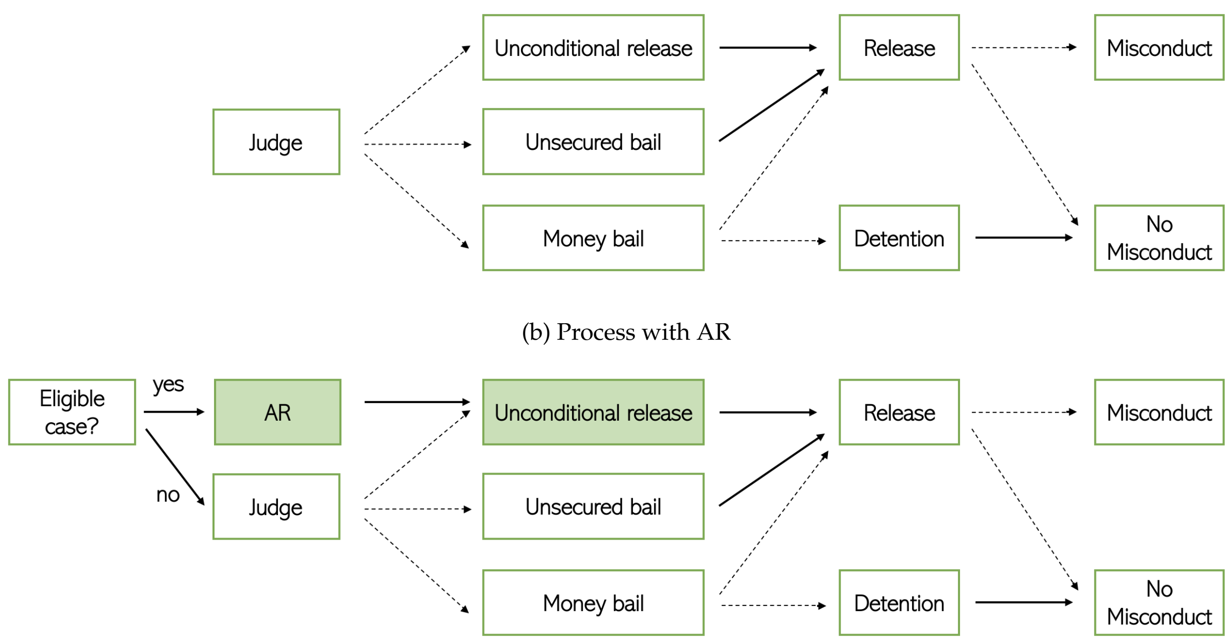
program's design and implementation provides quasi-experimental variation in program exposure as well as bail conditions, making it well suited for causal inference.

How AR impacts the pretrial process: If the AR program is not yet in place in a given county, a pretrial officer presents information about the defendant and alleged offense to a judge. The judge then makes a bail decision within a few minutes and the flow of outcomes follows the illustration in Figure 1a. If the AR program is in place, what happens depends on case eligibility. Figure 1b shows that eligible cases are assigned unconditional release without the involvement of a judge, while ineligible cases go through the system as usual – pretrial officers present information to judges and judges make bail decisions.

Figure 1: The Pretrial Process

(a) Process in the absense of AR

(b) Process with AR



Notes: Figure 1a demonstrates that in the absense of AR, judges can choose between the three conditions: unconditional release, unsecured bail, and money bail. Unconditional release and unsecured bail lead to certain release (solid arrows), but money bail leads to uncertain release or detention (dashed arrows). If released, there is some possibility of misconduct (dashed arrows). If detained, there is no possibility of misconduct (solid arrow). Figure 1b shows that, if a case is eligible, the AR program bypasses the judge and assigns unconditional release. If a case is not eligible, the process is the same as what is in Figure 1a.

As such, the program shrinks the scope of bail setting. Jurisdictions are increasingly considering similar automatic release programs for people arrested on low-level offenses.[22] In fact, Proposition 25 in California (voted down in November 2020) which would have implemented a program based on Kentucky's AR program across all of California.

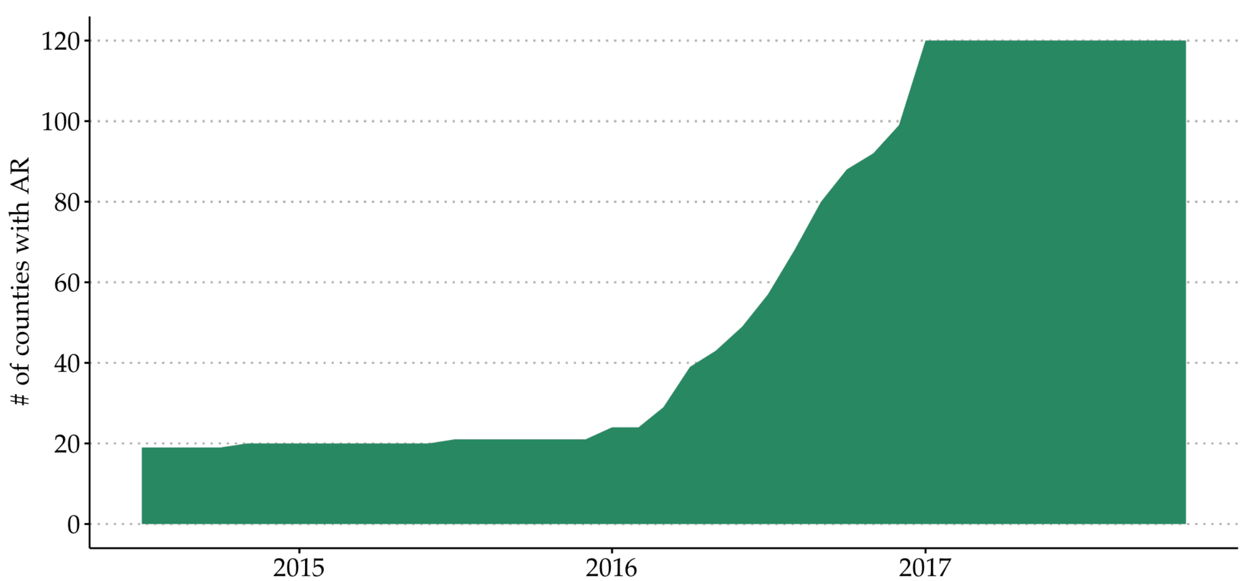
Most bail reform efforts and programs rely on judicial discretion, but AR intentionally limits

[22]These sorts of changes are also recommended by groups such as the ACLU (Woods and Allen-Kyle 2019).

judicial discretion. Limiting discretion means more binding changes since judges often deviate from recommended actions (Stevenson 2018; Albright 2019; Stevenson and Doleac 2019). In fact, activists involved with organizing for Illinois's Pretrial Fairness Act, which will make Illinois the first state to end money bail in 2023, explained that judicial discretion in prior bail reform waves made it "increasingly clear [...] that a more binding, statewide policy change was needed" (Grace 2021). Due to this observation, the Pretrial Fairness Act intentionally creates "bright-line rules that [take] away carceral tools from judges instead of trusting them to use such tools sparingly" (Grace 2021).

AR policy timing across the state: The AR program was phased in across the state between September 2013, when it was first piloted in a small group of counties, and 2017, when it became mandatory across the state (Supreme Court of Kentucky 2013, 2017a). Figure 2 demonstrates AR take-up timing across the counties – it is an area plot showing the number of counties with the AR program in place over time.[23] In late 2014, about 20 counties had AR in place. The Figure shows the majority of counties (about 80 of them) took up at various dates between January 2016 and December 2016. The remaining 20 counties took up when the program became mandatory statewide in 2017.

Figure 2: AR Timing Across Kentucky Counties



Notes: This figure is an area plot that demonstrates the number of counties with AR in place at the start of each month between July 2014 and November 2017. About 20 have AR before the start of the sample window. All 120 have AR by January 2017 since that is when the program went statewide.

AR eligibility requirements: The details that determine eligibility into the AR program shifted over time. Originally, the pilot counties listed out county-specific eligibility conditions (Supreme

[23]There is no official record of the exact AR implementation dates by county. Therefore, I follow recommendations from administrative court staff and define AR to start in a county on the first day that there is an observation of a "pilot release" in the administrative bail data.

Court of Kentucky 2014). A Supreme Court Order in November 2015 standardized eligibility across counties (Supreme Court of Kentucky 2015). When I discuss eligibility I use the January 2017 eligibility requirements (Supreme Court of Kentucky 2017a), which captures eligibility well for all counties that took up AR after the November 2015 order (since the differences between the two orders are slight) (Supreme Court of Kentucky 2015, 2017a).[24] See Appendix A for details on the evolution of eligibility over time.

Eligibility is determined based on arrest type, charges, and risk score (Supreme Court of Kentucky 2017a). To be eligible, a case must meet the following three requirements:

1. the case must be associated with a "regular arrest"
   - An arrest is a "regular arrest" if it is not a rearrest, violations of condition arrest, bench warrant arrest, or indictment.
2. all associated charges must be in a predetermined set of "AR eligible charges"
   - AR eligible charges are non-violent non-sexual misdemeanors with some exceptions.[25] Given these constraints, common AR eligible charges are: driving offenses (driving without insurance, driving on a suspended license, driving without a license), drug paraphernalia (buy/possess), shoplifting, and disorderly conduct/public intoxication.[26]
3. the defendant must have a risk score below 8 (on a scale of 2-12)
   - A pretrial officer will calculate this risk score during the initial information collection stage after arrest. The risk score used is the Composite Public Safety Assessment (PSA) Score. An example of a person with a risk score of 8 would be someone who: is under 23, has failed to appear once in the last 2 years, and has a prior misdemeanor conviction which resulted in a sentence to incarceration. See Appendix A.2 for the details on how the risk score is calculated.[27]

The percentage of observations that fit these requirements are 68%, 34%, and 75%, respectively. Thus, the charges themselves are the biggest limiting factor for eligibility. I generally call these charges low-level offenses. On the whole, about 21% of cases are eligible on all three dimensions.

[24]The AR program became mandated across the state in January 2017, but the risk score eligibility guidelines changed in December 2017, thus my sample ends November 30, 2017 (Supreme Court of Kentucky 2017a, 2017b). (According to Kentucky Pretrial staff, as of 2017, the AR rules were followed in a standardized way. Before the 2017 order, there was a lack of clarity in how counties followed stated rules, according to administrative court staff.)

[25]AR eligible charges are non-violent non-sexual misdemeanors excluding the following charges: failure to appear, bail jumping, violation of a protective order, contempt of court, violations of probation or conditional discharge, DUIs with injuries or accident or any aggravated circumstances, and DUIs on a suspended license.

[26]The data from Kentucky AOC only includes cases where the top charge (most severe charge) is a felony or misdemeanor, so there is no need to discuss violations.

[27]See Appendix B for background on risk score usage across the US.

3 Data and Identification Strategy

Direct evidence on the effects of eliminating money bail is naturally limited by policy changes and data availability. Reforms majorly reducing money bail usage are recent and their implementation often does not induce quasi-experimental variation. Moreover, most bail reform measures are subject to discretion by system actors, which can distill their intended impacts. Kentucky's AR program, therefore, provides a unique opportunity due to the policy relevance of the reform, availability of data, and its bypassing of judicial discretion.[28]

3.1 Data Construction using Kentucky Administrative Office Records

To leverage variation across time and eligibility in the AR program, I require case-level data on bail setting, detention and misconduct outcomes, and program eligibility. I am able to construct the necessary case-level data using a collection of datasets from the Kentucky Administrative Office of the Courts that span all criminal cases with felony or misdemeanor charges across all 120 Kentucky counties from July 1, 2009 through December 31, 2017.[29]

Bail Setting: I use data on the initial bail observation for each distinct case.[30] This includes the date of the bail decision (relevant for if the case is before or after AR implementation), the bail category (unconditional release, unsecured bail, money bail), bail amount in dollars (if applicable), and county (relevant for if the case is before or after AR implementation).

Detention Outcomes: I calculate the number of hours between the original booking date (time of booking into jail after arrest) and the eventual release date. Pretrial release is a consequence of making bail or case disposition.

Misconduct Outcomes: Failure to appear outcomes are directly recorded for each case in the administrative data. However, pretrial rearrest outcomes are not consistently captured, so I generate measures of these outcomes based on observable rearrests in the Kentucky data.[31] Therefore, this captures rearrests within the state only.[32] Note that pretrial rearrest does not include rearrests due to failing to appear (since that is not considered a new criminal offense). Therefore, the two types of misconduct are mutually exclusive.

Eligibility Status: Crucially, I need to identify cases as eligible or ineligible for the AR program.

[28]On policy relevance, jurisdictions are increasingly considering similar automatic release or diversion programs for people arrested of low-level offenses. In fact, Proposition 25 in California, though voted down in November 2020, would have implemented a program based on Kentucky's AR program across all of California.

[29]See Appendix D for details on data construction.

[30]Bail can be revisited if someone is in pretrial detention in Kentucky, which can mean multiple bail observations for a single case.

[31]Note the distinction between pretrial rearrest and pretrial interactions with the justice system. Specifically, a citation or summons that does not involve an arrest is distinct from a pretrial rearrest.

[32]For Appendix results where I differentiate between non-violent and violent rearrests, I define a pretrial rearrest as violent if any of the associated charges with the rearrest are considered violent based on Kentucky documentation.

This is not directly captured in the administrative data, so I perform this tagging myself. I tag cases as eligible or ineligible based on observable variables and the language of relevant Supreme Court orders, which describe AR implementation. I confirm my reading of the eligibility criteria via interviews with local practitioners, such as court staff members.[33] I define cases as AR eligible if they meet the following 3 criteria: the case is the result of a regular arrest, the case only includes eligible non-violent, non-sexual misdemeanor charges, and the case pertains to a defendant with a risk score below 8.[34]

There are likely instances where the observable data misses a detail that shifts a case in or out of eligibility. While arrest type and risk score components of eligibility are straightforward, the charge details are trickier. For example, charge codes in the administrative data are the Kentucky Uniform Crime Reporting Codes assigned by law enforcement officers, which can be different from the charge in the narrative record, which was used to determine eligibility before 2017. As another example, some arrests are the consequence of criminal warrants, which sometimes feature bail amounts set by judges.[35] If someone is arrested on a warrant with an amount specified, pretrial officers cannot release the person on AR; those cases have to go to a judge. Since the administrative data does not capture if someone is arrested on a warrant with a set bail amount, I cannot deem these observations ineligible.[36]

In short, the lack of eligibility status in the administrative data means that there is some rate of false positives (ineligible cases classified as eligible) and false negatives (ineligible cases classified as eligible). However, I will show strong evidence that my constructed eligibility tag is doing a good job capturing true program eligibility.

Sample Restrictions: I limit my sample due to constraints imposed by policy change details and available administrative data. The risk scores used for eligibility determination were first used on July 1, 2014. Since this is the first month when I can observe necessary risk scores for tagging eligibility, it marks the start of my sample period. Similarly, the risk score eligibility criterion changed in December 2017 (Supreme Court of Kentucky 2017b). Since eligibility categorization changes at that point, this marks the end of my sample period. Due to these two details, I require the initial bail decision date for a case to be between July 1, 2014 and November 30, 2017 for inclusion in the sample. Moreover, since November 2015 was when the key components of eligibility (as described in Section 2.3) were made consistent, I exclude cases from counties that implemented AR before November 2015. (Their eligibility criteria are distinct from the criteria that govern the later 99 counties.)[37]

[33] More details are in Appendix A.

[34] As such, ineligible cases are one of the following: they are the result of non-regular arrests, they include ineligible charges (say, felonies or violent/sexual misdemeanors), or they pertain to defendants with risk scores above 7.

[35] In interviews, an administrator estimated these sorts of arrests could compromise 10% of all arrests.

[36] For more details on criminal warrants, see Appendix A.

[37] Most of those 21 counties also only feature post-AR data in the sample window (see Figure 2), meaning they are not informative for a differences-in-differences approach anyway.

Socioeconomic Information: I also use information on defendant employment status and zip code. This data was acquired through special authorization from the Kentucky State Supreme Court (Supreme Court of Kentucky 2021).[38] The data was collected on interview forms during pretrial interviews or subsequent contacts between Pretrial Services and defendants.

3.2 Identification Strategy

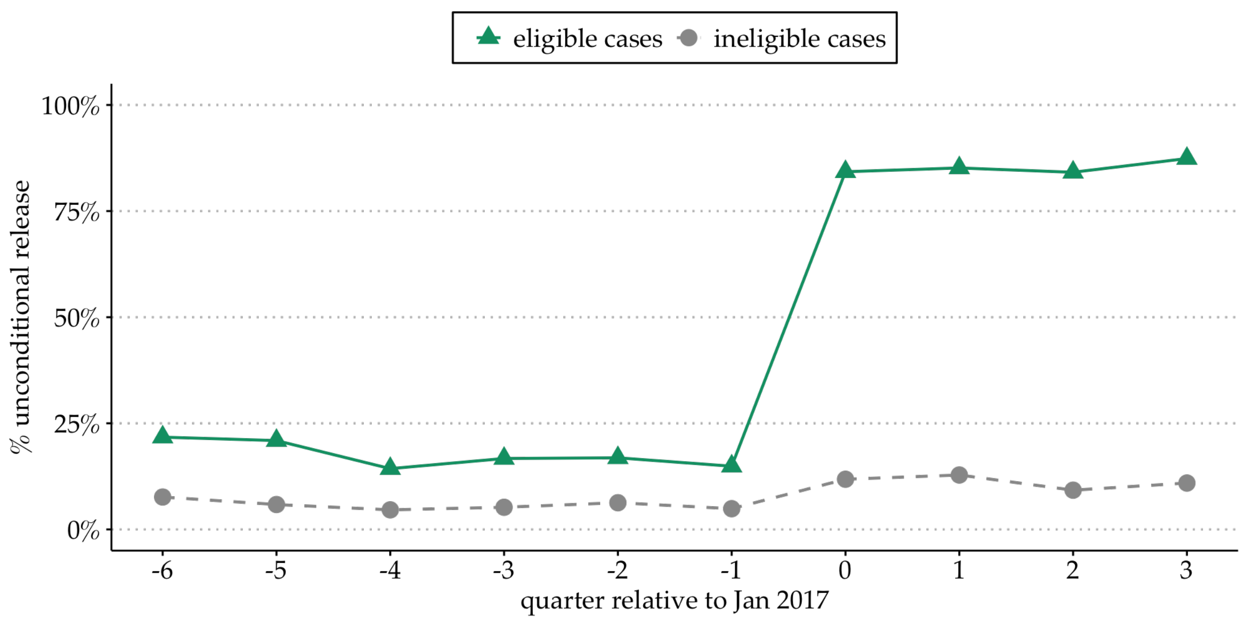
3.2.1 Differences-in-differences with *one* AR start date

Based on the program design described by Figure 1, the AR program should increase unconditional release for eligible cases after implementation (as long as judges were not giving these cases unconditional release at high rates already). Is this the case in the constructed data?

I provide visual evidence in Figure 3, which demonstrates changes in the set of counties that adopt AR in January 2017. I limit to counties with one AR start date to abstract away from concerns about staggered program timing and illustrate the identification strategy when there is one AR start date.

Figure 3 is a binned scatter plot showing the unconditional release rates for eligible and ineligible cases in the quarters before and after AR take-up in January 2017. AR take-up causes a dramatic increase in unconditional release for eligible cases but not ineligible cases.

Figure 3: AR Impacts Bail for Eligible Cases



Notes: Points illustrate the percentage of cases that receive unconditional release in each quarter relative to AR implementation. Eligible cases are represented by green triangles (connected by a regular line) and ineligible cases are represented by gray circles (connected a dashed line). Cases are limited to those in counties that adopt AR in January 2017.

[38]See the full Supreme Court order relevant to my data request here: https://kycourts.gov/Courts/Supreme-Court/Supreme%20Court%20Orders/202130.pdf

Eligible cases are more likely than ineligible cases to receive unconditional release before AR and the difference is similar over pre-periods, which is consistent with evidence of unconditional parallel trends in a differences-in-differences framework. After AR, 90% of eligible cases receive unconditional release (instead of around 20% just beforehand).

In theory, the unconditional release rate for eligible cases under AR should be exactly 100%. However, recall that I tag case eligibility using observable data, which might miss some factors that change case eligibility (as discussed in Section 3.1). The tagging is the primary explanation for the imperfect assignment to unconditional release.[39] However, the large jump in unconditional release for eligible (but not ineligible) cases demonstrates that the eligibility tag does a good job picking up program exposure.

Variation by eligibility and time in exposure to the AR program, as demonstrated in Figure 3, provides an opportunity to trace out program impacts using a differences-in-differences approach (and bail condition impacts using an instrumented differences-in-differences approach). This approach may be misleading if criminal justice system actors manipulated eligibility to shift people in and out of the program. If it became tougher to be classified as eligible after the program, then the eligibility classification itself shifted with the policy. This would mean eligible/ineligible cases before and after are not comparable. I use Appendix C to show that there is no evidence of discontinuous chances in charge eligibility or risk score eligibility around the time of program implementation. The fraction of defendants arrested on eligible charges does not change discretely around program implementation, nor does the fraction of defendants assigned eligible risk scores.

Each individual county (or county cohort based on AR date) provides an opportunity for a differences-in-differences approach with 1 program date to estimate the effects of automatic release. If all the counties took up AR at the same time, I could employ conventional two-way fixed effect and event-study differences-in-differences approaches. Under unconditional parallel trends, this would mean estimating:

$$y_{it} = \beta Eligible_i + \lambda_t + \delta^{DD}(Post_t \times Eligible_i) + \epsilon_{it} \tag{1}$$

$$y_{it} = \beta Eligible_i + \lambda_t + \sum_{q \neq -1} \delta_q^{DD} \Big[\mathbb{I}[t - AR = q] \times Eligible_i \Big] + \epsilon_{it} \tag{2}$$

where y_{it} is an outcome for case i at time t, $Eligible_i$ is an indicator for if case i is AR eligible, and λ_t are time fixed effects. In the pooled approach, $Post_t$ is an indicator for if t is after the time of AR take-up AR, and δ^{DD} is the differences-in-differences coefficient of interest. In the event-study approach, $\mathbb{I}[t - AR = q]$ is an indicator for if time t is q full quarters away from the date AR, and δ_q^{DD} is a vector of $q-1$ differences-in-differences coefficients of interest (where the quarter before

[39]The secondary explanation is relevant to counties that adopt AR earlier. Namely, administrative practices on AR were "messier" earlier, meaning that some eligible cases missed out on AR due to unobservable administrative learning and logistical difficulties, according to interviews with pretrial staff members.

AR, $q-1$, is the reference period).

However, since counties take up AR at different times, as illustrated in Figure 2, I need to address this complicating factor with a different sort of specification.

3.2.2 Differences-in-differences with *all* AR start dates

Since the 99 counties in my sample take up AR at different times, there is time-varying treatment. However, I do not need to leverage the staggered timing itself for identification since I have valid identification within each county independently.[40] Within any given county, eligible cases are treated units, ineligible cases in the same county are control units, and treatment turns on at the county's AR start date. (Figure 3 demonstrates this set-up for the counties that take up in January 2017.) Rather than use staggered timing for identification itself, I calculate an average effect across all 99 distinct AR implementations using a single set of treatment indicators.

To do this, I follow an existing approach to estimating a differences-in-differences specification with time varying treatment and treatment effect heterogeneity: "stacked regression."[41] I define relative time periods q based on the number of full quarters away the bail date is from the AR start date. Therefore, at the same moment in calendar time (same t), different counties may be in different quarters relative to AR (different q). To make sure county composition does not differ across relative quarters, I subset the data to 6 quarters before AR and 3 quarters after AR (i.e., $q \in [-6, 3]$ where $q=-1$ is the quarter before take-up) because cases from all counties are observed in those relative time periods.[42] My final dataset can be thought of as a stacked dataset where each event-specific dataset is just the observations associated with one of the 99 counties. The estimated pooled and event-study specifications are:

$$y_{itc} = \beta Eligible_{ic} + \lambda_{tc} + \delta^{DD}(Post_{tc} \times Eligible_i) + \epsilon_{it} \tag{3}$$

$$y_{itc} = \beta Eligible_{ic} + \lambda_{tc} + \sum_{q \neq -1} \delta_q \Big[\mathbb{I}[t - AR_c = q] \times \mathbb{I}(eligible_i)\Big] + \epsilon_{itc} \tag{4}$$

where case i in county c implements AR on date AR_c. As such, $Post_{tc}=1$ if and only if $t - AR_c \geq 0$. The difference between stacked specifications 3 and 4 (and their single county analogs 1 and 2) is that AR start dates now vary by counties (AR_c) and I saturate the eligibility indicator (similar to a unit fixed effect) and time fixed effects with indicators for counties. Cengiz et al. (2019) saturate their specification with stacked dataset indicators to calculate an average treatment effect across

[40] Inferential concerns about differences-in-differences designs that rely on staggered timing for identification is a growing area of study in the econometric and applied microeconomics literature (Baker, Larcker, and Wang 2021; Sun and Abraham 2020; Callaway and Sant'Anna 2021).

[41] See Cengiz et al. (2019) for a published example and Baker, Larcker, and Wang (2021) for a description.

[42] OLS weighting can be problematic if stacked samples don't have coverage for the full treatment effect range (Baker, Larcker, and Wang 2021).

all events in their study. In my context, I saturate with county indicators to calculate an average treatment effect across all 99 counties (or county-specific events). I report standard errors clustered by county since treatment (AR) is assigned at the county level.

4 What are the effects of AR on bail conditions, release, and misconduct?

I organize results on the effects of AR in the order that corresponds to the chronology of the pretrial process as shown in Figure 1: (1) bail conditions, (2) release outcomes, and (3) misconduct outcomes. I then discuss robustness to alternative sample choices.

4.1 What is the impact of AR on bail conditions?

Figure 4 demonstrates how AR impacted bail conditions. First, Figure 4a shows that AR increased unconditional release 50.5 p.p. relative to a baseline of 19.8% for eligible defendants. Gray estimates in the pre-period are precise zeroes and the point estimates do not demonstrate consistent pre-trends, providing strong evidence in favor of the parallel trends assumption. Estimates show evidence of dynamic effects – unconditional release effects are larger a few quarters after AR. According to administrative court staff, this is likely because counties improved in their administration of the program over time.[43]

Despite the fact that the eligible group is intended to be low-risk on a number of dimensions (arrest type, charges, risk scores), note that judges only assigned 19.8% of eligible cases unconditional release before AR. This descriptive fact is consistent with Ouss and Stevenson (2022)'s argument that judges experience "asymmetric penalties in errors" – misconduct is a bad outcome for judges that is observable to the public and can be blamed on lenient bail conditions, but setting unnecessarily restrictive conditions is unobservable. Assigning unconditional release might be seen as not adequately tailoring conditions to the person, which could impact judges negatively if they are blamed for resulting misconduct.

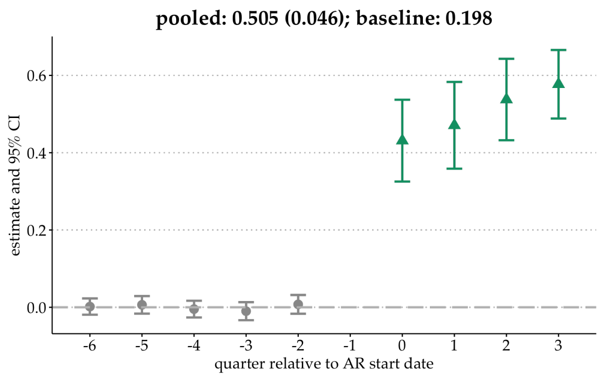
The increase in the unconditional release rate necessarily means a decrease in the usage of financial conditions (unsecured bail and money bail). Since unsecured bail is the next most strict after unconditional release, one might expect all the substitution to come from this category of bail. However, in fact, Figure 4b shows that money bail decreased by 20.6 p.p. off a baseline of 32.9% for eligible defendants.

Money bail comes with a particular bail amount required for release, meaning there is a continuous component of interest as well. In terms of dollars required for release, AR decreased that

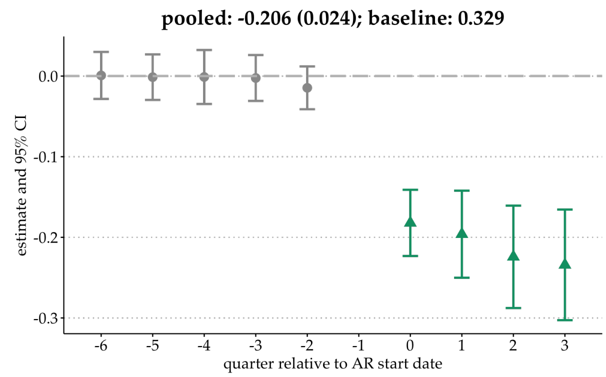
[43] There are no such dynamic effects for the counties that took up in 2017 when the program went statewide because processes were standardized and improved in advance of that policy date.

Figure 4: How AR Impacts Bail Conditions

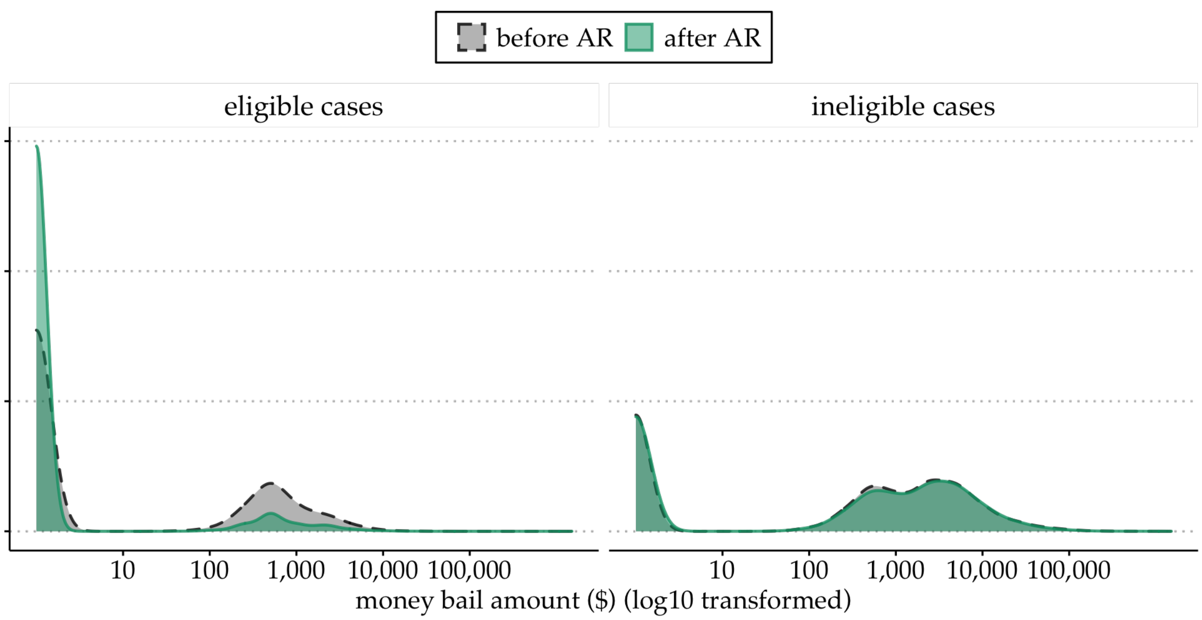
(a) Unconditional Release



(b) Money Bail



(c) Distribution of Dollars Required for Release



Notes: Figure 4a and 4b plot the event-time differences-in-differences estimates using methods described in Section 3.2.2. The outcome variable for Figure 4a is an indicator for unconditional release. The outcome variable for Figure 4b is an indicator for money bail. All figures that show event-time estimates include both point estimates and 95% confidence bands across quarters relative to AR start dates. The circular gray estimates are before AR implementation ($q \in [-6, -2]$), the triangular green estimates ($q \in [0, 3]$) are after AR implementation, and the quarter before AR ($q = -1$) is the omitted period. Figure 4c shows density plots for number of dollars required for pretrial release for eligible and ineligible cases both before and after AR. The x-axis is log10 transformed. The green shaded area (with a regular outline) is the distribution after AR and the gray shaded area (with a dashed outline) is the distribution before AR.

dollar amount by 76.9%.[44] Figure 4c demonstrates the distribution change. The figure plots the distribution of money bail amounts before and after AR for eligible and ineligible observations – the eligible group experiences a left shift away from values between $100 and $10,000 towards $0 (unconditional release or unsecured bail). The most common shift in levels for eligible cases is a change from $500 to $0.[45]

Meanwhile, the distributions before and after AR look identical for ineligible cases (they are right on top of each other), suggesting that judges are not changing their bail setting behavior as a result of the program. Moreover, there do not appear to be unintended consequences (as a result of judge behavior) that offset the AR program in the aggregate.[46]

4.2 What is the impact of AR on pretrial detention?

The most common money bail amounts for eligible cases before AR were $500, $1,000, $250, and $2,500. Since these amounts are relatively modest, it is not immediately obvious whether the program will have measurable release effects. Perhaps small money bail requirements for the eligible cases were not inducing much liquidity-driven pretrial detention, as in the case of Ouss and Stevenson (2022)'s evaluation of a prosecutor-focused bail reform. However, on the other hand, there is empirical evidence that some people are unable to cover even a $175 traffic ticket without credit consequences (Mello 2021).

My main outcome of interest is an indicator for release within 1 day of booking.[47] This is an intuitive cut-off since judges are required to set bail within 24 hours of booking during my sample time period. Therefore, if a defendant makes the first bail set by a judge, they are usually out within 24 hours. Thus, this outcome definition should focus on release changes due to liquidity constraints rather than changes due to shifts in the administrative process.

Figure 5a demonstrates that release within 1 day increases 13.7 p.p. off a baseline of 76.6%.[48] The baseline rate demonstrates that quick release was the norm within the eligible population before the program.[49] Regardless, the program's impact on release despite modest money bail requirements in its absence suggests that inability to put up a relatively modest sum is still an important constraint on pretrial release. Consistent with this interpretation, effects on release within 1 day are twice as

[44]See Appendix F for more details. The outcome variable in this case is the inverse hyperbolic sine of the money bail amount. If the observation does not receive money bail, the amount is 0. I use the inverse hyperbolic sine transformation since the distribution of amounts is right-skewed and includes zeroes.

[45]The total amount of money required for release in the year for eligible cases in my sample before AR was about $4.16 million. The AR program corresponds to about $3.2 million less being required across this population annually.

[46]An example of an unintended consequence: if judges had become harsher in ineligible cases.

[47]Appendix F presents more outcome variables.

[48]I perform a decomposition exercise to verify that the release effect is due to changes in bail conditions rather than changes in administrative speed. 96.5% of the effect is due to unconditional release assignment (complier group) as opposed to speed changes within unconditional release (always-taker group). See Appendix E for details.

[49]The magnitude of the release effect is necessarily limited by the high baseline rate (release within 1 day cannot be higher than 100%).

large for unemployed defendants (17.9 p.p.) as employed defendants (9.6 p.p.). Effects are also larger for defendants in lower-income zip codes.

To interpret the 13.7 p.p. effect in terms of detention hours, I compute mean hours in detention for those released and those not released within 1 day before the program. The means are 12.2 and 169.2, respectively. On average, release within 1 day is associated with a decline of 157 hours. The program effect is then associated with a decrease of 21.5 hours in detention on average.

Alternatively, I can directly estimate effects on detention hours, which yields a 42.4% decrease in hours in detention.[50] Relative to the baseline mean of 48.9 hours, this implies a decrease of around 20.7 hours in detention (which is extremely similar in magnitude to the 21.5 hour estimate that comes from translating the binary outcome into hours). Figure 5b demonstrates the density of density hours before and after AR for eligible and ineligible cases. While there is barely any change for ineligible cases, the eligible cases experience a shift left. Detention stays for longer than 12 hours become less frequent, while detention stays for less than 12 hours become more frequent. Part of the shift in the full distribution is driven by changes in bail conditions (more unconditional release) and another part is driven by speed in the administrative process (since AR does not require contacting a judge).[51]

4.3 What is the impact of AR on pretrial misconduct?

There are different types of pretrial misconduct, which carry different costs and policy implications. I provide results on failure to appear in court and pretrial rearrest since, according to formal guidelines, the objective of bail is to set the least restrictive bail to ensure appearance at court and avoid rearrest (American Bar Association Criminal Justice Standards Committee 2007).[52] Failure to appear in court means the defendant does not show up for the bail hearing scheduled for their arrested offense. Pretrial rearrest means the defendant is arrested on a new offense during the pretrial period (after pretrial release but before the case is concluded).[53]

Figure 6a demonstrates the estimates on failure to appear. Unlike in the prior cases where the parallel trends assumption seems clearly supported, the change in failure to appear here could be attributed to pre-trends. Nonetheless, all four of the coefficients after AR are positive and have confidence intervals that do not include 0. Taking the point estimate at face value, AR increased failure to appear by 3.3 p.p. (relative to a baseline of 10.7%). However, 6b shows that when it comes to pretrial rearrest the point estimate is close to 0 (0.7 p.p.) and is insignificant at conventional

[50] See Appendix 17 for these results, which set the outcome variable as the inverse hyperbolic sine of detention hours. Similar to the case of money bail amount, this is useful due to the right-skew of the data and the inclusion of zeroes. (Estimates using levels in both cases do not satisfy parallel trends.)

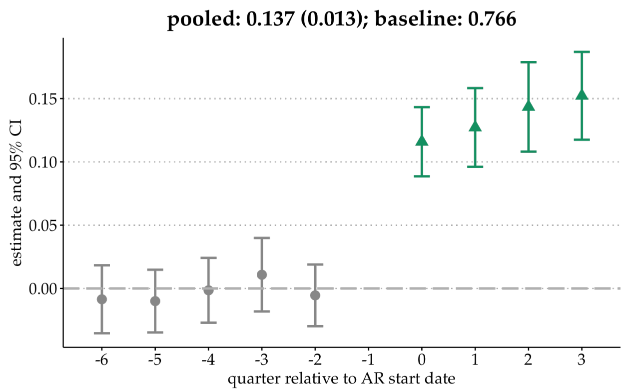
[51] In Kentucky, judges and pretrial officers have a regular schedule of bail calls either once or twice per day. If judges were available for bail calls 24/7, then the administrative speed gains of this program would be negligible.

[52] In Appendix F I also split pretrial rearrest based on violence since violent rearrests are particularly salient when it comes to bail reform backlash.

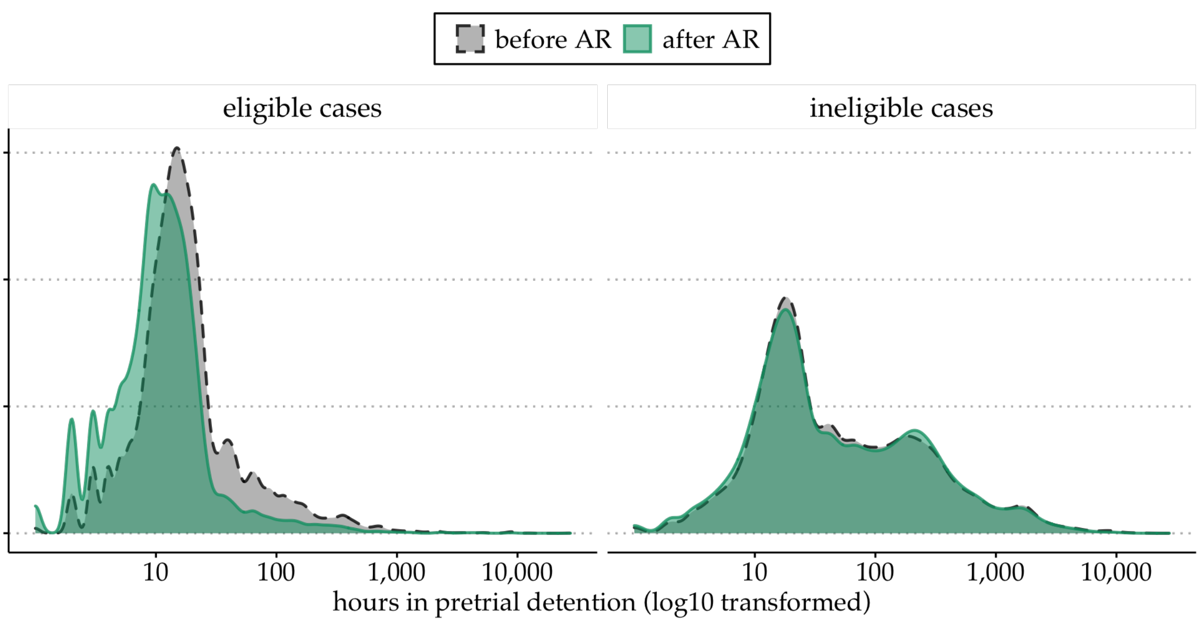
[53] This arrest cannot be for violation of pretrial conditions or failing to appear since those are not new offenses.

Figure 5: How AR Impacts Pretrial Release

(a) Release in 1 day



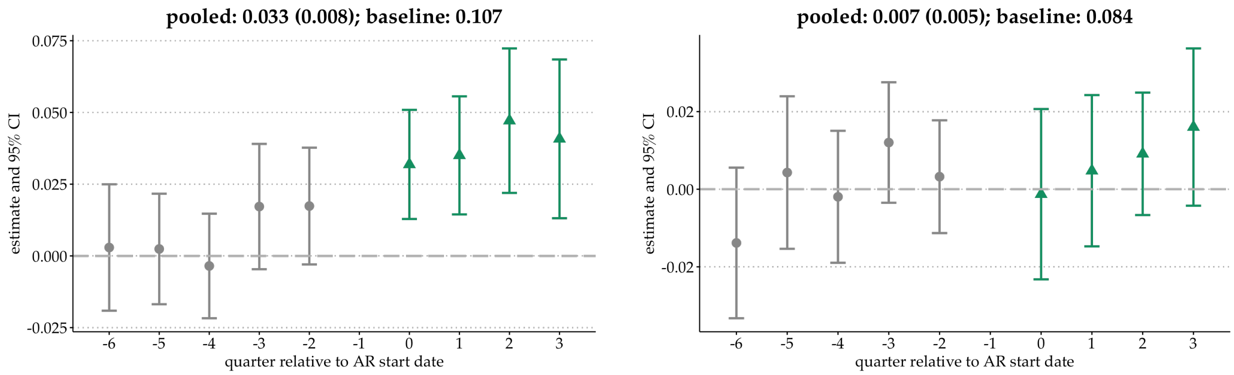
(b) Distribution of Detention Hours



Notes: Figure 5a plots the event-time differences-in-differences estimates using methods described in Section 3.2.2. The outcome variable is an indicator for release within 1 day. All figures that show event-time estimates include both point estimates and 95% confidence bands across quarters relative to AR start dates. The circular gray estimates are before AR implementation ($q \in [-6, -2]$), the triangular green estimates ($q \in [0, 3]$) are after AR implementation, and the quarter before AR ($q = -1$) is the omitted period. Figure 5b shows density plots for number of hours in pretrial detention for eligible and ineligible cases both before and after AR. The x-axis is log10 transformed. The green shaded area (with a regular outline) is the distribution after AR and the gray shaded area (with a dashed outline) is the distribution before AR.

Figure 6: How AR Impacts Pretrial Misconduct

(a) Failure to appear (b) Pretrial rearrest



Notes: Figure 6a and 6b plot the event-time differences-in-differences estimates using methods described in Section 3.2.2. The outcome variable for Figure 6a is an indicator for failure to appear in court. The outcome variable for Figure 6b is an indicator for pretrial rearrest. All figures that show event-time estimates include both point estimates and 95% confidence bands across quarters relative to AR start dates. The circular gray estimates are before AR implementation ($q \in [-6, -2]$), the triangular green estimates ($q \in [0, 3]$) are after AR implementation, and the quarter before AR ($q = -1$) is the omitted period.

levels. The 95% confidence interval includes a range of effects, including a decrease of 0.28 p.p. and an increase of 1.68 p.p. (relative to a baseline of 8.4%).[54] I can rule out increases larger than 1.68pp at the 5 percent level.

4.4 Robustness to alternative sample choices

In these main results, I use the maximum possible sample of all cases in 99 counties. However, I can run the same differences-in-differences approach homing in on cases that are only pushed in and out of eligibility due to risk scores. This means subsetting down to cases that are associated with regular arrests and eligible charges. As such, the only change is that the control group is smaller – it is then similar cases and offenses but the associated defendants have risk scores that are too high for eligibility.

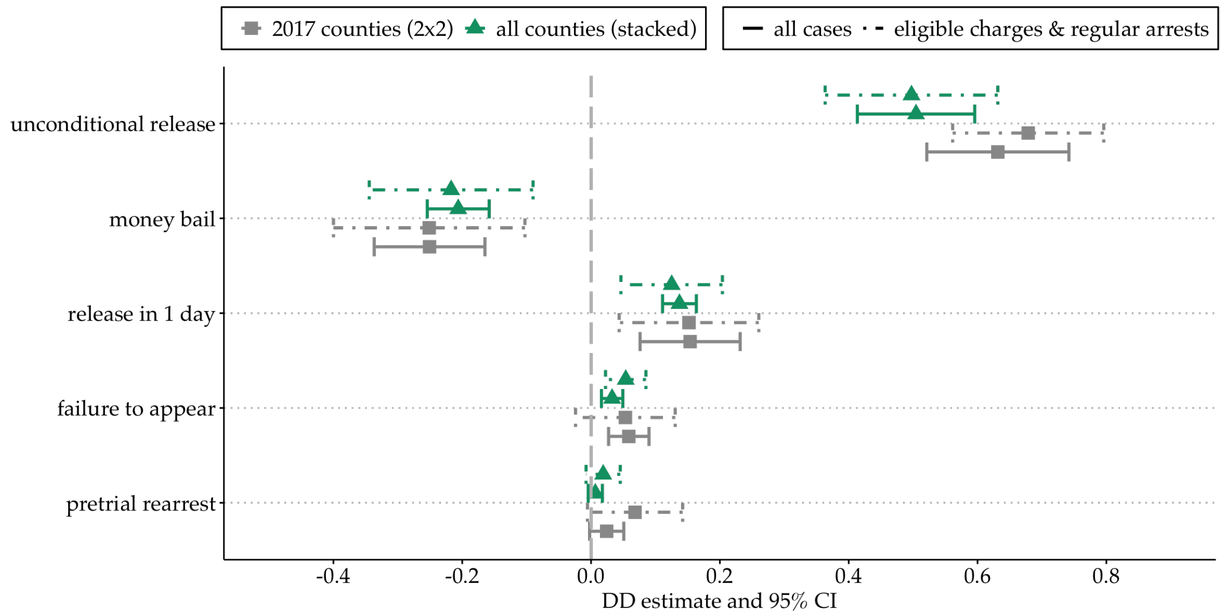
Moreover, to test out how robust my results are to avoiding staggered timing altogether, I can also generate results using only the group of counties that take up in 2017. I do this for all cases in the 2017 counties and then also only the 2017 counties that are associated with regular arrests and eligible charges.

Figure 7 demonstrates the pooled results across combinations of county samples (2017 only or full sample) and case samples (only eligible charges and regular arrests or all cases). Results for 2017 counties are slightly larger for bail conditions and pretrial rearrest. However, overall estimates look consistent across sample choices. The choice of how to refine the sample of cases or counties does

[54]For results broken up by non-violent and violent pretrial rearrest, see Appendix Figure 18.

not meaningfully change conclusions.

Figure 7: Differences-in-differences estimates using alternative samples



Notes: This figure demonstrates estimated differences-in-differences results across a range of samples and outcome variables. Square gray estimates are two-way fixed effect estimates using only 2017 counties. Triangular green estimates are those from the stacked regression approach using all 99 counties of data. Estimates with dotted lines restrict the sample to regular arrests and eligible charges (meaning risk scores are the only component that drives eligibility). Estimates with regular lines use all cases (so eligibility is defined by the 3 criteria of regular arrest, eligible charges, and risk scores).

5 What are the effects of eliminating financial conditions on release and misconduct?

While section 4 outlines reduced-form effects of AR on bail, release, and misconduct, the AR program also presents an opportunity to estimate the effects of bail conditions themselves on misconduct outcomes and time in pretrial detention. In an econometric framework, I use AR as an instrument for treatment, where treatment is the bail condition. Combining an instrumental variables approach with the differences-in-differences set-up requires two additional assumptions (on top of the conventional parallel trends assumption), which I make explicit with the following potential outcomes framework.

5.1 Potential outcomes framework

Consider a population of courts, indexed by i, each with a single defendant. Each court can assign its defendant to a bail type: unconditional release (u), unsecured bail (c) or money bail (m).

Let $Z_{it} \in \{0,1\}$ capture whether court i is covered by AR or not. AR coverage varies by time $t \in \{t_0, t_1\}$ and case eligibility status $e \in \{e_0, e_1\}$. t_0 is the time period before AR for given court i and t_1 is the time period after AR for given court i. e_0 is the group of defendants (and thus courts) who are ineligible under AR rules and e_1 is the group of defendants who are eligible under AR rules. Thus, $Z_{it} = 1$ for (e_1, t_1) only and $Z_{it} = 0$ for all other combinations – i.e., (e_0, t_1), (e_1, t_0), and (e_0, t_0).

Let $B_{it}(Z_i) \in \{u, c, m\}$ denote the defendant's potential treatment status (bail type) as a function of AR coverage.

The AR program maps onto theoretical restrictions on substitution patterns. AR coverage should induce defendants who would have otherwise received unsecured bail (c) or money bail (m) to receive unconditional release (u) instead. No court should switch between unsecured bail (c) and money bail (m) in response to bail reform coverage, and no court should be induced by bail reform coverage to switch a defendant away from unconditional release. In other words, the only way bail reform coverage should change bail setting is to shift those receiving c or m to u. This is an extended monotonicity assumption (assumption 1) and can be expressed by the condition below:[55]

$$B_{i1}(1) \neq B_{i1}(0) \rightarrow B_{i1}(1) = u$$

Under this assumption, the full population of courts is characterized by the following groups:

1. c-compliers: $B_{i1}(1) = u, B_{i1}(0) = c$
2. m-compliers: $B_{i1}(1) = u, B_{i1}(0) = m$
3. c-never takers: $B_{i1}(1) = c, B_{i1}(0) = c$
4. m-never takers: $B_{i1}(1) = m, B_{i1}(0) = m$
5. always takers: $B_{i1}(1) = u, B_{i1}(0) = u$

Because of the trio of bail condition options, complier and never taker groups are split into two subgroups, unlike in conventional instrumental variable set-ups. When covered by AR, the c- and m- compliers switch to unconditional release from unsecured bail and money bail, respectively. The two groups of never takers are never given unconditional release regardless of AR coverage. Always takers manage to receive unconditional release even when they aren't covered by AR – the court grants them unconditional release with judicial discretion in absence of the program. The key extended monotonicity assumption means there are no defiers who switch away from u and there are no AR-induced shifts between c and m.

Consistent with Figure 1, the later-stage outcomes of interest are misconduct and release. Call these $M_{it}(b)$ and $R_{it}(b)$. For $Y \in \{R, M\}$, we can write the reduced form effects of AR on Y as: $E[Y_{i1} - Y_{i0} | Z_i = 1] - E[Y_{i1} - Y_{i0} | Z_i = 0]$ if I make an additional assumption. Specifically, omitting

[55] This is also a condition in Kline and Walters (2016)'s evaluation of Head Start in the face of multiple alternatives. The condition extends the monotonicity assumption of Imbens and Angrist (1994) to a setting with multiple counterfactual treatments.

the bail condition information from the Y_{it} notation requires an exclusion restriction (assumption 2). This assumption means that the only way AR coverage impacts court appearance and detention outcomes is through the bail type (i.e., the only treatment channel is the bail condition category).

Assumptions (1) and (2) paired with the validation of the parallel trends assumption allow for identification with instrumented differences-in-differences. The DD-IV estimand can be written:

$$\frac{E[Y_{i1} - Y_{i0}|Z_i = 1] - E[Y_{i1} - Y_{i0}|Z_i = 0]}{E[1\{B_{i1} = u, B_{i0} \neq u\}|Z_i = 1] - E[1\{B_{i1} = r, B_{i0} \neq u\}|Z_i = 0]}$$

Because of parallel trends and the fact that $Z_{i0} = 0$ for all i (Hudson, Hull, and Liebersohn 2017),

$$= \frac{E[Y_{i1}(B_{i1}(1)) - Y_{i1}(B_{i1}(0))]}{E[1\{B_{i1}(1) = u, B_{i1}(0) \neq u\}]} = E[[Y_{i1}(u) - Y_{i1}(B_{i1}(0))|B_{i1}(1) = u, B_{i1}(0) \neq u]$$

Intuitively, this effect is acquired by attributing the entire reduced form effect to the complier group (those who are spared bail conditions due to AR).[56]

5.2 Using instrumented differences-in-differences to estimate effects of removing financial conditions

Specifications 5 and 6 demonstrate the first-stage and second-stage regressions in the stacked regression set-up. Table 1 demonstrates the results from instrumented differences-in-differences approach instrumenting for unconditional release with AR coverage (the interaction of case eligibility and relative time being after AR adoption).

$$unconditional_{itc} = \beta Eligible_{ic} + \lambda_{tc} + \delta^{DD}(Post_{tc} \times Eligible_i) + \epsilon_{it} \tag{5}$$

$$y_{itc} = \beta Eligible_{ic} + \lambda_{tc} + \delta^{DD-IV}\widehat{unconditional}_{itc} + \epsilon_{it} \tag{6}$$

I also demonstrate results instrumenting for money bail, assuming that the switch from unsecured bail to unconditional release has no effects (i.e., all AR effects are attributable to reduced use of money bail). This set of estimates can be thought of as an upper bound on the effects of eliminating money bail (in favor of unconditional release).

Table 1 shows that 27.2% of those spared financial conditions avoid spending 1 or more days in detention. This result highlights the prevalence of detention consequences even for those arrested on low-level offenses and is consistent with previous evidence on the large costs of even small fines and fees in the justice system (Mello 2021). Meanwhile, about 6.5% of those spared money-related conditions will fail to appear, which means 94.5% of that population did not have appearance ensured by conditions. Using Ouss and Stevenson (2022)'s language on error types, 94.5% of the

[56] To identify point estimates, the effects of unconditional release on detention and misconduct can be derived by simply rescaling the reduced form effects by the first-stage effects (of AR on unconditional release).

complier population under the status quo receive Type II errors (too harsh), while 6.5% of the complier population under AR receive Type I errors (too lenient).[57] Results on pretrial rearrest remain insignificant.

Table 1: Instrumented differences-in-differences estimates

	Release in 1 day		Failure to appear		Pretrial rearrest	
	(1)	(2)	(3)	(4)	(5)	(6)
Unconditional release	0.2720***		0.0648***		0.0130	
(instrumented)	(0.0255)		(0.0149)		(0.0109)	
Money bail		-0.6656***		-0.1585***		-0.0318
(instrumented)		(0.0577)		(0.0330)		(0.0253)
Observations	136,917	136,917	136,917	136,917	136,917	136,917

Notes: This table demonstrates instrumented differences-in-differences results; it demonstates estimated effects of bail conditions on the outcomes of: release in 1 day, failure to appear, and pretrial rearrest. In columns (1), (3), (5), the first-stage follows equation 5 and the second-stage follows equation 6. In columns (2), (4), (6), the first-stage follows equation 5 but the left-hand side is an indicator for money bail and the second-stage follows equation 6 but the instrumented endogenous variable is money bail. Standard errors are clustered at the county-level. (* p<0.1, ** p< 0.05, *** p<0.01)

5.3 Did judges correctly identify less risky cases before AR?

The potential outcomes framework gives me the machinery to provide descriptive evidence on this question. I can compare failure to appear rates for always takers (cases assigned unconditional release by judges before AR) to the implied rates for compliers (cases assigned unconditional release after AR only due to the program). The rate of failure to appear for cases unconditionally released before AR was 0.105. After AR, cases unconditionally released are composed of different types of cases (they include always takers – cases that would have gotten AR before regardless – and compliers – cases that only get unconditional release because of the reform) and the rate is higher at 0.173.

Assuming the always takers behave the same way, I can solve for the implied failure to appear rate for compliers (x):

$$0.173 = P(u,a)(.105) + P(u,c)(x)$$

where $P(u,a)$ is the share of unconditional releasees who are always takers, and $P(u,c)$ is the share of unconditional releasees who are compliers. Since 0.196 of the eligible population received unconditional release before AR and 0.75 received unconditional release after, then the compliers failure to appear rate is 0.197. This means the compliers are around twice as risky on failure to

[57] In this case, "too harsh" or "too lenient" refers to whether the conditions induced changes in failure to appear behavior. It is a different question whether the conditions are worth imposing due to simultaneous detention changes. This is modeled and addressed explicitly in Section 7.

appear than the always takers. As such, the always takers were correctly identified as less risky by judges even within the eligible case group.

5.4 How responsible is incapacitation vs. deterrence for effects on misconduct?

In this context, incapacitation refers to pretrial detention induced by financial restrictions, while deterrence refers to changes in behavior conditional on pretrial release (induced by different financial incentives). Misconduct can be higher without financial conditions due to more people released (less incapacitation) and also due to less financial incentives (less deterrence).

Since unconditional release always means less detention and less conditions simultaneously, it is not possible to separate out how incapacitation and deterrence effects contribute to the misconduct results in a causal framework. However, I can provide an accounting exercise building off the potential outcomes framework to demonstrate combinations that are consistent with empirical estimates. For details on this approach, see Appendix H.

The key intuition is that some compliers (cases that receive unconditional release due to AR) are released in the absence of AR while others are detained in the absence of AR. The change in misconduct for the always released compliers is only the consequence of a deterrence effect. Estimating the deterrence effect, therefore, requires subtracting out the misconduct change due to the newly released compliers. That change is the relevant share of cases that are newly released, which is empirically observable, multiplied by their misconduct rate under unconditional release, which is unknown.

Based on assumptions about the risk of newly released compliers, I can estimate the relative importance of incapacitation and deterrence in explaining misconduct effects. For incapacitation to be the sole source of the aggregate failure to appear effect, the newly released need to be around 6 times as risky as the always takers (cases that receive unconditional release even when judges choose bail conditions). In this case, the newly released would need to fail to appear more than 60% of the time when given unconditional release. Even if the newly released are more than three times as risky as always takers, deterrence would still be responsible for about half of the aggregate failure to appear effect. If I assume that the newly released are equally likely to fail to appear under unconditional release as never takers under financial conditions, then deterrence is responsible for 81% of the aggregate effect and incapacitation 19%. Since 60% is a very high misconduct rate, it is likely that at least some of the effects are due to deterrence.[58]

[58] If bail conditions impact defendant behavior beyond the incapacitation channel, then this is a complicating factor for judge instrument designs in bail settings, which rely on the assumption that judges only impact outcomes through detention or release.

6 What are the effects of eliminating money bail on release and misconduct? (What about unsecured bail?)

The AR program generates variation that can be used to identify a number of parameters. The effects of unconditional release, estimated with DD-IV in Section 5.2, are policy relevant for marginally increasing unconditional release in that state (say by marginally expanding eligibility). However, the estimated causal effects are a mix of two underlying parameters: the effect of unconditional release relative to unsecured bail and the effect of unconditional release relative to money bail. In the potential outcomes framework language, there are two distinct complier groups who drive the aggregate effect – c-compliers and m-compliers, as defined in Section 5.1.

Let $LATE_u^Y = E[Y_{i1}(u) - Y_{i1}(B_{i1}(0))|B_{i1}(1) = u, B_{i1}(0) \neq u]$ be the estimated local average treatment effect of unconditional release for outcome Y where $Y \in \{M, R\}$ = {misconduct, release}. Then, following Kline and Walters (2016), $LATE_u^Y$ is a weighted average of "subLATEs" which measure the effects of unconditional release for compliers with different counterfactual alternatives. Specifically,

$$LATE_u^Y = S_m LATE_{mu}^Y + (1 - S_m) LATE_{cu}^Y$$

where $LATE_{mu}^Y$ is the local average treatment effect of eliminating money bail (and replacing it with unconditional release), $LATE_{cu}^Y$ is the local average treatment effect of eliminating unsecured bail (and replacing it with unconditional release), and S_m is the fraction of compliers that are m-compliers. Based on the reduced form results in Figure 4, I know that 40.8% of compliers are money bail compliers and 59.2% are unsecured bail compliers.[59] Therefore, the effect of unconditional release is a 40-60 mix of two distinct treatment effects:

$$LATE_u = (0.408) LATE_{mu}^Y + (0.592) LATE_{cu}^Y$$

Since other states and jurisdictions use different mixes of bail conditions, the aggregate effect of unconditional release is limited in its external validity. For instance, while 27.2% of cases in Kentucky are assigned unsecured bail, only 4% of felony cases nationally are assigned unsecured bail. Intuitively, implementing unconditional release only means the same thing across environments if the counterfactual bail condition is the held constant. Therefore, it is worthwhile to explore the differences between the two counterfactual-specific treatment effects.[60] Moreover, investigating the two underlying treatment effects is informative as to the mechanisms behind the broad program effects and provides insights on financial incentives and behavior.

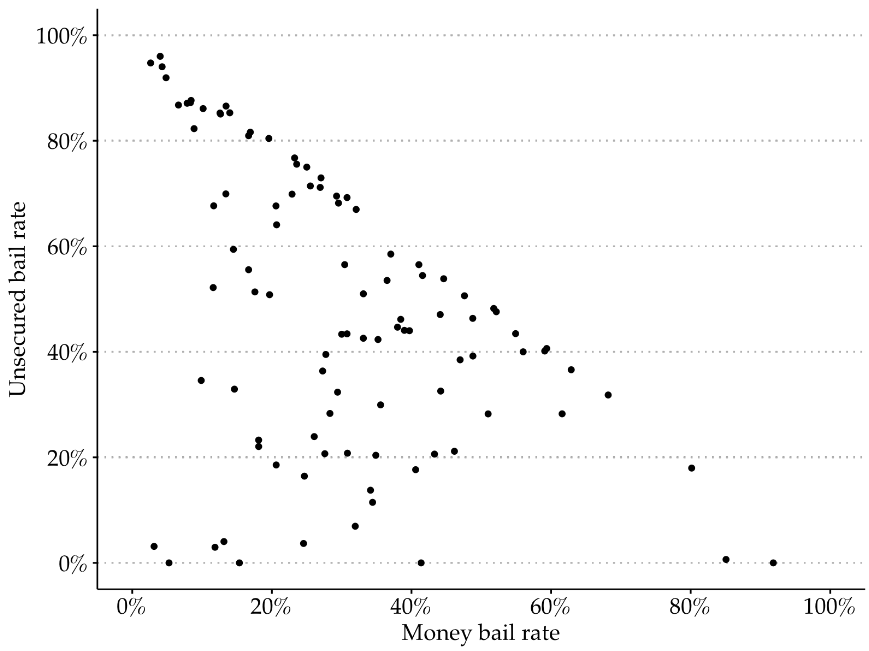
[59] 40.8% comes from dividing the magnitude of the effect of AR on money bail, seen in Figure 4a, by the effect of AR on unconditional release, seen in Figure 4b. Another method of calculating population shares is outlined in Appendix G.

[60] Moreover, the parameter that is more universally relevant is the effect of unconditional release relative to money bail. Not only is money bail more consistently used across the country, but it also is the primary target of bail reform conversations due to its incapacitation effects and salient financial implications.

I use two methods to identify the underlying counterfactual-specific effects.[61] Both use variation in bail setting across counties that is due to variation across judges in where they work.

Method (1): Figure 8 plots all Kentucky counties by their usage of money and unsecured bail for eligible cases before AR. The x-axis illustrates the money bail rate and the y-axis illustrates the unsecured bail rate. Since money bail, unsecured bail, and unconditional release fully characterize bail conditions and are mutually exclusive, the three rates will sum to 100%. Points that are on the diagonal between a 100% money bail and a 100% unsecured bail rate denote counties that only use those two conditions (no unconditional release). Points inside that boundary use some combination of the three.

Figure 8: Variation across counties in bail setting before AR



Notes: This figure plots counties in Kentucky by their usage of money bail (x-axis) and unsecured bail (y-axis). The sample is limited to eligible cases before AR. These the two rates and the unconditional release rate (which is not plotted) will sum to 100%.

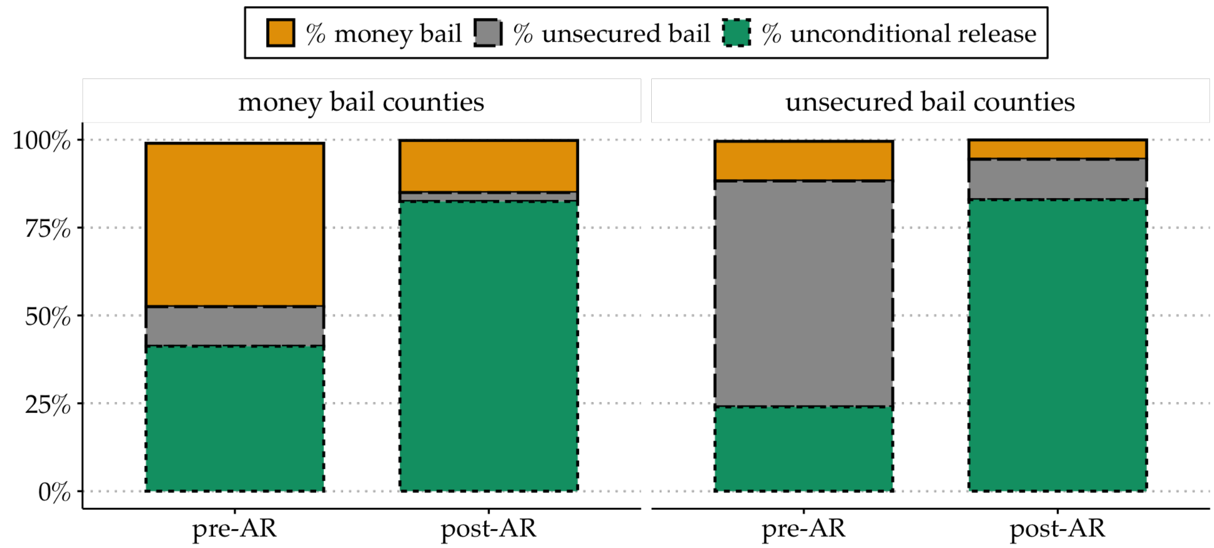
I define subgroups so I can focus on two distinct duos of bail types (and thus avoid the complications of substitution across a trio of bail types). I define "money bail counties" as counties where less than 20% of eligible cases receive unsecured bail pre-reform. "Unsecured bail counties" are counties where less than 20% of eligible cases receive money bail pre-reform.

Figure 9 then demonstrates the bail substitution patterns for eligible cases in these two county groups as a result of AR. The left pair of stacked bars illustrate substitution for money bail counties and mainly feature substitution from money bail (orange solid bars) to unconditional release (green dotted bars). The right pair of stacked bars illustrate substitution for unsecured bail counties and

[61] Appendix I provides a theoretical model of how release and misconduct respond to bail conditions and demonstrates the counterfactual-specific effects graphically under simplifying assumptions.

mainly feature substitution from unsecured bail (gray dashed bars) to unconditional release (green dotted bars). As intended (based on how I defined subgroups), money bail counties home in on the switch from money bail to unconditional release and unsecured bail counties home in on the switch from unsecured bail to unconditional release.

Figure 9: Bail substitution patterns across subgroups



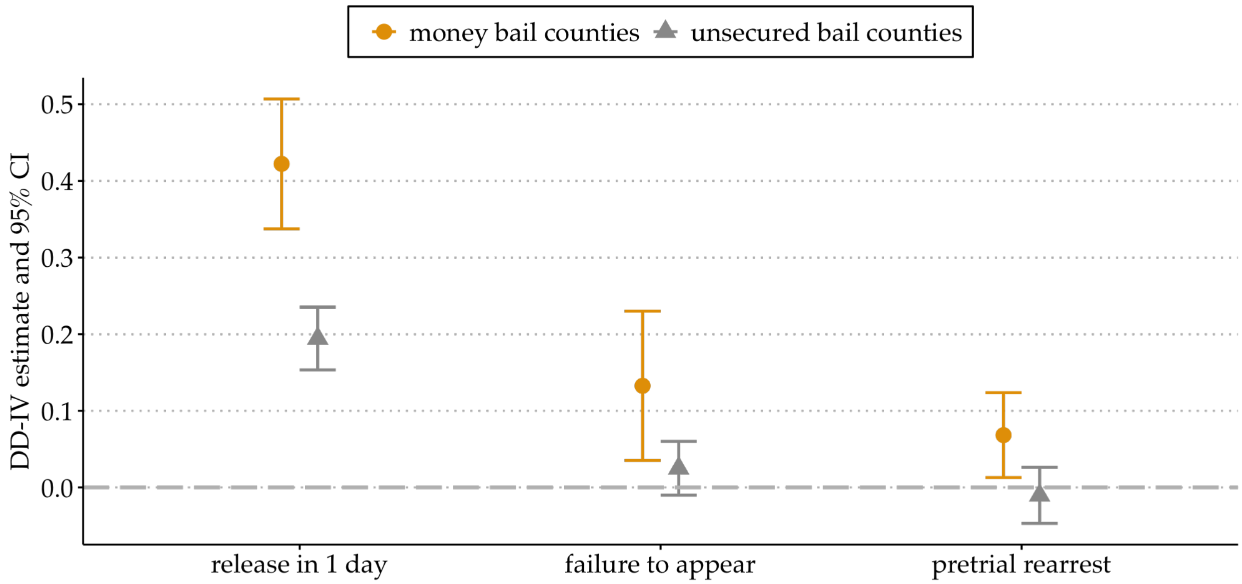
Notes: This figure plots the bail substitution patterns for money bail counties (counties with less than 20% of cases getting unsecured bail) and unsecured bail counties (counties with less than 20% of cases getting money bail). The sample is limited to eligible cases. As intended, money bail counties mainly experience substitution away from money bail due to AR and unsecured bail counties mainly experience substitution away from unsecured bail due to AR.

One concern with leveraging pre-reform variation is that case characteristics might differ substantially across place. If money bail counties assign money bail more due to riskier cases then attributing the difference in effects to the pre-existing bail conditions is problematic. However, Figure 19 demonstrates that the AR eligible cases pre-AR are, if anything, riskier in the unsecured bail counties than in money bail counties.

For both subsamples, I provide instrumented differences-in-differences estimates instrumenting for unconditional release. The assumption is that in the money bail counties the instrumented effect mainly captures the movement from money bail to unconditional release and in the unsecured bail counties the effects mainly capture the movement from unsecured bail to unconditional release. Figure 10 plots both sets of estimates across the three outcomes of interest (release within 1 day, failure to appear, and pretrial rearrest). The circular estimates in orange are for the money bail counties and the triangular estimates in gray are for the unsecured bail counties.

As expected, money bail counties feature larger effects of removing conditions on release. Cutting financial conditions by 10% in money bail counties increases release within 1 day by 4.2%. In unsecured bail counties, the same cut increases release by less than 1.9%. This is consistent with money bail posing a barrier to release due to ex ante posting requirements. The larger effect

Figure 10: Estimates across subsamples



Notes: Figure plots instrumented differences-in-differences estimates for removing financial conditions for the two county samples. Money bail county estimates are represented by orange circles and unsecured bail countiy estimates estimates are represented by gray triangles. Confidence bands are at the 95% level. Standard errors are clustered at the county-level.

in money bail counties demonstrates that the program is not solely improving release due to administrative speed improvements.

Money bail counties also feature larger effects in both misconduct measures. Cutting financial conditions by 10% in money bail counties increases failure to appear and pretrial rearrest by 1.3% and 0.7%, respectively. In unsecured counties, the increases are insignificant at conventional levels and are 0.2% and -0.1% in magnitude. The effect of removing conditions in unsecured bail conditions on misconduct (especially so for failure to appear) is weak.

The results demonstrate that removing financial conditions matters more in money bail counties than unsecured bail counties across all three outcomes. Under the assumption that pre-reform bail norms are not correlated with unobservables that drive the larger effects, these results are consistent with a stronger causal interpretation: money bail is more impactful than unsecured bail in reducing misconduct.

Method (2): Instead of estimating effects separately across two subgroups, I can estimate results using a fuller range of county variation. Following Kline and Walters (2016), I use two-stage least squares estimation treating unconditional release and unsecured bail as two separate endogenous variables.

To generate instruments, I interact AR coverage (which is the interaction of a case being AR eligible and the time period being after AR implementation) with county indicators. The intuition is again to take advantage of different bail norms pre-AR across counties. This is similar to interacting experimental program assignment with observed covariates or site indicators, as in Kling, Liebman, and Katz (2007) and Abdulkadiroğlu, Angrist, and Pathak (2014). This approach relies on an assumption of constant effects, meaning the counterfactual-specific effects themselves should not vary over the interacting groups (Hull 2018).

The set-up in this two-stage least squares context is similar to the set-up in specifications 5 and 6 but there are two distinct endogenous variables predicted in the first-stage: unconditional release and unsecured bail. Also, the interaction of eligibility and post (as well as eligibility and post separately) is interacted with a full set of county indicators. This yields two coefficients: δ_u^{DD-IV} (unconditional) and δ_c^{DD-IV} (unsecured).

In this framework, δ_u^{DD-IV} yields the local average treatment effect of unconditional release relative to money bail. Meanwhile, δ_c^{DD-IV} yields the local average treatment effect of unconditional release relative to money bail minus the local average treatment effect of unconditional release relative to unsecured bail (Kline and Walters 2016).

Therefore, if unsecured bail has measurable effects, it should be the case that $\delta_u^{DD-IV} > \delta_c^{DD-IV}$. On the other hand, if all of the effects of unconditional release are due to substitution away from money bail, it should be the case that $\delta_u^{DD-IV} = \delta_c^{DD-IV}$.

Table 2 demonstrates two-stage least squares estimates of separate effects of unconditional release and unsecured bail using AR coverage and its interaction with county indicators that capture heterogeneity in bail substitution patterns. The county interaction instruments yield significant independent variation in both unconditional release and unsecured bail – the partial F-stats are about 97.7 and 25.2, respectively. Moreover, the overidentification test in my context does not reject the null that overidentifying restrictions are valid. I do not reject the constant effects assumption and continue to assume that the underlying effects are constant across counties.

The estimated δ_u^{DD-IV} and δ_c^{DD-IV} coefficients are similar for pretrial rearrest. This is consistent with the interpretation that unconditional and unsecured bail effects are likely homogeneous for pretrial rearrest and bail type substitution attenuates estimates of the effect of unconditional release (if the counterfactual of interest is money bail). Since effects of unsecured bail are negligible, this is suggestive evidence that threats of additional fines are not effective in changing pretrial rearrest behavior (and may simply impose additional court debt on defendants in the event of misconduct).

However, for release and failure to appear, $\delta_u^{DD-IV} > \delta_c^{DD-IV}$. Interpretation of the point estimates means that unsecured bail has about 25% of the effect of money bail in impacting release. The fact that the results are larger for money bail are reassuring in showing that inability to pay small amounts of money is a large driver of pretrial detention in this context. The results for money bail in failure to appear are stronger in magnitude (by a factor of 3) and statistical significance than the results for unsecured bail.

Table 2: Two-Stage Least Squares Estimates with County Interaction Instruments

	Release in 1 day (1)	Failure to appear (2)	Pretrial rearrest (3)
Unconditional release	0.4652***	0.1208***	0.0460*
(instrumented)	(0.0358)	(0.0432)	(0.0267)
Unsecured bail	0.3452***	0.0837	0.0470
(instrumented)	(0.0415)	(0.0540)	(0.0354)
Observations	136,917	136,917	136,917

Notes: The table reports two-stage least squares estimates of the effects of unconditional release and unsecured bail. Unconditional release and unsecured bail are treated as two separate endogenous variables. Instruments are generated by interacting AR coverage (which is the interaction of a case being AR eligible and the time period being after AR implementation) with county indicators. Coefficients are reported with standard errors (clustered by county) in parentheses. (* p<0.1, ** p< 0.05, *** p<0.01)

Estimates imply that going from only money bail to no money bail means a 46.5 p.p. higher rate of release within 1 day. Meanwhile, going from only money bail to no money bail means a 16.6 p.p. higher rate of misconduct with most of (12 p.p.) that change stemming from failure to appear behavior. Recall that on average release within 1 day is associated with a drop in detention hours of 157. In this context, one instance of misconduct needs to be at least 18 times as costly as a day in detention for money bail to be worth its costs in this context.

The heterogeneity in effects between money bail and unsecured bail is attributable to a mix of the incapacitation effects of money bail and different behaviors due to payment timing (money is required ex ante for money bail but ex post for unsecured bail). For incapacitation to be the only channel that matters for money bail effects, it also needs to explain the majority of program effects (since money bail effects constitute the majority of the aggregate program effects).

Recall that for incapacitation to be the only channel that matters for program effects, marginally released defendants need to be around 6 times as risky as those always released (meaning they need to fail to appear more than 60% of the time). Since this is a very high failure to appear rate, it is likely that ex ante payment binds behavior more than the threat of financial collection.

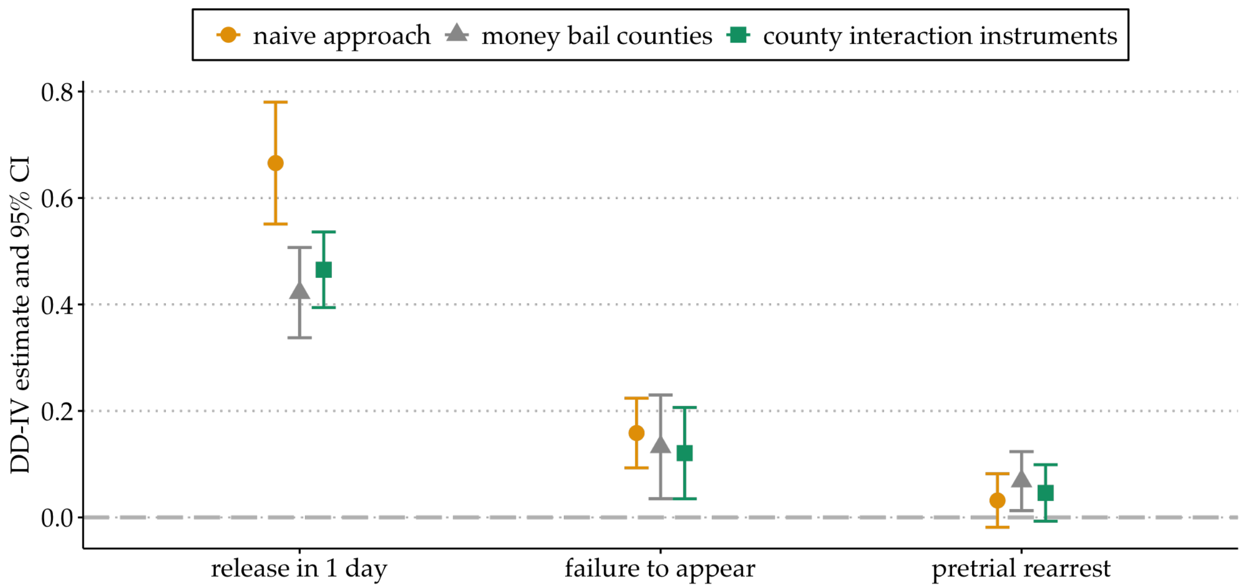
In fact, the weak unsecured bail effects suggest defendants act as though chance of financial collection is low (perhaps due to low chance of re-apprehension or low chance of collection conditional on re-apprehension).[62] In the money bail case, since posting was required for release, money is more saliently on the line. Therefore, even though the two types of financial conditions are similar in theory under misconduct, the logistical hurdles around collection ex post likely weaken its financial incentives.

How do results on the effect of money bail vary over estimation methods? I use Figure 11 to

[62] Appendix I provides a simple model of potential outcomes under different bail conditions related to this discussion.

compare the effects of eliminating money bail (in favor of unconditional release) across methods. With orange circles, I present the naive DD-IV estimates acquired from simply instrumenting for money bail in the original set up (presented in Table 10). With gray triangles, I present the estimates from instrumenting for unconditional release in money bail counties (where we know most of the counterfactual is money bail). With green squares, I present the estimates for instrumenting for unconditional release in the specification with 2 endogenous variables and county interaction instruments.

Figure 11: Effects of eliminating money bail



Notes: This Figure demonstrates the estimated effects of eliminating money bail on 3 outcomes of interest: release in 1 day, failure to appear, and pretrial rearrest. The plot depicts estimates from three different methods. The first (orange circles) instruments for money bail with AR coverage in the full main specification and takes the negative of that result. The second (gray triangles) instruments for unconditional release with AR coverage in the money bail county sample. The third (green squares) instruments for both unconditional release and unsecured bail with interactions of AR coverage and county indicators. Confidence bands are at the 95% level. Standard errors are clustered at the county-level.

Assuming the entire reduced form effects can be attributed to the elimination of money bail (as done in Table 10) generates similar misconduct results as the two methods using pre-reform county variation. All three methods provide similar estimates with ample overlap in their 95% confidence intervals. Estimates range in magnitude from 0.12-0.16 for failure to appear and 0.03-0.07 to pretrial rearrest. Effects on failure to appear are 2-5 times as large as the effects on pretrial rearrest, suggesting that more minor forms of misconduct are more responsive to money bail.[63]

However, the release results show that some of the original release in 1 day results were coming from unsecured bail substitution too. Attributing all of the aggregate release effect to money bail yields an estimate of 0.665; using pre-reform county variation methods gives estimates of 0.42 and

[63]The difference in effect sizes is not attributable to baseline levels since failure to appear (baseline: 10.7%) is not 2-5 times as frequent as pretrial rearrest (baseline: 8.4%).

0.47. The majority of the release effects are attributable to eliminating money bail, but that is not the only channel.

7 How do courts value detention relative to misconduct?

How do courts value competing outcomes (detention and misconduct)? Since the automatic release program reveals counterfactual outcomes under the removal of money-related bail conditions, the resulting estimates provide an opportunity to estimate court preferences before the program. I outline a theoretical revealed preference framework that builds on my developed potential outcomes framework to clarify the necessary assumptions for this exercise and provide explicit interpretations on court values.

7.1 Revealed preference framework

I build on the set-up outlined in Section 5. Again, there is a population of courts indexed by i (each with a single defendant). Each court assigns its defendant one of the following bail types: unconditional release (u), unsecured bail (c) or money bail (m). $Z_{it} \in \{0, 1\}$ captures whether court i is covered by AR or not. $B_{it}(Z_i) \in \{u, c, m\}$ denotes the defendant's potential treatment status (bail type) as a function of AR coverage. Each court has utility over bail type options given by $U_{it}(b, z)$ where $b \in \{u, c, m\}$ indexes bail types and $z \in \{0, 1\}$ indexes AR coverage status.

Again, I make an exclusion restriction and (the only way bail reform coverage impacts court appearance and detention outcomes is through bail type) and extended monotonicity assumption (the only way AR induces changes in bail type is through assignment to unconditional release).

Now, if $z = 0$, I assume court i's utility depends on potential outcomes $M_{it}(b)$ and $R_{it}(b)$. The former is the potential pretrial misconduct outcome of defendant i in time t if assigned bail type b. The latter is the potential pretrial release outcome of defendant i in time t if assigned bail type b. Specifically, I assume the court's utility function when $z = 0$ is a weighted sum of the expected potential outcomes where the relative weights are cost terms that are specific to court i, $-C_i^M$ and $-C_i^D$.

$$U_{it}(b, 0) = -C_i^M E(M_{it}(b)) - C_i^D E(1 - R_{it}(b))$$

This utility function assumes that (1) only detention and misconduct outcomes matter to courts, (2) detention and misconduct have constant costs, and (3) courts are correctly informed about how detention and misconduct respond to bail conditions. In other words, courts may vary in their preferences, but not in their predictions.[64]

[64] For an in-depth exploration of the distinction between preferences and predictions in the context of bail, see Rambachan (2021).

Since court i picks the bail condition that maximizes its utility, if $z = 0$,

$$B_{it}(0) = \underset{b \in \{u,c,m\}}{\operatorname{argmin}} C_i^M E(M_{it}(b)) + C_i^D E(1 - R_{it}(b))$$

For the set of courts that switch to unconditional release due to AR coverage (i.e., $B_{i1}(0) \neq u, B_{i1}(1) = u$), then for that set of courts, I know by revealed preference that:

$$U_{i1}(u,1) > U_{i1}(B_{i1}(0),0) > U_{i1}(u,0)$$

Since I defined the functional form of $U_{it}(b,0)$ and $U_{i1}(B_{i1}(0),0) > U_{i1}(u,0)$, I can write:

$$C_i^M E\big[M_{i1}(B_{i1}(0))|B_{i1}(0) \neq u, B_{i1}(1) = u\big] + C_i^D E\big[1 - R_{i1}(B_{i1}(0))|B_{i1}(0) \neq u, B_{i1}(1) = u\big] <$$

$$C_i^M E\big[M_{i1}(u)|B_{i1}(0) \neq u, B_{i1}(1) = u\big] + C_i^D E\big[1 - R_{i1}(u)|B_{i1}(0) \neq u, B_{i1}(1) = u\big]$$

In rearranging terms, the costs of misconduct induced by unconditional release must be larger than the costs of detention avoided by unconditional release for this (complier) population. Accordingly,

$$\frac{C_i^M}{C_i^D} > \frac{E\big[R_{i1}(u) - R_{i1}(B_{i1}(0))|B_{i1}(0) \neq u, B_{i1}(1) = u\big]}{E\big[M_{i1}(u) - M_{i1}(B_{i1}(0))|B_{i1}(0) \neq u, B_{i1}(1) = u\big]} \tag{7}$$

How does this map onto previously estimated coefficients? For both potential outcomes $Y \in \{R, M\}$, the reduced form coefficients from Section 4 are:

$$\delta^Y = E[Y_{i1} - Y_{i0}|Z_i = 1] - E[Y_{i1} - Y_{i0}|Z_i = 0]$$

I can write potential outcomes without the function of the bail type since I assume the only way Z_i impacts outcomes is through that bail assignment (exclusion restriction).

By parallel trends, which assumes that $Y_{i1}(B_{i1}(z)) - Y_{i0}(B_{i0}(0))$ are mean independent of Z_i for each $z = 0, 1$ (Hudson, Hull, and Liebersohn 2017), δ^Y can be rewritten as:

$$E[Y_{i1}(B_{i1}(1)) - Y_{i0}(B_{i0}(0))|Z_i = 1] - E[Y_{i1}(B_{i1}(0)) - Y_{i0}(B_{i0}(0))|Z_i = 0] = E[Y_{i1}(B_{i1}(1)) - Y_{i0}(B_{i1}(0))]$$

By extended monotonicity ($B_{i1}(1) \neq B_{i1}(0) \rightarrow B_{i1}(1) = u$),

$$E[Y_{i1}(B_{i1}(1)) - Y_{i0}(B_{i1}(0))] = E[Y_{i1}(u) - Y_{i0}(B_{i1}(0))]$$

$$= P[B_{i1}(0) \neq u, B_{i1}(1) = u] \times E\big[Y_{i1}(u) - Y_{i0}(B_{i1}(0))|B_{i1}(0) \neq u, B_{i1}(1) = u\big]$$

Therefore, I can rewrite the ratio of reduced form coefficients as follows,

$$\frac{\delta^R}{\delta^M} = \frac{P[B_{i1}(0) \neq u, B_{i1}(1) = u] \times E\left[R_{i1}(u) - R_{i0}(B_{i1}(0)) | B_{i1}(0) \neq u, B_{i1}(1) = u\right]}{P[B_{i1}(0) \neq u, B_{i1}(1) = u] \times E\left[M_{i1}(u) - M_{i0}(B_{i1}(0)) | B_{i1}(0) \neq u, B_{i1}(1) = u\right]}$$

$$= \frac{E\left[R_{i1}(u) - R_{i0}(B_{i1}(0)) | B_{i1}(0) \neq u, B_{i1}(1) = u\right]}{E\left[M_{i1}(u) - M_{i0}(B_{i1}(0)) | B_{i1}(0) \neq u, B_{i1}(1) = u\right]}$$

Since this expression is the same as the right-hand side in equation 7, I can write:

$$\frac{C_i^M}{C_i^D} > \frac{\delta^R}{\delta^M}$$

In words, the ratio of the reduced form effects (of regressing outcomes on Z_i) provides insights on the court-perceived costs of two outcomes the bail system seeks to avoid (detention and misconduct).

7.2 Implications of empirical estimates on court valuations

Since the main effects on rearrest are not statistically distinguishable from 0, the main tradeoff I focus on is between detention and failure to appear. My results from Section 4 demonstrate that

$$\frac{\delta^R}{\delta^M} = \frac{\text{0.137 more people released within 1 day}}{\text{0.033 more failure to appears}}$$

On average, release within 1 day is associated with a decline of 157 hours. If we assume aggregate detention hours are what matters, then

$$= \frac{\text{21.5 less detention hours}}{\text{0.033 more failure to appears}} \approx \frac{\text{27 less days in detention}}{\text{1 more failure to appear}}$$

In this framework, the courts put the costs of 1 court non-appearance at the cost of more than 27 days in detention. Is this relative valuation high or low based on empirical estimates of the two types of costs?

I provide estimates of the fiscal costs to courts of detention hours and failure to appear to help inform interpretation of the relative valuation result.

On detention, Levitt (1996) finds judicial system costs of jailing a defendant between \$84-\$126 dollars per day. Note that these estimates are only focusing on the court costs side and do not include the costs of pretrial detention to defendants (in terms of the increased convictions and decreased employment and government benefits) (Leslie and Pope 2017; Dobbie, Goldin, and Yang 2018). Therefore, taking the midpoint of Levitt (1996)'s range (\$105, as Abrams and Rohlfs (2011) do as well) is necessarily an underestimate of detention overall social costs. Since non-court costs of detention are large, ignoring these biases the exercise in the direction of understating detention costs relative to misconduct.

The costs of failure to appear consist of re-apprehension and additional court administration costs. On re-apprehension costs, Abrams and Rohlfs (2011) ask bail bond experts for fiscal estimates and receive two estimates: $500 or 5% of the bail amount. Since 5% of a $0 bail amount (the amount under unconditional release) is also $0, I use the $500 estimate for re-apprehension. Bierie (2007) reports that the cost of a minor hearing (of which an additional bail hearing is an example) is $560. The total estimated fiscal cost of each failure to appear is $1,060.

Taking these prior estimates seriously means that failure to appear costs ($1,060) about 10 times as much as one day in detention ($105), which does not validate the valuation of court non-appearance at 27 times the cost of a day in detention. This result suggests that courts overvalue averting misconduct relative to averting detention.[65]

Moreover, translating my reduced form estimates into annual changes implies that AR yielded around 9,187 less detention days and 385 more instances of court non-appearance. The total fiscal costs are, therefore, $964,583 saved in terms of detention and $407,664 lost in terms of court non-appearance; based on these results, the program is cost-saving to the government. In order for the status quo (before automatic release) to be preferable in terms of total social costs, the non-government costs of failure to appear would need to outweigh the equivalent costs of pretrial detention, which include the myriad harms pretrial detention imposes on detainee's legal and labor market outcomes.

8 Conclusion

Motivated by growing waves of bail reform across the country, this paper studies the effects of reducing financial bail conditions on the countervailing outcomes of pretrial detention and misconduct. I use administrative data from a unique program in Kentucky that was designed to eliminate financial bail conditions for a set of people arrested on low-level offenses.

I find that the program increased unconditional release by 50.5 pp and had meaningful impacts on pretrial release even though the average money bail amount for the impacted group was relatively modest in magnitude (around $360) in the absence of the program. Release effects are larger for unemployed defendants and defendants in low-income zip codes. This empirical result supports the notion that the gains to reducing the scope of bail are largest for socioeconomically disadvantaged populations. In terms of pretrial misconduct, failure to appear increases by 3.3 pp, but results on pretrial rearrest are indistinguishable from 0. I can rule out increases of more than 1.68 pp at the 5% level.

[65] Appendix J provides an alternative way of conceptualizing costs based on Stevenson and Mayson (2021)'s measures of preferences for averting experiencing misconduct and detention.

I develop a useful potential outcomes framework to evaluate the impact of bail conditions themselves, using program coverage as an instrument. I demonstrate that the majority of the automatic release program effects come from substitution away from money bail rather than unsecured bail (which only requires payment in the event of misconduct). In 17% of cases, money bail had desirable impacts on misconduct (induced a change from non-appearance or rearrest to appearance or no rearrest, respectively).[66] However, in almost half (47%) of cases money bail had undesirable effects on time in pretrial detention. One instance of misconduct needs to be at least 18 times as costly as a day in detention for money bail to be worth its costs in this context.

Lastly, I use my estimates for a revealed preference exercise. How do courts value detention relative to misconduct? My estimates paired with my theoretical framework suggest courts valued averting 1 failure to appear more than 27 days in detention before the program. Based on fiscal costs estimates, this implies courts overvalue averting misconduct relative to detention. This is consistent with judges suffering asymmetric penalties (more penalties due to lenience than due to harshness), as theorized by Ouss and Stevenson (2022).

This paper focuses on eliminating money bail in favor of unconditional release, but other proposed reforms to the bail system involve substitution to different alternatives, such as supervision or electronic monitoring. Since unconditional release is the most lenient form of bail, my money bail estimates present a worst case scenario for pretrial misconduct. These estimates are therefore informative for other policy environments since they represent an upper bound on effects of switching away from money bail.

[66] Therefore, in the majority of cases (83%), money bail did not ensure appearance or public safety, as proxied by rearrest.

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Appendix

A Automatic Release Eligibility

A.1 More on criminal warrants

In Kentucky, anyone can file a criminal complaint with the prosecutor's office. (This includes police officers, businesses, and private citizens). The citizen makes an allegation and signs a sworn statement. The prosecutor then requests that the court issue a summons or a warrant or declines to do either. The judge makes the decision to issue a warrant or a summons and sometimes they list a bail amount for a warrant. In those cases, cases cannot be AR eligible, but I cannot observe this in the data.

AR eligible charges that are "common circumstances" for criminal warrant arrests include theft and harassment.

A.2 Public Safety Assessment Risk Scores

As of Supreme Court of Kentucky (2015), defendants must have a Public Safety Assessment Composite score 2-7 for AR eligibility. This changed with Supreme Court of Kentucky (2017b) in December 2017 – eligibility was no longer based on the composite scores but underlying score levels.

The calculation of the Composite PSA score is illustrated in Figure 12: (1) raw Failure to Appear (FTA) and New Criminal Activity (NCA) scores are calculated based on the defendant's charge, criminal history, and age, (2) points are assigned to each possible response and summed to calculate the respective raw scores, (3) raw scores are then converted into scaled scores, and (4) the two scaled scores are added together to generate the composite risk score.

The raw FTA score is calculated based on:

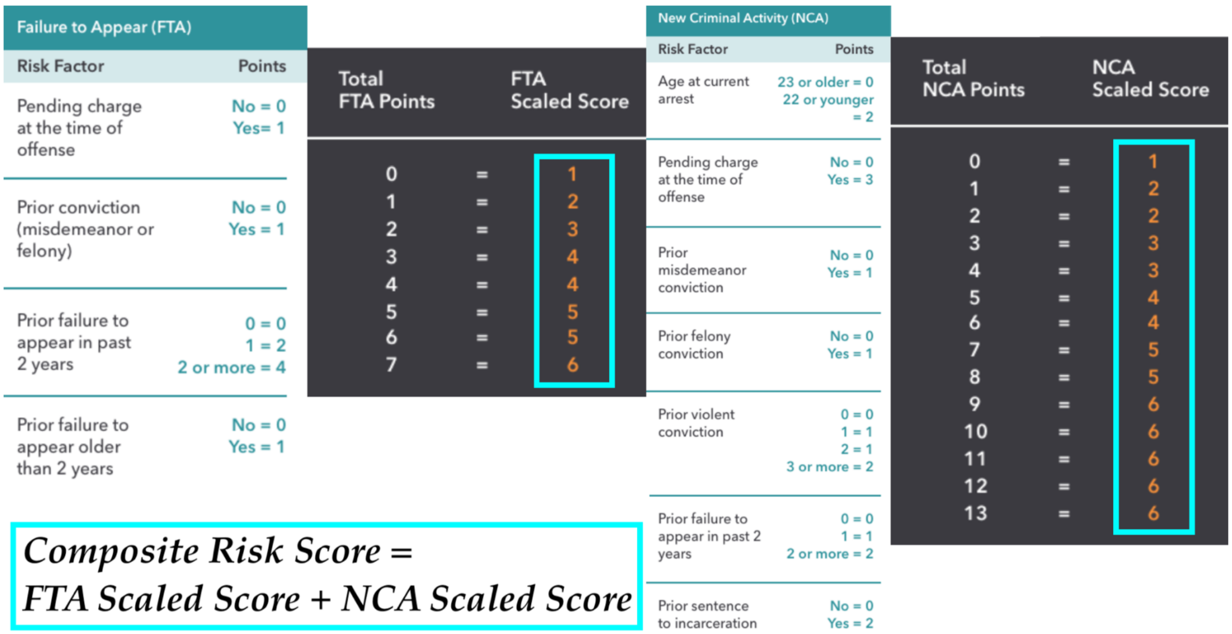
- whether the defendant has a pending charge at the time of offense
- whether the defendant has a prior conviction (misdemeanor or felony)
- how many times the defendant has failed to appear in the past 2 years
- whether the defendant has failed to appear more than 2 years ago

The raw NCA score is calculated based on:

- whether the defendant is 23 or older
- whether the defendant has a pending charge at the time of offense
- whether the defendant has a prior misdemeanor conviction
- whether the defendant has a prior felony conviction

- whether the defendant has a prior violent conviction
- how many times the defendant has failed to appear in the past 2 years
- whether the defendant has previously been sentenced to incarceration

Figure 12: Risk Score Calculation Methodology



Notes: This Figure demonstrates how the Composite Public Safety Assessment Score is calculated.

A.3 Charges

Supreme Court of Kentucky (2015), the first AR order that included the 2-7 risk score eligibility, also lists the following conditions for AR eligibility:

- charges are non-sexual/non-violent misdemeanors/violations
- defendant has not previously failed to appear on the charge
- defendant accepts pretrial services interview
- additional charges that render someone ineligible: (1) contempt of court or violations of probation or conditional discharge, (2) DUI with injuries or accident or any aggravated circumstances, (3) DUI on a suspended license

Entered December 2016, Supreme Court of Kentucky (2016) adds the following conditions:

- additional charges that render someone ineligible: (1) violation of a protective order, (2) bail jumping charges

Some slight changes in implementation:

- Supreme Court of Kentucky (2016) notes that pretrial officers can obtain approval from Pretrial Services Executive Officer (or designee) to present a defendant for judicial review.
- Supreme Court of Kentucky (2017a) mandates that pretrial officers base their review on the UOR code assigned by law enforcement. Previously, they were to base their review on the actual charge in the narrative/criminal record.

B Background on risk score usage across the US

The Public Safety Assessment (PSA) is used statewide in Arizona, New Jersey, Utah, and Kentucky. Other shaded states in Figure 13 include cities or counties that use the PSA, according to the Advancing Pretrial Policy and Research (APPR) organization (*Public Safety Assessment Sites* 2021). Figure 14 shows usage of risk scores nationally, as mapped by the Mapping Pretrial Injustice Project (*Where Are Risk Assessments Being Used?* 2021).

Figure 13: Public Safety Assessment Usage Across the US

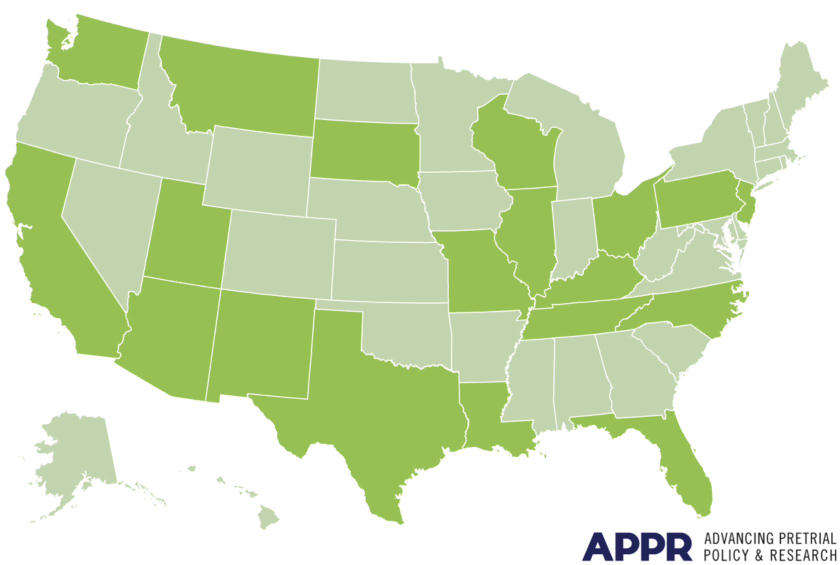
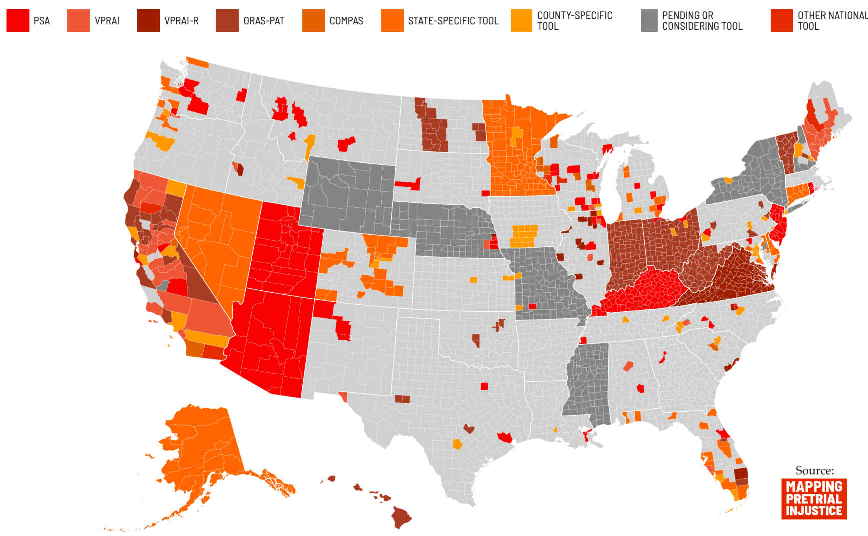


Figure 14: Pretrial Risk Score Usage Aross the US



C Testing for Strategic Manipulation

First, consider that there could be strategic charging by police to manipulate eligibility. While arrest type is mechanically determined, charge code assignment (which also factors into AR eligibility) is at the discretion of police officers. To address this, I plot the percentage of cases with AR eligible charges binned by month relative to AR take-up; Figure 15a shows that those percentages are stable around the policy change date for both county samples.

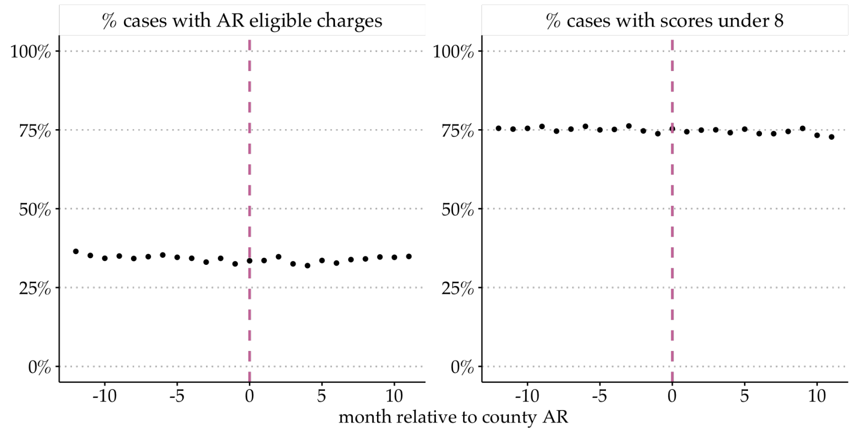
In addition, since pretrial officers input items for risk assessment score calculation (they are not automatically filled in by the court system), it is possible they could purposefully shift scores to impact AR eligibility.[67] To address this, I also plot the percentage of risk scores that are over 8 (ineligible) over time; Figure 15a shows the percentage is stable around the policy change.

Moreover, I also show that the share of defendants that has each underlying component feature is stable around AR implementation in Figure 15c. As such, there is not strong evidence of strategic actions to manipulate eligibility by police or pretrial officers around the policy date.

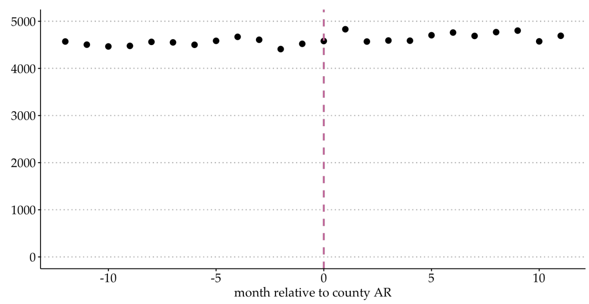
[67]However, this seems less likely since pretrial officers report to supervisors who can review their risk assessment accuracy, meaning there are incentives to be accurate for pretrial officers.

Figure 15: Testing for strategic actions

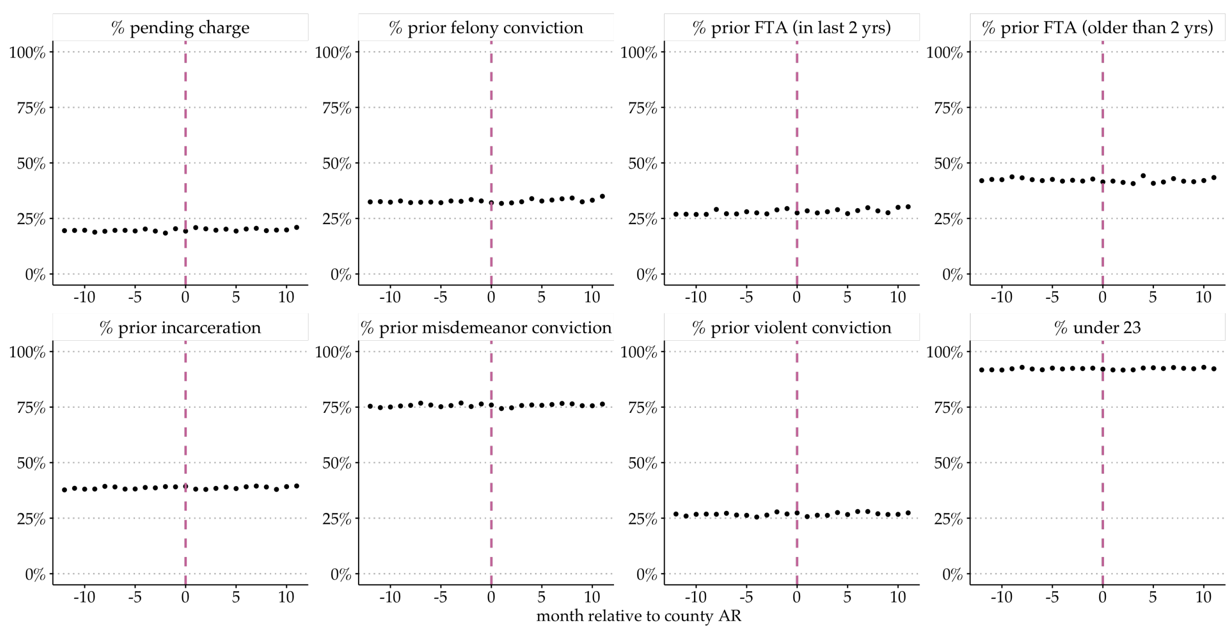
(a) Eligibility criteria



(b) Total cases



(c) Risk score components



Notes: All Figures are scatterplots binned by month relative to AR. They show the window 12 months before and after AR. Figures 15a plots percent of cases that have AR eligible charges and risk scores under 8 binned by month. Figures 15b plots total case count binned by month. Figures 15c plots percent of cases that have specific characteristics used in risk score calculation by month.

D Data Appendix

D.1 Sample restrictions

Initial decisions: I home in on initial bail decisions that pertain to a single case. Bail can be set case-by-case, so arrest-person level observations only have one outcome when there is one case. Note that cases can include multiple charges. I focus on initial bail decisions since a person can have multiple bail decisions over time for the same arrest. Kentucky will revisit bail if defendants are in detention for certain amounts of time.

Time period: My time period of interest starts July 1, 2014 since that was when the updated PSA scores were introduced in Kentucky pretrial and, therefore, this is the first month when I can observe relevant risk scores for defendants. My time period of interest ends November 30, 2017 since the risk score eligibility criterion changed in December 2017 (Supreme Court of Kentucky 2017b). The dates of interest are dates of initial bail decisions.

Counties: I use the samples of counties that took up AR after the November 2015 order. In the robustness results in Section 4.4 I also use the smaller group of counties that took up AR in 2017.[68] See Appendix D.2 for more on differences between county samples.

Certain cases to omit: I omit observations where there are holders or the defendant posted bail prior to the pretrial interview. If the defendant posted prior to bail being set then they don't go through the judge bail or pretrial officer steps I describe. Therefore, those observations don't work for my empirical strategy and I omit them.

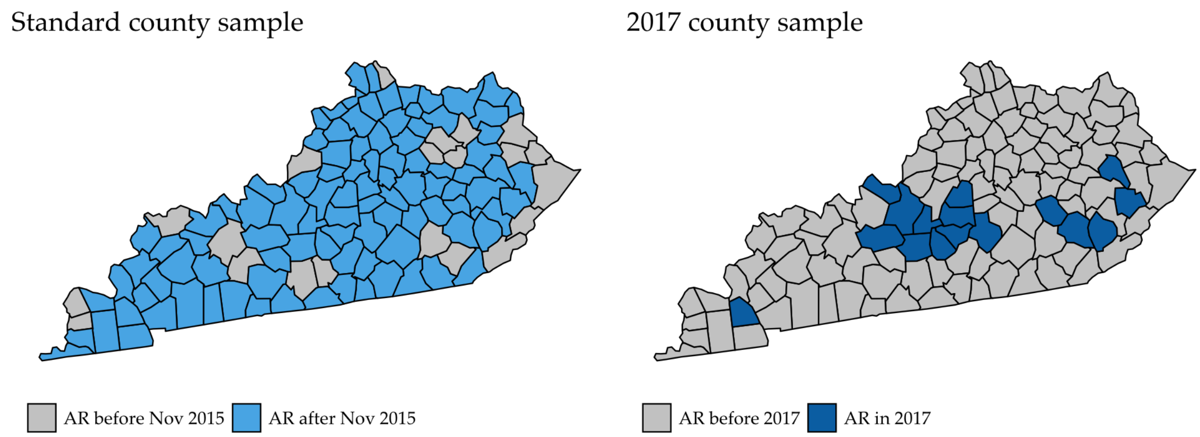
D.2 County samples

In the main text, I use data from all counties that took up AR after the November 2015 order. I can also home in on the counties that took up in January 2017, when the AR program went statewide. Figure 16 maps both sets of counties in Kentucky.

The full county sample yields a sample size that is an order of magnitude higher. However, that sample necessitates dealing with staggered timing with a stacked approach. Moreover, the effects are stronger in the 2017 sample since AR going statewide was accompanied with administrative improvements. In fact, counties that already had AR in effect got an additional bump to unconditional release in 2017. This points to improvements in administration over time, which explains the dynamic effects in the main text results. The 2017 sample does not feature dynamic effects, as illustrated with raw data in Figure 3.

[68] I omit counties that took up AR before November 2015 because (1) most are "always treated" and therefore don't help me identify the ATT of interest and (2) their eligibility requirements can be substantially different from what I can cover with my eligibility criteria definition.

Figure 16: Kentucky county samples



E Are release effects due to logistics or bail conditions?

Calculating DD coefficients using simple means yields: 0.505 (unconditional release)[69] and 0.132 (one day).[70]

How does the release within 1 day break out among compliers and always takers?

The change for the always takers is $[P(oneday|e, u, post) - P(oneday|e, u, pre)] - [P(oneday|ie, u, post) - P(oneday|ie, u, pre)] = 0.025$.[71].

The change for compliers is: $[P(oneday|e, \sim u, post) - P(oneday|e, u, pre)] - [P(oneday|ie, \sim u, post) - P(oneday|ie, \sim u, pre)] = 0.271$.[72]

Multiplying these by the estimated shares of each yields 0.14,[73], which is close to the 0.132 effect. Compliers are responsible for 96.5% of the effect in this estimation exercise.[74]

Therefore, I can attribute most of this effect to the channel of switching to unconditional release.

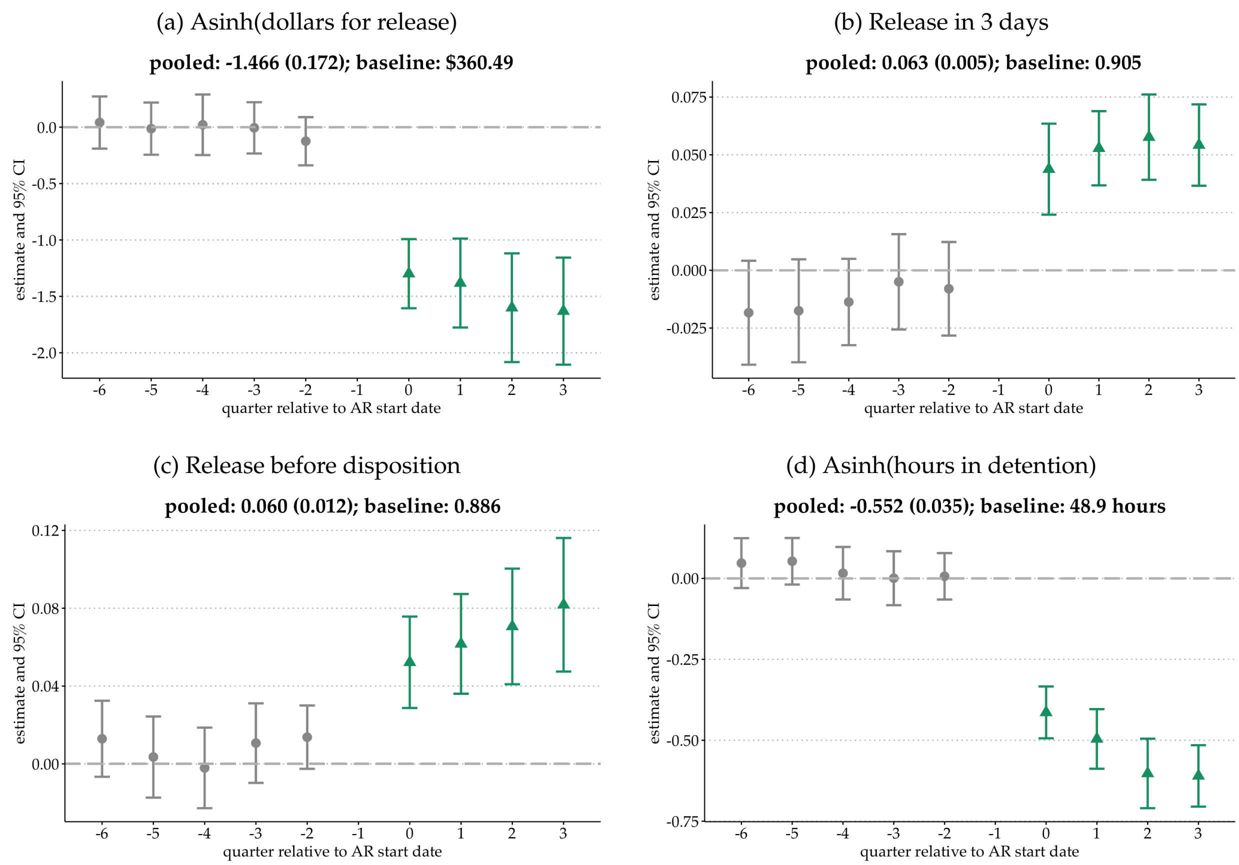
[69] $= (.755 - .197) - (.132 - 0.079)$
[70] $= (.906 - .766) - (.459 - .451)$
[71] $= (0.967 - 0.912) - (0.942 - .912)$
[72] $(0.967 - 0.723) - (.384 - .411)$
[73] $0.025 * 0.197 + 0.271 * 0.505$
[74] $(0.271 * 0.505)/(0.025 * 0.197 + 0.271 * 0.505)$

F Additional Figures and Tables

I provide results on AR program effects for additional bail and release outcomes in Figure 17: inverse hyperbolic sine of money bail amount (0 if no money bail), release in 3 days, release before disposition, and inverse hyperbolic sine of detention hours.[75]

I provide results for misconduct outcomes in Figure 18: pretrial non-violent rearrest and pretrial violent rearrest.

Figure 17: How AR Impacts Bail and Release (Extra Outcomes)

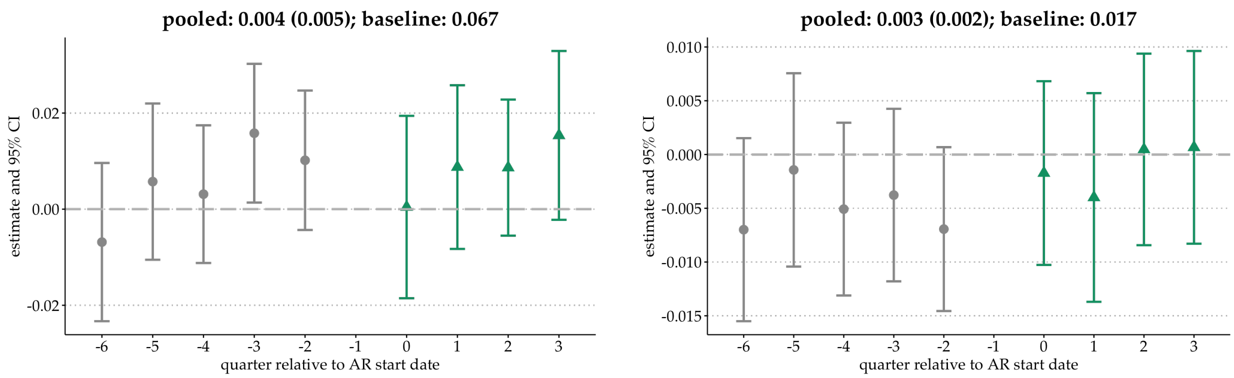


Notes: Figures 17a, 17b, 17c, and 17d plot the event-time differences-in-differences estimates using methods described in Section 3.2.2. The outcome variable for Figure 17a is the inverse hyperbolic of the money bail amount in dollars (0 if no money bail). The outcome variable for Figure 17b is an indicator for release within 3 days of booking. The outcome variable for Figure 17c is an indicator for release before case disposition. The outcome variable for Figure 17d is the inverse hyperbolic since of hours in detention. All figures that show event-time estimates include both point estimates and 95% confidence bands across quarters relative to AR start dates. The circular gray estimates are before AR implementation ($q \in [-6, -2]$), the triangular green estimates ($q \in [0, 3]$) are after AR implementation, and the quarter before AR ($q = -1$) is the omitted period.

[75] The inverse hyperbolic sine is defined as follows: $asinh(x) = ln(x + (x^2 + 1)^{1/2})$. To interpret coefficients in an asinh-linear equation with dummy variables, Bellemare and Wichman (2020) clarifies one can calculate the percent change as $(exp(\hat{\beta}) - 1) \times 100$ (as long as the untransformed means are larger than 10).

Figure 18: How AR Impacts Misconduct (More Outcomes)

(a) Pretrial non-violent rearrest (b) Pretrial violent rearrest



Notes: Figure 18a and 18b plot the event-time differences-in-differences estimates using methods described in Section 3.2.2. The outcome variable for Figure 18a is an indicator for pretrial non-violent rearrest. The outcome variable for Figure 18b is an indicator for pretrial violent rearrest. All figures that show event-time estimates include both point estimates and 95% confidence bands across quarters relative to AR start dates. The circular gray estimates are before AR implementation ($q \in [-6, -2]$), the triangular green estimates ($q \in [0, 3]$) are after AR implementation, and the quarter before AR ($q = -1$) is the omitted period.

Figure 19: County subgroup characteristics

Comparing characteristic means across county subsamples

risk component	money bail counties	unsecured bail counties
violent charge	0.01	0.00
age	34.65	34.58
pending case	0.00	0.00
prior misdemeanor conviction	0.63	0.66
prior felony conviction	0.23	0.23
FTA in last 2 years	0.08	0.09
FTA in >2 years	0.32	0.34
prior violent convic	0.16	0.20
prior sentence to incar	0.26	0.31

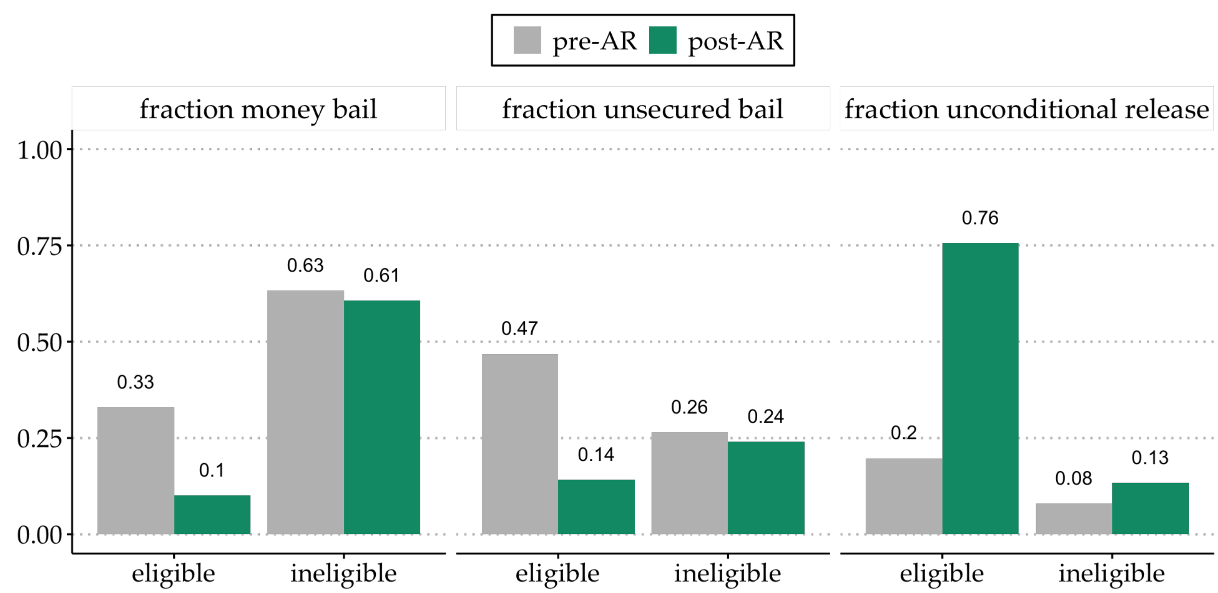
Subset to AR eligible cases before AR implemented.

G Bail Substitution Patterns

Jäger, Schoefer, and Zweimüller (2019) demonstrate how to estimate 3 group shares (always takers, never takers, compliers) in the context of a differences-in-differences potential outcomes framework. (Specifically, see Appendix C and Figure 4 in their paper.) I extend their framework, using the extended monotonicity assumption, to split out the never-taker and complier groups into their component c- and m- subgroups.

Never takers: The never-taker share is $P(B(1) \neq u|t_1, e_1)$.[76] Figure 20, generated with my data, shows that for t_1, e_1, $P(B(1) = u) = 0.76$, meaning the never taker share is 0.24. We know $P(B(1) \neq u|t_1, e_1) = P(B(1) = c|t_1, e_1) + P(B(1) = m|t_1, e_1)$. Figure 20 shows $P(B(1) = c|t_1, e_1) = 0.14$ and $P(B(1) = m|t_1, e_1) = 0.10$, so these are the u-never taker and s-never taker shares, respectively.

Figure 20: Bail types over subgroups



Always takers: The always-taker share is $P(D(0) = u|t_1, e_0) + P(D(0) = u|t_0, e_1) - P(D(0) = u|t_0, e_0)$.[77] Figure 20 shows this is 0.25.[78]

Compliers: The complier share is the remaining share of 0.51.[79] We know that the m-compliers and c-compliers shares will total to 0.51. Intuitively, substitution to unconditional release must come from equal substitution away from unsecured and money bail (due to the extended monotonicity assumption). Therefore,

[76] See (A15) from Jäger, Schoefer, and Zweimüller (2019).

[77] See (A14) from Jäger, Schoefer, and Zweimüller (2019).

[78] In words, the always taker share is the share ineligible-post receiving ROR plus the share eligible-pre receiving ROR less the share ineligible-pre receiving ROR. Plugging in descriptive statistics from Figure 20, $0.13 + 0.2 - 0.08 = 0.25$.

[79] I.e., $1 - 0.25 - 0.24 = 0.51$. Another way to compute this is $[P(B(1) = u|t_1, e_1) - P(D(0) = u|t_0, e_1)] - [P(D(0) = u|t_1, e_0) - P(D(0) = u|t_0, e_0)] = [0.76 - 0.2] - [0.13 - 0.08] = 0.51$.

$$[P(B(1)=u|t_1,e_1)-P(D(0)=u|t_0,e_1)]-[P(D(0)=u|t_1,e_0)-P(D(0)=u|t_0,e_0)]=$$
$$-\Big[[[P(B(1)=c|t_1,e_1)-P(D(0)=c|t_0,e_1)]-[P(D(0)=c|t_1,e_0)-P(D(0)=c|t_0,e_0)]]$$
$$+[[P(B(1)=m|t_1,e_1)-P(D(0)=m|t_0,e_1)]-[P(D(0)=m|t_1,e_0)-P(D(0)=m|t_0,e_0)]]\Big]$$

Plugging in from Figure 20,

$$=-\Big[\big[[0.14-0.47]-[0.24-0.26]\big]+\big[[0.1-0.33]-[0.61-0.63]\big]\Big]=-\Big[\big[-0.31\big]+\big[-0.21\big]\Big]$$

The share of c-compliers is 0.31 and the share of m-compliers is 0.21.[80]

Summary: the shares over the relevant 5 groups are:

- 14% c-never takers
- 10% m-never takers
- 25% always takers
- 31% c-compliers
- 21% m-compliers

H Deriving potential incapacitation and deterrence effects

Misconduct is only possible if defendants are out of pretrial detention before case disposition (case conclusion). Before the program, 88.6% of eligible defendants are released before disposition and 19.6% receive unconditional release (and are released before disposition), meaning a remaining 69% of eligible defendants receive financial bail conditions but are also released. I can write the failure to appear (FTA) rate in the pre-period as a weighted average of FTA rates across these two groups:

$$FTA^{pre}=0.196\underbrace{(FTA|u,r,pre)}_{\text{FTA rate for unconditional released pre-AR}}+0.69\underbrace{(FTA|\sim u,r,pre)}_{\text{FTA rate for released with financial conditions pre-AR}}$$

Descriptive statistics estimate that $(FTA|u,r,pre)=0.105$ and $(FTA|f,r,pre)=0.125$. Plugging these in yields $FTA^{pre}=0.106$, which matches direct estimates very well.

Unconditional release is 55 p.p. higher for the eligible group after the program and release after disposition is 7.6 p.p. higher.[81] As such, there are more people who are mechanically able to commit misconduct. Assuming that all the newly released defendants are newly released due to unconditional release receipt, I break down the post-AR FTA rate such that weights sum to the new

[80] Due to rounding they don't exactly sum to 0.51.

[81] The program increases release before disposition by 6 p.p. in the formal DD approach. See Appendix F.

total of people released pre-disposition as:

$$FTA^{post} = \underbrace{0.196(FTA|u,r)}_{\text{always takers}} + \underbrace{0.47(FTA|u,r)}_{\text{compliers, previously released}} + \underbrace{0.076(FTA|u,r)}_{\text{compliers, previously detained}} + \underbrace{0.216(FTA|f,r)}_{\text{never takers}}$$

Assuming that the always takers and never takers commit FTA at the same rates as they did before, the change in the FTA rate is then attributable to the change in FTA for compliers who no longer have financial conditions and the FTA rate for those who are now released (since they were previously detained their pre-FTA rate is assumed to be 0). These changes in rates multiplied by the relative share of the population correspond to deterrence and incapacitation effects, relatively.

$$\Delta FTA = FTA^{post} - FTA^{pre} = 0.05 = \underbrace{0.47((FTA|u,r) - (FTA|f,r))}_{\text{compliers, previously released}} + \underbrace{0.076(FTA|u,r)}_{\text{compliers, previously detained}}$$

I restrict $\underbrace{(FTA|u,r)}_{\text{compliers, previously detained}} \geq 0.125$ since that was the failure to appear rate for those receiving financial conditions in the pre-period. Assuming those previously detained are at least as likely to fail to appear than we can assume $\underbrace{(FTA|u,r)}_{\text{compliers, previously released}}, \underbrace{(FTA|u,r)}_{\text{compliers, previously detained}} \geq 0.125$. As such, there is a possible range of values of deterrence and incapacitation effects, which I list in Table 21.

Figure 21: Table of possible deterrence and incapacitation effects

FTA change for compliers (previously detained)	FTA change for compliers (previously released)	Deterrence effect	Incapacitation effect	fraction of effect due to deterrence	fraction of effect due to incapacitation
0.125	0.0736	0.0405	0.0095	0.810	0.190
0.225	0.0598	0.0329	0.0171	0.658	0.342
0.325	0.0460	0.0253	0.0247	0.506	0.494
0.425	0.0322	0.0177	0.0323	0.354	0.646
0.525	0.0184	0.0101	0.0399	0.202	0.798
0.625	0.0045	0.0025	0.0475	0.050	0.950
0.725	-0.0093	-0.0051	0.0551	-0.102	1.102
0.825	-0.0231	-0.0127	0.0627	-0.254	1.254
0.925	-0.0369	-0.0203	0.0703	-0.406	1.406

For incapacitation to be the sole source of the aggregate FTA effect, those newly released need to be around 6 times as risky as the always takers ($0.625 \approx 6 \times 0.125$). Even if the newly released are more than three times as risky ($0.325 > 3 \times 0.105$), the table shows that deterrence would still be responsible for about half of the aggregate FTA effect. If I assume that the newly released are equally likely to commit FTA under unconditional release as never takers under financial conditions (0.125), then deterrence is responsible for 81% (0.0405) of the aggregate effect and incapacitation 19% (0.0095).

I Demonstrating theoretical variation in responsiveness of misconduct and release to bail conditions

How do misconduct and release respond to bail conditions? Which quantity is more responsive?

I demonstrate the theoretical ambiguity of which is more responsive with a stylized one-period model. For now, define misconduct as failure to appear in court and define release as a binary outcome for release before case disposition. Assume a unit mass of defendants and three bail conditions: unconditional release (u), unsecured bail (c), and money bail (m).

For each defendant i, misconduct outcomes and release outcomes are a function of the bail conditions. Defendants choose whether or not to pay bail or fail to appear based on expected payouts. Defendants vary in their person-specific pain of jail time ϵ_i and person-specific difficulty of getting to court ϕ_i. At first, assume $\epsilon_i \sim U[0,1]$ and $\phi_i \sim U[0,1]$. All other parameters are fixed across the population. Y is the endowment, J is the cost of jail, B is the cost of bail, and F is the cost of a fine under a guilty conviction.

Figure 22 illustrates expected payouts under different decisions (decision paths are the arrow below blue boxes) and subsequent outcomes (outcome paths are the arrows below gray ovals).

Step 1 is posting bail. This is only relevant under money bail since we assume release under the other two conditions (unconditional release and unsecured bail). Under money bail, if person i does not post, they receive the endowment Y less jail cost J less their person-specific pain: $Y - J - \epsilon_i$. If they do post, they receive income less bail cost $Y - B$ and then move on to the court appearance decision.

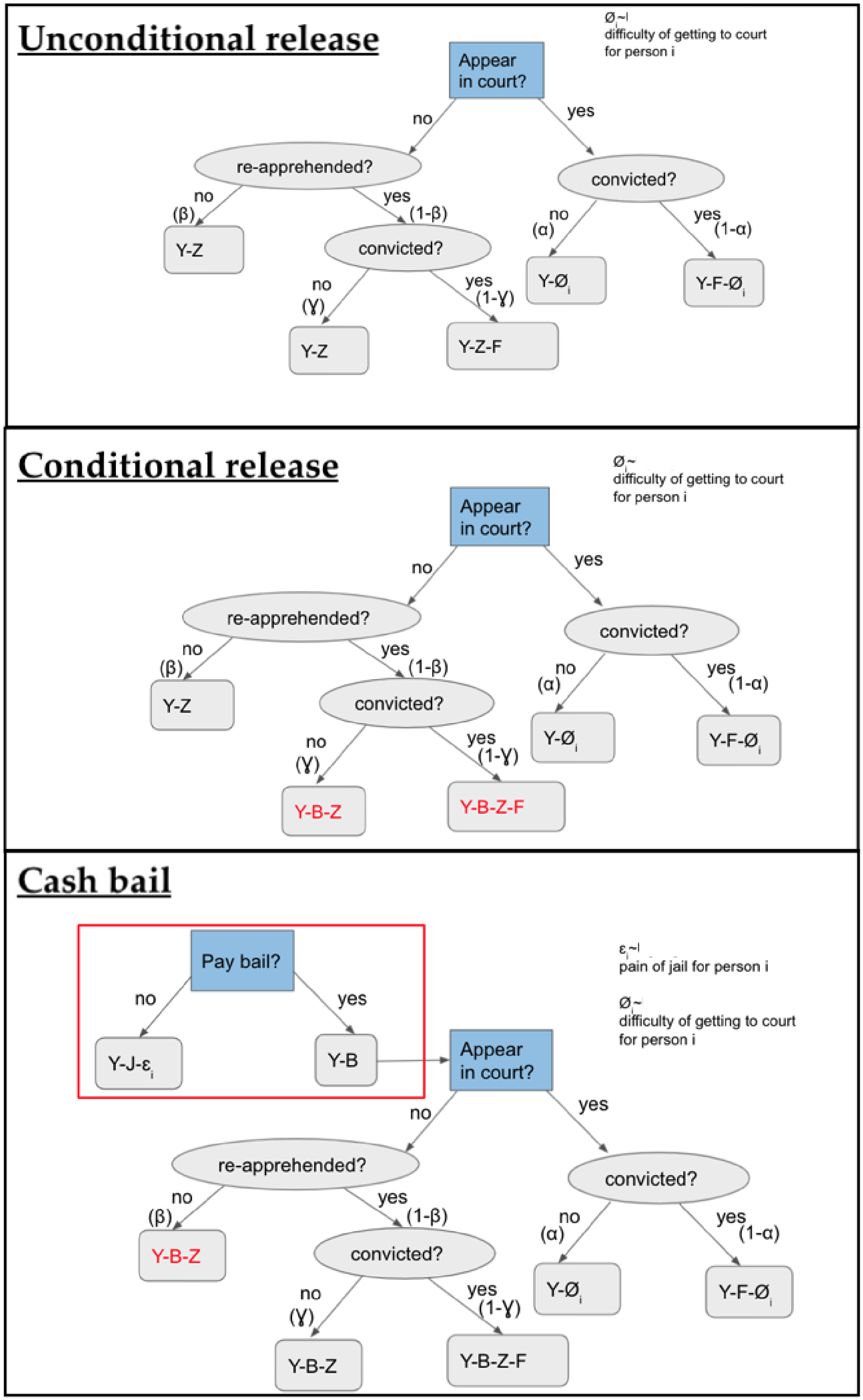
Step 2 is court appearance. If person i appears in court, the payout depends on conviction or acquittal. If acquitted (with α chance), they receive $Y - \phi_i$. If convicted (with $1 - \alpha$ chance), they receive $Y - F - \phi_i$.

If the person fails to appear, they might be reapprehended or not. If they are not reapprehended (with β chance), their outcome depends on the bail condition – under unconditional and unsecured bail, they receive $Y - Z$, which is endowment less disutility of living with a bench warrant; under money bail, they receive $Y - B - Z$, which is the endowment less warrant disutility less bail amount too. If they are reapprehended (with probability $1 - \beta$), the payout depends on conviction or acquittal – if they are acquitted (with γ chance), they receive $Y - B - Z$ if they are under unsecured bail or money bail or $Y - Z$ if they are under unconditional release; if they are convicted (with $1 - \gamma$ chance), they receive $Y - B - Z - F$ if they are under conditional release or money bail or $Y - Z - F$ if they are under unconditional release.

Based on this set-up, I can derive threshold rules that characterize outcomes in the model across bail conditions.

(1) Release outcomes: Call $R_i(b)$ the potential outcome for release under bail condition $b \in$

Figure 22: Payouts across different bail conditions



$\{u, c, m\}$ for person i. In line with institutional details, assume people are always released under unconditional release u and unsecured bail c, $R_i(u) = R_i(c) = 1$. However, under cash, release depends on the comparative magnitudes of $Y - B$ and $Y - J - \epsilon_i$. Person i will post bail if bail costs are less than jail costs or if when the person-specific pain of jail outweighs bail less jail costs – $R(m) = 1[B < J + \epsilon_i]$.

(2) Failure to appear in court outcomes: Call $FTA_i(b)$ the failure to appear potential outcome for release under bail condition $b \in \{u, c, m\}$ for person i. Based on the model payouts,

$$FTA_i(u) = 1\Big[[Z + F(1-\beta)(1-\gamma)] - F(1-\alpha) < \phi_i\Big]$$

Intuitively, losses under failing to appear are: the disutility of a warrant (Z) and the expected fine ($F(1-\beta)(1-\gamma)$). Losses under appearance are the expected fine $F(1-\alpha)$. If losses are greater under failing to appear, then person i needs a bigger shock ϕ_i to offset that difference.

$$FTA_i(c) = 1\Big[[Z + F(1-\beta)(1-\gamma) + B(1-\beta)] - F(1-\alpha) < \phi_i\Big]$$

Under unsecured bail, people who fail to appear have a chance of losing their bail amount, which shifts their decisions.

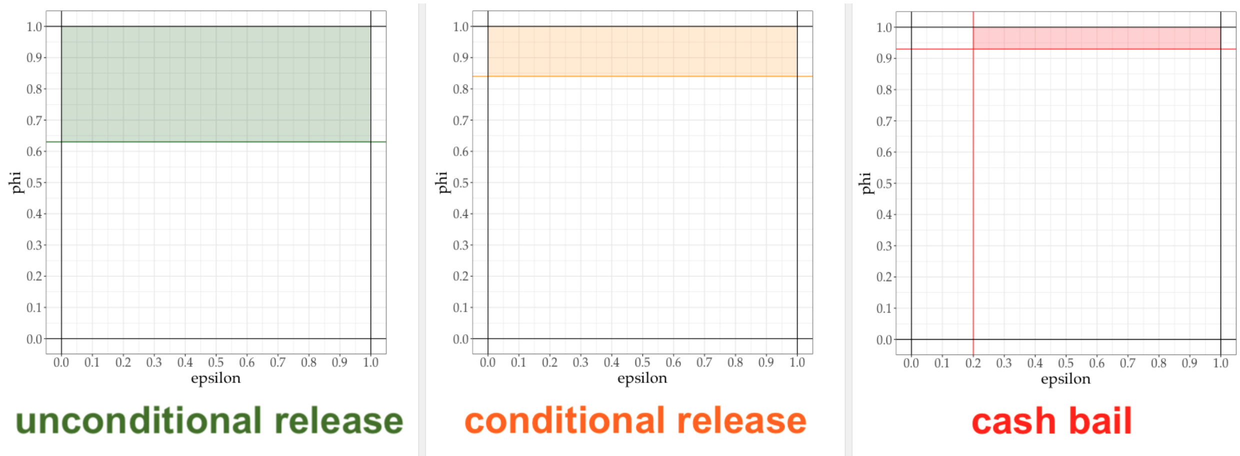
$$FTA_i(m) = 1[B - J < \epsilon_i] \times 1\Big[[Z + F(1-\beta)(1-\gamma) + B] - F(1-\alpha) < \phi_i]$$

Under money bail, failure to appear depends on posting bail as well as incentives once released. If $B < J$, everyone is released. If $B > J$, then only those with $\epsilon_i > B - J$ are released since the disutility of jail needs to offset the difference between bail and jail costs. If released, losses under failing to appear are: the disutility of a warrant (Z), the expected fine ($F(1-\beta)(1-\gamma)$), and expected sacrificed bail amount B. Again, losses under appearance are the expected fine $F(1-\alpha)$ and if losses are greater under failing to appear, then person i needs a bigger shock ϕ_i to offset that difference.

Graphically, Figure 23 demonstrates the range of people who fail to appear under each bail condition option with a set of person-invariant model parameters.[82] unsecured bail induces less failure to appear than unconditional release since the necessary ϕ_i threshold (to rationalize failure to appear) is shifted up. money bail induces less failure to appear since (1) the necessary ϕ_i threshold (to rationalize failure to appear) is shifted up and (2) there is now a necessary ϵ_i threshold for bail posting. The way the shifting of the ϕ_i (the horizontal) threshold impacts the red area is a deterrence effect, but the way the shifting of ϵ_i (the vertical) threshold impacts the red area is an incapacitation effect. Everyone to the left of the red vertical line is incapacitated under money bail.

[82]Setting: $\alpha = 0.9$, $\gamma = 0.1$, $\beta = 0.3$, $F = 1$, $B = 0.3$, $J = 0.1$, and $Z = 0.1$.

Figure 23: Model-based outcomes across bail conditions



If, as we initially assumed, ϵ_i and ϕ_i are both independently distributed on $U[0,1]$, then the average treatment effect of moving across bail conditions on failure to appear is simply the difference between the shaded areas. The average treatment effect of conditions on release is simply the rectangular area to the left of the red vertical line – that area is $max\{B - J, 0\}$.

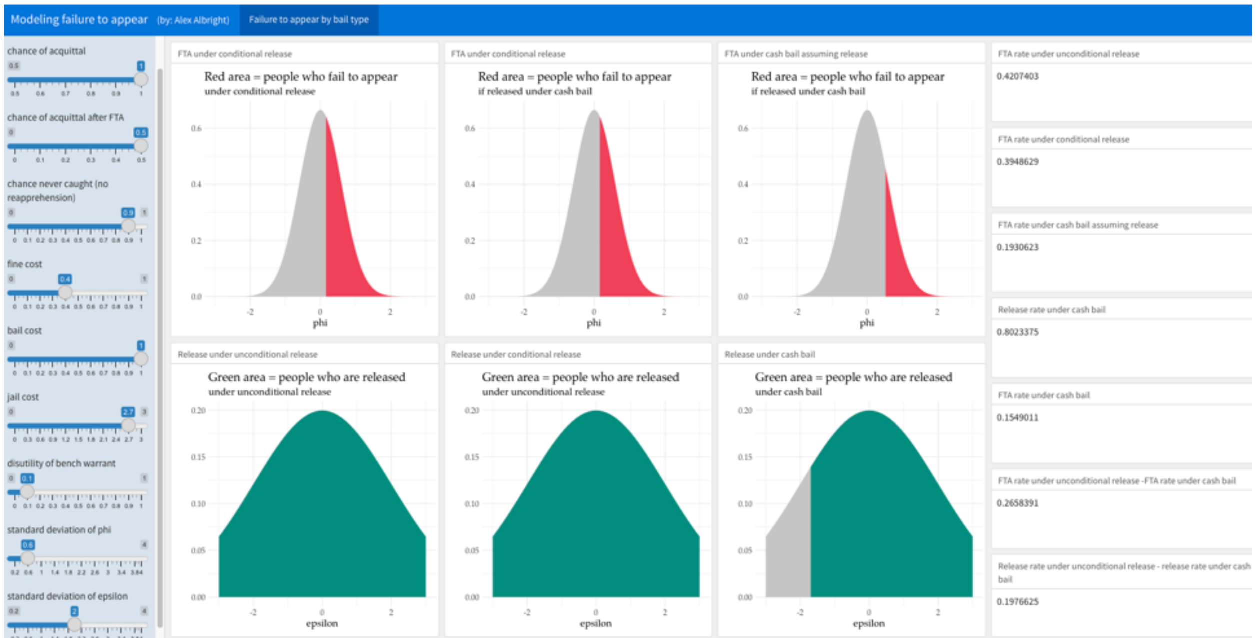
Under this distribution assumption, the deterrence effect of money bail relative to unconditional release is simply B, while the impact on release is $B - J$. In this case, misconduct is more responsive to conditions than is release. However, the theoretical magnitudes of bail conditions effects become ambiguous with different distributions of ϵ_i and ϕ_i.

For illustration, allow ϕ_i and ϵ to be normally distributed. I demonstrate that the same person-invariant model parameters can result in larger failure to appear or larger release changes depending on standard deviations of distributions. Graphically, Figure 24 displays the same threshold rules with $\epsilon_i \sim N(0,2)$ but the ϕ_i distribution varying between the upper and lower Figure. The same threshold therefore generates different deterrence effects due to the shape of the ϕ_i distribution.

In the upper Figure, with $\phi_i \sim N(0,1)$ and $\epsilon_i \sim N(0,2)$, unconditional release's (relative to money bail) impact on failure to appear is larger in magnitude than its impact on release. Both increase but failure to appear increases more. The only change in the lower Figure is that $\epsilon_i \sim N(0,3.4)$. In this context, unconditional release's (relative to money bail) impact on failure to appear is smaller in magnitude than its impact on release. Release increases more than failure to appear.

Figure 24: Model-based outcomes across bail conditions with varying shock distributions

FTA impact > Release impact



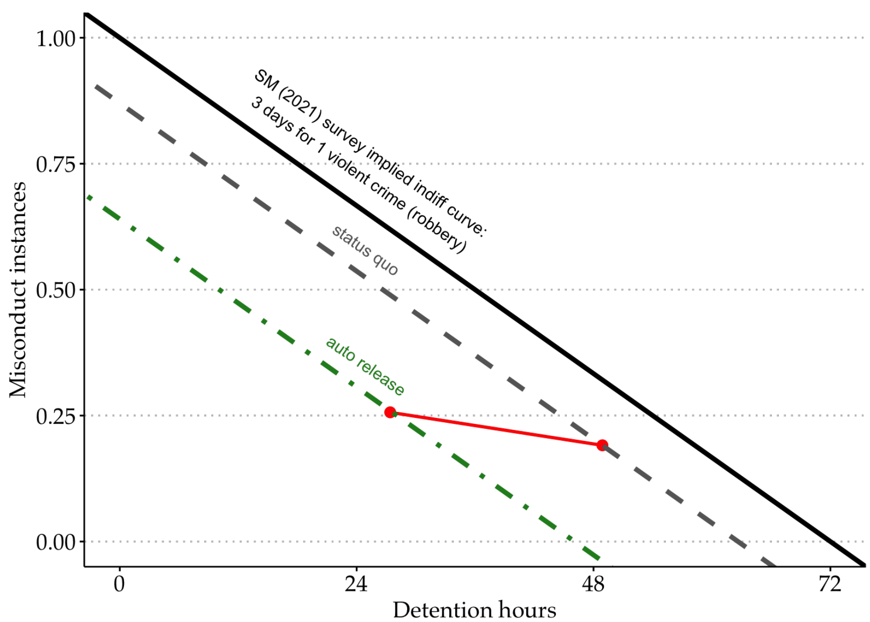
FTA impact < Release impact

J Comparing results with estimates of relative preferences

Another method of taking the reduced-form to prior estimates is to use existing evidence on social preferences in survey data. While this is not possible across all misconduct types, recent work by Stevenson and Mayson (2021) specifically surveys respondents on preferences over violent crime victimization and detention. They find that the median respondent finds 3 days in detention as costly as suffering a robbery, an example of a violent crime.

Conceptualizing Stevenson and Mayson (2021)'s estimated valuations of crime victimization and detention time as defining an indifference curve between two non-market "bads" (1 violent crime and 72 hours of detention), I use Figure 25 (a) to draw out that curve (in black) as well as the implied policy tradeoff from the upper bound of my empirical estimate with respect to misconduct.[83] The red line in Figure 25 demonstrates the shift between the status quo and AR based on reduced-form estimates.

Figure 25: Comparing policy-induced tradeoffs to survey evidence on tradeoffs



Interpreting the survey estimates as defining a valid indifference curve, the AR program shifts society to a preferred allocation of two non-market "bads" of detention hours and violent rearrest since the green dotted curve (which hits the AR-induced pair of outcomes) is to the left of the gray dotted curve (which hits the status quo-induced pair of outcomes).

[83] I use the highest value within the 95% confidence interval for failure to appear and pretrial rearrest (in Figure 6)., I add up the top of the 95% CI's for failure to appear and pretrial rearrest: $(.033) + (0.008 * 1.96) + (.007 + (1.96 * 0.005)) = 0.0655$. This value is 0.0655. Using the same procedure to that demonstrated in Section 7.2, this point estimates yields a policy tradeoff of 13.68 detention days to 1 misconduct event.