No Money Bail, No Problems?

Trade-Offs in an Automatic Pretrial Release Program

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Abstract

Many jurisdictions in the United States have reduced the use of money bail, which requires people to pay money to leave jail pretrial. This policy reduces the amount of time arrestees spend in jail, at the risk that they will offend or fail to appear in court. This paper estimates the detention-misconduct trade-off by studying a program in Kentucky that automatically released low-level arrestees with no bail. The program reduced total hours in detention by 43%, while it increased court non-appearance by 3.3 percentage points and had a small (0.7 percentage point) and insignificant effect on pretrial rearrest. For each new court non-appearance, the arrested population was spared an additional 26 days in detention. The program's effects operated through multiple channels: reducing the use of money bail, reducing the use of other bail types, and speeding up the release process. In the second part of the paper, I isolate the effects of eliminating money bail for low-level arrestees. For each new pretrial rearrest, I find that 10 people are spared longer jail stays by money bail elimination.

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1 Introduction

Pretrial detention – jailing arrested people before they are convicted of a crime – is common in the United States. On any given day, approximately 470,000 unconvicted people are held in jail (Zeng, 2023). This practice imposes substantial costs on both detained individuals and society. Pretrial detention increases the probability of conviction and decreases formal sector employment for detained people (Dobbie et al., 2018; Heaton et al., 2017; Leslie and Pope, 2017). Meanwhile, local governments spend $25 billion annually operating jails, where 70% of inmates have not been convicted (Zeng, 2023; Pew Charitable Trusts, 2021).

In the United States, defendants typically are in pretrial detention because they did not pay money bail (Reaves, 2013). Money bail is a bond that defendants can post that allows them to leave jail and is repaid if they avoid criminal activity and attend their court dates. Therefore, defendants who post bail have an additional financial incentive to appear in court and avoid criminal activity while they await trial outside of jail. However, using money bail to incentivize good conduct means pretrial detention for those who do not pay.

Public concern about widespread pretrial detention and its link to money bail fueled a wave of bail reform efforts across the country in the 2010s and early 2020s (Smith and Jorgensen, 2021). Advocates of bail reform contend that money bail has small effects on misconduct, which do not justify the costs of keeping people in jail. Opponents of bail reform counter that money bail provides necessary financial motivation for defendants to return to court and avoid criminal activity while awaiting trial. The debate often hinges on an empirical question: What are the effects of money bail reform on detention and misconduct?

There are several common challenges to studying this question with observational data from bail reforms. First, reforms often change multiple features of the bail system, which makes it difficult to determine which changes were responsible for observed effects. Second, many reforms impact everyone in a jurisdiction, leaving no defendants from whom to select a control group. Finally, studies are often identified from marginal policy changes or variation across judges, which may yield results that do not generalize to more binding reforms that would eliminate money bail for large swathes of the defendant population.

I confront these challenges by studying a unique bail reform program that featured automatic implementation, well-defined changes, and variation in eligibility across groups. Between 2013 and 2017, Kentucky implemented a bail reform program called Administrative Release (AR). Before the program, judges made bail decisions for all arrested individuals, which typically required either money bail (payment required for release) or unsecured bail (payment required only if pretrial misconduct occurred). The AR program created a two-track system based on eligibility. Individuals arrested for certain misdemeanors who had low risk scores based on their criminal history were eligible for AR and were automatically released on recognizance, which means they were released without money bail or unsecured bail requirements. Those who were ineligible – either because they faced more serious charges or because they had higher risk scores – continued through the traditional bail-setting process as usual.

The Administrative Release program is a useful natural experiment for studying bail reform because it automatically changed bail decisions for a substantial share of the defendant population. Using a differences-in-differences approach, I compare bail conditions, pretrial detention, and pretrial misconduct before and after AR between eligible defendants (the treatment group) and ineligible defendants (the control group) in the same county. I focus on these three outcomes because they capture both the program's direct goal of changing bail conditions and the central policy trade-off in bail reform debates: the trade-off between pretrial detention and pretrial misconduct. Under the assumption that these outcomes would have changed in parallel across defendant groups in the absence of AR, these comparisons identify the average treatment effects of bail reform on low-level arrestees.

The Administrative Release program dramatically changed bail conditions for eligible defendants. Release on recognizance rates (release without money bail or unsecured bail requirements) increased by 50 percentage points, more than tripling the pre-program rate. This represents a dramatically larger shift than those seen in previous studies. For comparison, Ouss and Stevenson (2023) examined a policy that increased recognizance release by 11 percentage points. By increasing release on recognizance, the AR program meaningfully reduced the rates of both money bail (by 20 percentage points) and unsecured bail (by 30 percentage points), resulting in $3.2 million less in required money bail payments annually for the group of eligible defendants.

The program significantly reduced pretrial detention, even though eligible defendants already had high release rates. Before the program, 76% of eligible defendants were released within one day of arrest. I find the program increased this rate by 13.7 percentage points. Overall, the eligible group spent 42% fewer hours in pretrial detention, which amounts to 223,000 fewer detention hours (or 25.5 fewer person-years) annually. In terms of pretrial misconduct, I find the program increased failure to appear rates (by 3.3 percentage points), but I find small (0.7 percentage point) and statistically insignificant effects on pretrial arrest. For each additional missed court appearance, the program reduced pretrial detention by 26 days across the eligible group. Using fiscal estimates from prior research, I find that these changes suggest net cost savings for the pretrial system.

These average treatment effects represent causal effects of the AR program itself, which are influenced by how counties treated low-level defendants before implementation. Before AR, counties had varying pretrial practices: some set money bail for most low-level offenders, while others primarily used unsecured bail (a type of bail that does not require up-front payment but involves potential fines for pretrial misconduct). These pre-existing bail patterns determine how many automatically released defendants would have otherwise faced immediate money bail requirements versus potentially delayed unsecured bail penalties. Building on methods from Kline and Walters (2016), I exploit these county-level differences to identify causal effects of automatic release, relative to both money bail and unsecured bail. These findings help generalize my results on reducing money bail to jurisdictions with different bail policies.

I find that automatic release reduces detention time, relative to both unsecured bail and money bail. Automatic release reduces detention time relative to unsecured bail because automatic release bypasses judges, which speeds up release even in the absence of money bail requirements. However, automatic release has larger effects relative to money bail. Among defendants no longer required to pay money bail, I find 47% are released within one day who otherwise would not have been. Since typical money bail amounts for this group are in the hundreds of dollars, this result suggests that low-level defendants are meaningfully liquidity constrained.

I find that automatic release has very different effects on pretrial misconduct depending on whether the counterfactual is unsecured bail or money bail. Relative to unsecured bail, I find that automatic release has minimal effects on pretrial misconduct. Since defendants are not incarcerated when unsecured bail is set, these results imply that unsecured bail has minimal deterrent effects on misconduct. Although unsecured and money bail theoretically impose the same penalties for pretrial misconduct, practical difficulties in collecting unsecured bail may weaken its effectiveness as a financial incentive. In my setting, unsecured bail slows release without any accompanying reductions in pretrial misconduct.

Meanwhile, I find that relative to money bail, automatic release increases failure to appear rates and (likely) pretrial rearrests, though the statistical significance of the latter depends on the estimation method. Among defendants no longer required to pay money bail, 12% of those who would have otherwise appeared miss court appearances, while 4.6% of those who would not have been rearrested are rearrested. The larger effects on court appearance suggest that money bail has larger effects for more minor forms of misconduct. While my estimates suggest increased rearrests from eliminating money bail, the vast majority of affected defendants (95.4%) are not rearrested as a result. For each new pretrial rearrest, I find that eliminating money bail spares 10 people longer jail stays.

This paper makes three contributions to the literature. First, I provide new causal evidence on the effects of non-marginal, policy-relevant changes in bail conditions.[1] Existing work by Ouss and Stevenson (2023) finds that money bail has limited effects on pretrial detention and misconduct. Their study is identified from a discretionary reform that decreased money bail rates for eligible defendants by 15% (5 percentage points). In this paper, I estimate effects in a more binding context: money bail rates for eligible defendants decreased by 63% (21 percentage points) in my setting. I find that money bail increases pretrial detention and – to a lesser extent – decreases pretrial misconduct. Therefore, I show that treatment effects identified from light touch reforms do not necessarily generalize to more binding environments.[2]

Second, I contribute to our understanding of how financial obligations operate in the criminal justice system. My findings reveal that unsecured bail – which defendants are supposed to pay in the event of misconduct but not up front – has minimal deterrent effects. This result relates to recent evidence from Pager et al. (2022) and Finlay et al. (2024) showing that post-sentencing fines and fees do not affect recidivism or employment. Though these financial obligations (unsecured bail and post-sentencing fines and fees) arise at different stages of the justice system, they share a crucial feature: collection is not certain. In an Oklahoma debt relief experiment, people in the control group repaid less than 5% of their outstanding court debt, while in Milwaukee, only 28% of newly charged dollars were collected after fee increases (with money bail forfeiture accounting for half of that collected amount) (Pager et al., 2022; Giles, 2023). Low collection rates may help explain otherwise surprising null effects of financial obligations in the justice system.[3]

[1] Most of the previous empirical evidence on the effects of bail in the economics literature uses judge leniency designs (Gupta et al., 2016; Dobbie et al., 2018). In contrast, Abrams and Rohlfs (2011) study effects of money bail amounts using random assignment of judges to bail guidelines, and Myers Jr. (1981) and Helland and Tabarrok (2004) demonstrate that less bail is associated with more failure to appear using regression analysis and propensity score matching, respectively.

[2] Relatedly, treatment effects from judge leniency designs on bail may differ from treatment effects from implemented policy for some of the same reasons (Rose and Shem-Tov, 2021). If a policy reform such as automatic release is more lenient than any judge, then the desired treatment effect is not attainable through a judge design. Additionally, there may be behavior changes in response to an overall policy change that would not be present in its absence when using judge leniency designs.

[3] Money bail presents a distinct case because it reverses the collection timeline: payment is required up

Third, I contribute to the evidence base on financial fragility in the United States. I find that money bail requirements in the hundreds of dollars meaningfully delay release for almost 50% of low-level defendants. This finding suggests that roughly half of this population lacks immediate access to even a few hundred dollars, aligning with previous research documenting low savings and self-reported inability to cover emergency expenses in the US population (Lusardi, 2011; Board of Governors of the Federal Reserve System, 2018). Most related is Mello's (2024) recent evidence showing that traffic fines averaging $195 increase unpaid bills in collections. Mello (2024) conceptualizes default as a "last resort" (and indicative of financial distress), and the same can be said of staying in jail when money bail is in the hundreds of dollars.

The rest of the paper proceeds as follows. Section 2 describes the Kentucky Administrative Release (AR) program and the administrative data used in this study. Section 3 describes the empirical strategy for estimating the effects of the AR program. Section 4 presents results on the causal effects of the AR program on bail conditions, pretrial release, and pretrial misconduct. Section 5 investigates the mechanisms underlying the program effects by estimating the effects of money bail and unsecured bail, relative to release on recognizance. Section 6 concludes.

2 The Kentucky Administrative Release Program and Administrative Data

2.1 Background on the Kentucky 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 pretrial release, also called "bail conditions." The exact process for bail setting varies widely across the country. 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).[4] Bail is meant to incentivize good pretrial conduct, but it can also lead to pretrial detention when defendants do not pay money bail.

In Kentucky, after someone is arrested and booked in one of the state's 120 counties, a

front. When defendants are released after posting money bail, the court already holds the funds.

[4]In this context, "public safety" usually means averting criminal offending in the pretrial period. In some jurisdictions, such as New York City, the legal objective of bail is limited to ensuring court appearance.

pretrial officer collects information about the arrested person and arrest incident to help facilitate pretrial decisions. Within 24 hours of booking, the pretrial officer will present this information to a judge, usually via a phone call.

There are three broad and mutually exclusive categories of bail in Kentucky.[5] In order of least to most restrictive, they are (1) release on recognizance, (2) unsecured bail, and (3) money bail:

1. Release on recognizance has no financial bail penalties. People who are released on recognizance 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 a 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.[6]

Figure 1a demonstrates how bail types feed into potential pretrial release, which, in turn, feeds into potential pretrial misconduct. Under release on recognizance, unsecured bail, and paid money bail, individuals are released. Since they are then free pretrial, it is possible for these people to commit pretrial misconduct (by failing to appear in court or being rearrested pretrial). If individuals do not post the required amount when assigned money bail, they are detained, and it is mechanically impossible 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 (2024) demonstrate that the causal effect of failing to appear varies across people 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. Therefore, there is always a potential

[5]On top of these three main categories, there is also bail denial. Bail denial is limited to the most severe cases and means that no amount of money can secure pretrial release. In Kentucky, only about 1.8% of cases have bail denied. Since bail denial is so rare (especially for the low-level arrestee population I study), I focus on the three remaining bail categories, which characterize nearly 100% of cases: release on recognizance, unsecured bail, and money bail.

[6]In Kentucky, if someone remains in jail for 24 hours after receiving money bail, 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.

cost of pretrial misconduct to people in the criminal justice system even if they are released on recognizance. When 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 predetermined bail amount.

There are two features of the Kentucky bail system that are notable relative to others in the US. First, initial bail decisions in Kentucky 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 based solely on information presented by pretrial officers. Judges make decisions about bail type and bail amount (if applicable) within a few minutes after getting information from pretrial officers.

Second, Kentucky does not have a commercial bail industry. In most states, if 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 arrested person (usually 10% of the total). However, in 1976 Kentucky became the first state to outlaw commercial bail bonding. Therefore, the arrested person or someone in their network needs to post the required bail amount.

2.2 Kentucky Administrative Release (AR) Program

From 2013 through 2017, Kentucky phased in the Administrative Release (AR) program to expedite pretrial release for people charged with non-violent, non-sexual misdemeanors (e.g., shoplifting, disorderly conduct, or criminal driving offenses). The goal of the program was to reserve resources for higher-risk cases by providing automatic release on recognizance for a subset of people who would normally have had bail set by a judge. The program's design and implementation generates quasi-experimental variation in program exposure, making it well suited for causal inference.

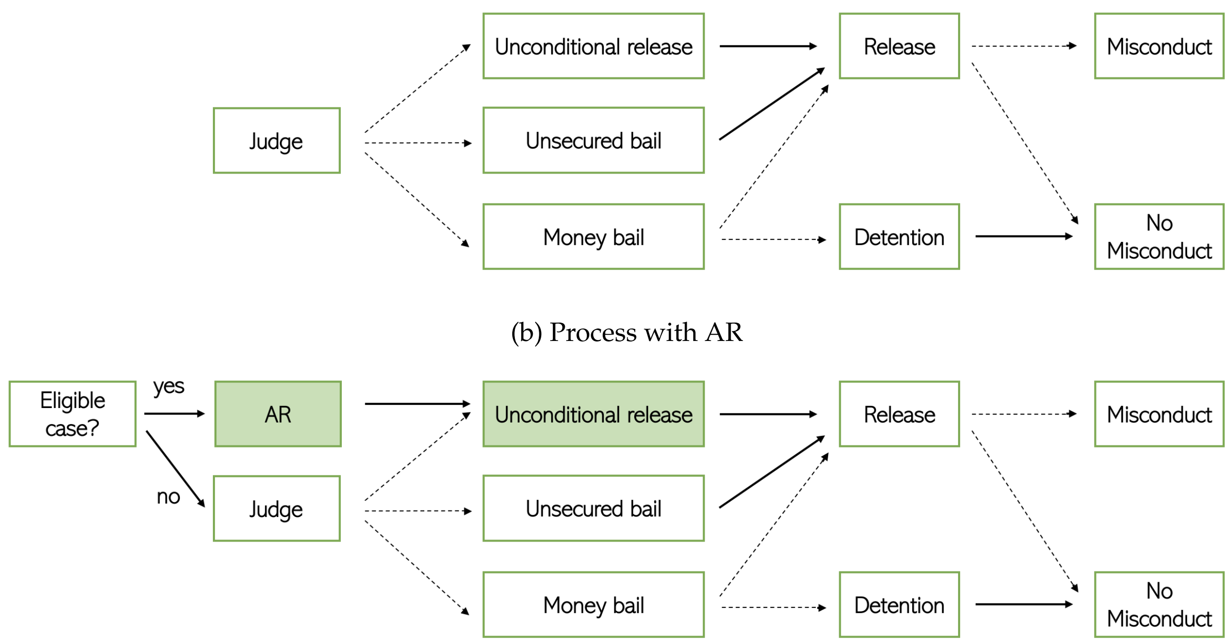
How AR Impacted the Pretrial Process: Before the AR program, a pretrial officer would present information about an arrested person and their alleged offense to a judge. The judge then would make a bail decision within a few minutes, and the flow of outcomes would follow the illustration in Figure 1a. After AR was implemented, what happened next depended on case eligibility, as demonstrated in Figure 1b. Eligible cases were assigned release on recognizance without the involvement of a judge, but ineligible cases go through the system as usual: pretrial officers presented information to judges, and judges made bail decisions.

While most bail reform efforts and programs rely on judicial discretion, AR intentionally limited judicial discretion. Limiting discretion means more binding changes since judges often deviate from recommended actions (Stevenson, 2018; Albright, 2024; Stevenson and Doleac, 2024). Other bail reforms have since emulated the binding nature of the AR program. Most notably, the 2023 Illinois Pretrial Fairness Act, which made Illinois the first state to end money bail, intentionally created "bright-line rules that [take] away carceral tools from judges instead of trusting them to use such tools sparingly" (Grace, 2021). Activists involved in advocating for the Illinois reform explained that judicial discretion in prior bail reform waves made it "increasingly clear . . . that a more binding, statewide policy change was needed" (Grace, 2021).

Figure 1: The Pretrial Process

(a) Process in the absense of AR

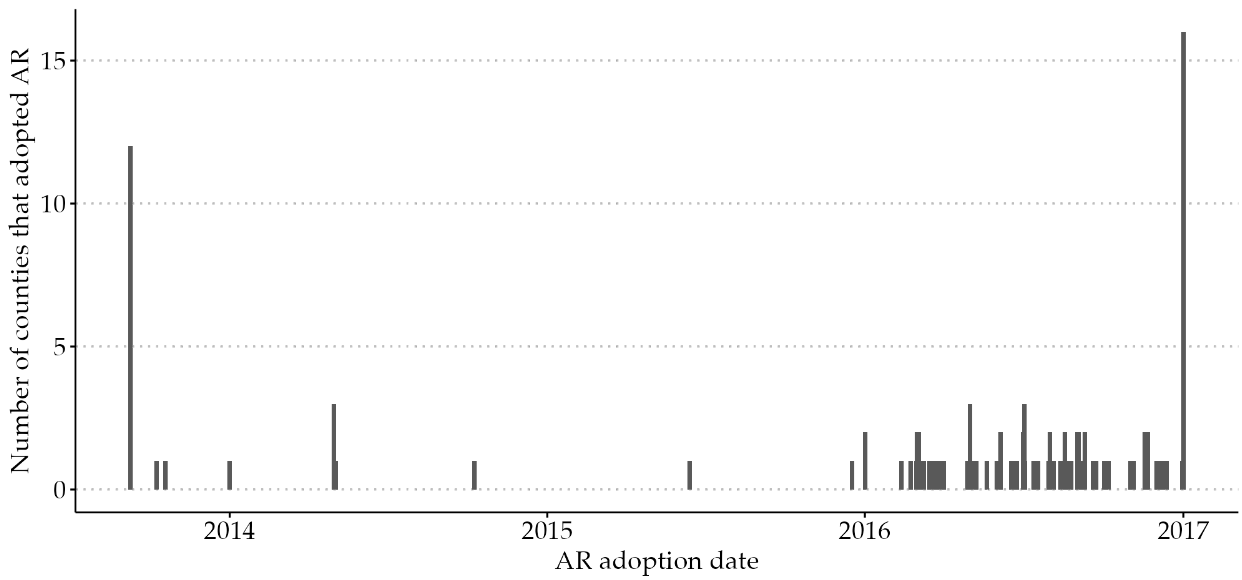
(b) Process with AR



Notes: Figure 1a demonstrates that in the absence of AR, judges can choose between the three conditions: release on recognizance, unsecured bail, and money bail. Release on recognizance 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 release on recognizance. If a case is not eligible, the traditional bail process shown in Figure 1a is followed.

AR Policy Timing Across the State: The Kentucky AR program was phased in across the state between September 2013, when it was first piloted in a small group of counties, and January 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 a histogram showing the number of counties that implemented the AR program at different dates.[7] While there were some early adopters, the majority of counties (about 80 out of 120) implemented AR between January 2016 and December 2016. The last 17 counties to adopt the program did so when it became mandatory statewide.

Figure 2: AR Timing Across Kentucky Counties



Notes: This figure is a histogram that demonstrates the number of counties that implemented AR at different dates between September 2013, when the program was first piloted, and January 2017, when the program became mandatory statewide.

AR Eligibility Requirements: The details that determined eligibility for the AR program changed over time. Originally, the pilot counties listed out county-specific eligibility conditions (Supreme Court of Kentucky, 2014). But in November 2015, a Supreme Court order standardized eligibility across counties (Supreme Court of Kentucky, 2015). When I discuss eligibility, I use the definition from the January 2017 Supreme Court order that mandated the program statewide (Supreme Court of Kentucky, 2017a). The January 2017 eligibility definition captures eligibility well for all counties that adopted AR after the November 2015 order, because the differences between the two orders are minor (Supreme

[7] 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 an AR release in the administrative data.

Court of Kentucky, 2015, 2017a).[8]

AR eligibility is determined by 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.[9] Given these constraints, common AR eligible charges are driving offenses (driving without insurance, driving on a suspended license, driving without a license), buying or possessing drug paraphernalia, shoplifting, and disorderly conduct/public intoxication.
3. The arrested person 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. The score is calculated by adding up points associated with a list of case and defendant characteristics, as detailed in Appendix A1.2. An example of a person with a risk score of 8 is someone who is under 23, has failed to appear once in the last 2 years, and has a prior misdemeanor conviction that resulted in a sentence to incarceration.

Charges are the biggest factor limiting AR eligibility. Only about 34% of cases are associated with AR eligible charges. Meanwhile, about 68% of cases are regular arrests, and 75% involve risk scores below 8. Overall, about 21% of all cases meet all three eligibility requirements for the AR program.

Why Study AR?: Kentucky's AR program has a number of useful features for studying the causal effects of money bail reform. For one, the program was automatic, which resulted in larger effects on bail conditions than most reforms that rely on the discretion of people in the criminal justice system. Second, the presence of both eligible and ineligible cases before and after the program provides me with useful variation for causal inference. Third, the program-eligible population is people arrested for low-level offenses, a population of

[8] I confirmed my understanding of the eligibility criteria in the Kentucky Supreme Court orders via interviews with local practitioners such as court staff members. See Appendix A1.1 for more details on how AR eligibility evolved over time.

[9] AR eligible charges exclude 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.

great interest when it comes to narrowing the scope of the criminal justice system.

Bail reform has been implemented in many different ways in the US (Smith and Jorgensen, 2021; Stemen and Olson, 2023; Craigie and Grawert, 2024). I study what happens when arrested people who normally would be assigned money bail or unsecured bail are automatically released on recognizance instead. In my case, the treated population (people with low risk scores arrested for low-level offenses) and the counterfactual to the status quo (release on recognizance) are well defined. In contrast, the policy proposal of completely banning money bail impacts a broader population, and the counterfactual to money bail is undefined: it is ambiguous what replaces money bail (e.g., outright detention,[10] supervision,[11] release on recognizance, electronic monitoring, etc.) and in what cases.

2.3 Administrative Data

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 construct the necessary case-level data using a collection of raw datasets from the Kentucky Administrative Office of the Courts that span all criminal cases with felony or misdemeanor charges across all of Kentucky's 120 counties. I outline data construction by variable category below.

Bail Setting: I use data on the initial bail observation for each distinct case.[12] I capture the date of this bail decision (relevant for if the case is before or after AR implementation), the bail category (release on recognizance, unsecured bail, or 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,

[10] Even though Illinois banned money bail in 2023, this has not ended the practice of pretrial detention. Sims (2025) finds that the policy had milder effects than anticipated on pretrial detention: she finds daily jail populations decreased by 16%-18%.

[11] Policies encouraging alternatives to money bail may also unintentionally reduce use of the most lenient conditions. For instance, Skemer et al. (2020) show that a New York City supervision program that reduced money bail rates also resulted in reduced release on recognizance.

[12] I study the initial bail decision because a person can have multiple bail decisions over time for the same arrest, but the AR program is relevant only to the initial bail decision (not later ones).

so I generate measures of these outcomes based on observable rearrests in the Kentucky data.[13] Therefore, I can measure rearrests only within the state of Kentucky. Pretrial rearrest does not include rearrests due to failing to appear, because they are not considered a new criminal offense. Therefore, the two types of misconduct are mutually exclusive.

Eligibility Status: The administrative data do not include information on which cases were eligible or ineligible for the AR program. Therefore, I categorize cases as either eligible or ineligible according to observable variables pertaining to arrest type, charges, and risk scores: eligible cases are the result of regular arrests, include eligible non-violent, non-sexual misdemeanor charges, and involve people with risk scores below 8 (Supreme Court of Kentucky, 2017a).

However, the observable data might not include details that impact eligibility classification. For example, charge codes in the administrative data are the Kentucky Uniform Crime Reporting Codes (assigned by law enforcement officers), but these can be different from the charge codes in the narrative record, which were 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. If someone is arrested on a warrant with an amount specified, pretrial officers cannot release the person on AR, and the case must go to a judge. Since the administrative data do not capture if someone is arrested on a warrant with a set bail amount, I cannot classify these observations as ineligible.[14]

Therefore, the absence of administrative eligibility records induces some classification errors: certain cases are incorrectly identified as eligible (false positives) or ineligible (false negatives). However, qualitative interviews with court staff suggest misclassification should be infrequent.

Sample Restrictions: I make two key restrictions for inclusion in my sample. First, I require a case's initial bail decision date to fall between July 1, 2014, and November 30, 2017. I make this restriction because the risk scores necessary for eligibility classification were first used in July 2014 and the risk score criterion for eligibility changed in December 2017 (Supreme Court of Kentucky, 2017b).[15] Second, I exclude cases from 21 counties that implemented AR before November 2015. I do this because November 2015 was when the

[13]There is a 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. I do not observe citations or summons interactions.

[14]For more details on criminal warrants, see Appendix A1.3.

[15]Cases before July 2014 are not useful, because I am not able to classify their eligibility. Cases from after December 2017 are not useful, because their eligibility is defined differently than eligibility for the rest.

key components of eligibility (as described in Section 2.2) were made consistent.[16]

3 Empirical Strategy

3.1 Differences-in-Differences for *a Single* AR Start Date

Kentucky counties adopted AR at different times (Figure 2), but I do not need to use this staggered timing for identification. Rather, within each cohort of counties that share an adoption date, I have a clean 2x2 differences-in-differences framework: every county cohort includes treated and control groups in the form of eligible and ineligible cases. With a single adoption date, I can employ a conventional two-way fixed effect differences-in-difference approach, as follows:

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

where y_{it} is an outcome for case i at time t, $Eligible_i$ is an indicator for if case i is AR eligible, λ_t are time fixed effects, $Post_t$ 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.

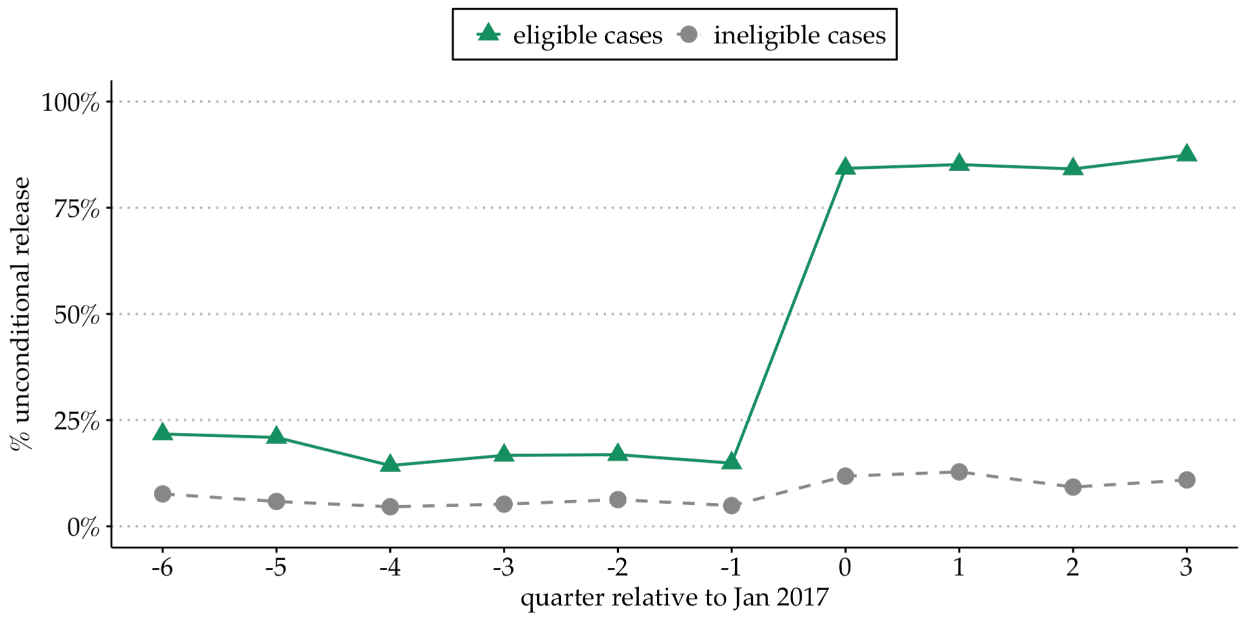
In this set-up, the identification assumption is that outcomes for the eligible and ineligible cases would have followed parallel trends in the absence of the AR program. This identification assumption is simpler and therefore preferable to the more demanding (and often problematic) parallel trends assumption required in staggered differences-in-differences approaches.[17]

I use Figure 3 to demonstrate the intuition of the differences-in-differences approach for one county cohort – counties that adopted AR in January 2017. Figure 3 is a binned scatter plot showing the release on recognizance rates for eligible and ineligible cases in the quarters before and after AR adoption. Eligible cases and ineligible cases move in parallel before AR is implemented (parallel trends), but AR impacts the two groups very differently. AR dramatically increased release on recognizance for eligible cases but not for ineligible cases. After AR, the rate of release on recognizance went from 20% to 90% for

[16] The eligibility criteria for these 21 counties were substantively different from the criteria for the later 99 counties. As an aside, most of those 21 counties only feature post-AR data in the sample window (see Figure 2), which means they are also not informative for my differences-in-differences approach.

[17] There are many inferential concerns about differences-in-differences designs that rely on staggered timing for identification (Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Baker et al., 2022).

Figure 3: AR Impacts Bail for Eligible Cases



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

eligible cases.[18] Therefore, for this cohort, the differences-in-differences effect of AR on release on recognizance would be about 70 percentage points.

3.2 Differences-in-Differences for *All* AR Start Dates

To calculate aggregate treatment effects across all 99 Kentucky counties in my sample, I use a stacked estimator as in Cengiz et al. (2019). 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

[18] In theory, the release on recognizance 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 2.3). Imperfect tagging is the primary explanation for the imperfect assignment to release on recognizance. (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 because of unobservable administrative learning and logistical difficulties, according to interviews with pretrial staff members.) However, the large jump in release on recognizance for eligible (but not ineligible) cases demonstrates that the eligibility tag does a good job picking up program eligibility.

in those relative time periods.[19] My final dataset is therefore a stacked dataset where each event-specific dataset is the observations from one of the total 99 counties. Thus, the estimated pooled specification is as follows:

$$y_{itc} = \beta Eligible_{ic} + \lambda_{tc} + \delta^{DD}(Post_{tc} \times Eligible_i) + \epsilon_{it}, \tag{2}$$

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 this stacked specification (equation 2) and its single date analog (equation 1) 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 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.

3.3 Addressing Threats to Identification

My differences-in-differences approach leverages variation by eligibility and time to estimate the effects of AR. However, this approach may be misleading if the composition of cases changed discontinuously at the time of program take-up. I describe two particular concerns below and provide evidence that neither threatens the identification strategy's validity.

(1) Change in Arrests: If bail is a large component of expected punishment for low-level offenses, then making bail more lenient could lead to increased offending. If there are more arrests as a result of the program, changes in the rates of detention and misconduct as a result of the program could be partially a result of that compositional change. In other words, if the population of arrested people changes because of the program, measured effects of the program on detention and misconduct are confounded by population changes. In Appendix A2, I show there is no evidence that the program changed the number of arrests, which alleviates concern about this potential threat to identification.

(2) Change in Eligibility Stringency: Another potential threat to identification is manipulation of program eligibility. For instance, if it became tougher to be classified

[19] I make this restriction because OLS weighting can be problematic if stacked samples don't have coverage for the full treatment effect range (Baker et al., 2022).

as eligible after the program (because of purposeful actions of people in the criminal justice system), then the eligibility classification itself shifted with the policy. This would mean eligible (and ineligible) cases before and after are not comparable, confounding the differences-in-differences approach. In Appendix A2, I show there is no evidence of this potential confounding: the results do not indicate that there were strategic actions to manipulate eligibility by police or pretrial officers around the time of AR implementation.

4 What Are the Effects of the AR Program?

I present the effects of AR following the order of the pretrial process illustrated in Figure 1: (1) bail conditions, (2) release outcomes, and (3) misconduct outcomes.

4.1 What is the Impact of AR on Bail Conditions?

Figure 4 demonstrates that AR dramatically increased release on recognizance, and analogously reduced money bail and unsecured bail. Figure 4a presents dynamic differences-in-differences coefficients for effects on release on recognizance. Pre-period estimates are precise zeros and do not demonstrate pre-trends, providing strong support of the parallel trends assumption. Overall, AR increased release on recognizance by 50.5 percentage points relative to a baseline of 19.8% for eligible cases. Compared with the results of other bail reform studies, this is a striking effect – for example, Ouss and Stevenson (2023) studied a policy that increased recognizance release by 11 percentage points. This difference comes from AR's binding nature. The discretionary nature of most other bail reforms results in more modest impacts.

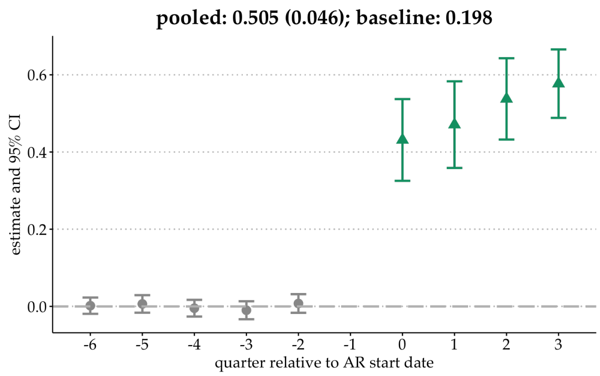
Although the eligible population was designed to be low risk across multiple dimensions (arrest type, charges, risk scores), judges assigned release on recognizance to only 19.8% of eligible cases before AR implementation. This descriptive fact aligns with Ouss and Stevenson's (2023) concept of "asymmetric penalties in errors," where judges face public scrutiny for observable misconduct resulting from lenient bail conditions, while overly restrictive conditions are less observable. Consequently, judges may have viewed release on recognizance as failing to adequately tailor conditions to individual defendants, which could negatively impact judges if they are blamed for resulting misconduct.

The post-AR estimates in Figure 4a reveal dynamic effects. Release on recognizance effects increase as time passes from AR implementation. According to court staff, this is likely because early-adopter counties improved program administration over time.

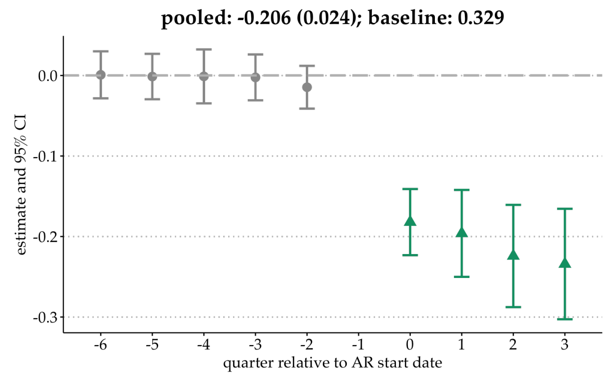
Supporting this explanation, Figure 3 demonstrates the absence of such dynamic effects in counties adopting AR in 2017 (when the program expanded statewide), as processes were standardized and refined prior to this policy implementation date.

Figure 4: How AR Impacts Bail Conditions

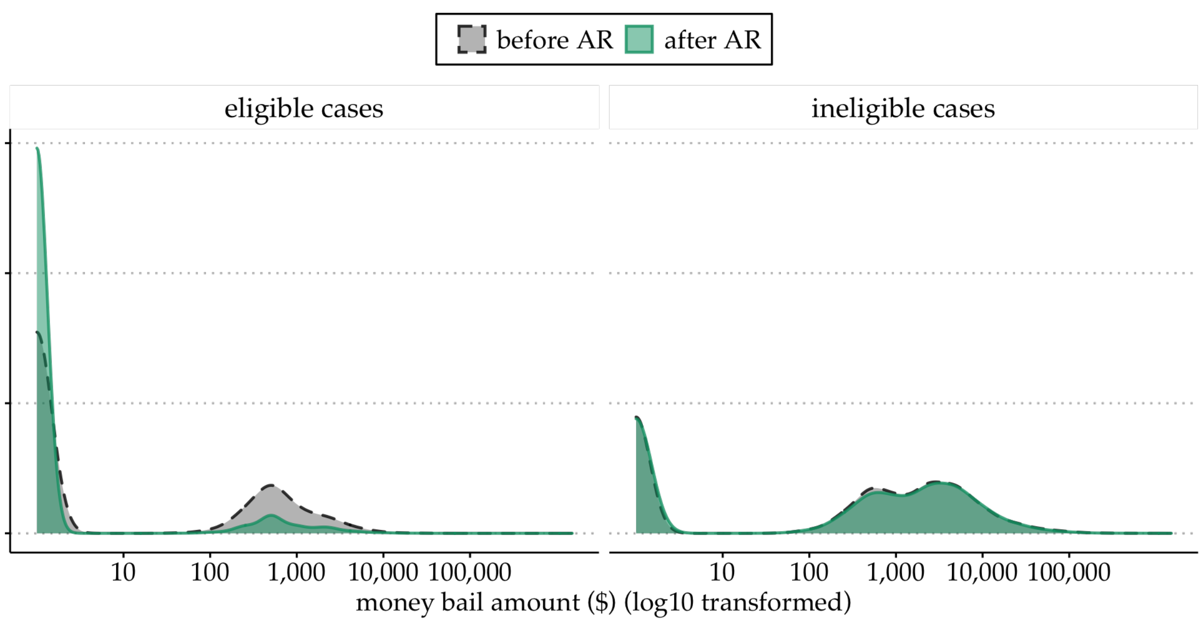
(a) Release on Recognizance



(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. The outcome variable for Figure 4a is an indicator for release on recognizance. 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 the 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 solid outline) is the distribution after AR and the gray shaded area (with a dashed outline) is the distribution before AR.

The increase in release on recognizance came from an analogous decrease in unsecured bail and money bail conditions. While one might expect substitution to occur primarily from unsecured bail – the next least restrictive option after release on recognizance – Figure 4b shows that money bail decreased by 20.6 percentage points from a baseline of 32.9% for eligible cases. Therefore, the AR program not only significantly reduced unsecured bail rates but also made substantial changes to the more restrictive money bail practices.

When money bail is set, judges assign a particular bail amount that defendants must post for release. I use Figure 4c to demonstrate how the number of dollars required for release changed before and after AR for eligible and ineligible cases. While the distribution of dollars required for release went unchanged for ineligible cases, the distribution for eligible cases shifted left.[20] The leftward shift came primarily from the transformation of money bail requirements in the hundreds of dollars into \$0 money bail requirements (release on recognizance). The most common shift in levels for eligible cases was a change from \$500 to \$0. Overall, total dollars required for release decreased by 76.9% for eligible cases, which meant about \$3.2 million less required for release in one year.[21]

4.2 What Is the Impact of AR on Pretrial Detention?

I estimate the effects of AR on pretrial detention with two different outcome variables. I examine effects on release within one day of booking ("one-day release") and total detention hours. I use these two outcomes to capture different margins of the programs' effects.

During the study period, judges were required to set bail within 24 hours of booking. Consequently, the vast majority of individuals granted unsecured bail or release on recognizance pre-AR should have been released in one day.[22] The one-day release measure therefore mostly captures changes attributable to reduced money bail requirements (as it takes time to post money bail) rather than administrative speed improvements made by the program. In contrast, total detention hours captures both effects: the reduction in money bail conditions and the expedited AR release process that bypasses judge decisions.

[20] One might have hypothesized judges would become harsher for ineligible cases because they know eligible cases are receiving release on recognizance automatically. There is no evidence of this sort of behavioral change in the data. There do not appear to be unintended consequences (as a result of judge behavior) that offset the AR program effects in the full population.

[21] The average money bail amount required before AR was \$360, so the 76.9% decrease corresponds to a drop down to \$83 after AR. See Appendix A3 for the dynamic differences-in-differences plot where the outcome variable is the inverse hyperbolic sine of the money bail amount.

[22] Figure A5 demonstrates this is true in practice: before AR, 92.3% of eligible cases assigned release on recognizance and 87.1% of eligible cases assigned unsecured bail were released in one day. However, only 49% of eligible cases assigned money bail were released in one day.

I first estimate the effects of AR on one-day release. Before AR, about 33% of eligible defendants were assigned money bail; the average amount was \$360. Section 4.1 showed that the rate of money bail dropped by 20.6 percentage points. If one expects that most of this population was able to quickly cover the \$360 bail, then it is possible the AR program did not change one-day release rates. However, if a \$360 money bail delays release for a meaningful share of arrestees, then one-day release could increase significantly. Figure 5a demonstrates that one-day release increases 13.7 percentage points from a baseline of 76.6%. Even though one-day release was already the norm before AR, the program significantly increased this rate, which suggests that arrestees' inability to post hundreds of dollars constrains pretrial release.[23]

Figure 5b demonstrates AR changed the overall number of hours people spent in pretrial detention. While the hours distribution did not change after AR for ineligible cases, the distribution moved to the left for the eligible cases. Detention stays for longer than 12 hours became less frequent, while detention stays for less than 12 hours became more frequent. Overall, the program caused a 42.4% decrease in hours in detention.[24] Relative to the baseline mean of 48.9 hours, this implies a decrease of around 20.7 hours in detention for the average low-level arrestee. In the year before AR, low-level arrestees in my sample were detained for a total of about 520,000 hours. The 42.9% drop translates to about 223,000 fewer person-hours in detention – equivalent to a reduction of roughly 9,300 person-days or 25.5 person-years in pretrial detention.

Overall, the AR program had large effects on pretrial detention. These effects stemmed from a combination of fewer money bail conditions and speedier release even in the absence of money bail (by bypassing judges). The one-day release effects demonstrate that money bail amounts in the hundreds constrain release, a result that is consistent with financial fragility in the arrested population. This evidence on financial fragility is related to recent work showing that traffic fines in the hundreds of dollars increase unpaid bills in collections (Mello, 2024). Like default, remaining in jail when faced with money bail requirements is indicative of financial distress.

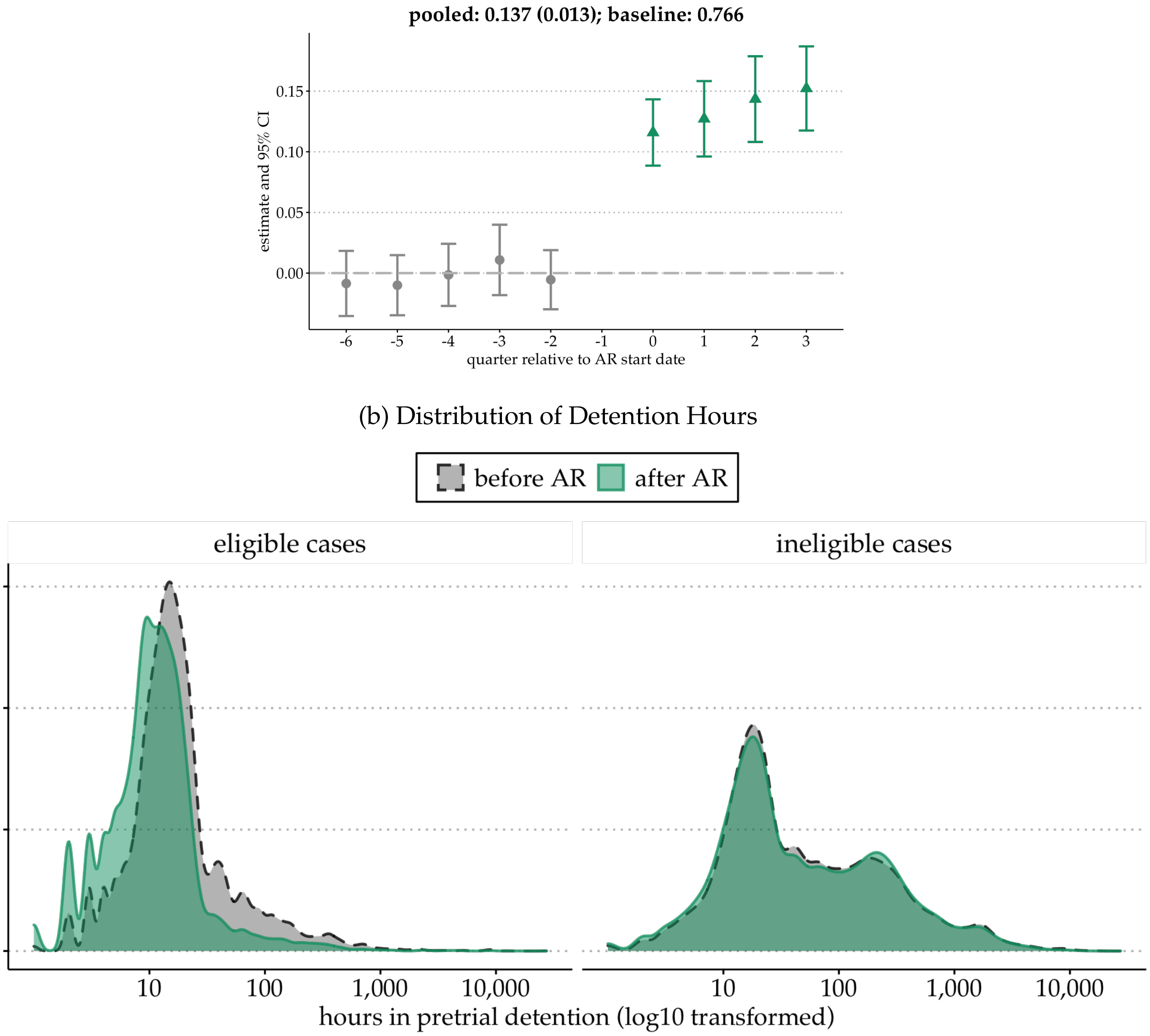
[23] I show in Appendix A3 that AR increases other measures of pretrial release as well: any pretrial release increases by 6 percentage points, while pretrial release within 3 days increases by 6.3 percentage points.

[24] See Appendix A3 for the dynamic differences-in-differences plot with the inverse hyperbolic sine of detention hours as the outcome variable.

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. 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.

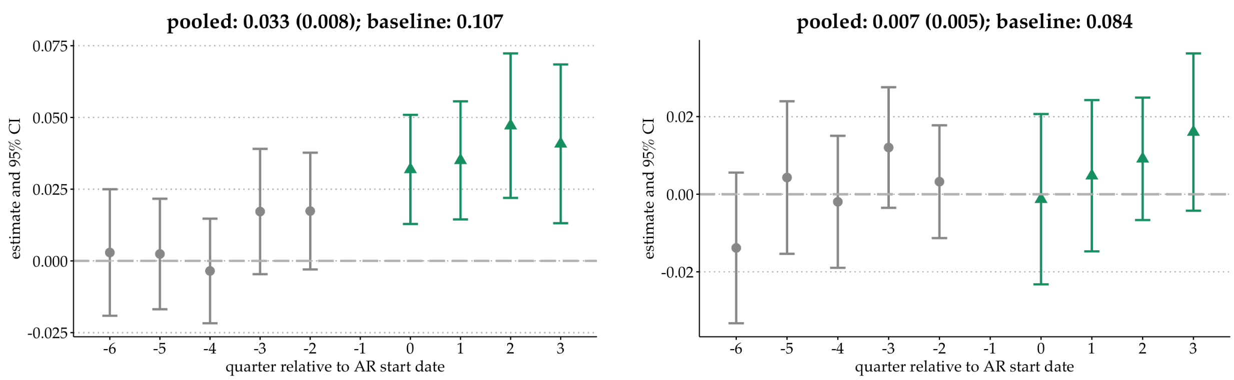
4.3 What Is the Impact of AR on Pretrial Misconduct?

The legal objective of bail is to set the least restrictive conditions to ensure appearance at court and public safety (American Bar Association Criminal Justice Standards Committee, 2007). People who are released from jail are given a court date for arraignment, where they enter pleas of guilty or not guilty and where misdemeanor cases may be resolved. Failure to appear in court means someone who has been released from jail does not show up for their scheduled court date. While court appearance is a measurable outcome, public safety is not. In this context, public safety usually means averting criminal offending in the pretrial period. Therefore, I estimate the effects of AR on two measures of pretrial misconduct: failure to appear in court and pretrial rearrest.[25]

Figure 6: How AR Impacts Pretrial Misconduct

(a) Failure to appear

(b) Pretrial rearrest



Notes: Figures 6a and 6b plot the event-time differences-in-differences estimates using methods described in Section 3.2. 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.

Figure 6 demonstrates that AR increases failure to appear but does not have statistically significant effects on pretrial rearrest. Figures 6a shows that AR increased failure to appear by 3.3 percentage points (relative to a baseline of 10.7%). Annually, this corresponds to an increase of about 364 court non-appearances. Figure 6b shows that the point estimate for pretrial rearrest is small (0.7 percentage points) and insignificant at conventional levels. Annually, this point estimate corresponds to an increase of about 79 rearrests. The 95% confidence interval includes a range of effects, including a decrease of 0.28 percentage

[25]Pretrial rearrest means someone who has been released from jail is arrested on a new offense while their original case is pending. This rearrest measure does not include rearrests for violation of pretrial conditions or failing to appear since those are not new offenses.

points and an increase of 1.68 percentage points (relative to a baseline of 8.4%). In Appendix A3, I show that estimates are even smaller (0.3 percentage points) and continue to be statistically insignificant if the outcome of interest is violent rearrest.

4.4 Implied Trade-Offs Between Pretrial Detention and Misconduct

The US bail system is supposed to keep both pretrial detention and misconduct low (American Bar Association Criminal Justice Standards Committee, 2007). If any reform decreases either detention or misconduct with no change to the other, the policy is unambiguously preferable to the status quo (it's a free lunch). However, under the AR program, pretrial detention decreases, but pretrial misconduct increases as well, although to a lesser extent. Therefore, the program generates measurable trade-offs, which are informative for policy discussions on bail reform.

In terms of annual levels, AR decreased the total number of pretrial detention hours by 223,000 hours (about 25.5 years), while it increased the number of pretrial misconduct instances by 439 instances (360 court non-appearances and 79 pretrial rearrests, 34 of which were violent). Therefore, for each instance of misconduct, the eligible population was spared 21 fewer days in detention in aggregate. If I focus only on the non-appearance results, which are the statistically significant misconduct results, then the implied trade-off is 26 fewer days in detention for each additional court non-appearance. If pretrial rearrest is the object of interest and I focus on the noisy point estimate, the implied trade-off is 118 fewer detention days for each additional pretrial rearrest. Finally, the implied (noisy) trade-off is 273 fewer detention days for each additional pretrial violent rearrest.

Whether any of these trade-offs are desirable depends on which outcome variables we incorporate into social welfare (total detection hours, instances of misconduct, etc.) and their social costs. Appendix A5 explores whether the AR policy is desirable in fiscal and broader welfare terms. I find evidence that the AR program was cost-saving to the government and aligns with survey results on relative preferences between detention or misconduct.[26]

[26] Bail reforms may have goals that are not captured by broad measures of pretrial detention or misconduct. For instance, bail reform may intend to alleviate the inequalities in the justice system, which would make the reform's impact on racial and socioeconomic inequality important for social welfare. Appendix A6 demonstrates the effects of the AR program on racial and socioeconomic gaps.

5 What Drives the Program's Effects?

The results presented thus far demonstrate the reduced form effects of Kentucky's AR program. However, there is a distinction between this policy's overall effects and the specific treatment effects relevant to the broader bail reform debate. AR operated through multiple channels: reducing use of money bail, reducing use of unsecured bail, and speeding up the release process via bypassing judges. In contrast, most bail reform debate focuses on the specific effects of *reducing money bail*. This section of the paper moves beyond reduced form analysis to estimate the effects of specific bail conditions, allowing for a deeper examination of both the mechanisms driving program outcomes and the development of more generalizable treatment effects.

I use an instrumented differences-in-differences approach to estimate the effects of different bail conditions. I formalize this approach and the necessary assumptions with a potential outcomes framework in Section 5.1. I then estimate the effects of release on recognizance in Section 5.2. In Section 5.3, I leverage geographic variation in bail patterns before AR to separately estimate the effects of release on recognizance *relative to unsecured bail and relative to money bail*.

5.1 Potential Outcomes Framework

Consider a population of courts, indexed by i, each with a single arrested person. Each court can assign its arrested person to a bail type: release on recognizance (r), unsecured bail (u), 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 $e \in \{e_0, e_1\}$. The time period before the implementation of AR for a given court i is denoted by t_0, and t_1 is the time period after AR is implemented for given court i. The group of people (and thus courts) who are ineligible under AR rules is denoted by e_0, and e_1 is the group of people 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).

Finally, let $B_{it}(Z_i) \in \{r, u, m\}$ denote the arrested person'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 people who would have otherwise received unsecured bail (u) or money bail (m) to receive release on recognizance (r) instead. No court should switch between unsecured bail (u) and money bail (m) in response to bail reform coverage, and no court should be induced by bail reform coverage to switch an arrested person away from release on recognizance. In other words, the only way bail reform coverage should change bail setting is to shift those receiving u or m to r (Assumption 1). This is an extended monotonicity assumption[27] and can be expressed by the condition below:

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

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

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

Because there are three bail condition options, the complier and never taker groups are split into two subgroups; this feature is a departure from the conventional instrumental variable set-up. When covered by AR, the u- and m- compliers switch to release on recognizance from unsecured bail and money bail, respectively. The two groups of never takers are never given release on recognizance, regardless of AR coverage. Always takers manage to receive release on recognizance even when they aren't covered by AR: the court grants them release on recognizance with judicial discretion in absence of the program. The key extended monotonicity assumption means there are no defiers who switch away from release on recognizance (r) and there are no AR-induced shifts between unsecured bail (u) and money bail (m).

Later-stage outcomes of interest – pretrial release and pretrial misconduct – are a function of bail conditions. Therefore, I write them as $R_{it}(b)$ and $M_{it}(b)$, respectively. If I make one additional assumption, then I can write the reduced form effects of AR on Y (such that $Y \in \{R, M\}$) as

$$E[Y_{i1} - Y_{i0}|Z_i = 1] - E[Y_{i1} - Y_{i0}|Z_i = 0].$$

The above expression omits bail condition information from the Y_{it} notation, which requires an exclusion restriction (Assumption 2). The exclusion restriction requires that the only

[27] Extended monotonicity is also a condition in Kline and Walters's (2016) 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.

way AR coverage impacts detention and misconduct outcomes is through the bail type (i.e., the only treatment channel is the bail condition category).

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

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

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

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

Intuitively, the DD-IV approach attributes the entire reduced form effect to the complier group (those who are spared unsecured bail or money bail conditions and instead released on recognizance because of AR). The effects of release on recognizance on detention and misconduct can therefore be derived by simply rescaling the reduced form effects (effects of AR on pretrial misconduct, release) by the first-stage effect (effect of AR on release on recognizance).

5.2 What Are the Effects of Release on Recognizance?

In this subsection, I use instrumented differences-in-differences to estimate the effects of release on recognizance. Specifically, I instrument for release on recognizance with AR coverage (the interaction of case eligibility and relative time being after AR adoption). Specifications 3 and 4 demonstrate the consequent first-stage and second-stage regressions:

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

$$y_{itc} = \beta Eligible_{ic} + \lambda_{tc} + \delta^{DD-IV}\widehat{ROR}_{itc} + \epsilon_{it}. \tag{4}$$

Table 1 presents the results for the δ^{DD-IV} coefficients of interest across three binary outcomes of interest: pretrial release in one day, failure to appear in court, and pretrial rearrest.[28] The results show that 27.2% of those spared unsecured or money bail avoid

[28] In Section 4.2, I explained that the one-day release measure mostly captures changes attributable to reduced money bail requirements. Meanwhile, the total detention hours measure captures changes due to changed bail conditions and bypassing judges (a channel outside of bail conditions). Therefore, to satisfy the exclusion restriction in the instrumented differences-in-differences setting, I estimate pretrial detention

spending one or more days in detention as a result. Meanwhile, about 6.5% of those spared unsecured or money bail conditions failed to appear in court as a result. This means 93.5% of the complier group did not have appearance ensured by bail conditions under the status quo. In other words, 93.5% of decisions for the complier population before AR were Type II errors (too harsh), while 6.5% of decisions for the population after AR were Type I errors (too lenient).[29]

Table 1: Effects of Release on Recognizance (Instrumented Differences-in-Differences)

	Release in 1 Day (1)	Failure to Appear (2)	Pretrial Rearrest (3)
Release on Recognizance (instrumented)	0.2720*** (0.0255)	0.0648*** (0.0149)	0.0130 (0.0109)
Observations	136,917	136,917	136,917

Notes: This table shows instrumented differences-in-differences estimates for the effects of release on recognizance on three binary outcomes: release in 1 day, failure to appear, and pretrial rearrest. Specifically, I instrument for release on recognizance with AR coverage (the interaction of case eligibility and relative time being after AR adoption). The first-stage is shown by equation 3 and the second-stages are shown by equation 4. Standard errors are clustered at the county-level. (* p<0.1, ** p< 0.05, *** p<0.01)

Before AR, about 20% of cases received release on recognizance at the discretion of judges. After AR, an additional 50.5 percentage points of cases received release on recognizance (without judge discretion). Were the people judges released on recognizance before AR lower risk in terms of court appearance than the people released because of AR?[30] I use my potential outcomes framework to address this question in Appendix A7. I estimate that the complier group is about twice as likely to fail to appear relative to the always taker group. Therefore, while the vast majority of compliers (93.5%) did not have their court appearances ensured by harsher conditions, the group assigned harsher bail ex ante was riskier than the group with more lenient bail ex post. Judges did identify a lower risk group (with respect to court appearance) in their decisions before AR.

Table 1 shows that the share of people spared unsecured or money bail conditions who are rearrested as a result is small, at 1.3%. Moreover, the result is not statistically significant, so I cannot reject the null that release on recognizance does not impact rearrest. In the next subsection (Section 5.3), I probe this result with the different bail alternatives (money bail and unsecured bail) in mind.

effects with one-day release as my outcome measure going forward.

[29]In this context, a decision was "too harsh" if it set unnecessary bail conditions to induce court appearance, and a decision was "too lenient" if someone failed to appear in response to more lenient bail conditions.

[30]This is analogous to asking if the always takers were lower risk than the compliers.

Why does release on recognizance impact pretrial misconduct? Bail conditions can impact misconduct through both incapacitation (people cannot commit misconduct if they are detained on money bail) and deterrence effects (people avoid misconduct to avoid financial penalties that result from unsecured or money bail). So, release on recognizance can increase misconduct because it leads to fewer people detained (incapacitation) and fewer financial incentives (deterrence). Since release on recognizance always means less detention and fewer financial incentives simultaneously, it is not possible to cleanly disentangle how incapacitation and deterrence effects contribute to the misconduct results. However, I can use my potential outcomes framework to provide a range of incapacitation and deterrence effect magnitudes that are consistent with empirical estimates. I outline the core intuition and results below (see Appendix A8 for details).

The key intuition is that some compliers (cases that receive release on recognizance due to AR) are released in the absence of AR, while others are detained in the absence of AR. Therefore, the change in misconduct for the always released compliers is solely a consequence of deterrence. 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 release on recognizance, which is an unknown parameter.

I can outline the relative importance of incapacitation and deterrence based on varied assumptions about the risk of newly released compliers. For incapacitation to be the sole source of the aggregate failure to appear effect, the newly released need to be around 7 times as risky as the always takers (i.e., the newly released would need to fail to appear more than 70% of the time when released on recognizance), and the always released group, which now is released on recognizance, would need to become less likely to fail to appear. Since these two requirements are unlikely, deterrence is likely responsible for some of the aggregate effect on court non-appearance.[31] This result has broader implications for the bail literature, as it suggests that bail conditions influence arrested individuals' behavior through mechanisms beyond mere incapacitation. This result challenges the exclusion restriction in many judge leniency designs, which assumes judicial decisions affect outcomes solely through pretrial detention.

[31] 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 instead assume that the newly released are equally likely to fail to appear under release on recognizance as never takers under financial conditions, then deterrence is responsible for 80% of the aggregate effect and incapacitation 20%. (See Appendix A8 for the details on calculations.)

5.3 What Are the Effects of Eliminating Money Bail? What About Unsecured Bail?

The AR program generates variation that can be used to identify a number of parameters. The effects of release on recognizance, estimated with DD-IV in Section 5.2, are policy relevant for marginally increasing release on recognizance in Kentucky (by, for instance, marginally expanding eligibility for the AR program). However, the estimated causal effects of release on recognizance are a mix of two distinct underlying effects: the effects of release on recognizance *relative to unsecured bail* and the effects of release on recognizance *relative to money bail*. Since much policy debate centers on the specific effects of eliminating money bail, it is useful to disentangle these two effects.

In the language of my potential outcomes framework, there are two distinct complier groups that drive the aggregate effects of release on recognizance: u-compliers (who switch away from unsecured bail) and m-compliers (who switch away from money bail), as defined in Section 5.1.

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

$$LATE_r^Y = S_m LATE_{mr}^Y + (1 - S_m) LATE_{ur}^Y,$$

where $LATE_{mr}^Y$ is the local average treatment effect of eliminating money bail (and replacing it with release on recognizance), $LATE_{ur}^Y$ is the local average treatment effect of eliminating unsecured bail (and replacing it with release on recognizance), and S_m is the fraction of compliers that are m-compliers. From 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.[32] Therefore, the effect of release on recognizance is a 40-60 mix of two distinct treatment effects:

$$LATE_r^Y = (0.408) LATE_{mr}^Y + (0.592) LATE_{ur}^Y.$$

Since other states and jurisdictions use different mixes of bail conditions, the aggregate effect of release on recognizance is limited in its external validity. Intuitively, implementing

[32] 40.8% comes from dividing the magnitude of the effect of AR on money bail, seen in Figure 4b, by the effect of AR on release on recognizance, seen in Figure 4a. Another method of calculating these shares is outlined in Appendix A9.

release on recognizance means the same thing across environments only if the counterfactual bail condition is the held constant. Therefore, it is worthwhile to explore the differences between the two counterfactual-specific treatment effects.[33] 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.

I use two methods to estimate the underlying counterfactual-specific effects. Both leverage geographic variation in pre-AR bail setting norms. Before AR was implemented, bail patterns looked very different across Kentucky. This is because different judges worked in different counties and had different bail practices. "You just get used to how your local judge does it," according to the deputy director of Kentucky's public defenders office (Musgrave and Desrochers, 2019). In some counties in my sample, the dominant alternative to release on recognizance was money bail. In others, the dominant alternative was unsecured bail.

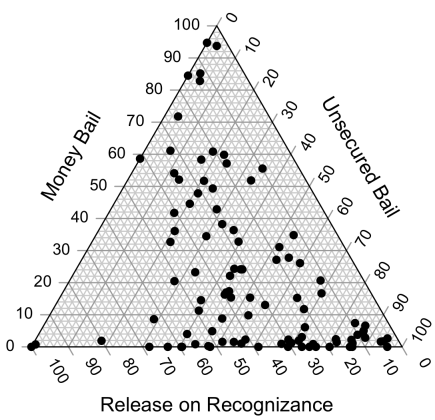
Figure 7a demonstrates this variation across counties in bail setting patterns before AR. Each county is shown as a point in three-dimensional space, where the three dimensions are the rates of release on recognizance, money bail, and unsecured bail for AR eligible cases. Counties near the lower left vertex used release on recognizance almost 100% of the time (counties at the right side of the triangle never used release on recognizance), counties near the top vertex used money bail almost 100% of the time (counties at the bottom side of the triangle never used money bail), and counties near the lower right vertex used unsecured bail release almost 100% of the time (counties at the left side of the triangle never used unsecured bail). Since money bail, unsecured bail, and release on recognizance fully characterize bail conditions and are mutually exclusive, the three rates sum to 100%.

Method 1: In my first method using this variation, I define two subgroups of counties to separate the two distinct alternatives to release on recognizance: money bail and unsecured bail. I define "money bail counties" as counties where less than 20% of eligible cases receive unsecured bail pre-reform, and I define "unsecured bail counties" as counties where less than 20% of eligible cases receive money bail pre-reform. These groups are illustrated in Figure 7b. Money bail counties are highlighted in orange and are near the triangle's left side, while unsecured bail counties are highlighted in gray and are near the triangle's bottom side.

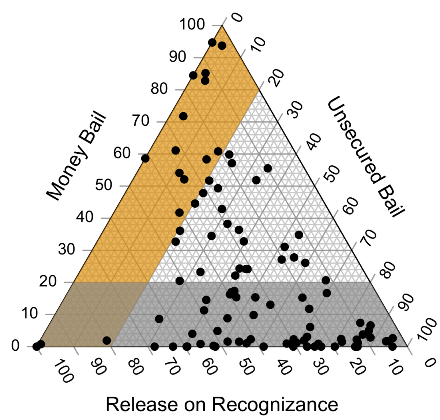
[33] Moreover, the parameter that is more universally relevant is the effect of release on recognizance relative to money bail. Not only is money bail more consistently used across the country, but it also is the primary target of bail reform conversations because of its incapacitation effects and salient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

Figure 7: Pre-AR Bail Setting Across Counties and Consequent Bail Substitution

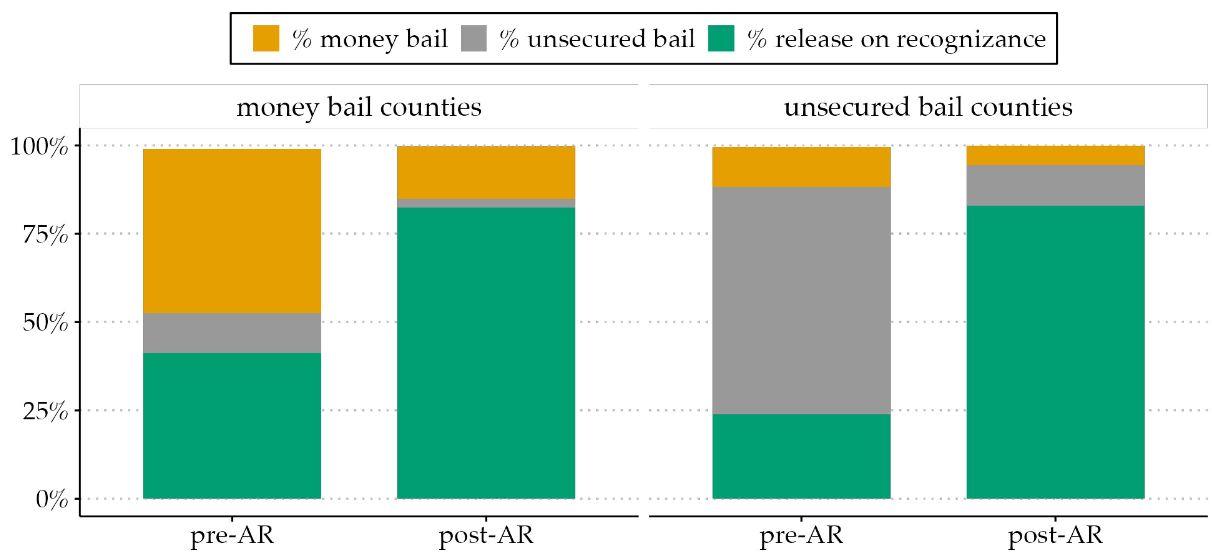
(a) Pre-AR County Variation in Bail Setting



(b) Two County Subgroups



(c) Bail Substitution Patterns for the Two County Subgroups



Notes: Figure 7a is a ternary plot demonstrating variation across counties in bail setting before AR. Each dot is one of the 99 counties in my sample. Each dot is positioned based on its rates of money bail, unsecured bail, and release on recognizance for AR eligible cases before AR. The three rates sum to 100% for each county since they fully characterize bail conditions and are mutually exclusive. Figure 7b demonstrates the county subgroups of interest: money bail counties are highlighted in orange; unsecured bail counties are highlighted in gray. Figure 7c shows the composition of bail outcomes before and after AR separately for eligible cases for money bail counties unsecured bail counties.

Figure 7c demonstrates how the AR program's impact on bail conditions differed across the two county groups. The left pair of stacked bars shows bail patterns for money bail counties before and after AR, and the right pair of stacked bars shows the patterns for unsecured bail counties before and after AR. The figure shows that AR primarily caused substitution from money bail to release on recognizance in money bail counties, but primarily caused substitution from unsecured bail to release on recognizance in unsecured bail counties.

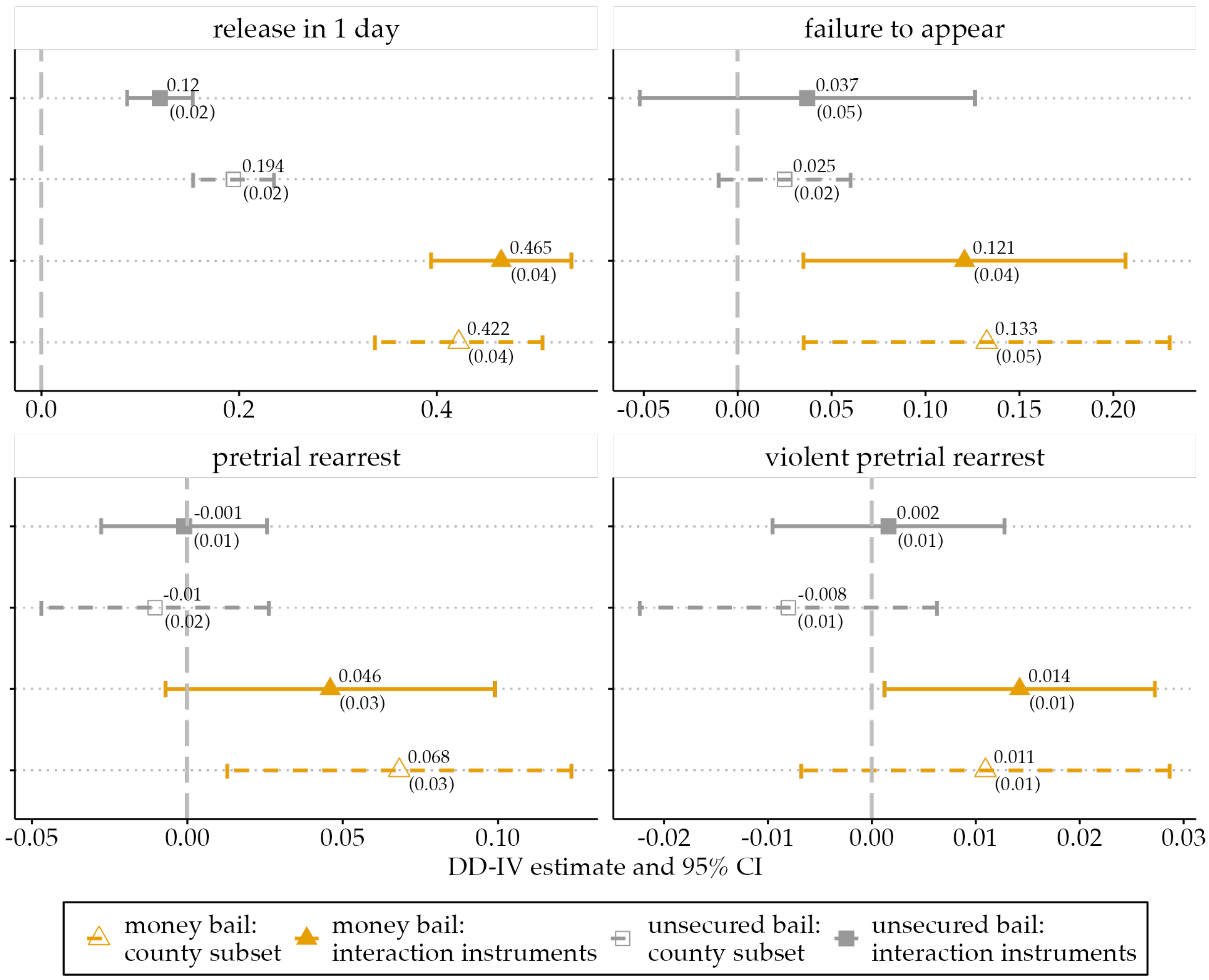
For each sample ("money bail counties" and "unsecured bail counties"), I use instrumented differences-in-differences and instrument for release on recognizance with AR coverage. This method is equivalent to estimating reduced form effects on misconduct in each subsample and then rescaling by the corresponding first-stage effects on release on recognizance in each subsample. The assumption underlying this approach is that in the money bail counties, the instrumented effects are driven by the substitution from money bail to release on recognizance, and in the unsecured bail counties, the effects are driven by the substitution from unsecured bail to release on recognizance.

Method 2: In the second approach, I leverage the full range of county variation rather than estimating results in particular subsamples. I follow generalizable methods from Kline and Walters (2016) to estimate treatment effects in the face of multiple alternatives. Using this approach, I generate a series of instruments by interacting AR coverage and county indicators, and use two-stage least squares estimation treating release on recognizance and unsecured bail as two separate endogenous variables. Appendix A10 describes the details of this approach. The central assumption is a constant effects assumption: counterfactual-specific effects should be similar across counties.

Figure 8 demonstrates the estimated effects of removing bail conditions on four binary outcomes of interest: pretrial release in one day, failure to appear in court, pretrial rearrest, and pretrial violent rearrest. Effects are estimated for two types of bail conditions – money bail (shown with orange triangles) and unsecured bail (shown with gray squares) – and using two estimation methods – county subsets (transparent shapes and dashed lines) and county interaction instruments (regular lines and shapes). The county interaction instruments approach is my preferred methodology because it leverages the full spectrum of variation rather than relying on ad hoc thresholds. Consequently, the following discussion centers on these estimates, though I include the county subset results in Figure 8 to provide additional context and robustness validation.

I find that eliminating money bail has large effects on one-day release. Reducing the money

Figure 8: Causal Effects of Removing Bail Conditions



Notes: I demonstrate the estimates of the effects of eliminating money bail and eliminating unsecured bail on four binary outcomes of interest: pretrial release in one day, failure to appear in court, pretrial rearrest, and violent pretrial rearrest. The plot is separated into the four outcomes of interest. I demonstrate effects across two bail types, money bail and unsecured bail, and two methods, using subgroups and using continuous county variation. Money bail estimates are in orange and shown with triangles. Unsecured bail estimates are in gray and shown with squares. Estimates from the subgroup method are shown with transparent shapes and dashed lines. Confidence intervals are at the 95% level. Standard errors are clustered at the county-level.

bail rate by 10 percentage points in favor of release on recognizance increases one-day release by 4.7 percentage points. In other words, about 47% of the impacted group is released in one day as a result, in spite of the fact that the money bail amounts required for release at baseline are usually in the hundreds of dollars (see Figure 4c). This estimation of counterfactual-specific treatment effects sharpens the previous reduced form evidence to show that moderate money bail amounts keep people in jail. Longer jail stays serve as direct evidence of binding liquidity constraints, just as default can be studied as a measure of financial distress (Mello, 2024).

I also show that eliminating money bail has larger effects on pretrial release than does eliminating unsecured bail. Reducing the unsecured bail rate by 10 percentage points in favor of release on recognizance increases speedy release by 1.2 percentage points. The AR program made release speedier even when money was not required ex ante, because the program bypassed judges for eligible cases. However, the fact that the release effects for money bail elimination are about four times as large as those for unsecured bail elimination clearly demonstrates that AR did not improve pretrial release outcomes *only* by bypassing judges – rather, reducing money bail rates was a crucial channel.

Figure 8 also shows results on misconduct, revealing a few key takeaways. First, across all misconduct measures, eliminating money bail yields effects that are larger in magnitude than eliminating unsecured bail. The heterogeneity in misconduct effects between eliminating 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), which Section 5.2 and Appendix A8 showed is unlikely. Therefore, it is likely that ex ante payment binds behavior more than the threat of financial collection.

Moreover, the effects of eliminating unsecured bail on all types of misconduct are weak. None of the estimates are statistically significant, and their magnitudes are small – sometimes even negative. Cutting unsecured bail rates by 10 percentage points yields increases of 0.3, -0.01, and 0.02 percentage points in court non-appearance, pretrial rearrest, and violent pretrial rearrest, respectively. These weak effects suggest defendants perceive a low probability of financial collection; this outcome aligns with recent evidence on low collection rates for justice system obligations from Giles (2023) and Pager et al. (2022). Unlike unsecured bail, money bail requires upfront payment, giving courts control over funds potentially forfeited upon misconduct. The minimal impact of eliminating unsecured bail in this context indicates that financial penalty threats may not effectively reduce pretrial misconduct (and may simply impose additional court debt), as is consistent with recent studies showing null effects of criminal justice financial obligations on recidivism or employment (Finlay et al., 2024; Pager et al., 2022).

Since unsecured bail has minimal effects on misconduct, I find that AR mainly impacted misconduct by eliminating money bail. I find significant effects of money bail elimination on court non-appearance, but results of mixed significance for rearrest and violent rearrest, depending on estimation method.[34] In terms of magnitudes, the results on court non-appearance are larger than results on pretrial rearrest (or violent pretrial rearrest) in both percentage point and percentage terms. This suggests that more minor forms of misconduct (court non-appearance) are more responsive to money bail than more serious misconduct (pretrial rearrest).

Finally, I use my estimates from Figure 8 to compare the trade-off induced by eliminating money bail in this setting. I find a decrease of 10 percentage points in the money bail rate induces a 4.7 percentage points increase in one-day release but a 1.7 percentage point increase in the rate of misconduct; most (1.2 percentage points) of that misconduct effect comes from failure to appear. Another way to interpret the results is that 17% of people assigned money bail have better conduct ensured as a result of the requirement, but 47% of people assigned money bail experienced additional jail time as a result of the requirement. In other words, for every one person who has better conduct as a result, about three people experience longer jail stays. If pretrial rearrest is the primary object of policy interest, then 4.6% of people assigned money bail have better conduct ensured as a result, but 47% of people experience additional jail time as a result. For each additional pretrial rearrest that comes from money bail elimination, about 10 people experience longer jail stays.

These results are informative on the release-misconduct trade-off induced by reducing money bail use in the US. Since release on recognizance 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 or electronic monitoring, as studied by Rivera (2024)). Non-monetary interventions can positively impact the behavior of people in the criminal legal system,[35] so pairing money

[34] In Section 5.2, when I estimated the effects of release on recognizance, misconduct effects were smaller in magnitude. This is to be expected because those release on recognizance effects were diluted versions of the money bail elimination effects (diluted by the smaller unsecured bail elimination effects). Recall that $LATE_r^Y = (0.408)LATE_{mr}^Y + (0.592)LATE_{ur}^Y$, so because $LATE_{ur}^Y$ is near zero, then it must be the case that $LATE_{mr}^Y >> LATE_r^Y$.

[35] For instance, court reminders have meaningful effects on court appearance (Emanuel and Ho, 2024;

bail reduction with non-monetary interventions could yield smaller misconduct increases.

6 Conclusion

This paper studies the causal effects of money bail reform. I study a unique program in Kentucky, Administrative Release (AR), which automatically released and eliminated financial bail conditions for a group of low-level arrestees. The program reduced pretrial detention and – to a lesser extent – increased pretrial misconduct. For each additional missed court appearance, the program reduced pretrial detention by 26 days across the eligible group. Fiscal estimates from prior research suggest that these changes create net cost savings for the pretrial system.

Beyond the reduced form evidence, I leverage variation in bail setting before the program to estimate the effects of distinct bail conditions. My findings reveal that eliminating money bail drives most program impacts, while unsecured bail has limited deterrent effects – possibly because of practical challenges in collection that weaken its financial incentives. Money bail has large effects on detention – about 50% of people spared money bail by the program are released in one day and would not have been otherwise – and smaller effects on pretrial misconduct. For each additional pretrial rearrest that comes from money bail elimination, I find that about 10 people experience longer jail stays.

This paper makes three contributions to the literature. First, I provide evidence on a non-marginal bail reform, showing that previous evidence from discretionary reforms does not necessarily generalize to more binding environments. Second, I contribute to our understanding of justice system financial obligations by showing that the threat of future financial sanctions (i.e., unsecured bail) has minimal effects. Lastly, I contribute new evidence on financial fragility, by showing that the inability to quickly post money bail in the hundreds of dollars is common. Just as default is indicative of financial fragility, so too is remaining in jail when faced with money bail requirements.

This paper is consistently framed around bail's stated legal objective: to set the least restrictive conditions to ensure appearance at court and public safety (American Bar Association Criminal Justice Standards Committee, 2007). While bail's legal purpose is to prevent pretrial misconduct, recent evidence suggests it may be used for different ends. In particular, money bail may be used as a debt collection mechanism in the face of low collection rates. Giles (2023) found that nearly 50% of new fee revenue after a Milwaukee

Fishbane et al., 2020).

reform came from the automatic application of posted cash bail. Future research should consider the interplay between the pretrial bail system and court system funding.

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Appendix

A1 Administrative Release Program Details

A1.1 Changes to AR Eligibility Rules over Time

Risk Scores: From November 2015 onward, a Public Safety Assessment Composite score of 2-7 was required for AR eligibility (Supreme Court of Kentucky, 2015). This changed in December 2017, after which eligibility was no longer based on the composite scores but on underlying score levels (Supreme Court of Kentucky, 2017b).

Charges: The November 2015 order, which included the 2-7 risk score eligibility, listed following charge conditions for AR eligibility (Supreme Court of Kentucky, 2015):

- The charges are non-sexual/non-violent misdemeanors/violations.
- The arrested person has not previously failed to appear on the charge.
- The arrested person accepts pretrial services interview.
- The arrested person does not face any of the following 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, or (3) DUI on a suspended license.

In December 2016, two additional charges were added that rendered someone ineligible for AR (Supreme Court of Kentucky, 2016): (1) violation of a protective order, and (2) bail jumping charges.

In January 2017, the program mandated that pretrial officers base their review on the UOR code assigned by law enforcement (Supreme Court of Kentucky, 2017a). Previously, they had based their review on the actual charge in the narrative/criminal record.

Other: In December 2016, a new Supreme Court Order noted that pretrial officers can obtain approval from Pretrial Services executive officer (or designee) to present an arrested person for judicial review.

A1.2 Public Safety Assessment Risk Scores

Figure A1 shows how calculation of the Composite Public Safety Assessment (PSA) score works. First, raw Failure to Appear (FTA) and New Criminal Activity (NCA) scores are calculated from risk factors related to the arrested person's charge, criminal history, and age. Second, raw scores are converted into scaled scores. Finally, the two scaled scores are added together to generate the composite risk score.

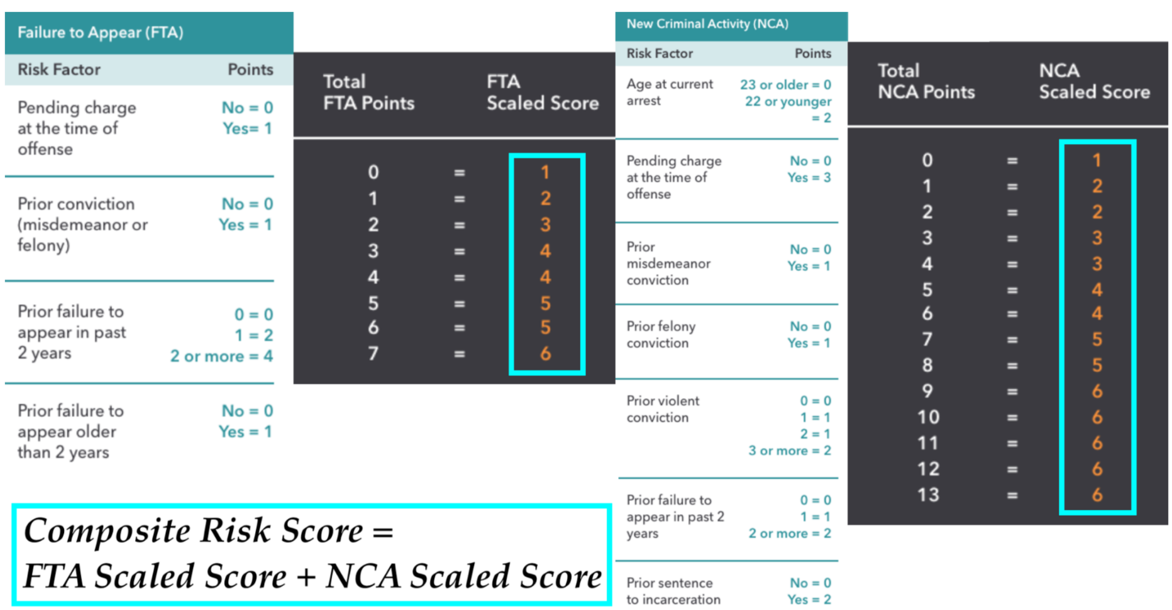
The raw FTA score is calculated based on

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

The raw NCA score is calculated based on

- whether the arrested person is 23 or older,
- whether the arrested person has a pending charge at the time of offense,
- whether the arrested person has a prior misdemeanor conviction,
- whether the arrested person has a prior felony conviction,
- whether the arrested person has a prior violent conviction,
- how many times the arrested person has failed to appear in the past 2 years,
- whether the arrested person has previously been sentenced to incarceration.

Figure A1: Risk Score Calculation Methodology



Notes: This figure demonstrates how the Composite Public Safety Assessment Score is calculated. Defendant and cases characteristics (called risk factors) are associated with a set number of points. Those points are added up to produce raw scores for two different types of risks. Those raw scores are converted into scaled scores and then added together to produce the composite score.

A1.3 Context on Criminal Warrants

If someone is arrested on a warrant with a bail amount already set by a judge, pretrial officers cannot release the person on AR, and the case must go to a judge. However, in the administrative data, I cannot observe if an arrest is a consequence of a criminal warrant.[36]

What does it mean for an arrest to be the consequence of a criminal warrant? 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 so. The judge makes the decision to issue a warrant or a summons (and sometimes they list a bail amount for said warrant).

In an informal interview, a court administrator estimated criminal warrant arrests could compromise 10% of all arrests. That said, they did not have an estimate as to what percentages of arrests may occur under warrants that also included listed bail amounts.

[36]According to court staff, AR eligible charges that are "common circumstances" for criminal warrant arrests include theft and harassment. So, this could be more of an issue for those charge categories.

A2 Evidence on Threats to Identification

My key differences-in-differences approach may be misleading if the composition of cases changed discontinuously at the time of program take-up. In this appendix, I test if the AR program impacted total arrests or eligibility determination.

To do so, I take advantage of the staggered timing of the AR program to estimate program effects on these outcomes. Specifically, I generate a balanced panel of all 99 counties over 14 quarters (relative to county-specific AR adoption dates) that includes averages for the outcomes of interest for each county-quarter. I use the methodology developed by Callaway and Sant'Anna (2021) to compare groups of counties with AR with other counties that have not yet adopted AR. I generate group-time treatment effect estimates for each set of counties that adopt AR in the same quarter and aggregate these effects into event-study estimates, which are average treatment effects at different lengths of time since exposure to treatment.

Figure A2 plots these estimates from 6 quarters before AR adoption to 3 quarters after AR adoption. The underlying identifying assumption is that outcomes in treated counties and not-yet-treated counties would have evolved similarly in the absence of the AR program.

(1) Change in Arrests: Figure A2a shows the results on overall arrests. There is no evidence that the program changed the number of arrests, which alleviates concern about this potential threat to identification. This result is also important beyond its relevance to identification. While this paper's main results focus on effects on pretrial detention and misconduct (American Bar Association Criminal Justice Standards Committee, 2007), other effects of bail reform may be relevant to the public debate of its efficacy – effects on overall offending are one salient example. I do not find evidence that offending (as proxied by total arrests) changed because of more lenient bail conditions.[37] There may be limited effects on overall crime of bail reform programs that target people arrested on low-level offenses.

(2) Change in Eligibility Stringency: The next possible confounding factor to address is manipulation of eligibility. Eligibility is determined by the actions of police officers and pretrial officers. While arrest type is mechanically determined, charge code assignment (which also factors into AR eligibility) is at the discretion of police officers. Therefore,

[37] An empirical concern is that these null arrest effects are due to decreased propensity to arrest offsetting a true increase in offending. If this were the case, we would expect to see the proportion of cases with eligible charges decrease (because police are opting to not arrest for low-level offenses). However, Figure A2b shows that this proportion does not change after AR implementation. This allays concerns about changes to discretionary arrest rates.

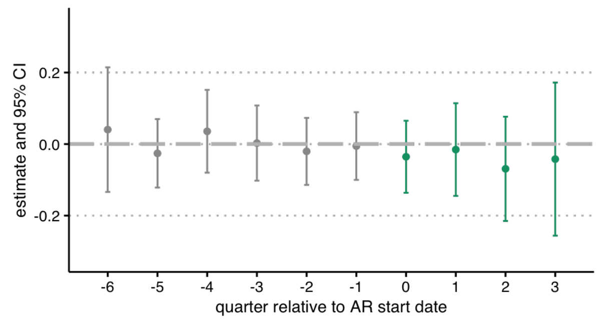
charges could be purposefully chosen to shift people in or out of program eligibility. To test this possibility, I estimate the effects of the program on the share of cases with eligible charge types. Figure A2b shows that there is no clear evidence of AR impacting charge types.

After police assign charges, pretrial officers input items that are used in the risk assessment score calculation. Since score inputs are not automatically filled in by the court system, it is possible pretrial officers could purposefully alter inputs and scores to impact program eligibility.[38] To test this, I estimate the effects of the program on the share of cases with eligible charges that have eligible risk scores. I focus on cases with eligible charges because the score manipulation impacts inclusion in the program only if the charges are eligible. Figure A2c shows the percentage is stable around the policy change. Overall, there is no strong evidence of strategic actions to manipulate eligibility by police or pretrial officers around the policy date.

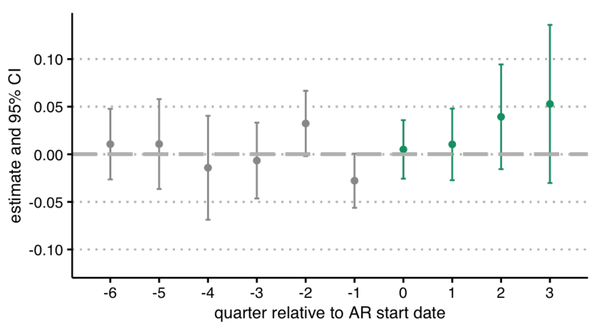
[38] However, owing to institutional details, this seems unlikely. Pretrial officers report to supervisors who can review their risk assessment accuracy, which means there are strong incentives to accurately scoring cases.

Figure A2: Testing for Threats to Identification

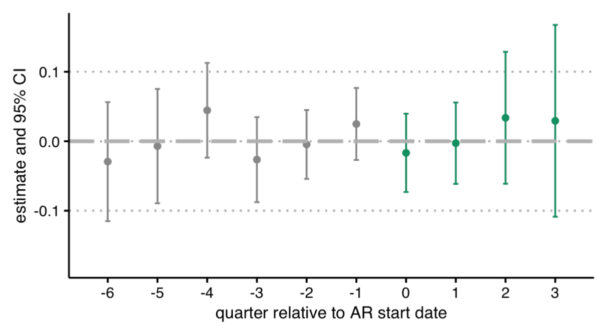
(a) Asinh(number of arrests)



(b) Fraction of cases with eligible charges



(c) Fraction of eligible charge cases with eligible risk scores



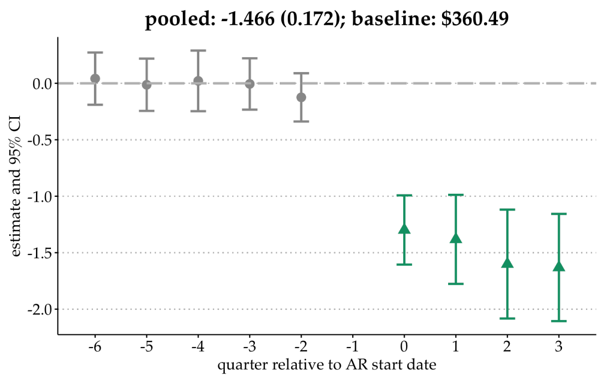
Notes: All figures are event-study differences-in-differences plots using Callaway and Sant'Anna's (2021) methods. In Figure A2a, the dependent variable is the inverse hyperbolic sine of the number of arrests in county-quarter. In Figure A2b, the dependent variable is the fraction of all cases with eligible charges. In Figure A2c, the dependent variable is the fraction of charge-eligible cases that have eligible risk scores.

A3 AR Effects on Additional Outcome Variables

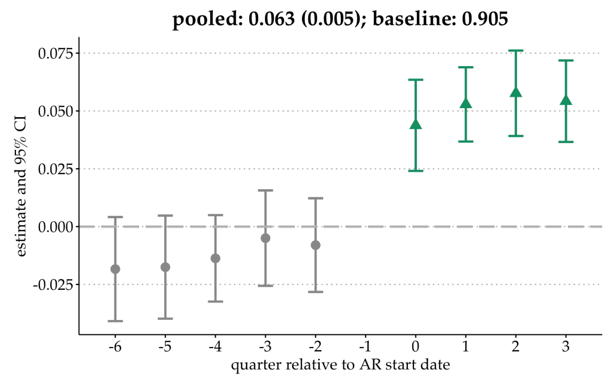
I provide results on AR program effects for additional bail and release outcomes in Figure A3: inverse hyperbolic sine of money bail amount (0 if no money bail), release in 3 days, release before disposition, and inverse hyperbolic sine of detention hours.[39]

Figure A3: How AR Impacts Bail and Release (More Outcomes)

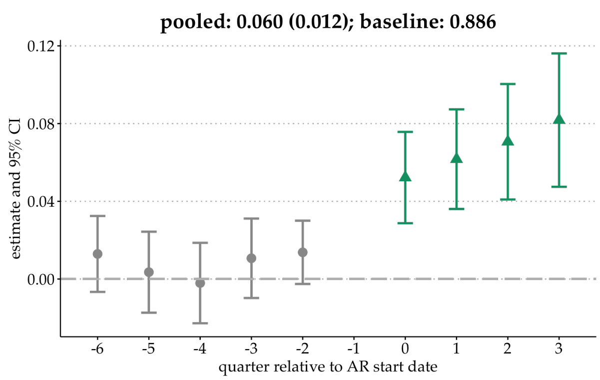
(a) Asinh(Dollars for Release)



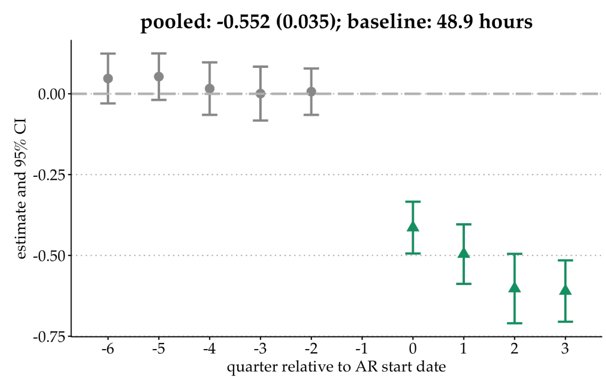
(b) Release in 3 Days



(c) Release Before Disposition



(d) Asinh(Hours in Detention)



Notes: Figures A3a, A3b, A3c, and A3d plot the event-time differences-in-differences estimates using methods described in Section 3.2. The outcome variable for Figure A3a is the inverse hyperbolic of the money bail amount in dollars (0 if no money bail). The outcome variable for Figure A3b is an indicator for release within 3 days of booking. The outcome variable for Figure A3c is an indicator for release before case disposition. The outcome variable for Figure A3d 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.

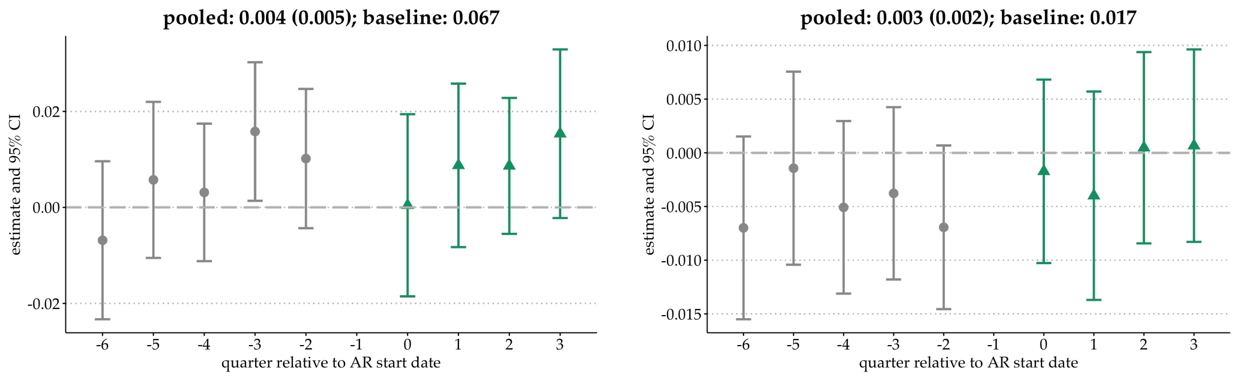
[39] I use the inverse hyperbolic sine transformation since these distributions are right-skewed and include zeros. The inverse hyperbolic sine is defined as follows: $asinh(x) = ln(x + (x^2 + 1)^{1/2})$. To interpret coefficients in an asinh-linear equation with dummy variables, Bellemare and Wichman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

I provide results for misconduct outcomes for pretrial non-violent arrest and pretrial violent arrest in Figure A4.

Figure A4: How AR Impacts Misconduct (More Outcomes)

(a) Pretrial Non-Violent Rearrest

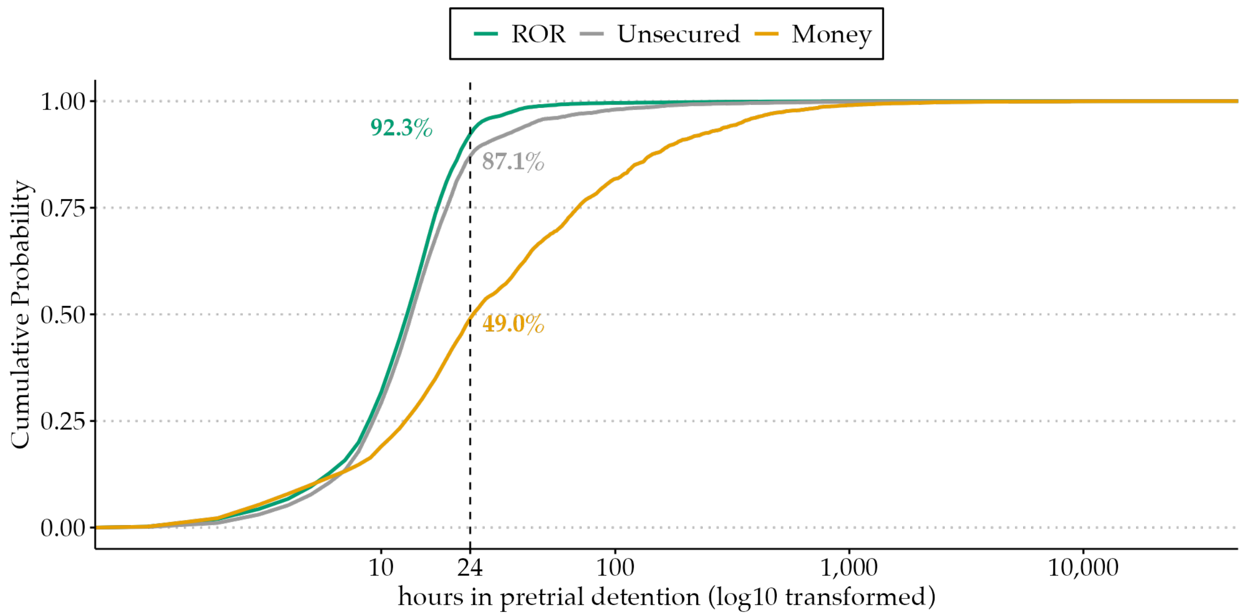
(b) Pretrial Violent Rearrest



Notes: Figure A4a and A4b plot the event-time differences-in-differences estimates using methods described in Section 3.2. The outcome variable for Figure A4a is an indicator for pretrial non-violent rearrest. The outcome variable for Figure A4b 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.

A4 Pretrial Detention and Bail Conditions

Figure A5: Cumulative Density of Pretrial Detention Hours Across Bail Types



Notes: This figure plots the cumulative density function of hours in pretrial detention across three distinct bail types: ROR (green line), unsecured bail (gray line), and money bail (orange line). The sample is AR eligible cases in the pre-AR time period. The CDF shows the percentage of cases that are detained for less than a certain number of hours. The x-axis is log10 transformed. The dashed line shows CDF value for each bail type for 24 hours of pretrial detention.

A5 Interpreting Relative Trade-Offs

A5.1 Trade-Offs in Terms of Fiscal Costs

To inform interpretation, I first consider whether the AR program saved the courts money. To do this, I use estimates of the fiscal costs of pretrial detention and misconduct from Kentucky reports and the prior academic literature.

Kentucky's Department of Corrections publishes an annual report with a cost of incarceration per day. In 2016-2017, the cost per defendant at a county jail without state inmates was $31.45, and the cost with state inmates was $40.45 (Kentucky Department of Corrections, 2017). I use the midpoint of these two – $35.95 – as the cost of jailing someone for one day pretrial.[40]

Compared with quantifying other court-related costs, quantifying the fiscal impact of failure to appear presents unique challenges. Whereas Kentucky's Department of Corrections reports incarceration expenses annually, there are no analogous reports documenting the costs of failure to appear. In the context of cases eligible for AR, which involve low-level offenses, the costs do not include re-apprehension expenses. The primary fiscal burden comes instead from court administration costs (more hearings may be needed). The closest related estimate available is from Bierie (2007), who estimates the cost of a minor court hearing at $560. However, this figure may overestimate the actual cost in cases where matters can be dismissed or adjudicated without the defendant's presence.[41]

Translating my reduced form estimates into annual changes implies that AR yielded around 9,187 fewer detention days and 385 more instances of court non-appearance. Plugging in the relevant cost estimates from Kentucky Department of Corrections (2017) and Bierie (2007) yields $330,273 saved in terms of detention and $215,600 lost in terms of court non-appearance. These figures suggest the program saves Kentucky's pretrial system costs.

In order for the status quo to be preferable over AR in terms of *overall* social costs, the non-fiscal costs of failure to appear would need to outweigh the equivalent costs of pretrial

[40] This estimate captures only the direct fiscal costs to the justice system and does not capture the costs of pretrial detention to defendants (in terms of the increased convictions and decreased employment and government benefits) (Leslie and Pope, 2017; Dobbie et al., 2018). Therefore, this is necessarily an underestimate of the overall social costs of pretrial detention.

[41] Relatedly, Graef et al. (2023) find that non-defendant failure to appear is more common than defendant failure to appear. It is worth noting that failure to appear is a systemic issue in the justice system beyond defendant behavior.

detention, which include the myriad harms pretrial detention imposes on detainee's legal and labor market outcomes.

A5.2 Trade-Offs in Terms of Surveyed Preferences

Another way to interpret the magnitude of the trade-offs between pretrial detention and misconduct is by asking, *Is this trade-off desirable according to reported preferences*? To my knowledge, there is no documented survey evidence on voters' preferences between different quantities of pretrial detention and misconduct. However, recent work by Stevenson and Mayson (2022) surveyed respondents on their preferences over hypothetically experiencing violent crime victimization or incarceration. They find that the median respondent finds three days in detention as costly as suffering a robbery, an example of a violent crime.

In the AR case, I find (using noisy point estimates) that for one more violent pretrial rearrest, the population experiences 273 fewer detention days. If I interpret Stevenson and Mayson's (2022) survey evidence as defining an indifference curve between two non-market "bads" (one violent crime and three days of detention), then the AR program shifts society to a preferred allocation of two non-market "bads." This is because the measured AR trade-off (273 detention days for each violent crime) is less steep than the survey reported trade-off (3 detention days for each violent crime). Even if I take the upper-bound of my 95% confidence interval, the implied trade-off is still much less steep than the one derived from Stevenson and Mayson's (2022) survey.

However, it is worth noting that the survey in Stevenson and Mayson (2022) asks people about their preferences assuming they (the respondent) would be the person hypothetically detained as well as the person hypothetically victimized. In practice, voter preferences are likely informed by whether they perceive themselves or their networks to be impacted by policies that may change detention or misconduct rates. In other words, perceived exposure to victimization and detention likely creates a wedge between the responses to this survey and the preferences people have in practice in terms of justice system severity.

A6 How Did AR Impact Racial and Socioeconomic Gaps?

While the main results in this paper focus on overall effects on detention and misconduct, other effects are relevant to the bail reform policy discussion. The money bail system can have disparate impacts on disadvantaged groups, such as Black people or people who are low income. Bail reform therefore often seeks to alleviate the inequalities that money bail generates. To contribute to the work on reforms and social inequality, I investigate how the AR program impacts racial and socioeconomic gaps in bail and detention.

The standard administrative court data include information on arrested peoples' race but not information on socioeconomic status. To acquire data on socioeconomic status, I requested data on arrested peoples' employment status through special authorization from the Kentucky State Supreme Court (Supreme Court of Kentucky, 2021). Employment status data was collected on interview forms during pretrial interviews or subsequent contacts between Pretrial Services and people.

I use Figure A6 to show how racial gaps in money bail and pretrial release evolve over time for different cases. While the Black-white gaps stay stable for the population of ineligible cases, the gaps shrink for the eligible cases (those impacted by AR). The gap in money bail assignment dropped from 10 to 2 percentage points, and the pretrial release gap shrank from 5.6 to 1.2 percentage points.

However, if I calculate how the gaps change in the full population, the effects are muted. First, this is because only 20% of cases are eligible for the program. Moreover, white people are slightly more likely to be program eligible (21.5% vs. 19.1%) than Black people. This is because Black people are slightly less likely to be arrested for eligible charges (32.6% vs. 35.3%) and less likely to have low enough risk scores (73.7% vs. 75.5%).

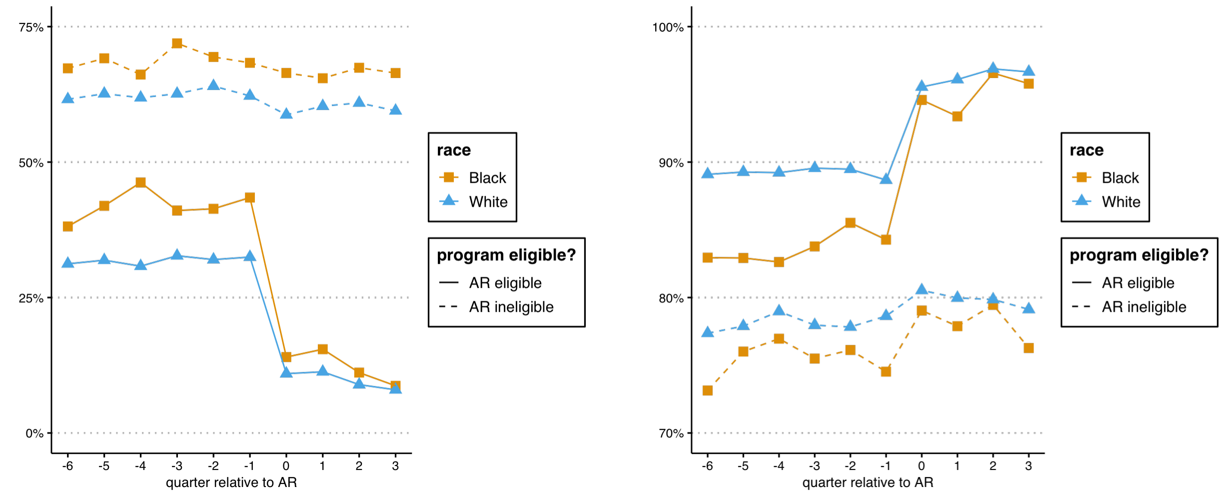
In Figure A7, I turn to socioeconomic gaps in bail and release. Specifically, I plot how gaps between employed and unemployed people evolve over time for eligible and ineligible cases. The gaps remained stable for ineligible cases but shrank dramatically for eligible cases. The gap in money bail assignment decreased from 8.4 to 0.7 percentage points, while the release gap decreased from 12 to 3.5 percentage points. Notably, before the adoption of AR, pretrial release for program-eligible unemployed people was lower than for program-ineligible employed people. This was no longer the case after AR.

As was the case with the racial gaps, the program's effects on gaps are muted in the full population. Again, this is because only 20% of cases are eligible and because employed people are more likely to be program eligible (20.6% vs. 18.1%). Unemployed people's

Figure A6: How AR Impacted Black-White Pretrial Gaps

(a) Percent money bail

(b) Percent pretrial release



Notes: Both figures are binned scatter plots grouped by quarters relative to AR adoption date. Figure A6a shows the percentage of cases that are assigned money bail, and Figure A6b shows the percentage of cases that result in pretrial release. Blue lines with triangles show the rates for white people, while orange lines with squares show the rates for Black people. Solid lines are eligible cases, while dotted lines are ineligible cases.

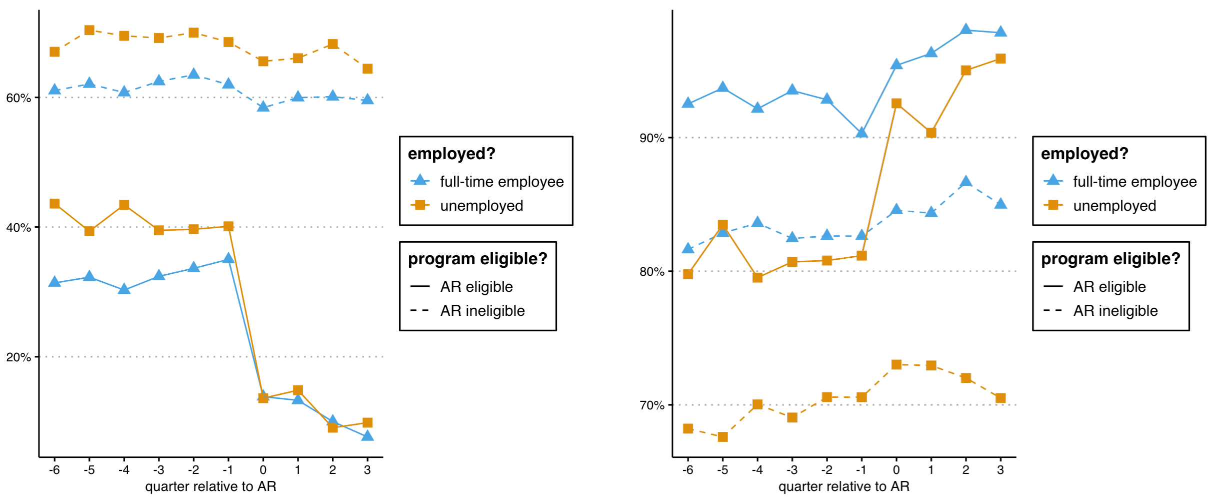
higher likelihood of being arrested for repeat offenses and having higher risk scores drives this result.

Overall, these results demonstrate that automatic release programs can close racial and socioeconomic gaps for eligible populations. However, these changes might not translate to the full population because of program size and differences in program eligibility across groups.

Figure A7: How AR Impacted Unemployed-Employed Pretrial Gaps

(a) Percent money bail

(b) Percent pretrial release



Notes: Both figures are binned scatter plots grouped by quarters relative to AR adoption date. Figure A6a shows the percentage of cases that are assigned money bail, and Figure A6b shows the percentage of cases that result in pretrial release. Blue lines with triangles show the rates for people who are employed full time, while orange lines with squares show the rates for unemployed people. Solid lines are eligible cases, while dotted lines are ineligible cases.

A7 Did Judges Set ROR for Less Risky Cases Before AR?

I use the potential outcomes framework outlined in Section 5.1 to provide descriptive evidence on whether judges assigned release on recognizance for less risky cases before AR. The objective is to compare failure to appear rates for always takers (cases assigned release on recognizance by judges before AR) with the implied rates for compliers (cases assigned release on recognizance after AR only due to the program).

The rate of failure to appear for cases released on recognizance before AR was 10.5%. After AR, the rate for cases released on recognizance is higher, at 17.3%. However, the cases in this group are different before and after AR. After AR, cases released on recognizance include always takers (cases that would have gotten AR before regardless) and compliers (cases that only get release on recognizance because of the reform).

Assuming the always takers behave the same way before and after AR, I can solve for the implied failure to appear rate for compliers (call this rate x). The new FTA rate for the group getting ROR should be a weighted average between the rate for the complier group and the always taker group:

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

In this case, $P(u, a)$ is the share of people granted release on recognizance who are always takers, and $P(u, c)$ is the share of people granted release on recognizance who are compliers. Since 19.6% of the eligible population received release on recognizance before AR and 75% received release on recognizance after, then $P(u, a) = 0.261$ and $P(u, c) = 0.739$. Plugging these shares into the above equation and solving for x yields $x = 0.197$. Therefore, the compliers' resulting failure to appear rate is 19.7%. This implies that the compliers are about twice as risky with respect to failure to appear than the always takers.

Thus, the always takers were correctly identified as less risky by judges even within the eligible case group.

A8 Bounding Incapacitation and Deterrence Effects

Pretrial misconduct is only possible if people are out of pretrial detention before case disposition (case conclusion). Before the AR program, 88.6% of eligible people were released before disposition ($R_{i0} = 1$). Those 88.6% of eligible people were split into two groups: 19.6% received release on recognizance ($B_{i0} = r$), while the remaining 69% did not ($B_{i0} \neq r$) and received unsecured or money bail conditions instead. I can write the failure to appear (FTA) rate in the pre-period (FTA_0) as a weighted average of FTA rates across these two mutually exclusive groups:

$$FTA_0 = 0.196(FTA_0|B_{i0} = r, R_{i0} = 1) + 0.69(FTA_0|B_{i0} \neq r, R_{i0} = 1).$$

Given the monotonicity assumption in the DD-IV approach, there should be no defiers (no switches away from release on recognizance because of AR). This means I can characterize the first group by their potential outcomes in the post-period too:

$$FTA_0 = 0.196(FTA_0|(B_{i0} = r, B_{i1} = r),(R_{i0} = 1, R_{i1} = 1))$$

$$+0.69(FTA_0|B_{i0} \neq r, R_{i0} = 1).$$

Meanwhile, the latter group is cases that are released but not given release on recognizance before AR. If we assume an additional monotonicity assumption with respect to release (AR should not induce detention conditional on the same bail condition), then this group can be split into two subgroups based on potential outcomes in the post-period:

1. compliers who are always released: $(B_{i0} \neq r, B_{i1} = r),(R_{i0} = 1, R_{i1} = 1))$.
2. never takers who are always released: $(B_{i0} \neq r, B_{i1} \neq r),(R_{i0} = 1, R_{i1} = 1))$.

Group (1) is compliers with respect to release on recognizance, but always takers with respect to release. Group (2) is never takers with respect to release on recognizance, but always takers with respect to release.

Plugging in empirically observable shares, I can then write out the pre-period FTA rate as

$$FTA_0 = 0.196(FTA_0|(B_{i0} = r, B_{i1} = r),(R_{i0} = 1, R_{i1} = 1))$$

$$+0.47(FTA_0|(B_{i0} \neq r, B_{i1} = r),(R_{i0} = 1, R_{i1} = 1))$$

$$+0.216(FTA_0|(B_{i0} \neq r, B_{i1} \neq r),(R_{i0} = 1, R_{i1} = 1)).$$

After AR, the FTA rate can be written with a very similar expression but with (1) time indexed to after AR, and (2) a new term for the compliers who are newly released after AR – i.e., cases such that $(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 0, R_{i1} = 1))$. These cases were not relevant in the pre-period because they were unable to fail to appear due to incapacitation. The share of eligible cases released before disposition is 7.6 percentage points higher after AR, so I can write FTA after AR as follows:

$$FTA_1 = 0.196(FTA_1|(B_{i0} = r, B_{i1} = r), (R_{i0} = 1, R_{i1} = 1))$$

$$+0.47(FTA_1|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 1, R_{i1} = 1))$$

$$+0.216(FTA_1|(B_{i0} \neq r, B_{i1} \neq r), (R_{i0} = 1, R_{i1} = 1))$$

$$+0.076(FTA_1|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 0, R_{i1} = 1)).$$

I then make two logical assumptions. First, I assume that the always takers behave similarly before and after AR. This means $FTA_1 = FTA_0$ for cases such that $(B_{i0} = r, B_{i1} = r), (R_{i0} = 1, R_{i1} = 1)$. Second, I assume never takers who are always released also behave similarly before and after AR, which means $FTA_1 = FTA_0$ for cases such that $(B_{i0} \neq r, B_{i1} \neq r), (R_{i0} = 1, R_{i1} = 1)$. Given these two assumptions, I can write the difference between FTA before and after AR as

$$FTA_1 - FTA_0 = 0.47(FTA_1|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 1, R_{i1} = 1))$$

$$+0.076(FTA_1|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 0, R_{i1} = 1))$$

$$-0.47(FTA_0|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 1, R_{i1} = 1)).$$

I can simplify this down to the following expression:

$$FTA_1 - FTA_0 = 0.47((FTA_1 - FTA_0)|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 1, R_{i1} = 1))$$

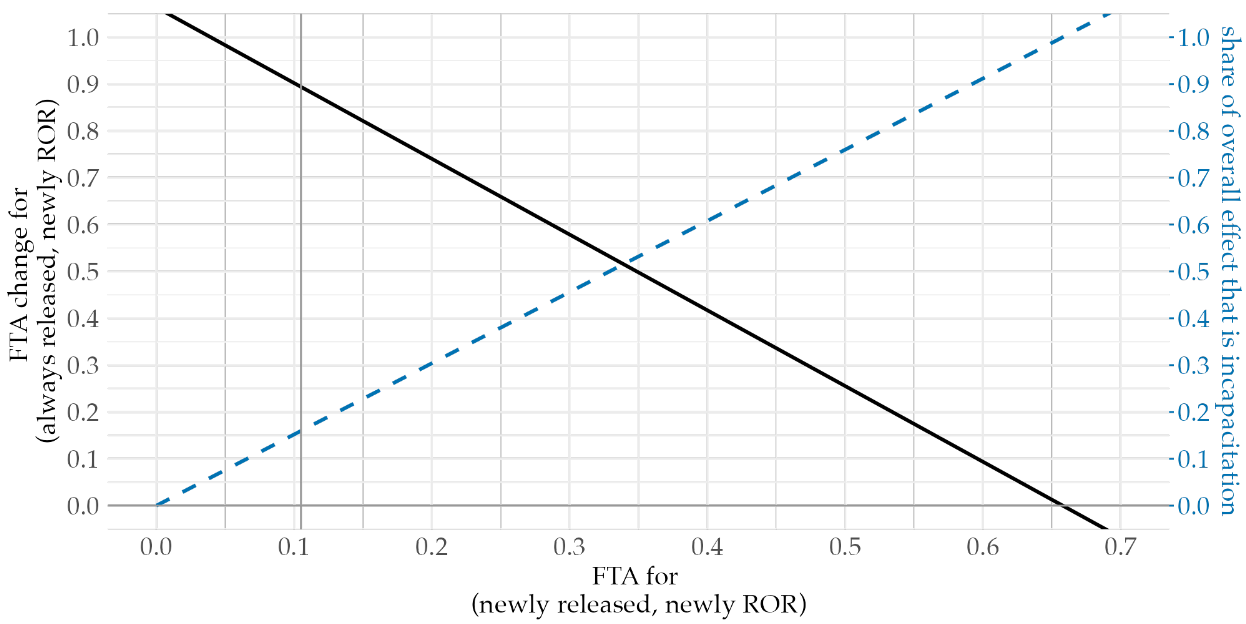
$$+0.076(FTA_1|(B_{i0} \neq r, B_{i1} = r), (R_{i0} = 0, R_{i1} = 1)).$$

In words, the change in FTA is attributable to two groups: those who were always released but are newly given release on recognizance, and (2) those who are newly released and newly given release on recognizance. The two pieces of the above expression for $FTA_1 - FTA_0$ map nicely onto deterrence and incapacitation effects, respectively. Since I know that $FTA_1 - FTA_0 = 0.05$, this bounds the possible incapacitation and deterrence effects.

However, there are still many combinations that satisfy the condition $0.47Y + 0.076X = 0.05$.

Using institutional knowledge, I expect that newly released compliers should be riskier than always takers, which is supported by the evidence in Appendix A7. Therefore, to narrow the range of incapacitation and deterrence effects in a logical way, I assume that $X \in [0.105, 1]$ and $Y \in [0, 1]$. Using this restriction, Figure A8 plots as a solid black line a range of possible values for X and Y that satisfy the necessary equation. Figure A8 also then plots the share of the misconduct effect that is due to incapacitation (with the remainder due to deterrence). This share is calculated as $\frac{0.076X}{0.05}$ and is represented as the dashed blue line.

Figure A8: Bounding Incapacitation and Deterrence Effects



Notes: This figure takes a range of hypothetical values for the failure to appear (FTA) rate for newly released compliers and then plots 2 consequent values: (1) the consequent change in the FTA rate for those who are always released but newly get release on recognizance (ROR), and (2) the consequent share of the overall pretrial misconduct effect that is due to incapacitation. For each value on the x-axis, the black solid line shows (1) the consequent change in the FTA rate for those who are always released but newly get release on recognizance (ROR). Meanwhile, for each value on the x-axis, the blue dashed line shows (2) the consequent share of the overall pretrial misconduct effect that is due to incapacitation.

Figure A8 makes a number of points clear. First, for incapacitation to be the sole source of the aggregate FTA effect, those newly released need to be about 7 times as risky as the always takers ($0.7 \approx 7 \times 0.105$), and the FTA change for the always released compliers also needs to be negative, both of which seem unlikely. Even if the newly released are more than three times as risky ($0.35 > 3 \times 0.105$), Figure A8 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 release on recognizance as never takers (0.105), then incapacitation is responsible for less than 20% of the aggregate FTA effect.

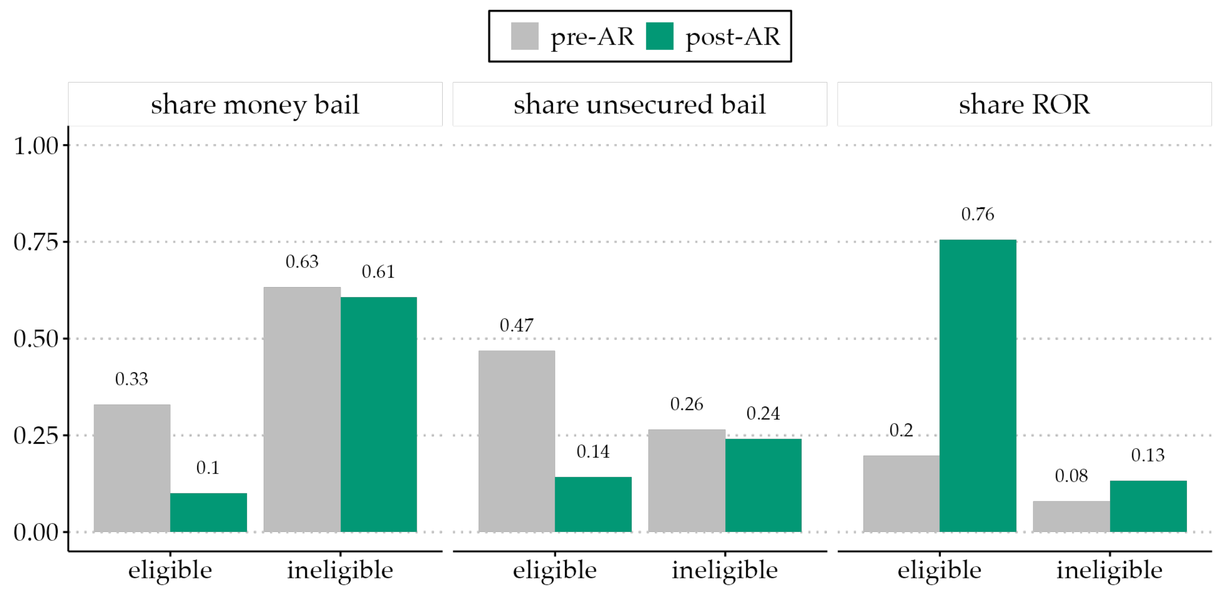
A9 Deriving Complier Shares from Bail Descriptives

Jäger et al. (2019) demonstrate how to estimate 3 group shares (always takers, never takers, compliers) in the context of a differences-in-differences potential outcomes framework (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 u and m subgroups.

Never takers: The never taker share is $P(B(1) \neq r | t_1, e_1)$.[42] Figure A9, generated with my data, shows that for t_1, e_1, $P(B(1) = r) = 0.76$, which means the never taker share is 0.24.

We know $P(B(1) \neq r | t_1, e_1) = P(B(1) = u | t_1, e_1) + P(B(1) = m | t_1, e_1)$. Figure A9 shows $P(B(1) = u | t_1, e_1) = 0.14$ and $P(B(1) = m | t_1, e_1) = 0.10$, so these are the u-never taker and m-never taker shares, respectively.

Figure A9: Bail Types over Subgroups



Notes: This figure demonstrates the shares of eligible and ineligible cases that receive different bail conditions (money bail, unsecured bail, release on recognizance) both before and after AR.

Always takers: The always taker share is $P(B(0) = r | t_1, e_0) + P(B(0) = r | t_0, e_1) - P(B(0) = r | t_0, e_0)$.[43] In words, the always taker share is the share of ineligible cases in the post-period receiving ROR plus the share of eligible cases in the pre-period receiving ROR minus the share of ineligible cases in the pre-period receiving ROR. Plugging in

[42] See (A15) from Jäger et al. (2019).
[43] See (A14) from Jäger et al. (2019).

descriptive statistics from Figure A9 yields $0.13 + 0.2 - 0.08 = 0.25$. Therefore, the always taker share is 0.25.

Compliers: The complier share is the share remaining after accounting for always takers and never takers. Therefore, the complier share is $1 - 0.25 - 0.24 = 0.51$.[44]

We know that the m-compliers and u-compliers shares will total to 0.51. Intuitively, substitution to release on recognizance must come from substitution away from unsecured and money bail (due to the extended monotonicity assumption). Therefore,

$$[P(B(1) = r|t_1, e_1) - P(B(0) = r|t_0, e_1)] - [P(B(0) = r|t_1, e_0) - P(B(0) = r|t_0, e_0)] =$$

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

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

Plugging in the relevant shares shown in Figure A9, we get that

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

The share of c-compliers is 0.31 and the share of m-compliers is 0.21.[45]

Summary: the shares over the relevant 5 groups are

- 14% u-never takers,
- 10% m-never takers,
- 25% always takers,
- 31% u-compliers,
- 21% m-compliers.

Therefore, about 60% of compliers are unsecured bail compliers ($60\% \approx \frac{31}{(31+21)}$), and about 40% of compliers are money bail compliers ($40\% \approx \frac{21}{(31+21)}$).

[44] Another way to compute this is as follows: $[P(B(1) = r|t_1, e_1) - P(B(0) = r|t_0, e_1)] - [P(B(0) = r|t_1, e_0) - P(B(0) = r|t_0, e_0)] = [0.76 - 0.2] - [0.13 - 0.08] = 0.51$.

[45] Because of rounding, these two sum to 0.52 instead of 0.51.

A10 Leveraging Continuous Variation Across Counties

Instead of simply estimating effects separately across two subgroups ("money bail counties" and "unsecured bail counties"), I can also estimate results using a fuller range of county variation. Following Kline and Walters (2016), I use two-stage least squares estimation treating release on recognizance 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 to take advantage of different pre-AR bail norms across counties. This is similar to interacting experimental program assignment with observed covariates or site indicators, as in Kling et al. (2007) and Abdulkadiroğlu et al. (2014). This approach relies on an assumption of constant effects, which means 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 3 and 4, but there are two distinct endogenous variables predicted in the first stage: release on recognizance 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_r^{DD-IV} (release on recognizance) and δ_u^{DD-IV} (unsecured).

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

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

Table A1 demonstrates two-stage least squares estimates of separate effects of release on recognizance 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 release on recognizance 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.

Table A1: Two-Stage Least Squares Estimates with County Interaction Instruments

	Release in 1 day (1)	Failure to appear (2)	Pretrial rearrest (3)
Release on recognizance	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 release on recognizance and unsecured bail. Release on recognizance 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)

For the sake of presentation and interpretation, I plot and present δ_r^{DD-IV} and $(\delta_r^{DD-IV} - \delta_u^{DD-IV})$ in the main text with Figure 8. I do this because these terms are the analogs of the treatment effects of interest: δ_r^{DD-IV} is the effect of switching from money bail to release on recognizance, and $(\delta_r^{DD-IV} - \delta_u^{DD-IV})$ is the effect of switching from unsecured bail to release on recognizance.