No Money Bail, No Problems?

Trade-offs in a Pretrial Automatic Release Program

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Abstract

Many jurisdictions across the United States are implementing bail reform programs to reduce the use of money bail. Bail reform opponents argue that money bail is critical for averting pretrial misconduct, while proponents counter that the effects are small and not worth the consequent costs of pretrial detention. I examine this detention-misconduct trade-off using a program in Kentucky that automatically released people arrested for low-level offenses – people who usually would have had financial conditions for release from jail. I find that the program reduced total annual time in pretrial detention by over 25 person-years with no detectable effect on pretrial rearrest. Meanwhile, the program increased the number of annual court non-appearances by about 364. This trade-off is desirable if 1 court non-appearance is less costly than 26 days in detention.

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1 Introduction

In the United States, 65% of the approximately 500,000 people in jails each day are unconvicted and are typically detained because of an inability to post money bail (Zeng and Minton 2021; Reaves 2013). This pretrial detention necessitates $15 billion of spending per year on jails and harms the legal, financial, and labor market outcomes of detained people (Wagner and Rabuy 2017).[1] Concerns about pretrial detention's scale and harms have fueled a recent wave of bail reform policies that seek to reduce the use of money bail.

Alongside bail reform comes public debate about its effects and whether reforms induce desirable trade-offs. Bail reform opponents argue money bail is necessary to avert high rates of pretrial misconduct, which includes failure to appear in court and rearrest for criminal offenses while awaiting case disposition. Meanwhile, bail reform advocates argue that money bail's effects on misconduct are small and not worth the consequent effects on detention.

What are the causal effects of narrowing the use of money bail? I study this question by leveraging administrative data on a major bail reform program in Kentucky. The Automatic Release (AR) program automatically released a group of people who, in the absence of the program, would have normally been assigned financial conditions for release. The AR program is well-suited for this research topic for a few reasons. For one, the program was automatic, making it a powerful setting for studying bail reform – the usage of financial conditions dropped by more than 50 percentage points for the eligible population. Second, some people were eligible for the program while others were not, which generates variation in treatment I can leverage to estimate causal effects. Third, the program-eligible population is people arrested for low-level offenses (i.e., non-sexual non-violent misdemeanors), which is the target population for many reforms that seek to narrow the scope of the criminal justice system. Therefore, estimates for this population are especially policy-relevant.

To estimate the effects of this program, I use administrative data from the entire state of Kentucky and leverage the program design for causal inference. Before the program was implemented, all arrested people had bail set by judges. After program implementation, program-eligible people were automatically released without financial conditions, but program-ineligible people still had bail set by judges. Therefore, I can use the eligible people as a treatment group and the ineligible people as a control group in a differences-in-differences approach. I can estimate program effects for 99 distinct counties in Kentucky, which adopted the program at different times between 2015 and 2017. To use as much of my sample as possible and provide aggregated estimates, I use a stacked differences-in-differences approach with data from all 99 counties.

I find that the program dramatically shifted bail conditions for eligible cases – the use of financial

[1] The $15 billion estimate is from Wagner and Rabuy (2017). Horowitz, Velázquez, and Clark-Moorman (2021) estimate that county spending on all jail expenses (related to both pretrial detention and post-conviction jail incarceration) at $25 billion. To put the $25 billion sum in context, 6% of county spending goes towards jails, 6% goes towards county roads, and 10% goes towards K-12 education (Horowitz, Velázquez, and Clark-Moorman 2021).

bail conditions decreased by over 50 percentage points and the amount of money required for release decreased by 77%. In levels, the average amount required for release declined from $360 to $83. As a result of the program, the rate of pretrial release increased by 13.7 percentage points and total hours in detention decreased by 42%. These results demonstrate that financial difficulty with posting a few hundred dollars can meaningfully constrain release, which is consistent with research demonstrating tight liquidity constraints on people in the criminal justice system (Mello 2021). The 42% decrease in detention hours translates into about 223,000 less hours – or 25.5 less years – of pretrial detention per year.

I also estimate the effects of the program on two distinct types of pretrial misconduct: failure to appear in court and pretrial rearrest. I find that the program increased failure to appear rates by 3.3 p.p. (relative to a baseline of 10.7%). This point estimate translates into an additional 364 court non-appearances per year. Effects on pretrial rearrest are insignificant and I can rule out effects larger than 1.68 p.p. at a 5% level of confidence. Focusing specifically on more serious rearrests (violent rearrests), effects are again insignificant and I can rule out effects larger than 0.6 p.p. at a 5% level of confidence. In a simplified cost-benefit exercise, the detention-misconduct trade-off generated by the program is desirable if 1 court non-appearance is less costly than 26 detention days.

The paper's main results focus on effects on pretrial detention and misconduct because the legal objective of the pretrial system is to minimize these two outcomes (American Bar Association Criminal Justice Standards Committee 2007). However, other effects of bail reform may be relevant to the public debate of its efficacy. For instance, does bail reform impact new offending? If bail is a large component of expected punishment for low-level offenses, then making bail more lenient could lead to increased offending. I leverage the staggered adoption of the automatic release program across Kentucky counties to estimate the effect of the policy on arrests. I do not find evidence that the reform increased offending, which suggests that financial bail has limited general deterrence effects on offending behavior.

Another impact of bail reform that is relevant to policy debate is its capacity to alleviate inequality in the criminal legal system. I investigate how the program impacts racial and socioeconomic gaps in bail and detention. I find the program dramatically reduced Black-white and employed-unemployed gaps in bail setting and pretrial detention for eligible people. However, the changes are very muted in the full population because, first, most cases were not program-eligible and, second, white or employed people were slightly more likely to be eligible than Black or unemployed people, respectively.

Finally, I explore the mechanisms that drive the main set of results on program effects. Namely, how much of the program effects are due to substitution away from money bail (as opposed to substitution away from other bail types)? To address this question, I first set up a potential outcomes framework with program coverage as an instrument for financial conditions in an instrumented differences-in-differences identification strategy.[2] This framework allows me to estimate the effects of financial conditions. I then must complicate the set-up to accommodate two distinct types of financial conditions: money bail, which requires payment for release from jail, and unsecured bail, which requires payment after release if a person commits misconduct. To separate out the effects of these two types of bail (relative to release with no financial conditions), I leverage pre-program variation in bail setting across counties, which is attributable to different norms across judges.[3]

Using instrumented differences-in-differences, I show 27.6% of those spared financial bail avoid spending one or more days in jail as a result. Meanwhile, 6.5% of those same people fail to appear in court as a result. (In other words, 93.5% of that population does not change their court appearance behavior due to imposed financial conditions.) While I cannot separate deterrence from incapacitation in this institutional setting, I use an accounting exercise to demonstrate the it is unlikely the results are solely attributable to incapacitation.[4]

Accommodating multiple treatments, I show that substitution away from money bail is responsible for most of the Kentucky program's effects on pretrial misconduct. In fact, substitution away from unsecured bail has small effects that are often indistinguishable from zero, suggesting that deterrent effects of future financial sanctions may be weak.

This paper studies a recent bail reform to provide empirical evidence on the effects of limiting the use of financial bail conditions. Most of the previous empirical evidence on the effects of bail uses judge leniency designs (Gupta, Hansman, and Frenchman 2016; Dobbie, Goldin, and Yang 2018).[5] However, treatment effects from judge leniency designs may differ from the true policy change treatment effects of interest for a few reasons (Rose and Shem-Tov 2021). For one, 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. Second, 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.[6]

[2]In addition to the parallel trends assumption required for differences-in-differences, two additional assumptions are required. First, the program can impact bail conditions only by inducing substitution to unconditional release (extended monotonicity). Second, the program can impact pretrial release and misconduct only through changes in bail conditions (exclusion restriction).

[3]First, I estimate instrumented differences-in-differences results for two subsamples – counties that rarely used money bail and counties that rarely used unsecured bail – to focus on substitution from each alternative individually. Second, I treat unconditional release and unsecured bail as two distinct endogenous variables and instrument for them using AR program coverage interacted with county indicators. Both approaches extend Kline and Walters (2016)'s counterfactual alternatives framework to the instrumented-differences-in-differences context.

[4]Money bail's effects can be decomposed into incapacitation effects and deterrent effects, while unsecured bail effects only operate through deterrence. I find that the marginally released people would need to fail to appear more than 60% of the time for the different effects of money bail and unsecured bail to be solely due to incapacitation. Since this is a very high failure to appear rate, it is likely that the timing of payment matters for deterrence – an immediate and certain loss of pre-paid money is a greater deterrent than a potential financial loss in the future.

[5]In contrast, Abrams and Rohlfs (2011) studies effects of money bail amounts using random assignment of judges to bail guidelines. Earlier on, Myers Jr (1981) and Helland and Tabarrok (2004) demonstrate that less bail is associated with more failure to appear. However, these later papers do not rely on quasi-experimental variation.

[6]The compiler groups across the two approaches may be different. Compliers in the automatic release program context are the 50% of all program-eligible cases whose bail type changes as a result of the program. Compliers in a judge leniency context are those whose outcome would be different if assigned to the most lenient judge instead of the

The most closely related paper on this topic is Ouss and Stevenson (2022). They study a prosecutor-driven bail reform and find no effects of reduced money bail and supervision on pretrial detention or pretrial misconduct. Our institutional settings differ in a few ways. First, the Philadelphia program was discretionary, while the Kentucky program is automatic, leading to a first-stage effect on bail conditions that is higher in magnitude (50 p.p. instead of 10 p.p.).[7] Second, the Philadelphia reform does not impact pretrial detention, so their paper is focused on the deterrent effects of bail conditional on release rather than potential trade-offs between overall misconduct and detention.

Bail reform encompasses many different policy prescriptions. In this paper, I study when people arrested for low-level offenses, who normally would be assigned financial conditions, are automatically released without any financial conditions. In this case, the treated population (those arrested for low-level offenses) and the counterfactual to the status quo (no financial conditions) are well-defined. In contrast, the policy proposal of total elimination of money bail impacts a broader population and the counterfactual to money bail is undefined – it is ambiguous what replaces money bail (e.g., supervision, unconditional release, electronic monitoring, etc.) and in what cases.[8] This paper provides evidence on a specific and well-defined reform, which is an important piece of the broad money bail reform conversation.[9]

This paper builds on the literature addressing the interplay of financial health and the criminal legal system. Pretrial detention, which often results from money bail, increases the likelihood of conviction (through guilty pleas) and decreases formal sector employment and receipt of government benefits (Dobbie, Goldin, and Yang 2018; Heaton, Mayson, and Stevenson 2017; Leslie and Pope 2017). Another type of legal monetary sanction, fines and fees, can increase chances of default for low-income people and can induce new offending (Mello 2021; Giles 2022).[10] Contact with the criminal justice system and financial well-being can impact one another in a perverse feedback loop – Aneja and Avenancio-León (2020) shows incarceration reduces access to credit, which in turn increases recidivism.

most strict judge.

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

[8]Policies that aim to limit the usage of money bail (such as those that encourage electronic monitoring or supervision) may also, unintentionally, limit the usage of the least lenient conditions. For example, Skemer, Redcross, and Bloom (2020) show that a New York City supervision program successfully shifted some cases away from receiving money bail, however, the program also resulted in less unconditional release.

[9]My paper is the first to my knowledge to estimate causal effects for distinct categories of bail conditions. Also, since unconditional release is the most lenient form of bail, my money bail estimates theoretically present an upper bound on the effects of switching away from money bail for low-level offenses regardless of the exact alternative (e.g., supervision, electronic monitoring). The alternative is important to consider since there are non-monetary interventions that can positively impact the behavior of people in the criminal legal system. For instance, court reminders have meaningful effects on court appearance (Emanuel and Ho 2020; Fishbane, Ouss, and Shah 2020).

[10]Bail, fines, and fees all are forms of legal monetary sanctions, which have grown in usage over the decades – 1 in 5 people surveyed in a Philadelphia survey use some type of court debt (Harris 2017; Shapiro 2014).

My paper proceeds as follows. Section 2 describes the Kentucky Automatic Release program and the administrative data used this paper. Section 3 describes the empirical strategy for estimating program effects. Section 4 presents my main results on the causal effects of the AR program. Section 5 demonstrates how the program impacts racial and socioeconomic gaps. Section 6 investigates the mechanisms underlying the main program effects by estimating the effects of distinct bail conditions. Section 7 concludes.

2 The Automatic Release Program and Administrative Data

2.1 Background on the US Bail System

There are over 10 million arrests every year in the US (O'Toole and Neusteter 2019). After arrest, a judge or magistrate determines the conditions that govern a person's release pretrial, 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).[11] As such, bail is meant to incentivize good conduct pretrial, but it can also lead to pretrial detention due to individuals' liquidity constraints.

There are four broad and mutually exclusive categories of bail. In order of least to most restrictive, they are: (1) unconditional release, (2) unsecured bail, (3) money bail, and (4) bail denial.[12]

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

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

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

4. Bail denial means that no amount of money can secure pretrial release.

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

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

Figure 1a demonstrates how bail types feed into potential pretrial release, which, in turn, feeds into potential pretrial misconduct. Under unconditional release, unsecured bail, and money bail if paid, individuals are released. Since they are then free pretrial, it is possible for people to commit pretrial misconduct (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 (2020) 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 cost of pretrial misconduct to people in the criminal justice system even if there are no financial bail penalties (i.e., under unconditional release). If someone is released pretrial, bail conditions should be thought of as an additional layer of sanctions on top of the baseline criminal justice system penalty to pretrial misconduct. Under money bail, individuals forfeit their already posted bail amounts. Under unsecured bail, individuals may forfeit their predetermined bail amount.

2.2 Kentucky Bail System Background

After someone is arrested and booked in one of Kentucky's 120 counties, a pretrial officer (an employee of the statewide Pretrial Services agency) working in that county collects information about the arrested person and arrest incident to help facilitate pretrial decisions.[13] Within 24 hours of booking, the pretrial officer will present this information to a judge (usually during a phone call). In Kentucky, initial bail decisions are not subject to prosecutorial review, as they are in most other states. Therefore, judge decisions are not conditional on prosecutor actions; judges make decisions solely based on information presented by pretrial officers. Judges then make decisions about bail type and bail amount (if applicable) within a few minutes.[14]

In Kentucky, 16.8% of cases receive unconditional release, 27.2% receive unsecured bail, 54.3% receive money bail, and 1.8% have bail denied.[15] Since bail denial is so rare, I focus on the three remaining categories that characterize nearly 100% of cases: unconditional release, unsecured bail, and money bail.

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

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

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

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).[16] In Kentucky, there is no commercial bail bonds industry. Therefore, the arrested person or someone in their network needs to post the required bail amount.[17]

2.3 Kentucky Automatic Release Program

From 2013 through 2017, Kentucky phased in a program called Administrative Release, which I call Automatic Release (AR), to expedite pretrial release for people charged with non-violent, non-sexual misdemeanors (e.g., shoplifting, disorderly conduct, criminal driving offenses). The goal of the program was to reserve resources for higher-risk cases by providing automatic unconditional release for a subset of people who would normally have 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 impacts the pretrial process: If the AR program is not yet in place in a given county, a pretrial officer presents information about the arrested person and alleged offense to a judge. The judge then makes a bail decision within a few minutes and the flow of outcomes follows the illustration in Figure 1a. If the AR program is in place, what happens depends on case eligibility. Figure 1b shows that eligible cases are assigned unconditional release without the involvement of a judge, while ineligible cases go through the system as usual – pretrial officers present information to judges and judges make bail decisions.

The AR program shrinks the scope of bail setting. Jurisdictions are increasingly considering similar automatic release programs for people arrested on low-level offenses, making this study directly policy-relevant.[18] In fact, Proposition 25 in California (voted down in November 2020) which would have implemented a program based on Kentucky's AR program across all of California.

While most bail reform efforts and programs rely on judicial discretion, AR intentionally limits judicial discretion. Limiting discretion means more binding changes since judges often deviate from recommended actions (Stevenson 2018; Albright 2019; Stevenson and Doleac 2019). Bail reforms on the horizon seek to emulate the binding nature of the AR program. For example, the Illinois Pretrial Fairness Act, which will make Illinois the first state to end money bail in 2023, intentionally creates "bright-line rules that [take] away carceral tools from judges instead of trusting them to use such tools sparingly" (Grace 2021). Activists involved in this reform explained that judicial discretion

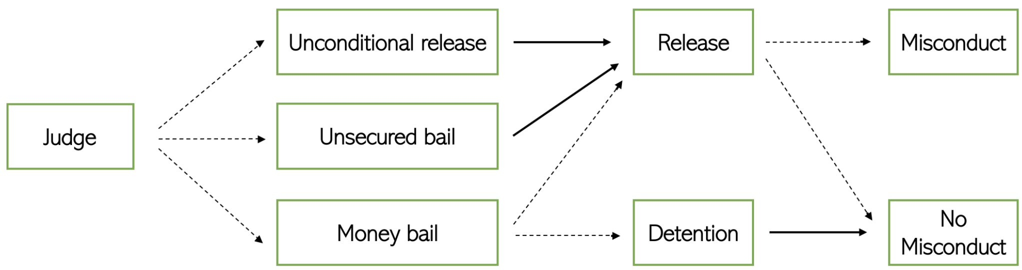
[16] Distinct from the commercial bail bonds industry, there are also non-profit organizations that post bail on people' behalf – there are more than 60 such bail funds nationally (Rahman 2020).

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

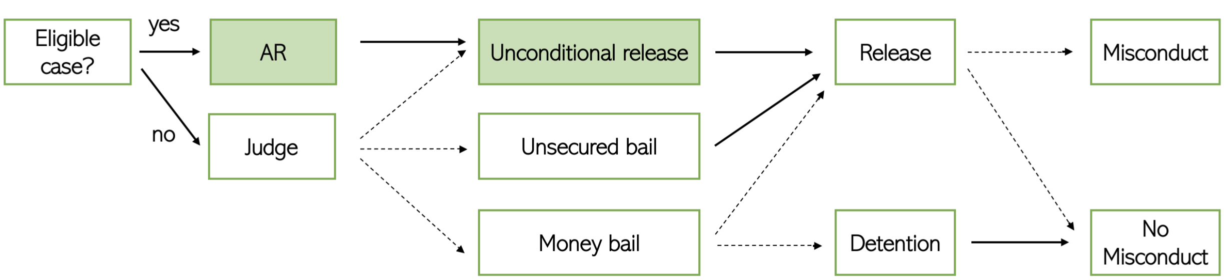
[18] These sorts of changes are also recommended by groups such as the ACLU (Woods and Allen-Kyle 2019).

Figure 1: The Pretrial Process

(a) Process in the absense of AR



(b) Process with AR

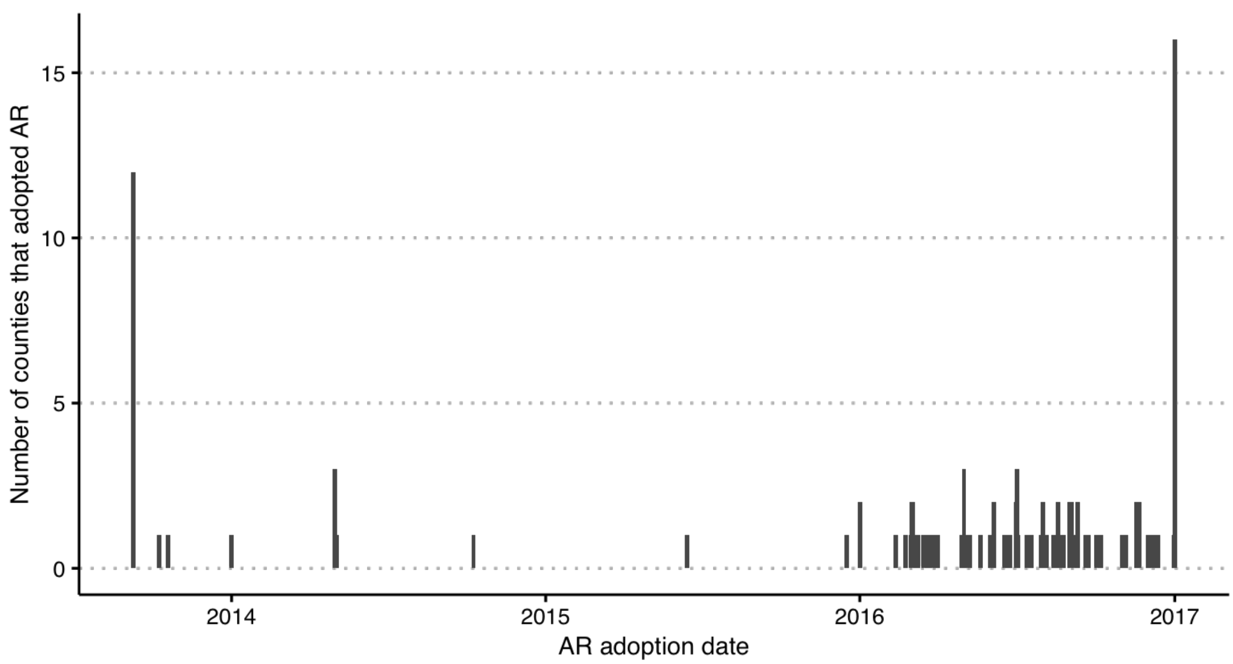


Notes: Figure 1a demonstrates that in the absense of AR, judges can choose between the three conditions: unconditional release, unsecured bail, and money bail. Unconditional release and unsecured bail lead to certain release (solid arrows), but money bail leads to uncertain release or detention (dashed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

in prior bail reform waves made it "increasingly clear [. . .] that a more binding, statewide policy change was needed" (Grace 2021).

AR policy timing across the state: The AR program was phased in across the state between September 2013, when it was first piloted in a small group of counties, and 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.[19] While there were some early adopters, the majority of counties (about 80 of them) took up at various dates between January 2016 and December 2016. The last 17 counties to adopt the program did so when it became mandatory statewide in January 2017.

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 and January 2017, when the program became mandatory statewide.

AR eligibility requirements: The details that determine eligibility into the AR program shifted over time. Originally, the pilot counties listed out county-specific eligibility conditions (Supreme Court of Kentucky 2014). A Supreme Court Order in November 2015 standardized eligibility across counties (Supreme Court of Kentucky 2015). When I discuss eligibility I use the January 2017 eligibility requirements (Supreme Court of Kentucky 2017a), which captures eligibility well for all counties that took up AR after the November 2015 order (since the differences between the two

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

orders are slight) (Supreme Court of Kentucky 2015, 2017a).[20] See Appendix A1 for details on the evolution of eligibility over time.

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

1. the case must be associated with a "regular arrest"
   - An arrest is a "regular arrest" if it is not a rearrest, violations of condition arrest, bench warrant arrest, or indictment.
2. all associated charges must be in a predetermined set of "AR eligible charges"
   - AR eligible charges are non-violent non-sexual misdemeanors with some exceptions.[21] Given these constraints, common AR eligible charges are: driving offenses (driving without insurance, driving on a suspended license, driving without a license), drug paraphernalia (buy/possess), shoplifting, and disorderly conduct/public intoxication.[22]
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. An example of a person with a risk score of 8 would be someone who: is under 23, has failed to appear once in the last 2 years, and has a prior misdemeanor conviction which resulted in a sentence to incarceration. See Appendix A1.2 for the details on how the risk score is calculated.[23]

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

Direct evidence on the causal effects of bail reform is naturally limited by policy changes and data availability. Kentucky's AR program is well-suited for this research topic for a few reasons. First, the program was automatic, which means its effects are large and not subject to judicial discretion. Second, the presence of both eligible and ineligible cases before and after the program allows me to use differences-in-differences for estimating effects. Third, the program-eligible population is those arrested for low-level offenses, a population of great interest when it comes to narrowing the scope of the criminal justice system.

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

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

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

[23] See Appendix A2 for background on risk score usage across the US.

2.4 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 am able to construct the necessary case-level data using a collection of datasets from the Kentucky Administrative Office of the Courts that span all criminal cases with felony or misdemeanor charges across all 120 Kentucky counties from July 1, 2009 through December 31, 2017. Appendix A3 describes details related to data construction.

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

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

Misconduct Outcomes: Failure to appear outcomes are directly recorded for each case in the administrative data. However, pretrial rearrest outcomes are not consistently captured, so I generate measures of these outcomes based on observable rearrests in the Kentucky data.[24] Therefore, I measure rearrests only within the state of Kentucky. Pretrial rearrest does not include rearrests due to failing to appear because that is not considered a new criminal offense. Therefore, the two types of misconduct are mutually exclusive.

Eligibility Status: Crucially, I must identify cases as eligible or ineligible for the AR program. This is not directly captured in the administrative data, so I perform this tagging myself. I tag cases as eligible or ineligible based on observable variables and the language of relevant Supreme Court orders, which describe AR implementation. I confirm my reading of the eligibility criteria via interviews with local practitioners, such as court staff members. I describe further details about eligibility in Appendix A1.

I define cases as AR eligible if they meet the following 3 criteria: the case is the result of a regular arrest, the case only includes eligible non-violent, non-sexual misdemeanor charges, and the case pertains to a person with a risk score below 8. Ineligible cases are one of the following: they are the result of non-regular arrests, they include ineligible charges (say, felonies or violent/sexual misdemeanors), or they pertain to people with risk scores above 7.

There are likely instances where the observable data misses a detail that shifts a case in or out of eligibility. While arrest type and risk score components of eligibility are straightforward, the charge

[24] 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.

details are trickier. For example, charge codes in the administrative data are the Kentucky Uniform Crime Reporting Codes assigned by law enforcement officers, which can be different from the charge in the narrative record, which was used to determine eligibility before 2017. As another example, some arrests are the consequence of criminal warrants, which sometimes feature bail amounts set by judges.[25] If someone is arrested on a warrant with an amount specified, pretrial officers cannot release the person on AR; those cases have to go to a judge. Since the administrative data does not capture if someone is arrested on a warrant with a set bail amount, I cannot deem these observations ineligible.[26]

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

Sample Restrictions: I limit my sample due to constraints imposed by policy change details and available administrative data. The risk scores used for eligibility determination were first used on July 1, 2014. Since this is the first month when I can observe necessary risk scores for tagging eligibility, it marks the start of my sample period. Similarly, the risk score eligibility criterion changed in December 2017 (Supreme Court of Kentucky 2017b). Since eligibility categorization changes at that point, this marks the end of my sample period. Due to these two details, I require the initial bail decision date for a case to be between July 1, 2014 and November 30, 2017 for inclusion in the sample. Moreover, since November 2015 was when the key components of eligibility (as described in Section 2.3) were made consistent, I exclude cases from counties that implemented AR before November 2015. (Their eligibility criteria are distinct from the criteria that govern the later 99 counties.)[27]

Socioeconomic Information: I also use information on arrested peoples' employment status. This data was acquired through special authorization from the Kentucky State Supreme Court (Supreme Court of Kentucky 2021).[28] The data was collected on interview forms during pretrial interviews or subsequent contacts between Pretrial Services and people.

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

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

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

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

3 Empirical Strategy

3.1 Addressing threats to identification

Variation by eligibility and time provides an opportunity to trace out AR program impacts using a differences-in-differences approach. However, this approach may be misleading if the composition of cases changed discreetly at the time of program take-up. For instance, 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 due to that compositional change. In other words, if the population of arrested people changes due to the program, measured effects of the program on detention and misconduct are confounded by population changes.

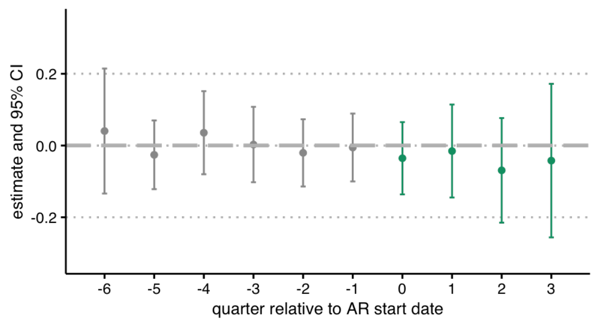
Another potential threat to identification is manipulation of program eligibility. For example, if it became tougher to be classified as eligible after the program (because of purposeful actions of people in the criminal justice system), then the eligibility classification itself shifted with the policy. This would mean eligible (ineligible) cases before and after are not comparable, confounding the differences-in-differences approach.

Due to these concerns, I use this subsection to test if the program impacted total arrests and eligibility determinations. 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 or 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 3 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.

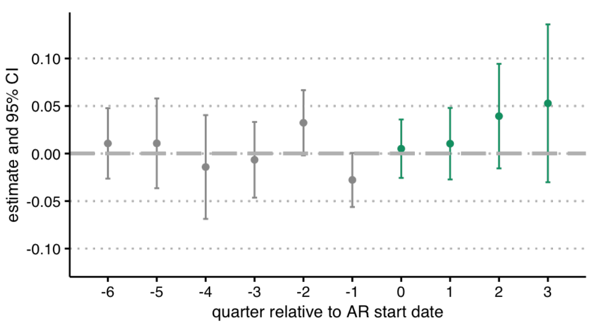
Figure 3a shows the results on overall offending. There is no evidence that the program changed the number of arrests, which alleviates concern about this potential threat to identification. This result is also important beyond its relevance to identification. While this paper's main results focus on 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. In probing my identification strategy, I have also established that overall offending did not change due to more lenient bail conditions. There may be limited effects on overall crime of bail reform programs that target people arrested on low-level offenses.

Figure 3: Testing for threats to identification

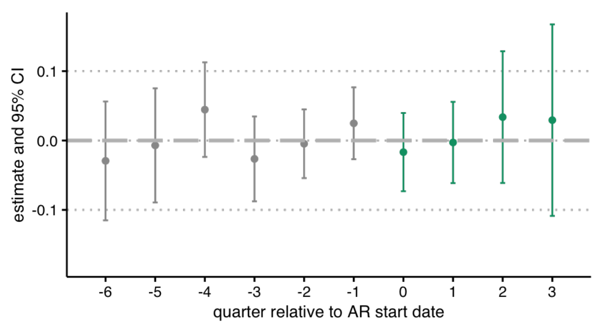
(a) Asinh(number of arrests)



(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 (2021) methods. In Figure 3a, the dependent variable is the inverse hyperbolic sine of the number of arrests in county-quarter. In Figures 3b, the dependent variable is the fraction of all cases with eligible charges. In Figure 3c, the dependent variable is the fraction of charge eligible cases that have eligible risk scores.

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, 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 3b shows that there is not 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.[29] 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 since the score manipulation only impacts inclusion in the program if the charges are eligible). Figure 3c shows the percentage is stable around the policy change. Overall, there is not strong evidence of strategic actions to manipulate eligibility by police or pretrial officers around the policy date.

3.2 Differences-in-differences with a single AR start date

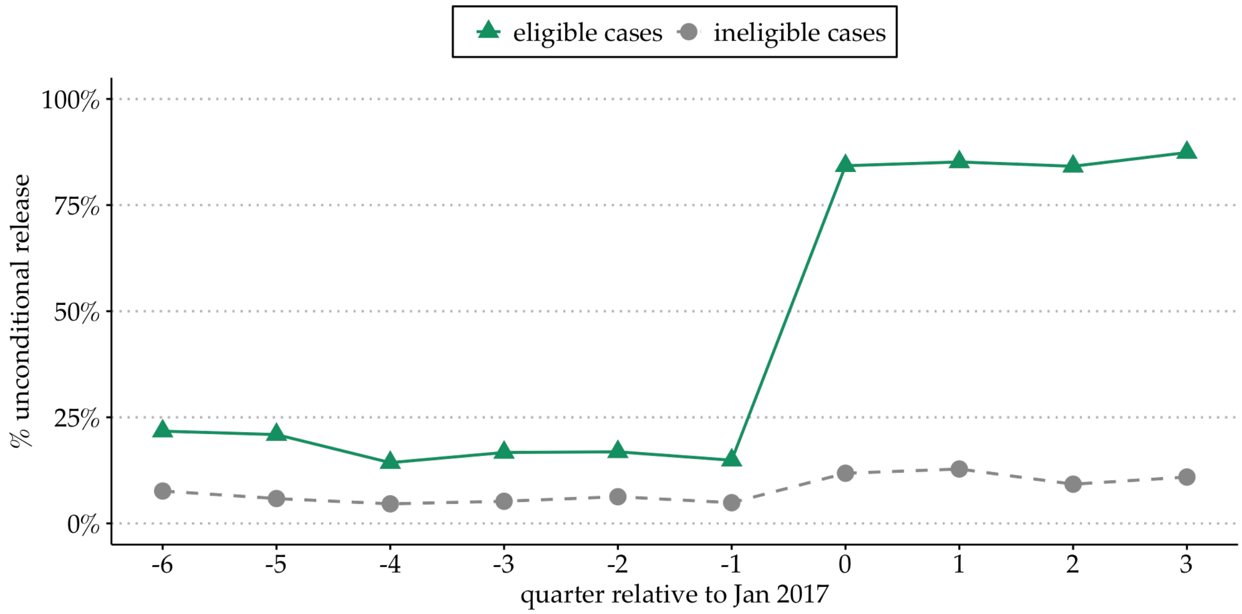
Based on the design of the AR program, the program should increase unconditional release for eligible cases. I use Figure 4 to demonstrate how the change in bail conditions looks for counties that adopt AR in January 2017. (I limit to counties with one AR start date to abstract away from concerns about staggered program timing and illustrate the identification strategy when there is one AR start date.) Figure 4 is a binned scatter plot showing the unconditional release rates for eligible and ineligible cases in the quarters before and after AR take-up in January 2017. AR take-up causes a dramatic increase in unconditional release for eligible cases but not ineligible cases. Eligible cases are more likely than ineligible cases to receive unconditional release before AR and the difference is similar over pre-periods, which is consistent with evidence of unconditional parallel trends in a differences-in-differences framework. After AR, 90% of eligible cases receive unconditional release (instead of around 20% just beforehand).

In theory, the unconditional release rate for eligible cases under AR should be exactly 100%. However, recall that I tag case eligibility using observable data, which might miss some factors that change case eligibility (as discussed in Section 2.4). Imperfect tagging is the primary explanation for the imperfect assignment to unconditional release.[30] However, the large jump in unconditional release for eligible (but not ineligible) cases demonstrates that the eligibility tag does a good job picking up program exposure.

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

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

Figure 4: AR Impacts Bail for Eligible Cases



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

Each individual county (or group of counties with the same AR adoption date) provides an opportunity for a differences-in-differences approach with 1 program date to estimate program effects. In the case of a single adoption date, I can employ conventional two-way fixed effect and event-study differences-in-differences approaches. Under unconditional parallel trends, this would mean estimating:

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

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

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

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

3.3 Differences-in-differences with all AR start dates

The 99 counties in my sample take up AR at different times. However, I do not need to leverage the staggered timing for identification because I have valid identification within each county independently.[31] Within any given county, eligible cases are treated units, ineligible cases in the same county are control units, and treatment turns on at the county's AR start date. Figure 4 demonstrates this intuition for the counties that take up in January 2017. Rather than use staggered timing for identification, I calculate an average effect across all 99 distinct AR implementations using a single set of treatment indicators.

To do this, I follow an existing approach to estimating a differences-in-differences specification multiple treatment dates: "stacked regression."[32] I define relative time periods q based on the number of full quarters away the bail date is from the AR start date. Therefore, at the same moment in calendar time (same t), different counties may be in different quarters relative to AR (different q). To make sure county composition does not differ across relative quarters, I subset the data to 6 quarters before AR and 3 quarters after AR (i.e., $q \in [-6, 3]$ where $q = -1$ is the quarter before take-up) because cases from all counties are observed in those relative time periods.[33] My final dataset can be thought of as a stacked dataset where each event-specific dataset is just the observations associated with one of the 99 counties. The estimated pooled and event-study specifications are:

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

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

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

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

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

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

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

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

4.1 What is the impact of AR on bail conditions?

Figure 5 demonstrates how AR impacted bail conditions. First, Figure 5a shows that AR increased unconditional release 50.5 p.p. relative to a baseline of 19.8% for eligible people. Gray estimates in the pre-period are precise zeroes and the point estimates do not demonstrate consistent pre-trends, providing strong evidence in favor of the parallel trends assumption. Estimates show evidence of dynamic effects – unconditional release effects are larger a few quarters after AR. According to administrative court staff, this is likely because counties improved in their administration of the program over time.[34]

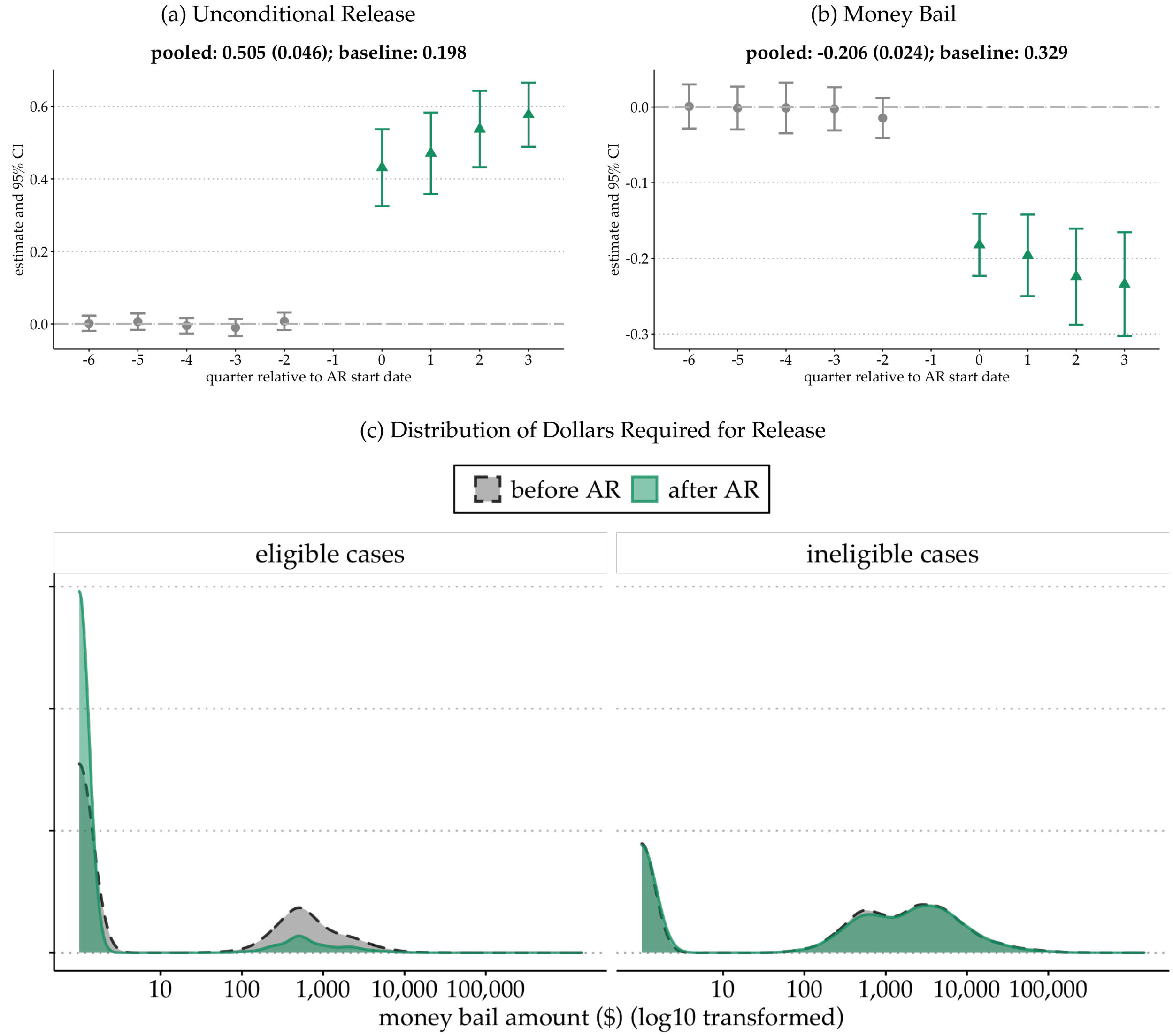
Despite the fact that the eligible group is intended to be low-risk on a number of dimensions (arrest type, charges, risk scores), note that judges only assigned 19.8% of eligible cases unconditional release before AR. This descriptive fact is consistent with Ouss and Stevenson (2022)'s argument that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

The increase in the unconditional release rate necessarily means a decrease in the usage of financial conditions (unsecured bail and money bail). Since unsecured bail is the next most strict after unconditional release, one might expect all the substitution to come from this category of bail. However, in fact, Figure 5b shows that money bail decreased by 20.6 p.p. off a baseline of 32.9% for eligible people.

Money bail comes with a particular bail amount required for release, meaning there is a continuous component of interest as well. In terms of dollars required for release, Figure 5c demonstrates how the distribution of this quantity changes. The figure plots the distribution of money bail amounts before and after AR for eligible and ineligible observations – the eligible group experiences a left shift away from values between \$100 and \$10,000 towards \$0 (unconditional release or unsecured bail). The most common shift in levels for eligible cases is a change from \$500 to \$0. Differences-

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

Figure 5: How AR Impacts Bail Conditions



Notes: Figure 5a and 5b plot the event-time differences-in-differences estimates using methods described in Section 3.3. The outcome variable for Figure 5a is an indicator for unconditional release. The outcome variable for Figure 5b 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 5c shows density plots for number of dollars required for pretrial release for eligible and ineligible cases both before and after AR. The x-axis is log10 transformed. The green shaded area (with a regular outline) is the distribution after AR and the gray shaded area (with a dashed outline) is the distribution before AR.

in-differences results show that AR decreased the dollar amount required for release by 76.9% for eligible cases.[35] The average amount required before AR was $360, so the 76.9% decreases corresponds to a drop down to $83. The total annual amount required for release before AR was $4.16 million for eligible cases – the AR program meant $3.2 million less required from this population.

Figure 5c also demonstrates that the distributions before and after AR look identical for ineligible cases, suggesting that judges are not changing their bail setting behavior as a result of the program. There do not appear to be unintended consequences (as a result of judge behavior) that offset the AR program effects in the full population. One might have hypothesized judges would become harsher for ineligible cases because they know eligible cases are receiving unconditional release automatically. There is not evidence of this sort of behavioral change in the data.

4.2 What is the impact of AR on pretrial detention?

How does the program impact pretrial detention? The average amount required for release from jail decreased from about $360 to $83 due to the AR program. It is possible this change in financial requirements may not induce much of a change in pretrial detention, as in the case of Ouss and Stevenson (2022)'s evaluation of a prosecutor-focused bail reform. This would be the case if most impacted people were already able to cover a $360 expense. However, on the other hand, a few hundred dollars can make a big difference for people making contact with the criminal justice system – Mello (2021) shows that even traffic tickets of $190 lead to unpaid bills in collections for drivers.

I first examine effects on an indicator for release within 1 day of booking. I focus on this definition of pretrial detention because judges are required to set bail within 24 hours of booking during my sample time period. Therefore, if a person makes the first bail set by a judge, they are usually out within 24 hours. The 1 day release definition, therefore, should focus on changes to release that are due to liquidity constraints rather than changes attributable to speedier administration induced by the program.

Figure 6a demonstrates that release within 1 day increases 13.7 p.p. off a baseline of 76.6%.[36] The baseline rate demonstrates that quick release was the norm within the eligible population before the program. Regardless, the program's impact on release despite modest money bail requirements in its absence suggests that inability to put up a relatively modest sum is still an important constraint on pretrial release. I show in Appendix A5 that AR increases other measures of pretrial release as

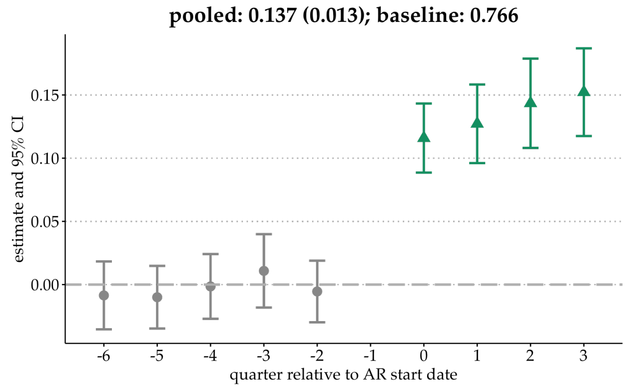
[35] See Appendix A5 for more details. The outcome variable in this case is the inverse hyperbolic sine of the money bail amount. If the observation does not receive money bail, the amount is 0. I use the inverse hyperbolic sine transformation since the distribution of amounts is right-skewed and includes zeroes.

[36] In Appendix A4, I perform a decomposition exercise to verify that the release effect is due to changes in bail conditions rather than changes in administrative speed. 96.5% of the effect is due to unconditional release assignment (complier group) as opposed to speed changes within unconditional release (always-taker group).

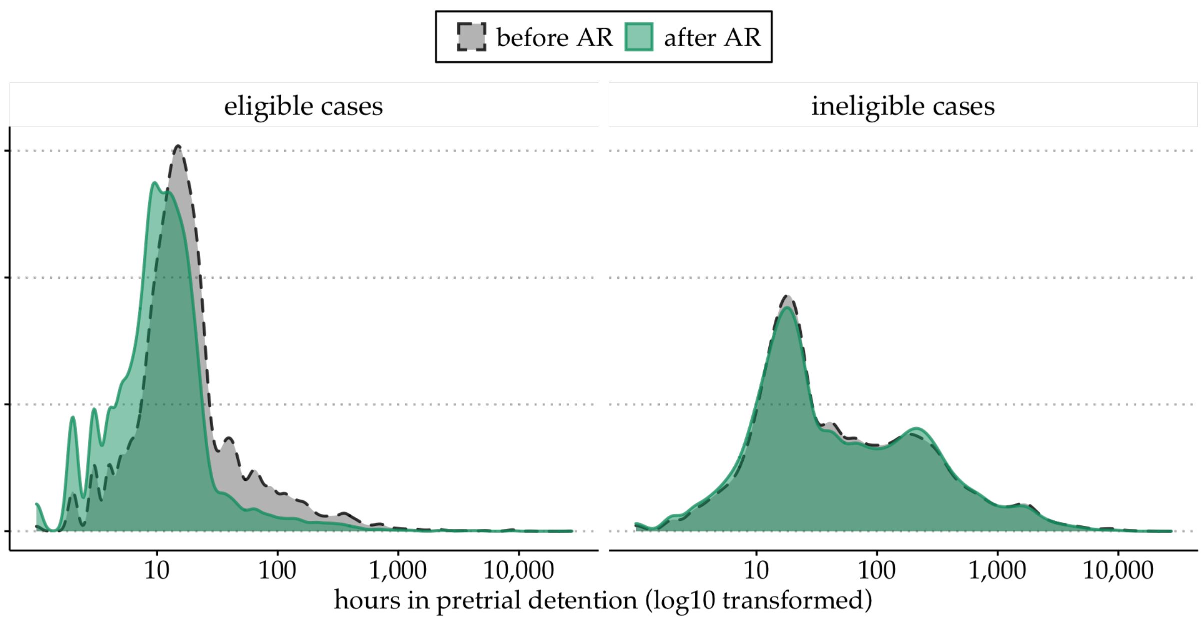
well – any pretrial release increases by 6 p.p. while pretrial release within 3 days increases by 6.3 p.p.

Figure 6: How AR Impacts Pretrial Release

(a) Release in 1 day



(b) Distribution of Detention Hours



Notes: Figure 6a plots the event-time differences-in-differences estimates using methods described in Section 3.3. 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 6b 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.

I also directly estimate effects on total detention hours. Figure 6b shows the densities of density hours before and after AR for eligible and ineligible cases. While there no visible change for ineligible cases, the eligible cases experience a shift left. Detention stays for longer than 12 hours become less frequent, while detention stays for less than 12 hours become more frequent. Overall, the program causes a 42.4% decrease in hours in detention.[37] Relative to the baseline mean of 48.9 hours, this implies an average decrease of around 20.7 hours in detention. In the year before AR, program eligible people in the 99 counties were detained for a total of about 520,000 hours. The 42.9% drop means a drop of around 223,000 person-hours – equivalent to roughly 9,300 person-days or 25.5 person-years – in detention.

Estimating effects on the total detention hours measures all the ways AR impacts release. Part of the change is due to changes in bail conditions (more unconditional release) and another part is driven by speed in the administrative process (since AR does not require contacting a judge). In contrast, the 1 day measure minimizes the importance of the administrative speed changes since all arrested people should have their initial bail set within 1 day.

4.3 What is the impact of AR on pretrial misconduct?

There are different types of pretrial misconduct, which carry different costs and policy implications. I provide results on failure to appear in court and pretrial rearrest since, according to formal guidelines, the objective of bail is to set the least restrictive bail to ensure appearance at court and avoid rearrest (American Bar Association Criminal Justice Standards Committee 2007). Failure to appear in court means someone who has been released from jail does not show up for their scheduled court date.[38] Pretrial rearrest means someone who has been released from jail is arrested on a new offense while their original case is pending.[39]

Figure 7a demonstrates the estimates on failure to appear. I find that AR increased failure to appear by 3.3 p.p. (relative to a baseline of 10.7%). Annually, this corresponds to an increase of about 364 court non-appearances. Figure 7b shows that the point estimate for pretrial rearrest is close to 0 (0.7 p.p.) and is 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 p.p. and an increase of 1.68 p.p. (relative to a baseline of 8.4%). In Appendix A5, I show that estimates are even smaller if the outcome of interest is violent rearrest. The point estimate of 0.3 p.p is insignificant and corresponds to an annual increase of 34 violent rearrests. The upper end of the 95% confidence interval is 0.6 p.p.

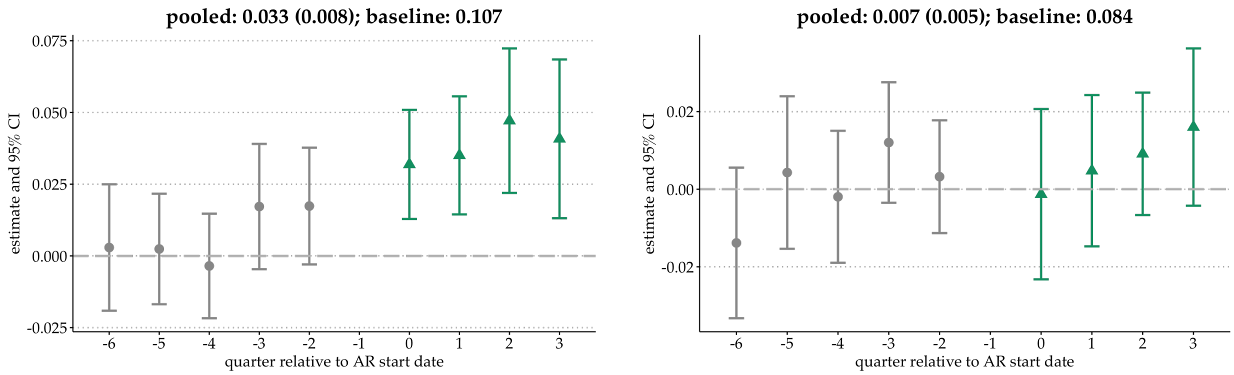
[37] See Appendix A5 for the event-time differences-in-differences estimates, which set the outcome variable as the inverse hyperbolic sine of detention hours. Similar to the case of money bail amount, this is useful due to the right-skew of the data and the inclusion of zeroes. (Estimates using levels in both cases do not satisfy parallel trends.)

[38] 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.

[39] The rearrest measure does not include rearrests for violation of pretrial conditions or failing to appear since those are not new offenses.

Figure 7: How AR Impacts Pretrial Misconduct

(a) Failure to appear (b) Pretrial rearrest



Notes: Figure 7a and 7b plot the event-time differences-in-differences estimates using methods described in Section 3.3. The outcome variable for Figure 7a is an indicator for failure to appear in court. The outcome variable for Figure 7b 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.

4.4 Do the effects generate a desirable trade-off?

In evaluating the effects of the program on bail conditions, pretrial release, and pretrial misconduct, a natural follow-up question is: do these effects constitute a desirable trade-off? Bail systems are meant to minimize pretrial detention and misconduct (American Bar Association Criminal Justice Standards Committee 2007). If a policy decreases one of these quantities with no change to the other, it is unambiguous that the policy is preferable to the status quo (it's a free lunch). However, in the context of the Kentucky AR program, detention decreases but misconduct increases as well. Understanding whether this trade-off is desirable requires assumptions about the nature of social welfare and the counterveiling costs of detention and misconduct.

As a simplified exercise, assume that social welfare is a function of total detention hours and total misconduct instances. (This treats 1 person in detention for 100 hours as equivalent to 10 people in detention for 10 hours each.) Then, let's focus on detention and court non-appearance results because the rearrest point estimate was indistinguishable from 0. The question of interest becomes: is 223,000 hours of detention hours more costly than 364 court non-appearances? Assuming constant costs (each hour of detention and each instance of misconduct has the same cost), the relevant question simplifies down to: is 26 days of detention more costly than one court non-appearance? If so, the program generates a desirable trade-off.

Depending on the misconduct measure of interest, I can provide a number of alternate conclusions. If policymakers would rather group misconduct types together, the program generates a desirable trade-off if 21 days of detention costs more than one instance of misconduct. If policymakers want to focus only on pretrial rearrest, the program generates a desirable trade-off if 118 days of detention costs more than one rearrest. Lastly, if violent rearrest is the misconduct type of interest, the program generates a desirable trade-off if 273 days of detention costs more than one violent rearrest.

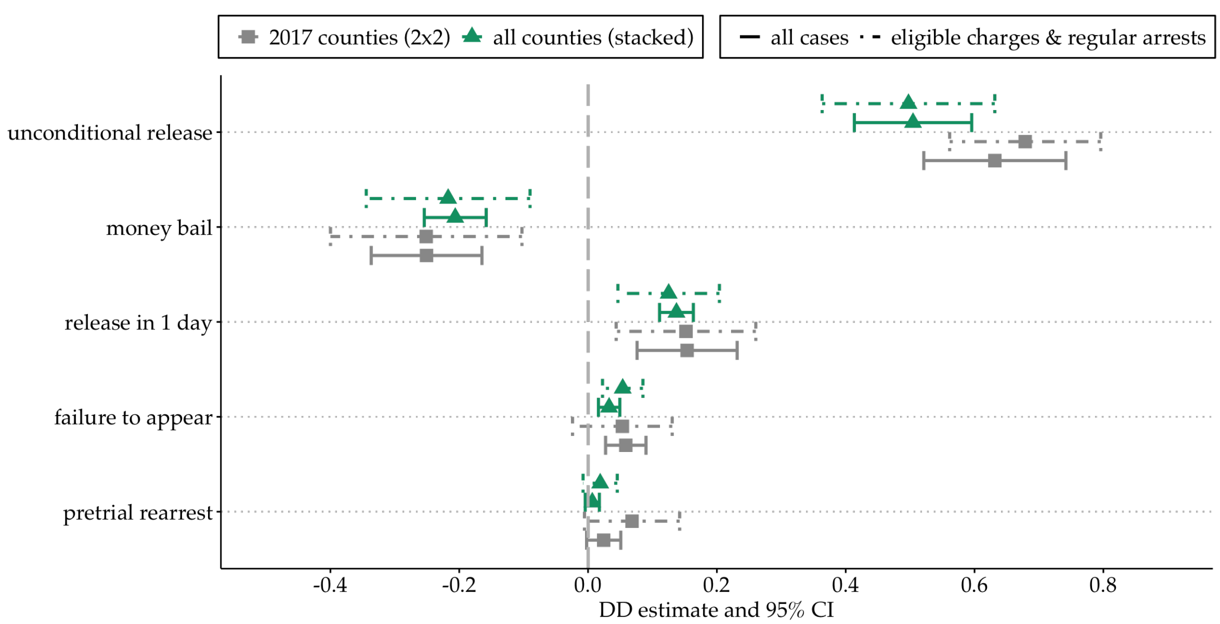
4.5 Robustness to alternative sample choices

In generating my main differences-in-differences results, I use the maximum possible sample of all cases in 99 counties. However, I can run the same differences-in-differences approach using different sample choices to provide evidence of the stability of my findings.

First, I subset to cases where eligibility is attributable to risk scores. In other words, I focus on cases associated with regular arrests and eligible charges. This makes the control group smaller – the control group becomes low-level cases where arrested people have risk scores that are above the eligibility cut-off.

Second, I test how results change if I avert avoiding staggered timing of AR across the state. I generate results using only the group of counties that take up in January 2017. I do this for all cases in the 2017 counties and then also only the 2017 counties that are associated with regular arrests and eligible charges.

Figure 8: Differences-in-differences estimates using alternative samples



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

Figure 8 shows the pooled differences-in-differences coefficients across combinations of county samples (2017 adopters only or full sample) and case samples (only eligible charges and regular arrests or all cases). Results for 2017 counties are slightly larger for bail conditions and pretrial rearrest. However, overall estimates look consistent across sample choices. The choice of how to refine the sample of cases or counties does not dramatically change the summary of results.

5 How did the program impact racial and socioeconomic gaps?

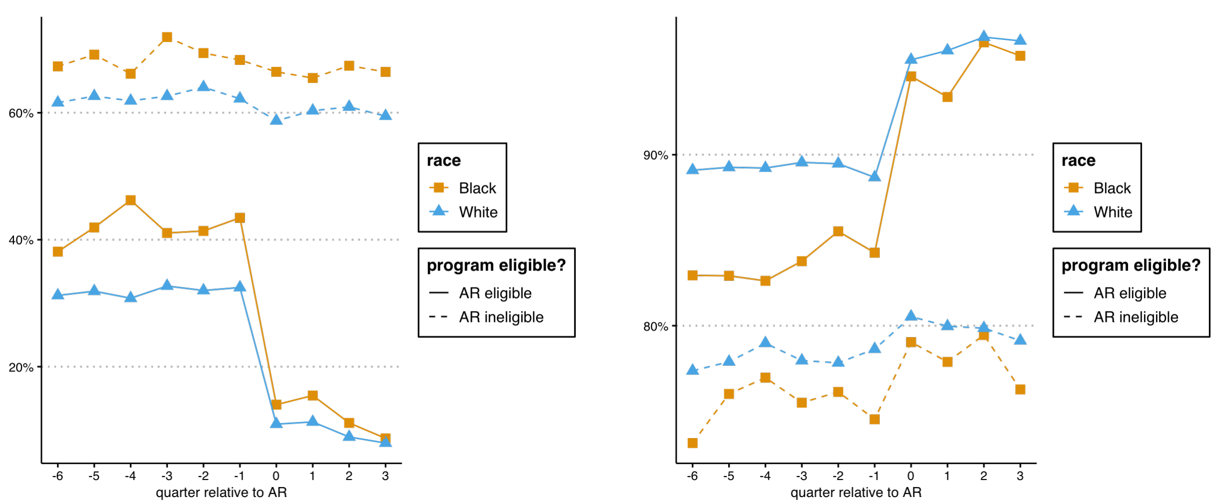
While the main results in this paper focus on overall effects on detention and misconduct, other effects are relevant to the bail reform policy discussion. The money bail system can have disparate impacts on disadvantaged groups, such as Black people or people who are low-income. As such, bail reform 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.

I use Figure 9 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.

Figure 9: How AR Impacts Black-White 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 9a shows the percentage of cases that are assigned money bail and Figure 9b 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.

However, if I calculate how the gaps change in the full population the effects are very muted. The change is muted because only 20% of cases are eligible for the program. Moreover, white people are slightly more likely to 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%).

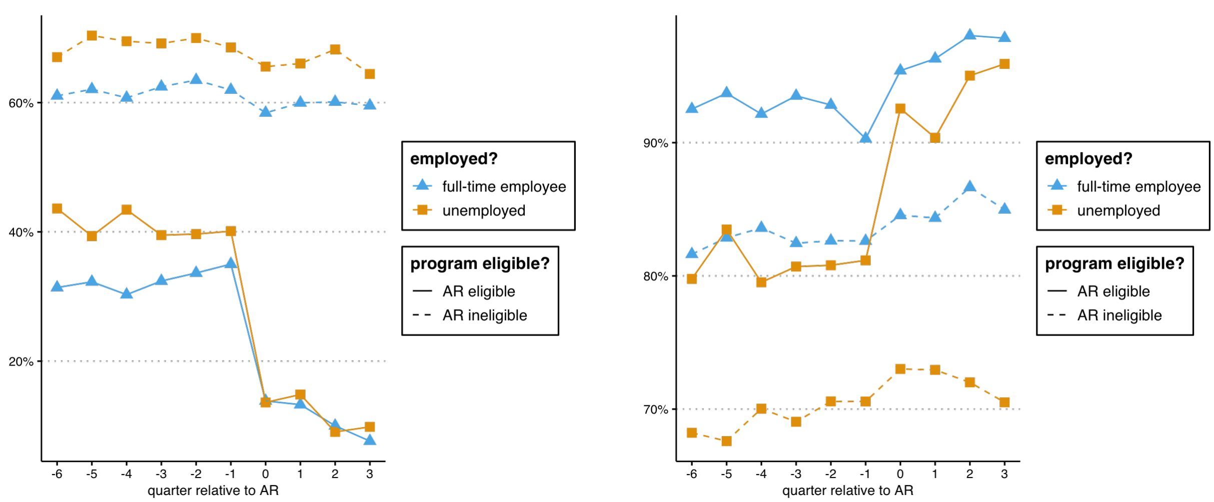
In Figure 10, I turn to socioeconomic gaps in bail and release. Specifically, I plot how gaps between employed and unemployed people evolve over time for eligible and ineligible cases. The gaps remained stable for ineligible cases, but shrunk dramatically for eligible cases. The gap in money bail assignment decreased from 8.4 to 0.7 percentage points, while the release gap decreased from 12 to 3.5 percentage points. Noticeably, pretrial release for program eligible unemployed people was lower than for program ineligible employed people before the program. This was no longer the case after AR.

As is 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%). This is driven by the fact unemployed people are more likely to be arrested for repeat offenses and are more likely to have high risk scores.

Overall, these results demonstrate that automatic release programs can close racial and socioeconomic gaps for eligible populations. However, these closures might not translate to the full population due to program size and disparities in program eligibility.

Figure 10: How AR Impacts Unemployed-Employed 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 9a shows the percentage of cases that are assigned money bail and Figure 9b 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.

6 Mechanisms

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

6.1 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: unconditional release (u), unsecured bail (c) or money bail (m).

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

Let $B_{it}(Z_i) \in \{u, c, m\}$ 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 (c) or money bail (m) to receive unconditional release (u) instead. No court should switch between unsecured bail (c) and money bail (m) in response to bail reform coverage, and no court should be induced by bail reform coverage to switch an arrested person away from unconditional release. In other words, the only way bail reform coverage should change bail setting is to shift those receiving c or m to u. This is an extended monotonicity assumption (assumption 1) and can be expressed by the condition below:[40]

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

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

1. c-compliers: $B_{i1}(1) = u, B_{i1}(0) = c$
2. m-compliers: $B_{i1}(1) = u, B_{i1}(0) = m$

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

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

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

Consistent with Figure 1, the later-stage outcomes of interest are misconduct and release. Call these $M_{it}(b)$ and $R_{it}(b)$. For $Y \in \{R, M\}$, we can write the reduced form effects of AR on Y as: $E[Y_{i1} - Y_{i0}|Z_i = 1] - E[Y_{i1} - Y_{i0}|Z_i = 0]$ if I make an additional assumption. Specifically, omitting the bail condition information from the Y_{it} notation requires an exclusion restriction (assumption 2). This assumption means that the only way AR coverage impacts court appearance and detention outcomes is through the bail type (i.e., the only treatment channel is the bail condition category).

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

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

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

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

Intuitively, this effect is acquired by attributing the entire reduced form effect to the complier group (those who are spared bail conditions due to AR).[41]

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

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

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

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

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

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

Table 1 shows that 27.2% of those spared financial conditions avoid spending 1 or more days in detention. This result highlights the prevalence of detention consequences even for those arrested on low-level offenses and is consistent with previous evidence on the large costs of even small fines and fees in the justice system (Mello 2021). Meanwhile, about 6.5% of those spared money-related conditions will fail to appear, which means 93.5% of that population did not have appearance ensured by conditions. Using Ouss and Stevenson (2022)'s language on error types, 93.5% of the complier population under the status quo receive Type II errors (too harsh), while 6.5% of the complier population under AR receive Type I errors (too lenient).[42] Results on pretrial rearrest remain insignificant.[43]

Table 1: Instrumented differences-in-differences estimates

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

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

In theory, financial conditions can induce both deterrence and incapacitation effects. As a result, it's ambiguous which of these channels is responsible for the estimated effects.[44] Since unconditional

[42] In this case, "too harsh" or "too lenient" refers to whether the conditions induced changes in failure to appear behavior. It is a different question whether the conditions are worth imposing due to simultaneous detention changes.

[43] In Appendix A7 I show that cases that judges assigned unconditional release before the program were less risky than the cases that are only assigned unconditional release because of the AR program.

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

release always means less detention and less conditions simultaneously, it is not possible to separate out how incapacitation and deterrence effects contribute to the misconduct results in a causal framework. However, I can provide an accounting exercise building off the potential outcomes framework to demonstrate combinations that are consistent with empirical estimates. The details of this approach are in Appendix A9 and I outline the core intuition below.

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

I can estimate the relative importance of incapacitation and deterrence based on beliefs 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 6 times as risky as the always takers (cases that receive unconditional release even when judges choose bail conditions). In this case, the newly released would need to fail to appear more than 60% of the time when given unconditional release. Even if the newly released are more than three times as risky as always takers, deterrence would still be responsible for about half of the aggregate failure to appear effect. If I assume that the newly released are equally likely to fail to appear under unconditional release as never takers under financial conditions, then deterrence is responsible for 81% of the aggregate effect and incapacitation 19%. Since 60% is a very high misconduct rate, it is likely that some of the effects are due to deterrence.[45]

6.3 How do the effects of eliminating money bail compare to the effects of eliminating unsecured bail?

The AR program generates variation that can be used to identify a number of parameters. The effects of unconditional release, estimated with DD-IV in Section 6.2, are policy relevant for marginally increasing unconditional release in that state (say by marginally expanding eligibility). However, the estimated causal effects are a mix of two underlying parameters: the effect of unconditional release relative to unsecured bail and the effect of unconditional release relative to money bail. In the potential outcomes framework language, there are two distinct complier groups who drive the aggregate effect – c-compliers and m-compliers, as defined in Section 6.1.

(less deterrence).

[45]Note that if bail conditions impact arrested persons' behavior beyond the incapacitation channel, then this causes an issue for judge instrument designs in bail settings, which rely on the assumption that judges only impact outcomes through detention or release.

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

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

where $LATE_{mu}^Y$ is the local average treatment effect of eliminating money bail (and replacing it with unconditional release), $LATE_{cu}^Y$ is the local average treatment effect of eliminating unsecured bail (and replacing it with unconditional release), and S_m is the fraction of compliers that are m-compliers. Based on the reduced form results in Figure 5, I know that 40.8% of compliers are money bail compliers and 59.2% are unsecured bail compliers.[46] Therefore, the effect of unconditional release is a 40-60 mix of two distinct treatment effects:

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

Since other states and jurisdictions use different mixes of bail conditions, the aggregate effect of unconditional release is limited in its external validity. For instance, while 27.2% of cases in Kentucky are assigned unsecured bail, only 4% of felony cases nationally are assigned unsecured bail. Intuitively, implementing unconditional release only means the same thing across environments if the counterfactual bail condition is the held constant. Therefore, it is worthwhile to explore the differences between the two counterfactual-specific treatment effects.[47] 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.

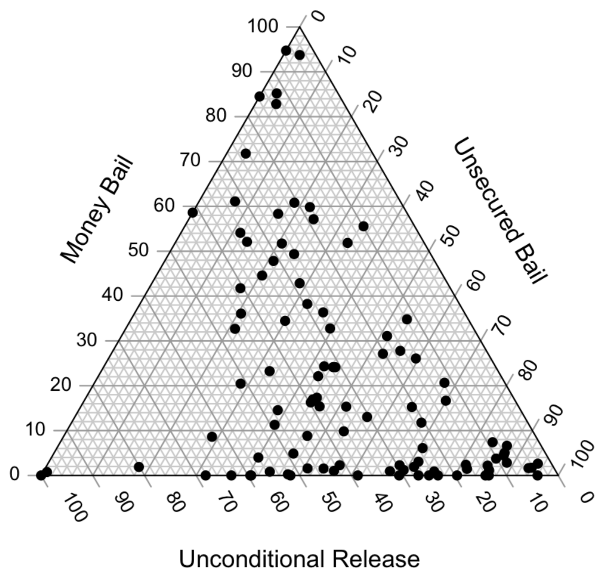
I use two methods to identify the underlying counterfactual-specific effects. Both use variation in bail setting across counties that is due to variation across judges in where they work.

Method (1): Figure 11 is a ternary plot demonstrating how all Kentucky counties by their usage of money bail, unsecured bail, and unconditional release for eligible cases before AR. Dots near the lower left vertex are counties that use unconditional release almost 100% of the time, while dots along the right side of the triangle almost never use unconditional release. Dots near the top vertex are counties that use money bail release almost 100% of the time, while dots along the bottom side of the triangle almost never use money bail. Dots near the lower right vertex are counties that use unsecured bail release almost 100% of the time, while dots along the left side of the triangle almost never use unsecured bail. Since money bail, unsecured bail, and unconditional release fully characterize bail conditions and are mutually exclusive, the three rates will sum to 100%.

[46]40.8% comes from dividing the magnitude of the effect of AR on money bail, seen in Figure 5a, by the effect of AR on unconditional release, seen in Figure 5b. Another method of calculating population shares is outlined in Appendix A8.

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

Figure 11: Variation across counties in bail setting before AR



Notes: This figure plots counties in Kentucky by their usage of money bail, unsecured bail, and unconditional release. The sample is limited to eligible cases before AR. These the three rates sum to 100% for each county since they fully characterize bail conditions and are mutually exclusive.

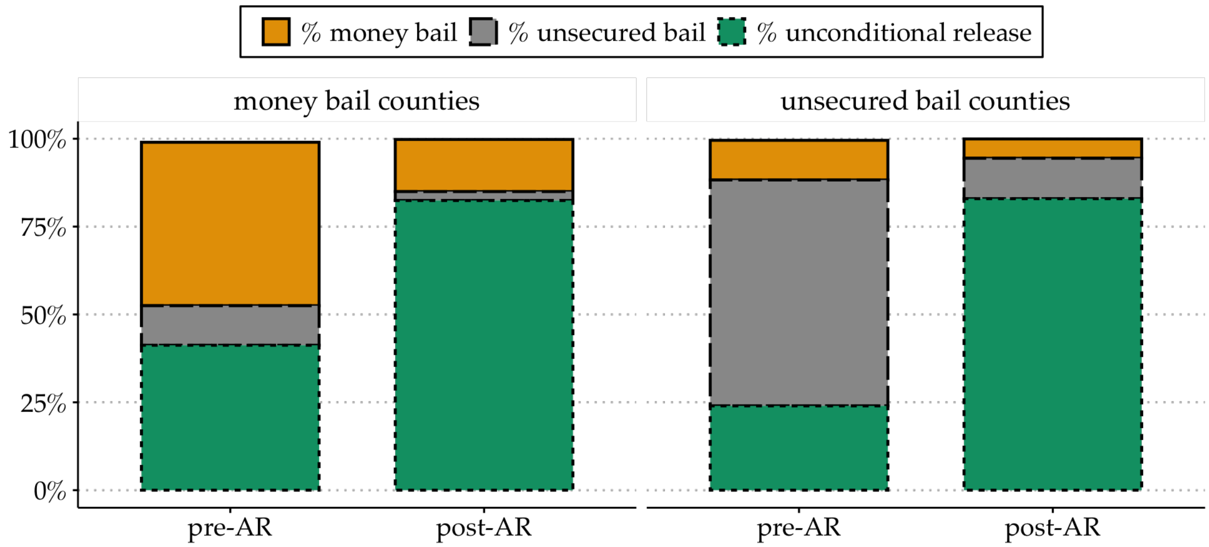
I define subgroups so I can focus on two distinct duos of bail types (and thus avoid the complications of substitution across a trio of bail types). I define "money bail counties" as counties where less than 20% of eligible cases receive unsecured bail pre-reform. "Unsecured bail counties" are counties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

Figure 12 then demonstrates the bail substitution patterns for eligible cases in these two county groups as a result of AR. The left pair of stacked bars illustrate substitution for money bail counties and mainly feature substitution from money bail (orange solid bars) to unconditional release (green dotted bars). The right pair of stacked bars illustrate substitution for unsecured bail counties and mainly feature substitution from unsecured bail (gray dashed bars) to unconditional release (green dotted bars). As intended (based on how I defined subgroups), money bail counties home in on the switch from money bail to unconditional release and unsecured bail counties home in on the switch from unsecured bail to unconditional release.

One concern with leveraging pre-reform variation is that case characteristics might differ substantially across place. If money bail counties assign money bail more due to riskier cases then attributing the difference in effects to the pre-existing bail conditions is problematic. However, I show in Appendix A6 that the AR eligible cases pre-AR are, if anything, riskier in the unsecured bail counties than in money bail counties.

For both subsamples, I provide instrumented differences-in-differences estimates instrumenting for unconditional release. The assumption is that in the money bail counties the instrumented effect mainly captures the movement from money bail to unconditional release and in the unsecured bail

Figure 12: Bail substitution patterns across subgroups



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

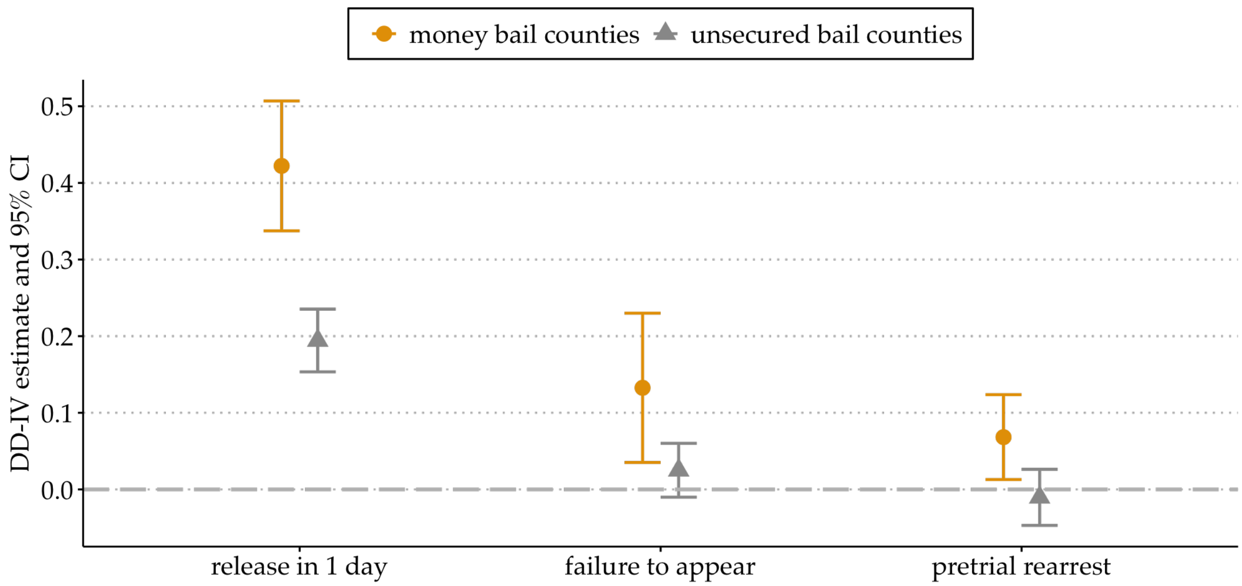
counties the effects mainly capture the movement from unsecured bail to unconditional release. Figure 13 plots both sets of estimates across the three outcomes of interest (release within 1 day, failure to appear, and pretrial rearrest). The circular estimates in orange are for the money bail counties and the triangular estimates in gray are for the unsecured bail counties.

As expected, money bail counties feature larger effects of removing conditions on release. Cutting financial conditions by 10 p.p. in money bail counties increases release within 1 day by 4.2 p.p. In unsecured bail counties, the same cut increases release by less than 1.9 p.p. This is consistent with money bail posing a barrier to release due to ex ante posting requirements. The larger effect in money bail counties demonstrates that the program is not solely improving release due to administrative speed improvements.

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

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

Figure 13: Estimates across subsamples



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

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

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

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

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

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

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

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

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

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

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

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

Estimates imply that a decrease of 10 p.p. in money bail use leads to a 4.7 p.p. higher rate of release within 1 day, which is similar to the results I got using method 1. Meanwhile, that change means a 1.7 p.p. higher rate of misconduct with most of (1.2 p.p.) the effect stemming 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.

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

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

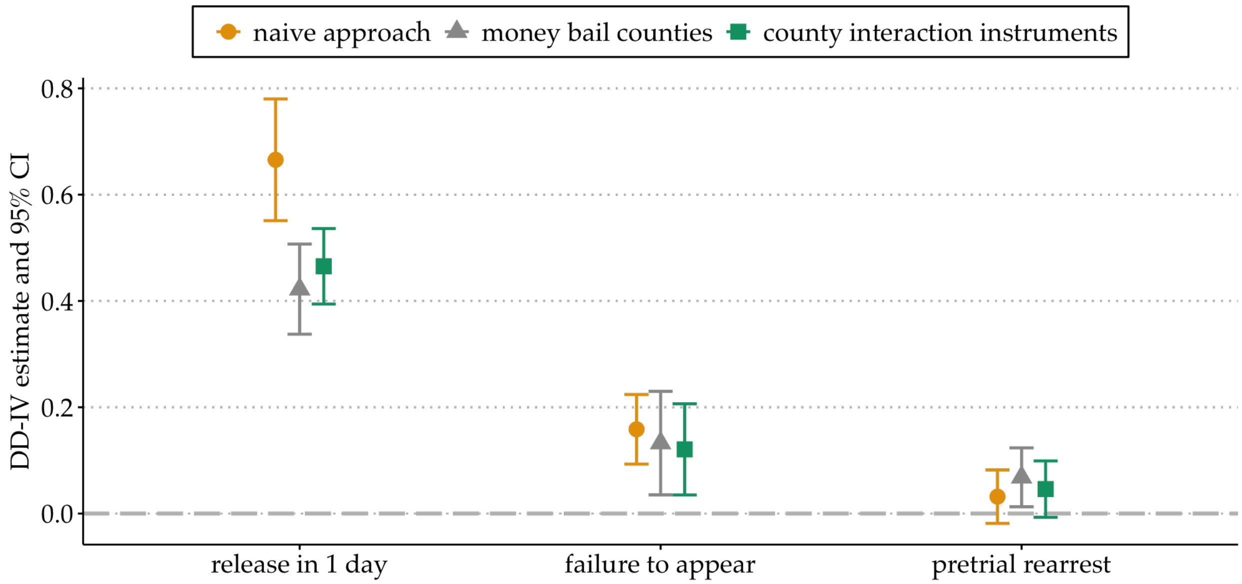
In fact, the weak unsecured bail effects suggest people act as though chance of financial collection is low (perhaps due to low chance of re-apprehension or low chance of collection conditional on re-apprehension). In the money bail case, since posting was required for release, money is more saliently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

How do results on the effect of removing money bail vary over estimation methods? I use Figure 14 to compare the effects of removing money bail (in favor of unconditional release) across methods. With orange circles, I present the naive DD-IV estimates acquired from simply instrumenting for money bail in the original set up (presented in Table 13). With gray triangles, I present the estimates from instrumenting for unconditional release in money bail counties (where we know most of the counterfactual is money bail). With green squares, I present the estimates for instrumenting for unconditional release in the specification with 2 endogenous variables and county interaction instruments.

Assuming the entire reduced form effects can be attributed to the elimination of money bail (as done in Table 13) generates similar misconduct results as the two methods using pre-reform county variation. All three methods provide similar estimates with ample overlap in their 95% confidence intervals. Estimates range in magnitude from 0.12-0.16 for failure to appear and 0.03-0.07 to pretrial rearrest. Effects on failure to appear are 2-5 times as large as the effects on pretrial rearrest, suggesting that more minor forms of misconduct are more responsive to money bail.[48]

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

Figure 14: Effects of eliminating money bail



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

However, the release results show that some of the original release in 1 day results were coming from unsecured bail substitution too. Attributing all of the aggregate release effect to money bail yields an estimate of 0.665; using pre-reform county variation methods gives estimates of 0.42 and 0.47. The majority of the release effects are attributable to eliminating money bail, but that is not the only channel.

7 Conclusion

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

I find that the program reduced financial conditions by 50.5 pp and meaningfully reduced pretrial pretrial detention. Total hours in detention decreased by 42%, which translates into a annual decrease of 223,000 hours, which is roughly equivalent to 25.5 years. Money bail requirements before the program were relatively modest in magnitude (around $360), which shows that liquidity constraints can be tight for people involved in the criminal justice system. In terms of pretrial misconduct, failure to appear increases by 3.3 pp, but results on pretrial rearrest are indistinguishable from 0 and I can rule out small increases. In one cost-benefit framework, the trade-off induced by the program is desirable if 26 days of pretrial detention costs less than 1 court non-appearance.

My core results focus on detention-misconduct trade-offs since the legal objective of bail is to minimize both these objects. However, this paper also addresses other bail reform effects that are relevant to policy debates. For one, I do not find evidence that the program increased new offending (through weakened deterrence as a result of the program). Moreover, I show that the program reduced bail and release gaps between Black and white people as well as employed and unemployed people (though these changes are muted in the full population).

Going beyond the reduced-form evidence, I use an instrumented differences-in-differences approach and variation in bail setting across counties before the program to estimate the distinct effects of distinct bail conditions. I find substitution away from money bail is responsible for most of the program effects. About 50% of people spared money bail by the program experience less detention time as a result.

Bail reform can mean many different things – its meaning has shifted over the decades (the 1960's reforms encouraged reducing money bail, but the 1980's reforms introduced preventative detention in the name of public safety) and even today, it encompasses a variety of policy prescriptions. In this paper, I study when people arrested for low-level offenses, who normally would be assigned financial conditions, are automatically released without any financial conditions. The treated population is those arrested for low-level offenses and the counterfactual to the status quo is defined (no financial conditions). The policy proposal of total elimination of money bail is distinct because the impacted population is larger and there the counterfactual to money bail is undefined – it is ambiguous what replaces money bail (e.g., supervision, unconditional release, electronic monitoring, etc.) and in what cases. In this way, my paper provides useful evidence on a specific and well-defined reform, which is an important piece of the broader money bail reform conversation.

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Appendix

A1 Automatic Release Eligibility

A1.1 More on criminal warrants

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

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

A1.2 Public Safety Assessment Risk Scores

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

The calculation of the Composite PSA score is illustrated in Figure A1: (1) raw Failure to Appear (FTA) and New Criminal Activity (NCA) scores are calculated based on the arrested person's charge, criminal history, and age, (2) points are assigned to each possible response and summed to calculate the respective raw scores, (3) raw scores are then converted into scaled scores, and (4) the two scaled scores are added together to generate the composite risk score.

The raw FTA score is calculated based on:

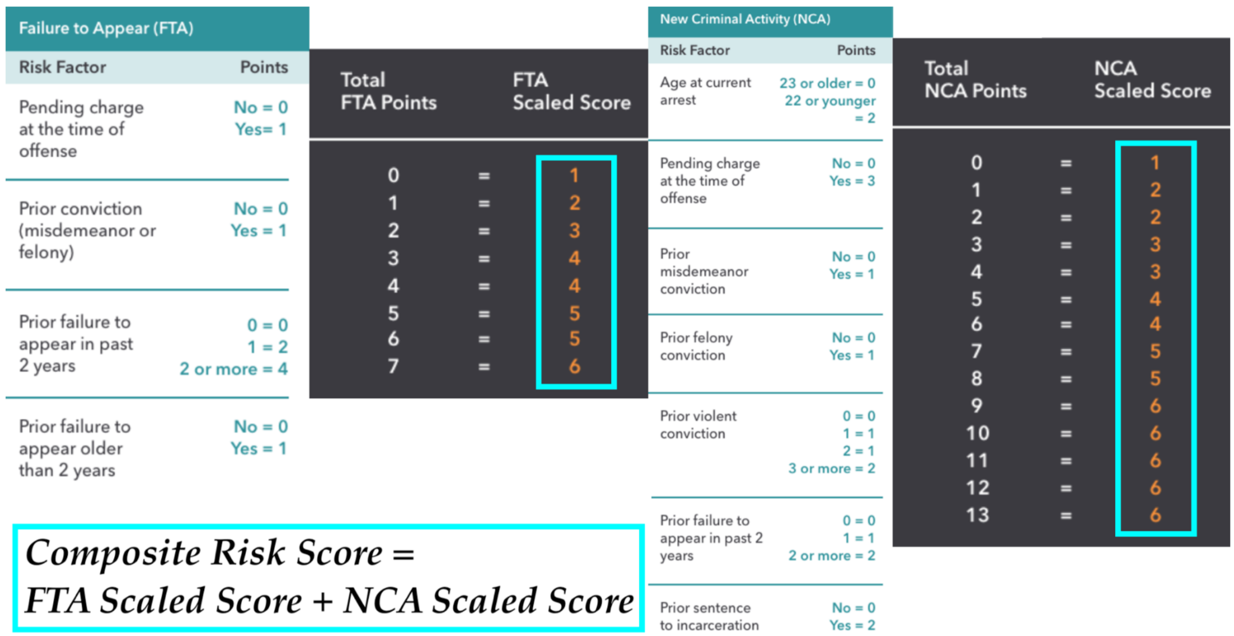
- whether the arrested person has a pending charge at the time of offense
- whether the arrested person has a prior conviction (misdemeanor or felony)
- how many times the arrested person has failed to appear in the past 2 years
- whether the arrested person has failed to appear more than 2 years ago

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.

A1.3 Charges

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

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

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

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

Some slight changes in implementation:

- Supreme Court of Kentucky (2016) notes that pretrial officers can obtain approval from Pretrial Services Executive Officer (or designee) to present an arrested person for judicial review.

- Supreme Court of Kentucky (2017a) mandates that pretrial officers base their review on the UOR code assigned by law enforcement. Previously, they were to base their review on the actual charge in the narrative/criminal record.

A2 Background on risk score usage across the US

The Public Safety Assessment (PSA) is used statewide in Arizona, New Jersey, Utah, and Kentucky. Other shaded states in Figure A2 include cities or counties that use the PSA, according to the Advancing Pretrial Policy and Research (APPR) organization (*Public Safety Assessment Sites* 2021). Figure A3 shows usage of risk scores nationally, as mapped by the Mapping Pretrial Injustice Project (*Where Are Risk Assessments Being Used?* 2021).

Figure A2: Public Safety Assessment Usage Across the US

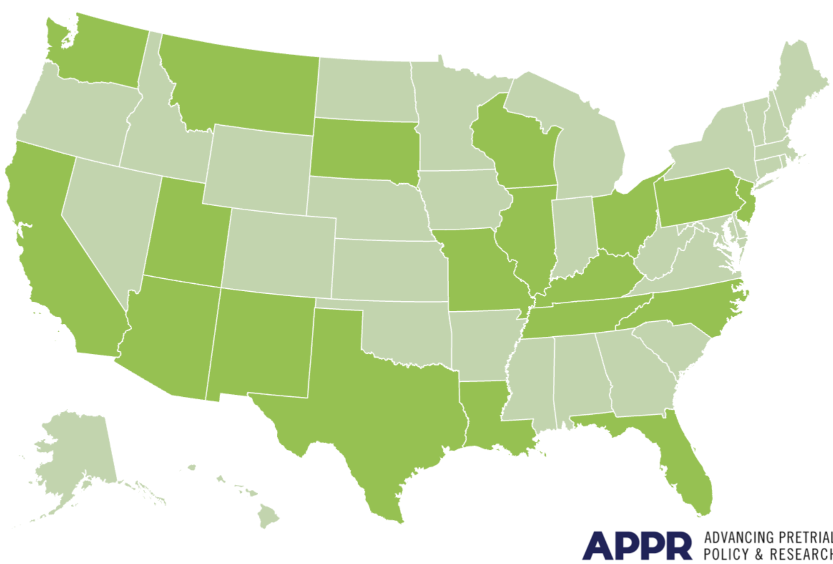
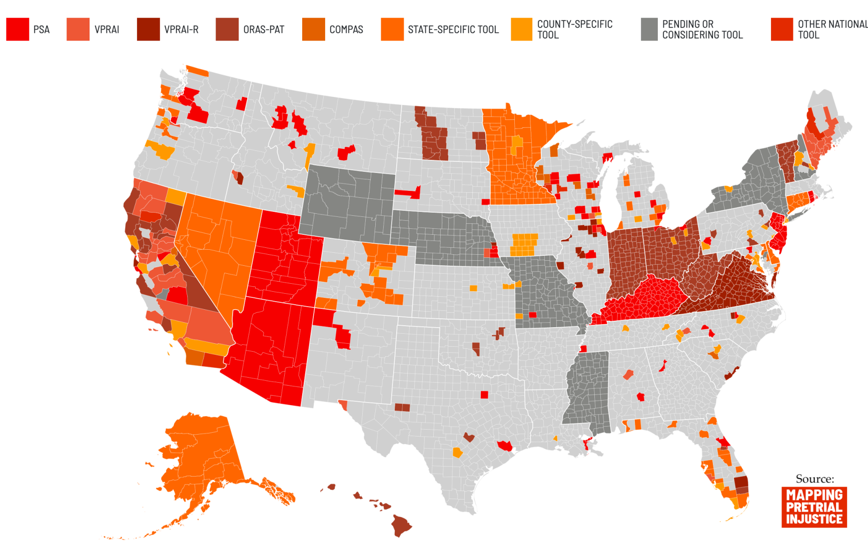


Figure A3: Pretrial Risk Score Usage Aross the US



A3 Data Appendix

A3.1 Sample restrictions

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

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

Counties: I use the samples of counties that took up AR after the November 2015 order. In the robustness results in Section 4.5 I also use the smaller group of counties that took up AR in 2017.[49] See Appendix A3.2 for more on differences between county samples.

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

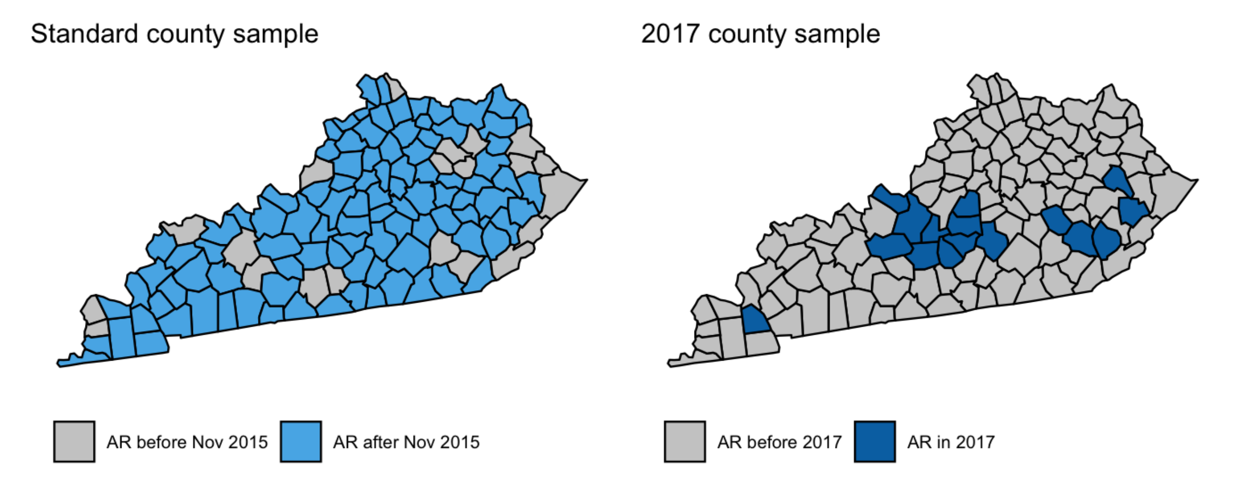
A3.2 County samples

In the main text, I use data from all counties that took up AR after the November 2015 order. I can also home in on the counties that took up in January 2017, when the AR program went statewide. Figure A4 maps both sets of counties in Kentucky.

The full county sample yields a sample size that is an order of magnitude higher. However, that sample necessitates dealing with staggered timing with a stacked approach. Moreover, the effects are stronger in the 2017 sample since AR going statewide was accompanied with administrative improvements. In fact, counties that already had AR in effect got an additional bump to unconditional release in 2017. This points to improvements in administration over time, which explains the dynamic effects in the main text results. The 2017 sample does not feature dynamic effects, as illustrated with raw data in Figure 4.

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

Figure A4: Kentucky county samples



A4 Are release effects due to logistics or bail conditions?

Calculating DD coefficients using simple means yields: 0.505 (unconditional release)[50] and 0.132 (one day).[51]

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

The change for the always takers is $[P(oneday|e,u,post) - P(oneday|e,u,pre)] - [P(oneday|ie,u,post) - P(oneday|ie,u,pre)] = 0.025$.[52].

The change for compliers is: $[P(oneday|e,\sim u,post) - P(oneday|e,u,pre)] - [P(oneday|ie,\sim u,post) - P(oneday|ie,\sim u,pre)] = 0.271$.[53]

Multiplying these by the estimated shares of each yields 0.14,[54], which is close to the 0.132 effect. Compliers are responsible for 96.5% of the effect in this estimation exercise.[55]

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

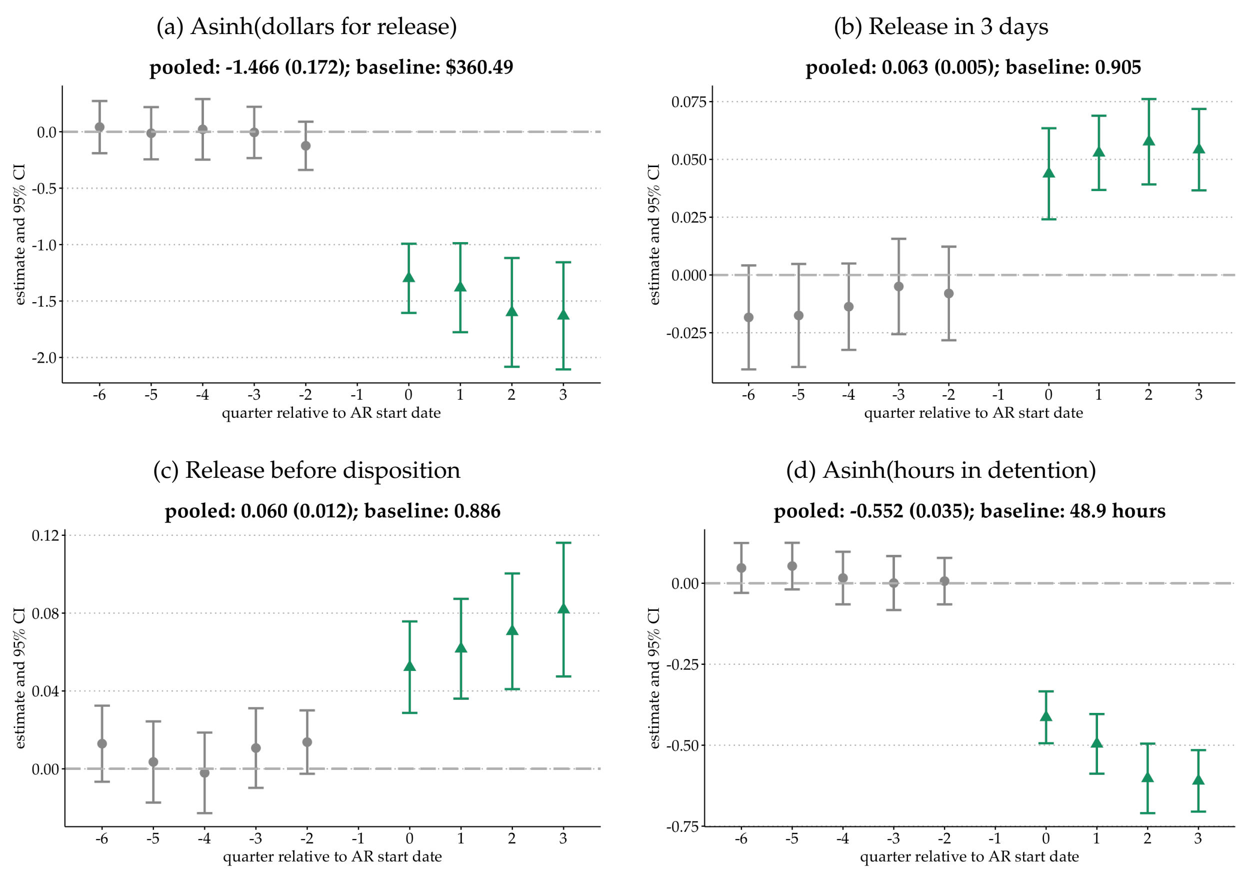
[50] $= (.755 - .197) - (.132 - 0.079)$
[51] $= (.906 - .766) - (.459 - .451)$
[52] $= (0.967 - 0.912) - (0.942 - .912)$
[53] $(0.967 - 0.723) - (.384 - .411)$
[54] $0.025 * 0.197 + 0.271 * 0.505$
[55] $(0.271 * 0.505)/(0.025 * 0.197 + 0.271 * 0.505)$

A5 Additional differences-in-differences outcome variables

I provide results on AR program effects for additional bail and release outcomes in Figure A5: 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.[56]

I provide results for misconduct outcomes in Figure A6: pretrial non-violent rearrest and pretrial violent rearrest.

Figure A5: How AR Impacts Bail and Release (Extra Outcomes)



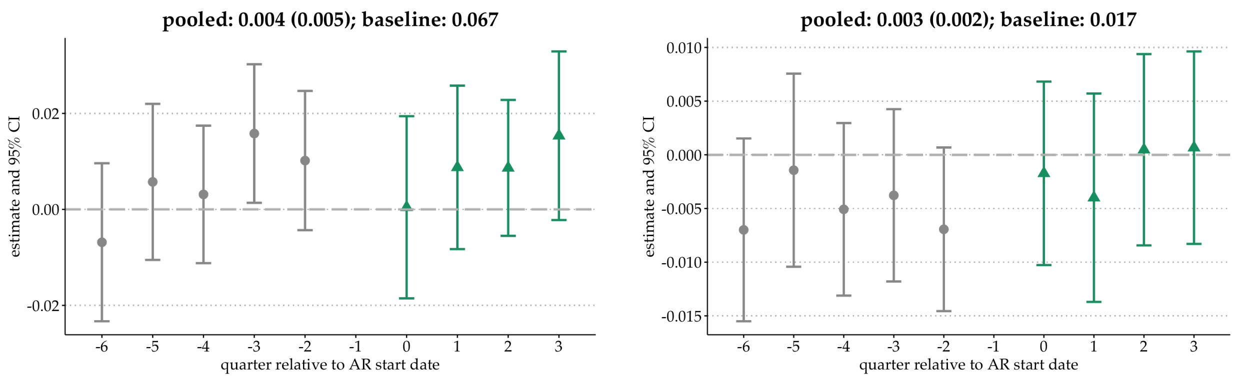
Notes: Figures A5a, A5b, A5c, and A5d plot the event-time differences-in-differences estimates using methods described in Section 3.3. The outcome variable for Figure A5a is the inverse hyperbolic of the money bail amount in dollars (0 if no money bail). The outcome variable for Figure A5b is an indicator for release within 3 days of booking. The outcome variable for Figure A5c is an indicator for release before case disposition. The outcome variable for Figure A5d 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.

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

Figure A6: How AR Impacts Misconduct (More Outcomes)

(a) Pretrial non-violent rearrest

(b) Pretrial violent rearrest



Notes: Figure A6a and A6b plot the event-time differences-in-differences estimates using methods described in Section 3.3. The outcome variable for Figure A6a is an indicator for pretrial non-violent rearrest. The outcome variable for Figure A6b 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.

A6 Comparing unsecured bail counties to money bail counties

Figure A7: County subgroup characteristics

Comparing characteristic means across county subsamples

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

Subset to AR eligible cases before AR implemented.

A7 Did judges correctly identify less risky cases before AR?

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

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

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

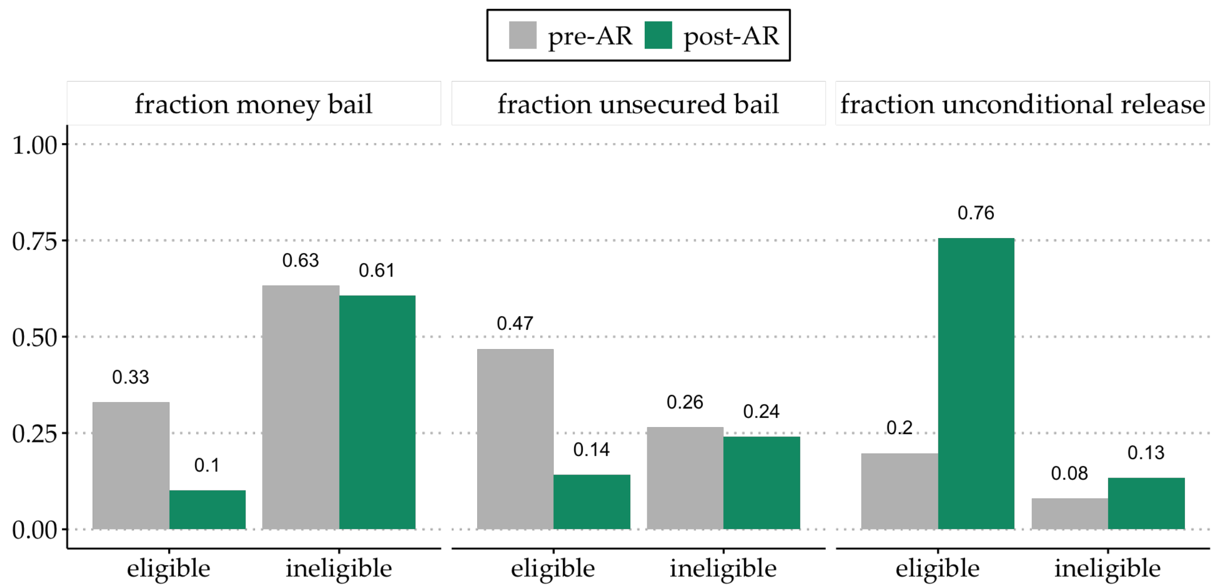
where $P(u,a)$ is the share of unconditional releasees who are always takers, and $P(u,c)$ is the share of unconditional releasees who are compliers. Since 0.196 of the eligible population received unconditional release before AR and 0.75 received unconditional release after, then the compliers failure to appear rate is 0.197. This means the compliers are around twice as risky on failure to appear than the always takers. As such, the always takers were correctly identified as less risky by judges even within the eligible case group.

A8 Bail Substitution Patterns

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

Never takers: The never-taker share is $P(B(1) \neq u|t_1, e_1)$.[57] Figure A8, generated with my data, shows that for t_1, e_1, $P(B(1) = u) = 0.76$, meaning the never taker share is 0.24. We know $P(B(1) \neq u|t_1, e_1) = P(B(1) = c|t_1, e_1) + P(B(1) = m|t_1, e_1)$. Figure A8 shows $P(B(1) = c|t_1, e_1) = 0.14$ and $P(B(1) = m|t_1, e_1) = 0.10$, so these are the u-never taker and s-never taker shares, respectively.

Figure A8: Bail types over subgroups



Always takers: The always-taker share is $P(D(0) = u|t_1, e_0) + P(D(0) = u|t_0, e_1) - P(D(0) = u|t_0, e_0)$.[58] Figure A8 shows this is 0.25.[59]

Compliers: The complier share is the remaining share of 0.51.[60] We know that the m-compliers and c-compliers shares will total to 0.51. Intuitively, substitution to unconditional release must come from equal substitution away from unsecured and money bail (due to the extended monotonicity assumption). Therefore,

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

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

[59] In words, the always taker share is the share ineligible-post receiving ROR plus the share eligible-pre receiving ROR less the share ineligible-pre receiving ROR. Plugging in descriptive statistics from Figure A8, $0.13 + 0.2 - 0.08 = 0.25$.

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

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

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

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

Plugging in from Figure A8,

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

The share of c-compliers is 0.31 and the share of m-compliers is 0.21.[61]

Summary: the shares over the relevant 5 groups are:

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

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

A9 Deriving potential incapacitation and deterrence effects

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

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

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

Unconditional release is 55 p.p. higher for the eligible group after the program and release after disposition is 7.6 p.p. higher.[62] As such, there are more people who are mechanically able to commit misconduct. Assuming that all the newly released people are newly released due to unconditional release receipt, I break down the post-AR FTA rate such that weights sum to the new total of people released pre-disposition as:

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

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

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

I restrict $\underbrace{(FTA|u,r)}_{\text{compliers, previously detained}} \geq 0.125$ since that was the failure to appear rate for those receiving financial conditions in the pre-period. Assuming those previously detained are at least as likely to fail to appear than we can assume $\underbrace{(FTA|u,r)}_{\text{compliers, previously released}}, \underbrace{(FTA|u,r)}_{\text{compliers, previously detained}} \geq 0.125$. As such, there is a possible range of values of deterrence and incapacitation effects, which I list in Table A9.

For incapacitation to be the sole source of the aggregate FTA effect, those newly released need to be around 6 times as risky as the always takers ($0.625 \approx 6 \times 0.125$). Even if the newly released

[62] The program increases release before disposition by 6 p.p. in the formal DD approach. See Appendix A5.

Figure A9: Table of possible deterrence and incapacitation effects

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

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