The Hidden Effects of Algorithmic Recommendations

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Abstract

Algorithms are intended to improve human decisions with data-driven predictions. However, algorithms provide more than just predictions to decision-makers — they often provide explicit recommendations. In this paper, I demonstrate these algorithmic recommendations have significant independent effects on human decisions. I leverage a natural experiment in which algorithmic recommendations were given to bail judges in some cases but not others. Lenient recommendations increased lenient bail decisions by 40% for marginal cases. The results are consistent with algorithmic recommendations making visible mistakes, such as violent rearrest, less costly to judges by providing them reputational cover. In this way, algorithms can affect human decisions by changing incentives, in addition to informing predictions.

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1 Introduction

Predictive algorithms are used in many high-stakes decisions. Algorithms predicting default are used in granting loans, algorithms predicting self-harm are used in mental health treatment, and algorithms predicting rearrest are used in criminal justice. Despite their prevalence, it is still the norm that humans (loan officers, therapists, judges) – not the algorithms – make the final decisions that govern outcomes. Therefore, understanding how algorithms change outcomes in these systems requires understanding how algorithms change human decisions.

The conventional wisdom is that algorithms impact human decisions because they provide decision-makers with data-driven predictions, but they can do more than that. They often give explicit recommendations: the loan algorithm can recommend rejection, the mental health algorithm can recommend hospitalization, and the pretrial algorithm can recommend release. These *algorithmic recommendations* are distinct from predictions; recommendations are the result of a normative mapping from predictions to actions, and many different recommendations can be consistent with identical underlying predictions. Despite the distinction between predictions and recommendations, they are usually conflated under the catch-all term of "algorithm." Therefore, most attempts to estimate the effects of algorithms muddle the impact of a new prediction technology with the impact of setting normative recommendations. In this paper, I disentangle the two to isolate the hidden effects of algorithmic recommendations on human decisions.

It is an empirical challenge to isolate the effects of algorithmic recommendations for a few reasons. For one, the institutional details around how predictive algorithms are developed and used in high-stakes settings are often opaque, which can impede careful study. Moreover, even if the details are transparent, algorithmic predictions and recommendations are often introduced simultaneously, which makes isolating the two difficult. I progress on this front by leveraging a unique setting in which algorithmic predictions were already present, but algorithmic recommendations were introduced. I highlight the importance of algorithmic recommendations by demonstrating their independent causal effects on high-stakes human decisions.

My empirical setting covers bail decisions in Kentucky from 2011 to 2013. Bail decisions are important in the US criminal legal system because they set the conditions for defendants' release from jail after arrest. For instance, it is common for judges to set money bail, which requires defendants to post money to be released from jail. During my study period, judges making money bail decisions received information on alleged incidents

and involved defendants. One piece of information available to them was an algorithmic prediction of pretrial misconduct (rearrest or failure to appear in court) for each case. Before June 2011, there were no recommendations based on these risk predictions. But, in June 2011, a new policy set recommendations for judges based on the predictions: judges were recommended to not set money bail when a case's predicted risk was low or moderate (rather than high). I call these recommendations "lenient bail" recommendations because not setting money bail is a more lenient decision than setting money bail. The institutional details yield useful variation for causal inference: lenient bail recommendations kicked in discontinuously across risk (under the "high" risk cut-off) and time (after June 2011).

Why might these recommendations change decision-maker behavior? I develop a model of bail decision-making in which judges make decisions based on their prediction of pretrial misconduct (a bad outcome) and the perceived costs of pretrial detention and misconduct. I demonstrate how introducing algorithmic recommendations changes judge behavior under two distinct theories. The first theory is that recommendations only impact decision-maker predictions of misconduct (as conventional wisdom assumes). The second theory is that recommendations can change the *cost* of misconduct because visible mistakes (e.g., a rearrest) become less costly for judges whose decisions adhere to recommendations and more costly for judges whose decisions deviate from them. (Decision-makers are less liable for mistakes when they go along with recommendations but more liable when they go against recommendations.) The two theories generate dueling testable predictions in the Kentucky empirical setting. If recommendations only change predictions, the new recommendations should have no effects on bail setting (because algorithmic predictions were already available to judges). But if recommendations change costs, then lenient bail should have increased for lower-risk cases (because they became newly covered by lenient recommendations).

To test these predictions, I estimate the causal effects of algorithmic recommendations. I leverage the fact that only some cases received lenient recommendations to implement differences-in-differences and differences-in-discontinuities designs. In the differences-in-differences approach, cases with low or moderate risk scores are the treated group because they experienced a change in recommendations at the policy date, while cases with high risk scores are the control group because they experienced no such change. The differences-in-differences design estimates causal effects for the entire distribution of cases in the low and moderate risk groups.

My other identification approach – differences-in-discontinuities – leverages the fact that the lenient recommendation kicks in at a sharp cut-off in the risk score distribution. After

June 2011, cases with the highest moderate risk scores received lenient recommendations, but similarly scoring cases with the lowest high risk scores did not. If the lenient bail recommendation were the only factor that changed discontinuously over the threshold during the post-period, then estimating a simple regression discontinuity would identify the desired lenient recommendation effect. However, other relevant factors changed discontinuously at that threshold as well. Since confounding factors around the threshold in the post-period were also present in the pre-period, I use a differences-in-discontinuities approach to recover the lenient recommendation effect. This method, in contrast to the differences-in-differences approach, estimates the effect of the lenient recommendation for marginal cases, those that are close to the critical moderate-high threshold.

Both the differences-in-differences and differences-in-discontinuities strategies leverage the fact that recommendations were introduced for some cases but not others. To correctly attribute these estimated effects to the causal effect of recommendations, it must be the case that at the time of the policy change, nothing else differentially impacted low and moderate risk cases relative to high risk cases. The calculation of risk was the same before and after the policy and risk levels were available in both periods, however, the policy also made it mandatory for judges to consider algorithmic predictions. Therefore, I take additional steps to align my estimated effects with the desired recommendation effects.

In the differences-in-discontinuity setting, I leverage a different discontinuity in the risk score distribution to estimate how often judges consulted risk levels before the policy and then adjust my estimates accordingly. I leverage the discontinuity at the low-moderate cut-off because risk levels change at this cut-off but recommendations do not (low and moderate risk cases receive the same recommendation). This procedure allows me to take the final necessary step from estimating a bundled treatment to an isolated treatment of the algorithmic recommendations.

In the differences-in-differences setting, I test whether recommendation effects matter in a unique subset of cases where the expected effect of risk levels is small. Specifically, I look at cases that are associated with misdemeanor charges and have zero associated risk factors (zero failures to appear, zero pending cases, zero convictions, etc.). Intuitively, the risk level does not provide new information to judges for these cases because they are obviously low risk. Therefore, the differences-in-differences result for this group should only capture the effect of algorithmic recommendations.

I find that algorithmic recommendations have independent effects on human decisions. First, I report my unadjusted differences-in-differences and differences-in-discontinuities

results, which are necessarily an upper-bound on the isolated effect of recommendations. The 2011 policy change increased lenient decisions by 50% for low and moderate risk cases. There is no evidence of pre-trends in the differences-in-differences approach, and results are nearly identical regardless of which controls (if any) are included in the specifications. The relative effects are similar at around 50% for the marginal moderate risk cases when using differences-in-discontinuities. These unadjusted results suggest that introducing algorithmic recommendations changes how prediction technologies impact human decisions.

I then adjust and test my original estimates to account for the fact that the 2011 policy might have also changed the visibility of risk levels. In the differences-in-discontinuities case, I can isolate the component of the original estimate that is the causal effects of algorithmic recommendations. I find that risk levels were used in about 80% of cases in the pre-period. Adjusting my original estimates, this finding implies that lenient recommendations increased lenient bail by 40% for marginal cases. Therefore, the majority of the original effects are attributable to the independent causal effects of algorithmic recommendations. In the differences-in-differences case, I focus on obviously low risk cases (misdemeanors with zero risk factors) to isolate an effect of algorithmic recommendations. Consistent with meaningful effects of algorithmic recommendations, I find that lenient bail increases by about 15 percentage points for this low-risk group. Overall, I show algorithmic recommendations have economically important effects on human decisions, and these effects are independent of any changes related to algorithmic predictions.

Bringing these results back to my testable predictions, my results are consistent with the theory that algorithmic recommendations change decision-maker costs (and therefore their incentives). In particular, lenient choices may become less costly when they adhere to lenient recommendations. If a lenient choice results in bad outcomes, some of the blame goes to the recommendation designer (the House legislature in the Kentucky case) rather than all of the blame going to the individual decision-maker (the judge in the Kentucky case). In this way, the communication of predictive algorithms can change human decisions by shifting incentives in addition to directly providing new prediction information.

Related literature: My paper contributes to the existing literature on algorithms and human decisions. When studying the effects of algorithms, researchers often contrast a world without algorithms, where humans have complete discretion, to one with algorithms where there is no human discretion (Berk 2017; Mullainathan and Obermeyer 2022; Kleinberg, Lakkaraju, et al. 2018; Cowgill 2018a). However, since humans usually make the final decisions even when algorithms are present, this is often not the policy-relevant comparison.

With that in mind, a growing literature compares outcomes in the absence of algorithms to outcomes when human decision-makers use algorithms at their discretion (Sloan, Naufal, and Caspers Forthcoming; Stevenson 2018; Stevenson and Doleac Forthcoming; Garrett and Monahan 2018; DeMichele et al. 2018; Cowgill and Tucker 2019; Davenport 2023). How algorithms are integrated into human decision-making varies greatly across these settings. For instance, some settings provide decision-makers with algorithmic predictions only, while others also provide explicit algorithmic recommendations. My paper demonstrates that these specifics matter for the causal effects of algorithms. In particular, algorithmic recommendations matter: they have independent effects on human decisions and merit independent attention in policy discussions and research.

My paper is one of the first to focus directly on the causal effects of algorithmic recommendations. In doing so, I complement two concurrent papers that also study these recommendations. First, McLaughlin and Spiess (2022) investigate the distinction between algorithmic predictions and recommendations by developing a theoretical model in which algorithmic recommendations may directly change preferences (rather than only changing beliefs).[1] While they develop theoretical results showing how recommendations can have independent effects on human decisions, I demonstrate these independent effects in practice. Second, Hausman (2024) studies how changes to algorithmic recommendations changed human decisions in another context: US Immigration and Customs Enforcement (ICE). He finds that ICE release rates decreased by 50% after release recommendations were removed – a result that is consistent with the large effects in my setting. However, ICE's algorithmic predictions changed at the same time as the recommendations, so it is not possible to disentangle the two effects in Hausman (2024)'s setting. My paper, therefore, progresses the evidence by isolating the causal effects of algorithmic recommendations.

I leverage a 2011 policy change in Kentucky to study the effects of algorithmic recommendations. This 2011 policy was first studied by Stevenson (2018) with interrupted time series methods. Her paper was one of the first to discuss the important distinction between how algorithms change outcomes in theory and practice. While Stevenson (2018) studied the effects of the 2011 policy, I leverage the policy to study the effects of *algorithmic recommendations*. Therefore, instead of demonstrating the effects of a treatment bundle (a few things changed with the 2011 policy), I isolate the effects of my treatment of interest

[1] A number of other papers discuss the idea that algorithms in general may change decision-maker incentives (Davenport 2023; Stevenson and Doleac Forthcoming; Stevenson and Doleac 2023), but McLaughlin and Spiess (2022) is unique in that they discuss algorithmic recommendations changing these incentives. In related work, Almog et al. (2024) studies how AI oversight changes the costs of errors to decision-makers. When decisions can be publicly overruled by AI, mistake costs increase.

(algorithmic recommendations only). Doing this requires a series of novel steps: using court reports to find a time period when the calculation of algorithmic predictions was unchanged but recommendations changed, using court report documents to calculate cases' underlying risk scores, and using data-driven approaches to address confounding policy changes (changes to arrests and changes to risk level visibility).

The algorithmic recommendations studied in this paper are closely related to simpler prediction tools used in the criminal justice system for many decades: sentencing guidelines. Sentencing guidelines were a tool used by legislatures to "[impose] structure on judicial discretion"; a similar idea underlies the design of algorithmic recommendations in the modern era (Bushway, Owens, and Piehl 2012). Bushway, Owens, and Piehl (2012) studied the causal effects of sentencing guidelines, holding other case characteristics constant. The authors leveraged calculation mistakes for identification and found that erroneously high or low recommendations have causal effects on sentencing. My paper shows that, even as prediction tools get more complex, advisory recommendations continue to have independent causal effects in the justice system.

More broadly, my paper contributes to a wide interdisplinary literature on how people use discretion when given algorithms. Ethnographic work demonstrates a "decoupling" between how algorithms are expected to be used and how they are used in practice (Christin 2017; Pruss 2023). Decision-makers frequently overrule algorithm recommendations (Hoffman, Kahn, and Li 2017; Gruber et al. 2020; Agarwal et al. 2023; Angelova, Dobbie, and Yang 2023) and may respond to algorithms differently according to the age or socioeconomic status of the people about whom they are making decisions (Skeem, Scurich, and Monahan 2019; Stevenson and Doleac Forthcoming).

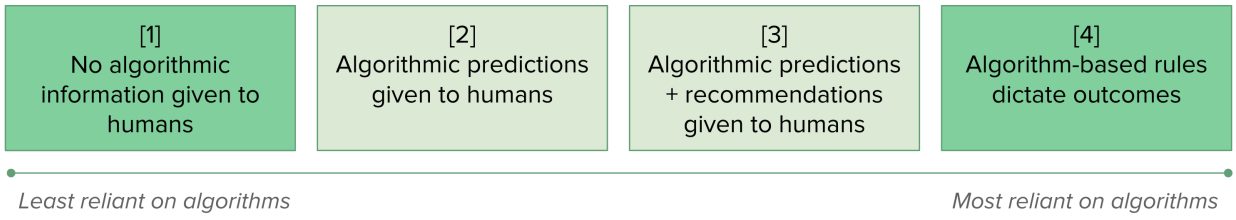
Roadmap: The remainder of the paper proceeds as follows. Section 2 provides background on algorithms and decision-making to highlight the underappreciated role of algorithmic recommendations. Section 3 describes my empirical setting – bail decisions in Kentucky – and the variation used for identification. Section 4 develops a toy model, which generates testable predictions to differentiate between dueling theories of the effects of algorithm recommendations. Section 5 describes the administrative court data. Section 6 presents my results, which isolate the causal effects of algorithmic recommendations. Section 7 concludes.

2 Background on Algorithms and Decisions

How do algorithms change decisions? The answer depends on the differences between the status quo and the new algorithm-based decision-making system. Algorithm-based decision-making systems vary widely. They include systems in which algorithm-based rules fully dictate decisions as well as systems in which humans are given some information from an algorithm with no direction on how to use the information. There is no singular algorithmic decision-making system – there is a spectrum of them.

In fact, Congress's Algorithmic Accountability Act defines an "automated decision-making system" as "a computational process, including one derived from machine learning, statistics, or other data processing or artificial intelligence techniques, that makes a decision or facilitates human decision making" (Lum and Chowdhury 2021). This definition includes decisions strictly dictated by algorithm-based rules as well as decisions weakly informed by algorithm information. It is vague enough to include many types of algorithmic decision-making environments.

Figure 1: Spectrum of Algorithm-Based Decision-Making Settings



Notes: This figure illustrates a theoretical spectrum of algorithm-based decision-making systems. There are four settings illustrated. Going from left to right, they are ordered from least to most reliant on algorithms.

In Figure 1, I illustrate a spectrum of algorithm-based decision-making settings to make the differences across potential settings explicit. From left to right, I list four settings, from least to most reliant on algorithms. In dark green, on the ends, are the two extremes: (1) no algorithmic information given to humans and (4) algorithm-based rules dictate outcomes. In the middle, in light green, are the two intermediate settings in which humans make the final decisions, but they have some information from an algorithm. In (2), human decisions are given information on algorithmic predictions but no algorithmic recommendations. In (3), decision-makers are also given algorithmic recommendations.

Using the spectrum shown in Figure 1, I can spatially situate different strands of the literature on algorithms and decision-making. Research showing that algorithms alone can outperform human decision-makers (Berk 2017; Mullainathan and Obermeyer 2022;

Kleinberg, Lakkaraju, et al. 2018; Cowgill 2018a) contrasts (1) with (4). Meanwhile, research showing how outcomes change when humans are given algorithms but have discretion (Sloan, Naufal, and Caspers Forthcoming; Stevenson 2018; Stevenson and Doleac Forthcoming; Garrett and Monahan 2018; DeMichele et al. 2018; Cowgill and Tucker 2019; Davenport 2023) contrasts (1) with either (2) or (3). My paper contributes to the literature by studying the underappreciated distinction between (2) and (3). In doing so, I highlight the hidden effects of algorithmic recommendations implicit in previous research.

2.1 Algorithms and Bail Decisions

Algorithms in the criminal justice system are prevalent and varied. They are used in pretrial risk assessment, sentencing, prison management, and parole. In a survey of state practices, the Electronic Privacy Information Center (2020) found dozens of different algorithms used in criminal justice systems across the country. Every state uses one in some capacity. These algorithms generally predict types of risk based on individual-level and case-level characteristics. For example, the Public Safety Assessment, used in over 40 counties, calculates pretrial misconduct risk by adding up integer weights based on nine risk factors (Laura and John Arnold Foundation 2018). The tool derives these weights by regressing misconduct measures on a slate of case-level characteristics in a dataset of 750,000 observations (Laura and John Arnold Foundation 2018). Meanwhile, the more complicated COMPAS algorithm, which also calculates pretrial misconduct risk, has hundreds of inputs and is a black-box machine learning model (Angwin et al. 2016; Stevenson and Slobogin 2018).

In the pretrial setting, algorithms are meant to help make bail decisions. After arrest, judges decide how to set bail for the arrested person. The bail decision stipulates the conditions the arrested person must meet for release from jail. There are a few reasons why bail is an important setting for studying algorithms' effects. For one, bail decisions directly affect pretrial detention, which has downstream effects on future outcomes, such as the likelihood of conviction (Dobbie, Goldin, and Yang 2018; Cowgill 2018b). Moreover, pretrial detainees "account for two-thirds of jail inmates and 95% of the growth in the jail population over the last 20 years," and as a result, bail decisions and subsequent pretrial detention have been a substantive contributor to US mass incarceration (Stevenson and Mayson 2018). Bail is also a promising environment for study because bail decisions are made quickly (in a matter of minutes), and the legal objective is well defined (Arnold, Dobbie, and Yang 2018). The legal objective of bail is to set the lowest possible bail to

ensure court appearance and public safety (American Bar Association Criminal Justice Standards Committee 2007). In this context, algorithms are designed to predict the risk of pretrial misconduct (failing to appear in court or rearrest).

While algorithms can vary greatly in how they predict misconduct, they share a common goal. The goal is to provide a "data-driven way to advance pretrial release." In other words, the goal is to reduce judges' prediction errors, allowing for the release of more people without compromising on misconduct. Prior research on risk assessments in bail settings highlights their potential in this regard. Kleinberg, Lakkaraju, et al. (2018) find that if bail decisions were delegated to a predictive algorithm, jail populations could be reduced by 42%, with no change in crime rates. A strictly preferred combination of jailing and crime rates is possible simply through better (algorithm-based) decision-making. However, algorithms' predictions give information about the ranking of individual cases by risk; they do not give information about which jailing rate judges should pick.

Bail is not something in fixed supply that judges simply allocate. Rather, judges pick the rate of bail setting (i.e., what percentage of the population receives money bail). This dimension of choice can be absent in high-stakes environments where algorithms are involved only in allocation. For instance, in the coordinated entry system, people are scored (according to housing need or readiness) and then ordered on a list by their score. The available housing is then allocated down the list by score until the housing runs out. The housing supply in that context is fixed; the algorithm cannot change that margin. In contrast, in the bail system, changing decision-making environments can change allocation (who gets which bail decisions) and the overall rate of bail settings (what percentage of defendants receive money bail).

3 Empirical Setting: Bail Decisions in Kentucky

In general, algorithmic predictions and recommendations are often introduced simultaneously, which makes it difficult to isolate the effects of recommendations. However, in Kentucky, algorithmic predictions were used both before and after the introduction of algorithmic recommendations. The nature of the introduction of algorithmic recommendations in Kentucky, therefore, provides a unique opportunity to estimate the independent effects of algorithmic recommendations.

The algorithmic predictions: From March 18, 2011 to June 30, 2013, Kentucky used one fixed algorithm to make predictions about cases. It was called the Kentucky Pretrial Risk

Assessment (KPRA) and made predictions about pretrial misconduct (rearrest or failure to appear in court).

Kentucky Pretrial Services created the KPRA in-house, fitting a regression model to predict pretrial misconduct using the existing Kentucky administrative data. The KPRA was not a complex black-box machine learning tool. Rather, it was a checklist tool that added points based on "yes" or "no" answers to a series of questions. The total number of points was then converted to score levels of "low," "moderate," or "high." Totals of 0-5, 6-13, and 14-24 corresponded to low, moderate, and high levels, respectively. During bail phone calls, pretrial officers told judges these risk levels rather than the underlying number of points.

The factors in the KPRA were mostly criminal history elements (e.g., prior failure to appear, pending case). The factors also include information about the current charge (e.g., whether the charge is a felony of class A, B, or C) and the defendant's personal history (e.g., verified local address, means of support).[2]

The algorithmic recommendations: In response to significant increases in the incarcerated population between 2000 and 2010, Kentucky House Bill 463 (HB463) went into effect on June 8, 2011. The law recommended release without the requirement to post money, "lenient bail," for defendants with low or moderate risk scores.[3] HB463 introduced recommendations for low and moderate risk cases but not for high risk cases. Importantly, the policy change did not change the calculation of the risk scores or levels; it introduced recommendations for how to use them.

Bail decisions before June 2011: In the pre-period, bail decisions were made as follows. After a defendant was booked into jail, a pretrial services officer (an administrative court employee) interviewed the defendant to collect information and calculate a risk score (the algorithmic prediction of misconduct risk). Within 24 hours of booking, the officer presented information about the defendant and the alleged incident to a judge over the phone. One piece of information that could be discussed was the KPRA risk level. After hearing this information, the judge made a bail decision in a few minutes.[4]

Bail decisions after June 2011: In the post-period, bail decisions were made in a similar

[2]See Appendix A.1.2 for more background and details on risk calculation in Kentucky.

[3]Judges' bail decisions determine conditions for people's pretrial release from jail. These conditions are frequently financial and require defendants to post some money for release from jail. Judges can choose not to require money for release, which is a more lenient decision. Throughout this paper, I discuss judges setting "lenient bail," which means not requiring money for release, or "harsh bail," which means requiring money for release or detention outright.

[4]See Appendices A.1.1 and A.1.3 for more background on the Kentucky bail setting.

way as before, but with two changes. First, the new recommendations were part of the judge conversations: cases with low or moderate KPRA risk levels were recommended lenient bail. Second, risk levels were a mandated part of the conversation (previously, they had been optional).

If judges wanted to override the recommendation, they could do so easily by providing a reason. In practice, this was as simple as saying a few words (e.g., "flight risk") to the pretrial officer on the phone. The policy change did not set a recommendation for high risk defendants. Therefore, the policy introduced a recommendation (lenient bail) for some defendants (people with low or moderate risk scores) but not others (people with high risk scores).

4 A Toy Model and Theoretical Predictions

In this toy model, I demonstrate the empirical predictions of introducing algorithm recommendations based on two distinct theories (whether they have incentive effects or prediction effects). This framework clarifies why we might or might not expect recommendations to change human decisions.

Status quo set-up: Under the status quo, judges make bail decisions using information about the case and algorithm predictions about misconduct.

The legal objective of bail is to set the lowest possible bail to ensure court appearance and public safety. To map onto the empirical setting, let judges choose whether to set money bail (or harsh bail: $b = h$) or no money bail (or lenient bail: $b = l$) for defendants. If the judge sets harsh bail, there is some probability the defendant is detained $Pr(d|b = h)$, and there is some probability the defendant is released $1 - Pr(d|b = h)$. If the defendant is detained, the judge incurs a cost $c(d|b = h)$, which is the financial cost of detaining someone in jail. If the defendant is released, they may commit misconduct with probability $Pr(m|b = h)$. If they don't commit misconduct, the judge faces no costs. If they do, the judge faces cost $c(m|b = h)$, which is the cost of misconduct, given the choice of harsh bail. In total, the judge incurs cost

$$C(b = h) = Pr(d|b = h)c(d|b = h) + (1 - Pr(d|b = h))Pr(m|b = h)c(m|b = h).$$

If the judge sets lenient bail, they incur costs based on the probabilities and costs of detention and misconduct again. However, there is no capacity for detention, so only

misconduct probabilities and costs show up:

$$C(b = l) = Pr(m|b = l)c(m|b = l).$$

How are probabilities and costs determined? We assume that costs to judges are solely the reputational blowback to their decision-making. They do not face costs when the public can validate their choices as correct. When judges set harsh bail and the defendant commits misconduct, the choice is seen as correct, which means they will not face any misconduct-related consequences for being harsh. Therefore, $c(m|b = h) = 0$. Accordingly, the expression for judge costs under harsh bail simplifies to

$$C(b = h) = Pr(d|b = h)c(d|b = h).$$

On the other hand, judges face blowback for setting lenient bail for people who commit misconduct, because the choice looks like a mistake, meaning $c(m|b = l) >> 0$. Meanwhile, there is no way for anyone to assess whether harsh bail was correct when defendants are detained, because they cannot commit misconduct mechanically. Therefore, $Pr(d|b = h) \neq 0$.

How do judges predict $Pr(m|l)$? They have a vector of case information X and algorithm-based risk level information. The risk level information is a mapping from $Pr^A(m|l)$ (the algorithm's prediction of misconduct under lenient choice) to r^A. Risk levels provide relative risk information rather than absolute risk information.[5] To align with the empirical environment, we assume $r^A \in \{low, moderate, high\}$. The judge prediction is some function of observables and the algorithm's risk level: $Pr(m|l) = f(X, r^A)$.

Therefore, judges choose to set bail based on the following threshold rule:

$$b = \begin{cases} h, & \text{if } \frac{c(d|b=h)}{c(m|b=l)} < \frac{Pr(m|b=l)}{Pr(d|b=h)}, \\ l, & \text{otherwise.} \end{cases} \tag{1}$$

Adding algorithmic recommendations: Now, I complicate the status quo set-up by introducing algorithmic recommendations. Call the algorithmic recommendation R. I define R to align with the recommendation introduced in my empirical environment, as

[5] I make this assumption to fit with common practice in the real world and in my empirical setting. If one case is "low risk" while another is "moderate risk," it is unknown what probabilities of misconduct these levels imply; however, it is clear that the "moderate risk" case has a higher predicted probability of misconduct than the "low risk" case.

described in Section 3. Therefore, it is based on algorithmic risk level r^A as follows:

$$R = \begin{cases} b = l, & \text{if } r^A \in \{low, moderate\}, \\ -, & \text{otherwise.} \end{cases} \tag{2}$$

In words, lenient bail is recommended to judges if the risk level is low or moderate. There is no recommendation otherwise. How does R impact judges' decisions?

- **If all recommendations do is inform judge predictions,** then the recommendation of lenient bail ($R = b = l$) communicates to the judge that the risk level is low or moderate ($r^A \in \{low, moderate\}$). However, under the status quo, judges already know risk levels and have integrated that information into misconduct predictions (since $Pr(m|l) = f(X, r^A)$). So, if the only channel through which recommendations matter is revealing algorithmic predictions, then we would expect no change to judge decisions in this setting.
- **If recommendations instead change payoffs**, the predictions are different. If recommendations change payoffs, then $c(m|l)$ becomes $c(m|l, R)$. Assume, in line with anecdotal evidence, that it is less costly to make a mistake when that mistake is consistent with a recommendation (because there is less liability). Then, $c(m|b = l, R = b = l) < c(m|b = l)$. Similarly, making a mistake that goes against a recommendation is more costly since this is seen as "going rogue." Then, $c(m|l, R = b = h) > c(m|l)$. In this case, judges choose to set bail based on two distinct threshold rules (one for when the recommendation applies and one for when the recommendation does not apply):

$$b = \begin{cases} R = b = l, \begin{cases} h, & \text{if } \frac{c(d|b=h)}{c(m|b=l,R=b=l)} < \frac{Pr(m|b=l)}{Pr(d|b=h)}, \\ l, & \text{otherwise;} \end{cases} \\ R = -, \begin{cases} h, & \text{if } \frac{c(d|b=h)}{c(m|b=l)} < \frac{Pr(m|b=l)}{Pr(d|b=h)}, \\ l, & \text{otherwise.} \end{cases} \end{cases} \tag{3}$$

Because $c(m|b = l, R = b = l) < c(m|b = l)$, the cost-ratio threshold when the recommendation applies ($\frac{c(d|b=h)}{c(m|b=l,R=b=l)}$) is larger than the cost-ratio under the status quo ($\frac{c(d|b=h)}{c(m|b=l)}$). In effect, the cost threshold shifts right under the lenient recommendation, making harsh decisions less frequent and lenient decisions more frequent.

Dueling predictions: Therefore, this toy model generates dueling predictions. If recom-

mendations only serve to communicate algorithmic predictions, then they should have no effects in my empirical setting. If they change payoffs to decision-makers, they should increase lenient bail setting when the recommendation applies.

Anecdotal evidence on liability and algorithmic recommendations: Recommendations can change misconduct costs in two ways. First, lenient recommendations may make lenient decisions less risky for decision-makers. The algorithm designer who sets the recommendation – in the Kentucky case, the state legislature – provides reputational cover to the judges. If someone commits misconduct, judges can point out that the lenient decision followed recommendations out of their control. Judges have made statements in court to this effect. For instance, in New York City, where there have been recent attempts at bail reform, judges "routinely stated that they only ordered people to be released ... because the law forced them to" (Covert 2022). Making such statements is a way to signal that lawmakers, not the judges, should be responsible for subsequent pretrial misconduct outcomes.

Second, suppose the recommendation is detention (harsh bail) and the judge releases the defendant (the judge is lenient). In that case, the judge sticks their neck out more than they would have without a recommendation. If a defendant commits misconduct, the judge could face higher costs through increased scrutiny and political backlash (loss of a future election).[6] This theory aligns with recent events in the US. The Milwaukee DA faced calls for removal after setting low bail for a person who later committed a violent crime, killing six people. Part of the political backlash was because the bail decision was "not consistent with ... the risk assessment of the defendant prior to the setting of bail" (Fung 2021). Similar anecdotal evidence exists in other contexts. For instance, medical professionals have expressed hesitation to deviate from algorithmic recommendations because of concerns around increased liability. As one school therapist put it, "You have this thing telling you someone is high risk, and you're just going to let them go?" (Khullar 2023).

5 Kentucky Administrative Court Data

I use administrative court data from Kentucky's Administrative Office of the Courts, which covers all criminal cases with felony- or misdemeanor-level charges in the state. I use the

[6] Angelova, Dobbie, and Yang (2023) find that judges make harsher decisions after an unrelated local defendant is arrested for a violent crime. This result could also be consistent with an error cost mechanism. If salient misconduct increases public scrutiny of lenient judges, then lenient choices become more costly to judges, which reduces their prevalence.

raw data to construct my final dataset using the following steps.

i. Defining the appropriate observation level: The raw data consists of many datasets at different levels of observation. My desired observation level is at the case-level. Since there can be multiple charges in a case, multiple cases in an pretrial interview, and multiple bail decisions (over time) for a cases, I take the following steps to define an interpretable and relevant level of observation. First, I aggregate data on charges up to the case level. Second, I subset to pretrial interviews with defendants where one case is at issue. (This is necessary to think about bail decisions that apply to a single well-defined case rather than a potential bundle of cases.) Third, I focus on the first bail setting for each case, commonly called initial bail.

ii. Sample restrictions: I impose several sample restrictions. First, I limit the sample to initial bail decisions made by district judges between March 18, 2011, and June 30, 2013. I make this restriction because that is the time period during which (a) the KPRA was used and (b) its calculation did not change. As such, there is no change to the calculation of risk levels during my chosen study period.

Second, I impose restrictions to eliminate concerns that HB463 changed the sample composition itself. One challenge to studying a change resulting from HB463 is that it was a large bill of about 150 pages and 110 sections (Kentucky Legislature 2011). The bill introduced more policy changes beyond introducing algorithmic recommendations to bail decision making. Therefore, a key empirical concern is incorrectly attributing estimated effects to the recommendations when they are instead due to concurrent policy changes.

Qualitative review of the bill, paired with interviews with practitioners, pinpointed a change to policing in the bill that is a potential empirical concern. According to a memo from the Louisville chief of police, the bill amended existing law "by requiring law enforcement officers to issue citations instead of making physical arrests" for many misdemeanor offenses.[7] In other words, some misdemeanor offenses may have no longer resulted in arrest after HB463.

To address any resulting change in the composition of cases, I omit cases from my sample that were supposed to result in citation after HB463 according to the bill's language. Therefore, my sample of cases excludes the group that could have been simultaneously impacted by policing policy changes. I use "Standard Operating Procedures" documentation (SOP

[7] However, there are exceptions to this requirement "which still allow officer discretion to make a physical arrest for certain offenses." The referenced memo is from Robert C. White on June 2, 2011, "Re: SOP 10.1, Enforcement - Revised General Order #11-013."

10.1) from the Louisville Metro Police Department to identify the relevant cases based on underlying charge codes. In short, I restrict my sample to cases with offenses that were arrestable before and after HB463.

iii. Constructing risk scores: The raw administrative data do not include the underlying KPRA risk scores; they only include the risk levels. However, the administrative data do have information on all the components are used to calculate risk scores. I use observation of these components combined with Austin, Ocker, and Bhati (2010)'s explanation of the corresponding weights to construct the underlying scores. Table A.1 demonstrates the weights and the components.

Therefore, as the researcher, I observe risk scores while judges observe only the more discretized risk levels. This granularity is necessary for my differences-in-discontinuities identification strategy.

Final dataset: The resulting dataset consists of about 131,000 observations. Each row is an observation at the case level and contains information about the defendant, the relevant charges, the initial bail decision, the bail judge, and the algorithm-based risk components, scores, and levels. As usual in the pretrial context, the administrative data do not indicate what specific information judges and pretrial officers discussed in each bail decision.

Motivational Descriptive Statistics: Figure A.1 demonstrates the distribution of risk scores across all cases in the administrative data. The distribution skews low risk: 90% of cases are in the low or moderate categories. Therefore, 90% of cases receive the lenient bail recommendation after the introduction of recommendations. However, only 32% of cases received lenient bail before the introduction of recommendations. Therefore, the new recommendations set a much lower threshold for lenient bail than implicitly existed beforehand. If the state wanted to set a threshold to align with the pre-existing level of bail setting, the lenient recommendation would have kicked in for cases with scores below 4 rather than below 14.

The chosen recommendation threshold was a normative decision on the part of the state rather than a natural consequence of any underlying risk-scoring system. Recall that many different decision thresholds are consistent with the same underlying risk rankings. In this way, algorithmic recommendations can be thought of as a form of what Cowgill and Stevenson (2020) call "algorithmic social engineering" – recommendations are derived from predictions, but manipulated to reflect some algorithmic designer's perspective.

The chosen threshold of 14 suggests that the state wanted judges to set lenient bail more frequently than they were doing under the status quo. Conversely, if the state had chosen a

threshold of 2, that would suggest the state wanted judges to set lenient bail less frequently. Both thresholds (14 and 2) are defined based on the same underlying algorithmic predictions, but have very different policy implications. While many researchers focus on how algorithms can change decisions in a "locally optimal" (or an "allocative") sense (Kleinberg, Lakkaraju, et al. 2018), these descriptive statistics hint at how algorithms may change the overall composition of decisions because of explicit algorithmic recommendations.

6 Algorithmic Recommendations Change Judge Decisions

To study the effects of algorithmic recommendations, I leverage a policy change implemented in June 2011 that impacted bail decisions in Kentucky. As a result of the policy, judges were given explicit recommendations on setting bail. The new recommendation was to set lenient bail (no money bail) for cases with low or moderate risk levels.

I leverage the fact that only some cases received lenient recommendations to implement both (a) differences-in-differences and (b) differences-in-discontinuities approaches. These estimated effects are the causal effect of recommendations if nothing else differentially impacted low and moderate risk cases relative to high risk cases at the time of the policy. Risk level calculation was the same before and after the policy and risk levels were available in both periods, however, the policy made it mandatory for judges to consider these algorithmic predictions. Therefore, I take additional steps to test and adjust the estimated effects to align with the desired recommendation effects.

In Section 6.1, I demonstrate the straight-forward (naive) differences-in-differences and differences-in-discontinuities results. In Section 6.2, I address concerns about potential confounding related to the usage of risk levels with two different approaches. In the end, I find that the majority of the naive (unadjusted) effects are attributable to the independent causal effects of algorithmic recommendations. Lenient recommendations increase judges' lenient bail decisions by 40% for marginal cases.

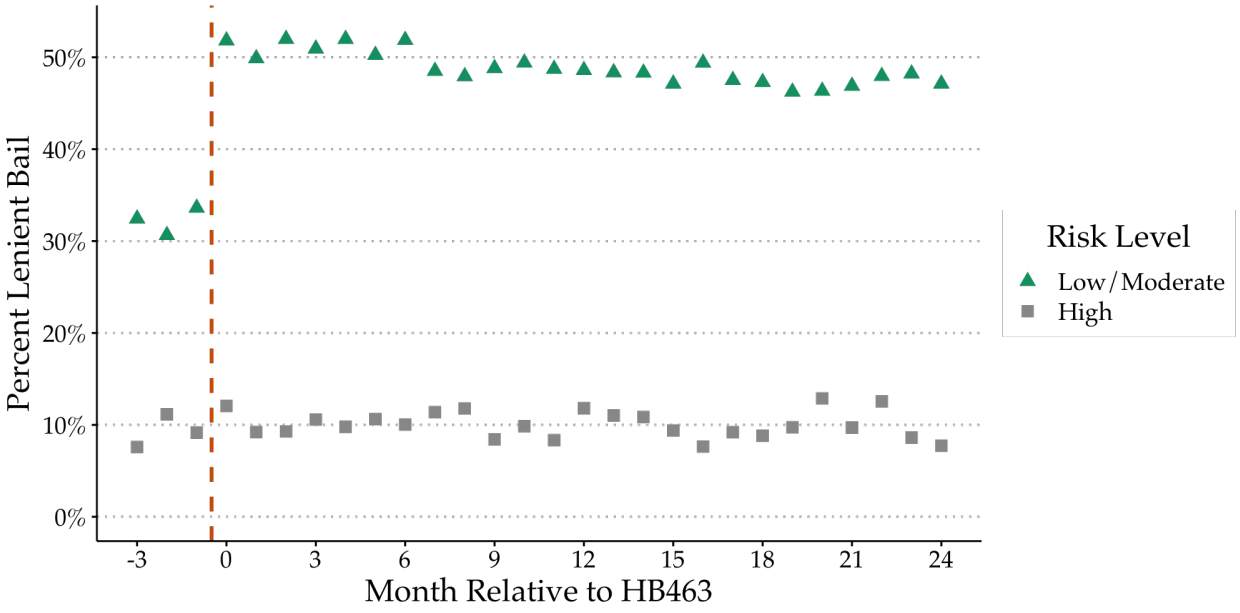
6.1 Naive Estimates

6.1.1 Differences-in-Differences Results

In my differences-in-differences framework, high risk cases are the control group because they experience no change in recommendations. In contrast, low and moderate risk cases are the treatment group because they experience a change in recommendations. Figure 2 illustrates the rate of lenient bail for low or moderate risk cases and high risk cases over

time.[8] Once recommendations go into effect, there is a stark increase in lenient bail for low/moderate cases of about 15-20 percentage points. There is no similar increase for the high risk group. The underlying assumption of using a differences-in-differences approach is parallel pre-trends. The raw visual evidence in Figure 2 provides promising evidence for this assumption.

Figure 2: Lenient Bail Rates by Risk Level over Time



Notes: This figure shows the rate of lenient bail over time by risk level groups. Months are indexed relative to the introduction of algorithmic recommendations. Cases with low and moderate risk level (risk scores below 14) are shown as green triangles, while cases with high risk level (risk scores at or above 14) are shown as gray squares. The orange dotted line shows when HB463 went into effect.

To formally estimate causal effects and test for pre-trends, I estimate a standard specification of the form

$$lenient_{itj} = \sum_{m \neq -1} [\beta_m \times I(score_i < 14)] + X_{itj} + \epsilon_{itj}, \tag{4}$$

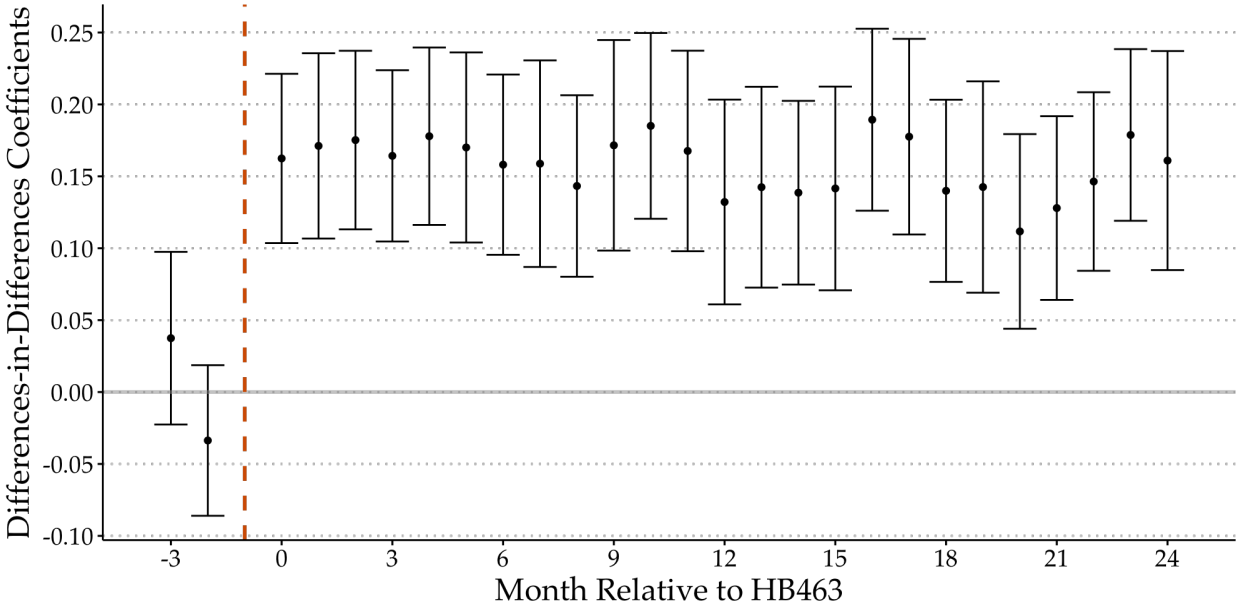
where $lenient_{itj}$ is an indicator for if the bail for case i at time t decided by judge j is lenient (no money bail) and $I(score_i < 14)$ is an indicator for if the risk score for case i is below 14, meaning the risk level is low or moderate (rather than high). Distinct coefficients are estimated for each month m relative to HB463 adoption, and $m = -1$ is the omitted group.

[8]Note that there are only a few pre-policy time periods because the method of calculating the KPRA risk score changed in March 2011, as described in Section A.1.2. To keep the meaning of risk scores and risk levels consistent, I exclude data from before March 2011 when constructing the analysis dataset (as previously detailed in Section 5).

I include a vector of controls X_{itj}, which includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, county, and other risk score components listed in Table A.1. I cluster standard errors by judge.

Figure 3 shows the dynamic differences-in-differences coefficients by plotting the values of β_m. Before the recommendation introduction, the coefficients are close to zero and do not demonstrate evidence of pre-trends. The results are not sensitive to the choice of control variables. Figure A.2 shows that results with zero detailed controls are nearly identical to those with controls based on all observed case variables.

Figure 3: Dynamic Differences-in-Differences Estimates



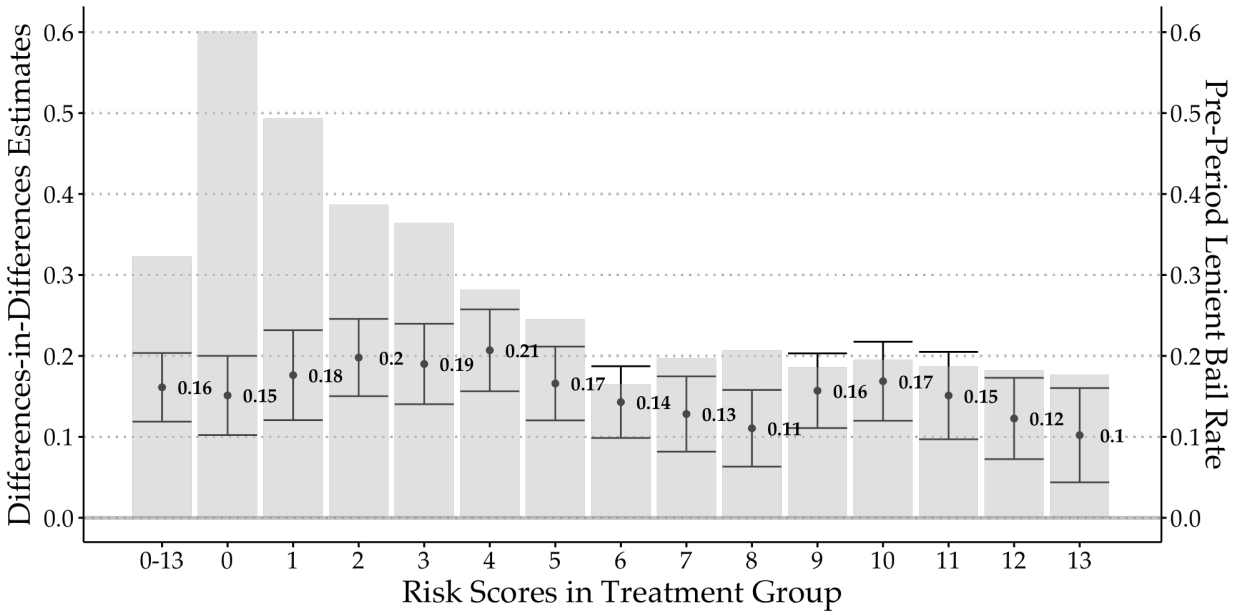
Notes: This figure shows the difference-in-differences coefficients for months relative to recommendation introduction. The orange dashed line denotes the omitted period of the month before recommendation introduction.

To obtain a summary coefficient, I estimate pooled differences-in-differences coefficients and present these results across specifications in Table A.3. Pooling time periods, I find that algorithmic recommendations increased lenient bail by 15 percentage points following the policy change, off of a baseline of 31%. Therefore, the recommendations increased lenient bail by about 50%. These economically meaningful results are consistent with the theory that algorithmic recommendations change the costs of errors to decision-makers.

How do effects vary across the risk score distribution? So far, estimated effects apply to the entire low and moderate risk score distribution. If the recommendations change the cost of errors, we should see results across the whole risk score distribution. To test

this, I estimate pooled differences-in-differences coefficients for each risk score in the low/moderate distribution – that is, scores between 0 and 13. In the raw data, each risk score group between 0 and 13 experienced a discontinuous increase in lenient bail at the time of HB463 (see Figure A.3). Figure 4 shows this result holds when estimating the differences-in-differences coefficients as well. The figure shows the pooled differences-in-differences coefficients and the baseline lenient bail rates (in shaded gray bars). There are statistically significant effects across the entire distribution, and the estimates range from 10 to 20 percentage points.

Figure 4: Pooled Differences-in-Differences Estimates across Risk Score Bandwidths



Notes: This figure shows the pooled difference-in-differences coefficients across different treatment groups based on risk scores. The control group consists of cases with high risk scores, and the treated group consists of cases with low or moderate risk scores (varying from 0 to 13). Specifications are estimated separately for all risk score treatment groups. The specification includes controls for day of week, month-year, exact risk score, top charge level/class, defendant demographics (race, gender), judge, county, and all risk score components listed in Table A.1 except for verified address and support. The black error bars show the 95% confidence interval for each differences-in-differences coefficient. The light-shaded gray bars show the baseline rate of lenient bail for that risk score group in the pre-period, which allows for relative interpretation of effect sizes.

Even though the point estimates are similar in magnitude across the distribution, the relative effects are larger near the moderate-high risk cut-off because they have lower lenient bail baseline rates. For illustration, the coefficient for cases with scores of 0 is 14.8 percentage points, a 25% relative increase off the 60% baseline rate. In comparison, the coefficient for cases with scores of 13 is 10.1 percentage points, a 60% relative increase off the 18% baseline rate. The estimated coefficients across the distribution are similar

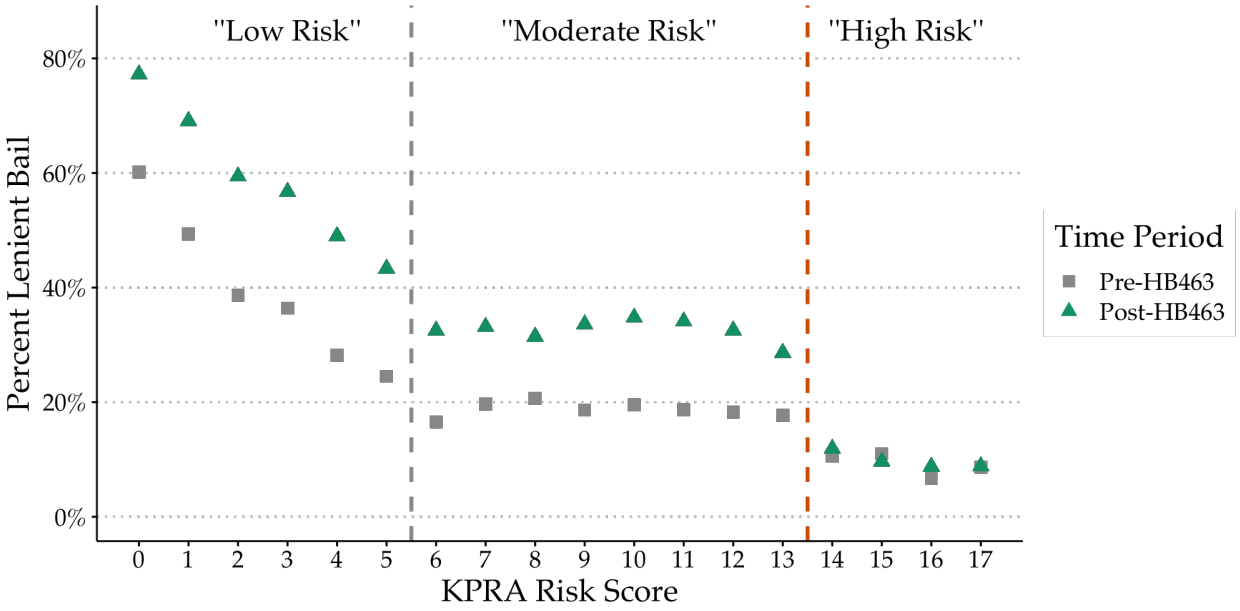
regardless of specification and control choices, as demonstrated by Figure A.4.

6.1.2 Differences-in-Discontinuities Results

I also estimate recommendation effects using a different identification strategy, focusing on marginal cases near the recommendation threshold. After June 2011, the lenient bail recommendation applied only to cases with risk scores below 14. Therefore, cases with similar risk scores received different recommendation treatments based on which side of the critical threshold they were on.

If the lenient bail recommendation were the only factor that changed discontinuously over the threshold, a simple regression discontinuity using the post-period data would identify the lenient recommendation effect. However, other relevant factors changed discontinuously at that threshold as well. Conveniently for identification, these confounding factors were also present in the pre-period. Therefore, I can leverage pre-period data at the same discontinuity to difference out confounding factors with a differences-in-discontinuities approach. This approach then allows me to isolate the effect of interest – the effect of the lenient bail recommendation – for cases near the threshold.

Figure 5: Percent Lenient Bail across Risk Scores and Time Periods



Notes: This figure demonstrates the percentage of cases that receive lenient bail across the risk score distribution, both before and after HB463. The orange dashed line marks the threshold between moderate and high risk. Before HB463, there were no bail recommendations. After HB463, cases with scores to the left of the orange line received a lenient bail recommendation, but those with scores to the right did not. The gray rectangles show the rates before HB463, while the green triangles show those after HB463.

Figure 5 demonstrates the differences-in-discontinuities approach visually. It shows the percentage of cases that received lenient bail based on cases' risk scores and the time period. Points on the left represent cases with the lowest risk scores, while points on the right represent cases with the highest risk scores.[9] I show lenient bail rates across the score distribution in the pre-period (before the introduction of recommendations) and post-period (after the introduction of recommendations).

There were no changes in recommendations for high risk cases (points to the right of the orange dashed line) across time periods, but there were changes for low or moderate risk cases (points to the left of the orange dashed line). For cases that did not experience a change in recommendation, lenient bail rates are nearly identical in the pre- and post-periods. However, for cases that did experience a change in recommendations, lenient bail rates are 10-20 percentage points higher in the post-period.[10] This raw visual evidence is consistent with lenient recommendations having a causal effect on lenient bail rates because rates increase discontinuously where the recommendation kicks in at the critical threshold (the orange dashed line) in the post-period, and the same increase is not present in the pre-period (before the introduction of recommendations).

To formally estimate the effect of the recommendation at the margin, I use a differences-in-discontinuities approach pioneered by Grembi, Nannicini, and Troiano (2016). I estimate regression discontinuity coefficients before and after HB463 and take the difference to isolate the effect of the lenient recommendation. Using data from the post-period, I estimate the effect of crossing the moderate-high threshold using nonparametric methods following Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2020) for optimal bandwidth selection and bias-corrected inference. My preferred estimate yields a 13.7 percentage point effect. This method uses a triangular kernel and the optimal bandwidth based on Calonico, Cattaneo, and Farrell (2020), but particular estimation

[9] I plot rates for the scores 0-17 instead of the entire distribution of 0-24 to focus on risk scores with sufficient observations before and after HB463. Figure A.1 shows few observations at the high end of the risk distribution: the number of observations is tiny for scores above 17, especially in the pre-HB463 period, because there are only two months of pre-period data. For instance, there are only 22 cases pre-HB463 with a score of 18.

[10] As an aside, Figure 5 also demonstrates a clear downward trend in lenient bail for the low risk scores as they get higher (from 0 to 5). However, moderate risk scores receive similar lenience across the score range (from 6 to 13). One likely explanation is that even though judges do not receive the underlying risk scores, it is obvious to them which cases are the lowest risk. In cases with the lowest risk (scores near 0), the person arrested has little or no criminal history background, which is quickly evident on their bail phone call with pretrial officers. Meanwhile, when an arrested person has a handful of risk factors, they necessitate a more extended conversation, making judges less likely to be able to tell the difference between someone who has a low score in the moderate group (e.g., a 6) and someone who has a high score in the moderate group (e.g., a 13).

choices do not erode the effect (see Figure A.5). Since cases with scores of 14 receive lenient bail only 16% of the time, crossing the threshold in the post-period makes the case almost twice as likely to receive lenient bail (even though the underlying risk prediction is very similar).

If the only factor that changed across the moderate-high threshold was the lenient bail recommendation, the regression discontinuity estimate would be equivalent to the recommendation effect of interest. However, two other factors change discontinuously across the threshold. First, the risk level given to judges for the case changes. Cases scored as 14 receive a *high risk* label and no recommendation, but cases scored as 13 receive a *moderate risk* level and a lenient recommendation. Second, Figure A.6 shows that while most characteristics do not display a sharp discontinuity around the critical threshold in the post-period, one exception is prior felony convictions. Defendants with cases that are marginally high risk are discontinuously more likely to have a prior felony conviction than defendants with cases that are marginally moderate risk. Therefore, the estimated 13.7 percentage point effect is some combination of the effect of changing risk levels, the effect of a prior felony conviction, *and* the effect of the recommendation.

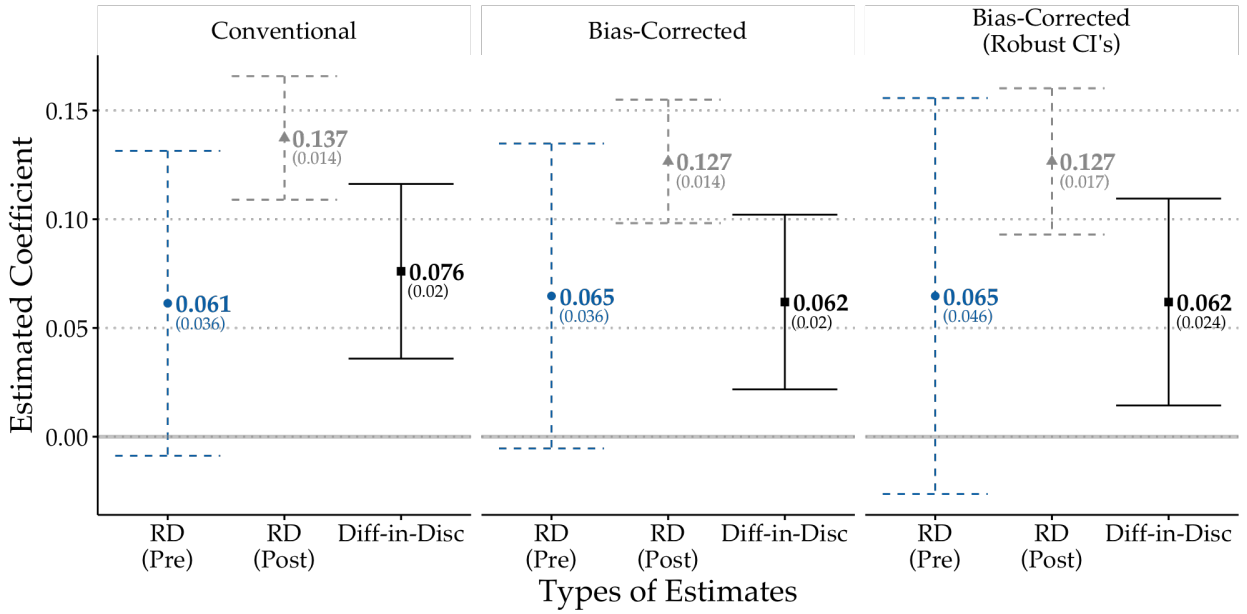
I can disentangle the recommendation effect from the other two components by leveraging the fact that I can observe bail decisions around the same discontinuity in the pre-period. In a regression discontinuity design, a central assumption is that nothing but the treatment (the presence of lenient recommendations, in my case) changes discontinuously at the threshold. My differences-in-discontinuities approach weakens this assumption, allowing for discontinuities at the threshold (confounders) as long as those same discontinuities are present in both time periods (Grembi, Nannicini, and Troiano 2016). Risk levels were present in the pre-period, and the movement in covariates across the risk score distribution was similar. In particular, Figure A.7 shows that the discontinuous uptick in the likelihood of prior felony conviction is nearly identical in the pre-period and the post-period, supporting the validity of differences-in-discontinuities assumptions in this setting.

The regression discontinuity estimate in the pre-period estimates the risk levels effect (the effect of switching from high to moderate) combined with the prior felony conviction effect. Again, I use nonparametric methods following Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2020) for optimal bandwidth selection and bias-corrected inference. My preferred estimate in the pre-period aligns with the methods for my preferred estimate in the post-period: I use a triangular kernel and the optimal bandwidth based on Calonico, Cattaneo, and Farrell (2020), and again the effect is similar

across different estimation choices (see Figure A.8). My preferred estimate in the pre-period is a 6.1 percentage point effect. This estimate is less than half the magnitude of the regression discontinuity in the post-period (13.7 percentage points). The pre-period estimate is meaningfully noisier than the post-period estimate because of the asymmetric nature of the data. There are many more months available for estimation in the post-period.

Finally, I can take the difference between the pre-period regression discontinuity estimate and the post-period regression discontinuity estimate to isolate the effect of the lenient recommendation. The preferred pre- and post-period regression discontinuity results yield a differences-in-discontinuities estimate of 7.6 percentage points. This estimate is statistically significant and economically meaningful: the lenient recommendation caused an almost 50% increase in lenient bail at the margin (an increase of 7.6 percentage points off a baseline of 16% for cases with scores of 14).

Figure 6: Regression Discontinuity and Differences-in-Discontinuities Estimates at the Moderate-High Threshold



Notes: The blue points and dotted lines show the coefficients and 95 percent confidence interval for pre-period regression discontinuity estimates at the moderate-high threshold. The gray points and dotted lines show the coefficients and 95 percent confidence intervals for post-period regression discontinuity estimates at the moderate-high threshold. The black dots and lines show the coefficients and 95 percent confidence intervals for differences-in-discontinuities estimates at the moderate-high threshold.

Figure 6 summarizes the pre-period regression discontinuity, post-period regression discontinuity, and differences-in-differences results across different estimation methods (conventional, bias-corrected, and bias-corrected with robust confidence intervals), following Calonico, Cattaneo, and Farrell (2020). Results are similar across these estimation op-

tions. Overall, the differences-in-discontinuities results are consistent with the estimated differences-in-differences results across the risk score distribution shown in Figure 4. The results further illustrate that algorithmic recommendations have independent effects, which is consistent with the theory that recommendations change the costs of errors to human decision-makers.[11]

6.2 Testing and Adjusting the Naive Estimates

Both the differences-in-differences and differences-in-discontinuities strategies leverage the fact that recommendations were introduced for some cases (low and moderate risk cases) but not others (high risk cases). To correctly attribute the estimated effects in Section 6.1 to recommendations, it must be the case that at the time of HB463, nothing else differentially impacted low and moderate risk cases relative to high risk cases.

In this vein, there is a potential identification concern due to the implementation of HB463. While the calculation of risk levels was the same before and after HB463, and the risk levels were available before and after HB463, the policy change made it *mandatory* for judges to consider them. Therefore, some judges and pretrial officers may not have discussed risk levels on the bail calls before HB463.[12] In that case, HB463 changed the presence of algorithmic predictions *and* recommendations, which complicates how we interpret the previous naive results. In the following subsections, I address concerns about this potential confounding in the context of both the (a) differences-in-discontinuities and (b) differences-in-differences approaches. I find that the majority of the naive (unadjusted) effects are attributable to the independent causal effects of algorithmic recommendations.

6.2.1 Differences-in-Discontinuities Results

In the differences-in-discontinuities approach, I estimate the differences-in-discontinuity coefficient at the moderate-high threshold to recover the effect of algorithmic recommendations, which I'll call R. Intuitively, I leverage the fact that the post-period regression discontinuity at this threshold is the sum of the recommendation effect (R), the levels effect at the threshold (L_{mh}, the effect of being labeled moderate instead of high risk for marginal

[11] My results are also consistent with previous research that showed that discontinuous changes in algorithm risk labels have causal impacts on criminal proceedings (Cowgill 2018b). Both sets of results show that *how* algorithms are communicated matters for human decisions.

[12] Because the administrative data do not indicate which information judges discussed in bail decisions, I cannot directly test this possibility using tabulations in the data.

cases), and the effect of increased prior felony conviction (F).[13] Meanwhile, the pre-period regression discontinuity at the threshold is the sum of the levels effect at the threshold (L_{mh}) and the effect of increased prior felony conviction (F). Therefore, the difference between the two (the differences-in-discontinuities coefficient) leaves the desired recommendation effect (R).

If $\omega \in [0, 1]$ is the share of judges who did not consider risk levels before HB463, then the interpretation of some of these estimates changes. The post-period regression discontinuity still recovers the desired recommendation effect plus the levels and increased prior felony effects, $R + L_{mh} + F$. However, the pre-period regression discontinuity coefficient recovers the increased prior felony effect plus a *diluted* version of the levels effect because only some judges considered levels in the pre-period. Assuming the ω share of judges is similar in their risk level responses to the remaining $1 - \omega$ share of judges, then the pre-period regression discontinuity estimates $\omega L_{mh} + F$ instead of $L_{mh} + F$. Accordingly, the difference-in-discontinuity approach estimates the recommendation effect plus an additional levels effect, $R + (1 - \omega)L_{mh}$ (instead of just the desired recommendation effect, R).

Since $\omega \geq 0$, the differences-in-discontinuities estimate is necessarily an upper bound for the recommendation effect. If ω is close to 1 (almost all judges considered risk levels before), then the extra term goes to 0, and the identification strategy recovers the recommendation effect well. If ω is close to 0 (almost no judges considered risk levels before), then the previous strategy does not recover recommendation effects unless L_{mh} is near 0.

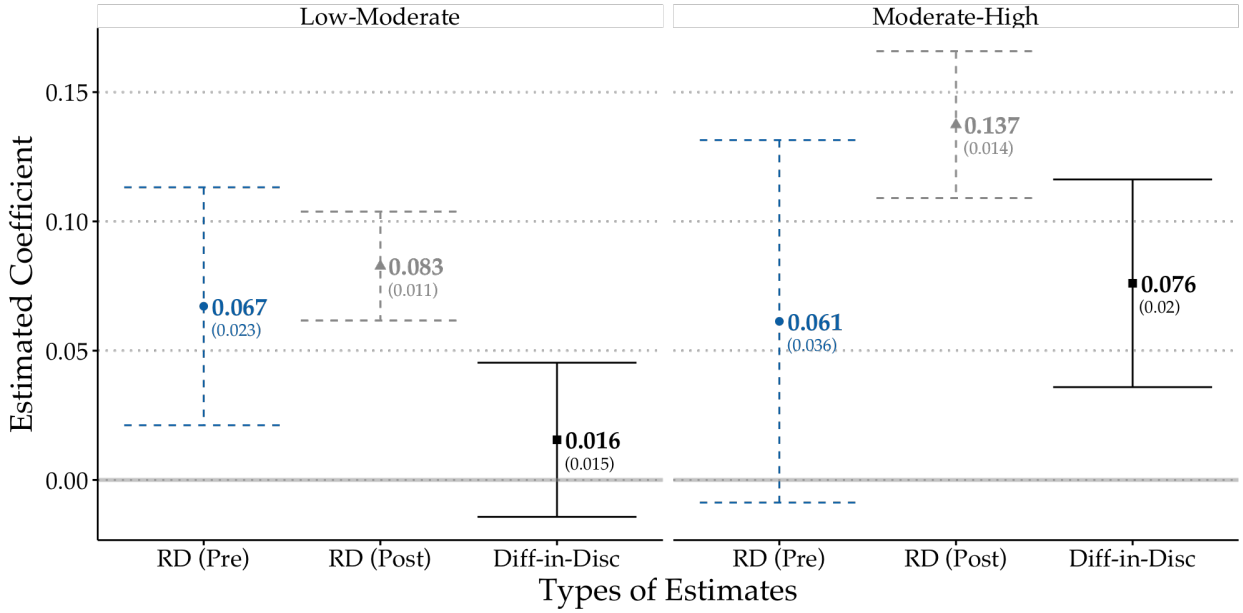
I can leverage another discontinuity in the risk score distribution to investigate the magnitude of ω. Cases also experience a discontinuous change in their risk level around the low-moderate discontinuity. Importantly, there is no change in the presence of recommendations over that discontinuity. Recommendations are either present for both groups, as in the post-period, or not, as in the pre-period. In the post-period, the regression discontinuity at this threshold recovers L_{lm}, the effect of being labeled low risk rather than moderate risk for marginal cases. Assuming that risk score consultation is similar around the low/moderate and moderate/high thresholds before HB463, then the pre-period regression discontinuity recovers ωL_{lm} and the differences-in-discontinuities estimate equals

[13] Recommendation and level changes are sharp discontinuities over the moderate-high threshold. But, the prior felony conviction change is a fuzzy discontinuity because the share of cases with prior felony convictions increases from around 40% to 60% when crossing the moderate-high threshold. For notational simplicity, I refer to F as the effect of increased prior felony conviction, but it could also be denoted $0.2F'$, where F' is the sharp effect of moving from 0% to 100% of cases with prior felony convictions.

$(1-\omega)L_{lm}$.[14]

Intuitively, if the differences-in-discontinuities estimate for the low-moderate threshold is near 0, then ω is near 1 and confounding is limited. To test this, I directly estimate the pre-period RD, post-period RD, and differences-in-discontinuities at the low/moderate threshold. Figure 7 shows the results. The differences-in-discontinuities coefficient is 1.6 percentage points in magnitude and is not statistically significant, which suggests this potential source of confounding is small in magnitude.

Figure 7: Regression Discontinuity and Differences-in-Discontinuities Estimates at Critical Thresholds



Notes: The blue dots and dotted lines show the point estimates and 95 percent confidence intervals for regression discontinuities using pre-period data. The gray dots and dotted lines show the 95 percent confidence intervals for regression discontinuities using post-period data. The black dots and lines show the point estimates and 95 percent confidence intervals for the differences-in-discontinuities results. I show estimates for both critical thresholds in the data: the low-moderate and the moderate-high thresholds.

Even though the results at the low-moderate threshold are not statistically significant, I use them to provide conservative bounds on my original estimates of the recommendation effect. The pre-period regression discontinuity at the low-moderate threshold recovers ωL_{lm}, while the post-period regression discontinuity recovers the undiluted L_{lm}. Therefore, it is also the case that $RD_{lm}^{pre} = \omega RD_{lm}^{post}$. Plugging in the estimates from Figure 7 yields $0.067 = \omega 0.083$, which implies $\omega = 0.81$. Therefore, using the empirical estimates to solve

[14]This assumption requires an assumption about the homogeneity of ω at both points in the risk score distribution. However, it does not require any assumptions about how the level effect around the low/moderate discontinuity (L_{lm}) relates to the level effect around the moderate/high discontinuity (L_{mh}).

this system of equations implies that risk levels were consulted in about 81% of cases.

I can use this estimated ω with the estimates at the moderate-high threshold to adjust the original recommendation effect estimates from Section 6.1.2. Table 1 compares the original estimates (assuming no confounding or $\omega = 1$) to adjusted estimates with $\omega = 0.81$. The effect of risk levels and increased prior felony conviction is slightly higher (7.5 percentage points instead of 6.1 percentage points), and the effect of recommendations is slightly lower (6.2 percentage points instead of 7.6 percentage points) with this adjustment. Therefore, the majority of the unadjusted results are attributable to the independent causal effects of algorithmic recommendations.

The adjusted results correspond with lenient recommendations increasing lenient decisions by 40% (instead of the previously estimated 50%). The effects continue to be economically meaningful. Returning back to the testable implications derived in Section 4, these results are consistent with the theory that recommendations change error costs for human decision-makers.

Table 1: Comparing Estimates with and without Estimated Confounding

Parameter	Original Estimate ($\omega = 1$)	Adjusted Estimate ($\omega = 0.81$)
$R + L_{mh} + F$	13.7	13.7
$L_{mh} + F$	6.1	7.5
R	7.6	6.2

Notes: This table compares the original estimates from the regression discontinuities and differences-in-discontinuities approaches at the moderate-high threshold with estimates adjusted with the estimated ω parameter. Data-driven estimation implies that $\omega = 0.81$. The first row is the sum of the recommendation effect (R), the levels effect at the threshold (L_{mh}, the effect of being labeled moderate instead of high risk for marginal cases), and the effect of increased prior felony conviction (F). The second row is the sum of the levels effect at the threshold (L_{mh}) and the effect of increased prior felony conviction (F). The final row is the desired algorithmic recommendation effect (R).

6.2.2 Differences-in-Differences Results

I can also address potential confounding in the differences-in-differences context. If $\omega \in [0, 1]$ is the share of judges who did not consider risk levels before HB463, then the differences-in-differences approach also picks up the effect of newly using risk levels for $(1 - \omega)$ of the cases. So, the pooled differences-in-difference coefficient (β^{DD}) is then a weighted average between our desired recommendation effect (R') and an effect of

levels (the effect the judge hearing the case risk level as opposed to not, L).[15] Specifically, $\beta^{DD} = R' + (1 - \omega)L$.

Since $\omega \geq 0$, β^{DD} is necessarily an upper bound for the recommendation effect. If ω is close to 1 (almost all judges considered risk levels before), then $\beta^{DD} \approx R'$, and the straightforward differences-in-differences strategy recovers the recommendation effect well. If ω is close to 0 (almost no judges considered risk levels before), then $\beta^{DD} \approx R' + L$, and the previous strategy does not recover recommendation effects *unless* we think $L \approx 0$.[16]

Therefore, I test whether recommendation effects still matter in a subset of cases where the expected effect of consulting risk levels is small ($L \approx 0$). Specifically, think of cases where the risk level does not provide new prediction information to the judges because they are obviously low risk. A prime example is cases that are due to misdemeanor arrests, an attribute that is very salient to judges, and cases that have risk scores of 0, meaning that the person affiliated with the case has zero risk factors (zero failures to appear, zero pending cases, zero convictions, etc.). In other words, these are cases associated with low-level offenses where the defendant has no criminal history. The bail phone call is short in these cases, and it is clear to judges that the relevant defendant is low risk. Since $L \approx 0$ intuitively in this case, then β^{DD} is a valid approximation for the recommendation effect for this group.

Misdemeanor cases with zero risk factors are 7% of cases in the data. Figure A.9 illustrates the rate of lenient bail for these cases in contrast to the rate of lenient bail for the high risk cases. Intuitively, judges know these cases are low risk because there are no risk factors to discuss on the bail call and the offense itself is a misdemeanor. Even if some judges had not consulted risk levels before the policy change, the new "low risk" label should not introduce new prediction information to the judge. Regardless, we see an increase of 10-15 percentage points around the policy date.

Using this set of obviously low risk cases, I estimate dynamic and pooled differences-in-differences coefficients following the methodology in Section 6.1.1. Figure A.10 shows that the coefficients increase after the policy change in a way that diverges from any existing pre-trends. The results are not sensitive to the choice of control variables. Figure A.11

[15] Note that the recommendation effect here is denoted as R' because this effect may differ from the recommendation from the prior section, R. The two effects may differ because they use different data samples for identification.

[16] While I have an estimate of ω from the prior differences-in-discontinuities section, I can recover R' only with an estimate of L, which I cannot estimate. I can estimate the effect of switching levels at the margin (L_{lm} and L_{mh}) with regression discontinuities. However, it is impossible to use the available observational variation to estimate the effect of hearing any risk level instead of not (L).

shows that results with no detailed controls are nearly identical to those with controls based on all observed case variables.

Table 2 shows the pooled differences-in-differences results across different sets of controls. The estimated coefficients are similar in percentage point terms to those estimated for the entire sample in Table A.3. However, the relative effects are smaller because the baseline lenient bail rates are higher for this low risk sample. The 14-15 percentage point increase in lenient bail is a 22% increase relative to the baseline of 66%. These results demonstrate recommendation effects survive in a sub-sample of cases where potential confounding should be minimal. Therefore, the recommendation effects in the differences-in-differences context survive concerns about confounding variation. Lenient recommendations increase judges' lenient bail decisions independent of changes to algorithmic predictions.

Table 2: Differences-in-Differences Results across Specifications (Treated Group: Lowest Risk Cases)

	Dependent variable: I(lenient bail)		
I(score<14) x Post	0.146***	0.147***	0.155***
	(0.026)	(0.026)	(0.027)
Pre-Mean Score<14	0.659	0.659	0.659
Time/Score FEs	Y	Y	Y
Charge/judge/county/demographic controls	Y	Y	N
Risk component controls	Y	N	N
Observations	18,904	18,904	18,904
R^2	0.552	0.552	0.490
Adjusted R^2	0.540	0.540	0.489

Notes: This table displays estimated differences-in-differences coefficients in specifications with lenient bail as the dependent variable. The control group consists of cases with high risk levels, and the treated group consists of misdemeanor cases with risk scores of 0. The table shows results across different specifications. The full set of controls includes fixed effects for month-year, day of week, exact risk score, top charge level and class, judge, county, defendant gender, and defendant race, and all the characteristics that factor into risk score, listed in Table A.1. Standard errors are always clustered at the judge level. *p<0.1; **p<0.05; ***p<0.01.

7 Conclusion

This paper studies how predictive algorithms impact human decisions. Conventional wisdom assumes that algorithms affect human decisions by providing people with data-driven predictions. In this paper, I demonstrate that algorithms matter in another way. Algorithmic predictions are often translated into recommendations for decision-makers,

and these algorithmic recommendations have their own independent effects on human decisions.

I demonstrate the importance of algorithmic recommendations by isolating their causal effects on human decisions. I use a unique setting in the US criminal justice system paired with administrative data to demonstrate that algorithmic recommendations have first-order effects on human decisions. In my setting, lenient recommendations increase judges' lenient bail decisions by 40% for marginal cases.

These economically meaningful effects are not attributable to changes in algorithmic predictions. Instead, the evidence is consistent with recommendations changing human decisions because they change the costs of errors to the people making decisions. Judges may become more lenient when their choices are consistent with recommendations, because the recommendation shields them from backlash.

In this way, recommendations can change more than just the allocation of decisions *(who gets which decision)* – they can change the overall composition *(how many decisions are lenient)*. If decision-makers and algorithm designers disagree about the costs of errors, then recommendations could better align decision-maker incentives with social planner objectives (e.g., less money bail) (McLaughlin and Spiess 2022). Algorithmic recommendations are, therefore, a type of what Cowgill and Stevenson (2020) call "algorithmic social engineering": recommendations are derived from algorithmic predictions, but adjusted to meet certain policy objectives.

These results help inform policy issues related to algorithms and human decisions. For instance, Obermeyer et al. (2019) found that a healthcare algorithm was "less likely to refer black people than white people who were equally sick to [programs] that aim to improve care." The company responsible for the algorithm's predictions and recommendations replied that the algorithm's recommendation (of whether to refer someone to care) is "just one of many data elements intended to be used to select patients for clinical engagement programs." In other words, because doctors consider more than just algorithmic recommendations when making decisions, the company argued the recommendations need not impact final outcomes. My results demonstrate that algorithmic recommendations have independent causal effects on human decisions even when many other pieces of data are available. Therefore, algorithmic recommendations (like the referral recommendation) merit independent study and scrutiny because they have strong and demonstrable effects on high-stakes decisions.

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Appendix

A.1 Background on Kentucky Bail Setting

A.1.1 Kentucky Compared to other US Settings

The Kentucky setting has a several features that distinguish it from other US bail settings. First, in many other states, pretrial data is managed locally, meaning that data needs to be collected at the county-level. However, Kentucky has one pretrial services agency serving all of its 120 counties, so I am able to use data from the entire state. Second, bail decisions are usually made in phone conversations between pretrial officers and judges rather than during in-person bail hearings, which are common in the US.[17] Because judges make bail decisions over the phone, defendants are not present. Third, police have full authority to charge in Kentucky, which means there is no prosecutorial review before the judge makes a bail decision. Thus, judges' bail decisions do not follow any prosecutor's actions. Finally, Kentucky does not have a commercial bail bonds industry – it is one of four states with this ban as of 2022 (Cornell Law School Legal Information Institute 2024). This means that if someone cannot afford money bail in Kentucky, they cannot contract with a bail bonds agent to make bail.[18]

A.1.2 Background on Kentucky Risk Assessment

Kentucky has used a few different risk assessment scoring tools over the years. The first tool was a six-question tool developed by the Vera Institute. In 2006, Kentucky moved to the Kentucky Pretrial Risk Assessment (KPRA) tool. In July 2013, Kentucky started using the Public Safety Assessment (PSA) tool, which the Laura and John Arnold Foundation developed.

Although Kentucky used the KPRA tool from 2006 to 2013, the algorithm changed slightly on March 18, 2011 (Austin, Ocker, and Bhati 2010). Because of these changes, I use data after March 18, 2011, but before adoption of the PSA tool to focus on a time period in which there were no changes to the algorithm.

[17] Kentucky has been using phone calls for pretrial services since 1976. Kentucky uses phone calls because people are very spread out in parts of the state, which would make in-person bail hearings costly in terms of commute time.

[18] However, in Kentucky, if the defendant has not posted bail within 24 hours of the initial decision, the pretrial officer informs the court, and the judge can change the bail decision to increase the chance that they can be released pretrial. If the defendant remains detained pretrial, the next time bail could be reconsidered is usually first appearance.

The KPRA is a checklist-style instrument. Table A.1 documents how to calculate the score for the post-March 18 version of the tool. There were 12 risk score factors, which took the form of "yes" or "no" questions. Each "yes" or "no" answer was associated with a set number of points. Pretrial officers calculated the total of the 12 numbers associated with the relevant questions to generate the final risk score between 0 (lowest) and 24 (highest). Pretrial officers then converted the risk scores into risk levels, which they provided to judges. Scores of 0-5 were categorized as "low risk," scores of 6-13 were categorized as "moderate risk," and scores of 14-24 were categorized as "high risk."

Table A.2 documents how the risk score was calculated before March 18. Relative to the post-March 18 method, this older one featured one additional question (Item 0, which is about references), and the weights for 7 question responses were different. In addition, the way risk scores were converted to levels was slightly different: scores of 0-5 were categorized as "low risk," scores of 6-12 were categorized as "moderate risk," and scores of 13-23 were categorized as "high risk."

A.1.3 Information Used in Bail Decisions

What information do judges have during bail decisions? Because bail decisions in Kentucky occur over the phone, I cannot directly observe the relevant conversations. However, in 2019, there were eight examples of judge calls available on the Kentucky pretrial website, which I listened to. These calls included the following information: name, age, risk score information, list of charges, and incident description. The incident description quoted information from the relevant police report.

Note that while demographic information on race or gender is not explicitly in the calls, these details may be implicitly included. Gender is revealed through the use of pronouns (e.g., "he" and "she") when the pretrial officer discusses the defendant. Meanwhile, names (especially in combination with the county) can signal information about race. Moreover, race and ethnicity were on judge forms about cases during my time period of interest, meaning they could be explicitly observed if judges looked at said forms in their decision-making. (However, these details have since been removed from judge forms.)

Table A.1: Kentucky Pretrial Risk Assessment Factors (After March 18, 2011)

Factor #	Risk Score Question	"Yes" Points	"No" Points
1	Does the defendant have a verified local address and has the defendant lived in the area for the past twelve months?	0	2
2	Does the defendant have a verified sufficient means of support?	0	1
3	Is the defendant's current charge a Class A, B, or C Felony?	1	0
4	Is the defendant charged with a new offense while there is a pending case?	7	0
5	Does the defendant have an active warrant(s) for Failure to Appear prior to disposition? If no, does the defendant have a prior FTA for felony or misdemeanor?	2	0
6	Does the defendant have a prior FTA on his or her record for a criminal traffic violation?	1	0
7	Does the defendant have prior misdemeanor convictions?	2	0
8	Does the defendant have prior felony convictions?	1	0
9	Does the defendant have prior violent crime convictions?	1	0
10	Does the defendant have a history of drug/alcohol abuse?	2	0
11	Does the defendant have a prior conviction for felony escape?	3	0
12	Is the defendant currently on probation/parole from a felony conviction?	1	0

Notes: This table shows the weights associated with risk score factors in the KPRA after March 18, 2011. To calculate total risk score, pretrial officers added up the points associated with each answer. Item 1 was a "yes" if at least five people (reached via the defendant's cell phone) were able to verify the defendant's local address and confirm they had lived in the area for the past twelve months. Item 2 was a "yes" if a defendant was one or more of the following: employed full-time, the primary caregiver of a child or disabled relative, a Social Security/disability recipient, employed part-time or a part-time student, a full-time student, retired, or living with someone who supported them. Item 11 was a "yes" if the defendant had 3 or more drug- or alcohol-related convictions in the last 5 years (a longer period was considered if the defendant had been incarcerated at some point).

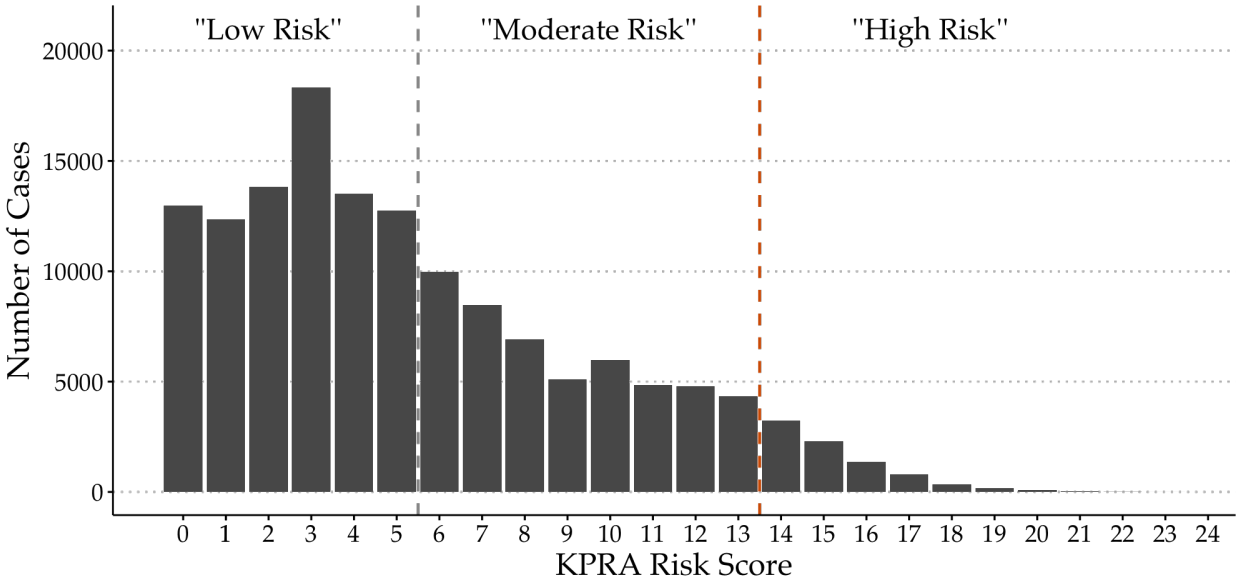
Table A.2: Kentucky Pretrial Risk Assessment Factors (Before March 18, 2011)

Factor #	Risk Score Question	"Yes" Points	"No" Points
0	Did a reference verify that he or she would be willing to attend court with the defendant or sign a surety bond?	0	1
1	Does the defendant have a verified local address and has the defendant lived in the area for the past twelve months?	0	1
2	Does the defendant have a verified sufficient means of support?	0	1
3	Is the defendant's current charge a Class A, B, or C Felony?	1	0
4	Is the defendant charged with a new offense while there is a pending case?	5	0
5	Does the defendant have an active warrant(s) for Failure to Appear prior to disposition? If no, does the defendant have a prior FTA for felony or misdemeanor?	4	0
6	Does the defendant have a prior FTA on his or her record for a criminal traffic violation?	1	0
7	Does the defendant have prior misdemeanor convictions?	1	0
8	Does the defendant have prior felony convictions?	1	0
9	Does the defendant have prior violent crime convictions?	2	0
10	Does the defendant have a history of drug/alcohol abuse?	2	0
11	Does the defendant have a prior conviction for felony escape?	1	0
12	Is the defendant currently on probation/parole from a felony conviction?	2	0

Notes: This table shows the weights associated with risk score factors in the KPRA before March 18, 2011. To calculate total risk score, pretrial officers added up the points associated with each answer. Item 1 was a "yes" if at least five people (reached via the defendant's cell phone) were able to verify the defendant's local address and confirm they had lived in the area for the past twelve months. Item 2 was a "yes" if a defendant was one or more of the following: employed full-time, the primary caregiver of a child or disabled relative, a Social Security/disability recipient, employed part-time employee or a part-time student, a full-time student, retired, or living with someone who supported them. Item 11 was a "yes" if the defendant had 3 or more drug- or alcohol-related convictions in the last 5 years (a longer period was considered if the defendant had been incarcerated at some point).

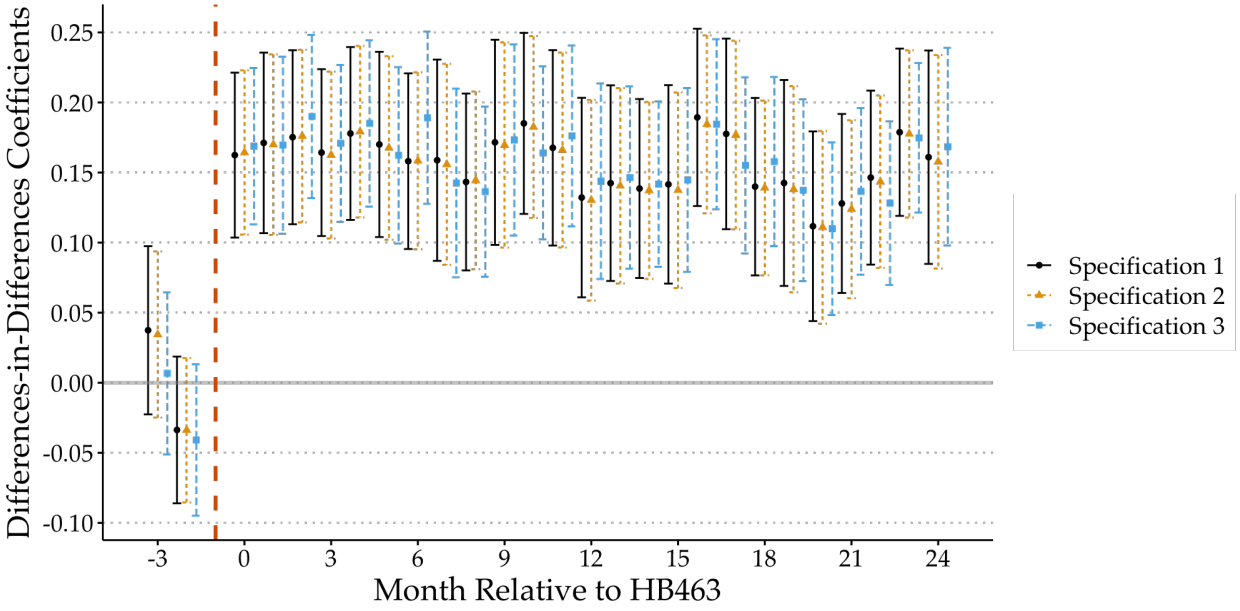
A.2 Supplementary Figures and Tables

Figure A.1: The Risk Score Distribution



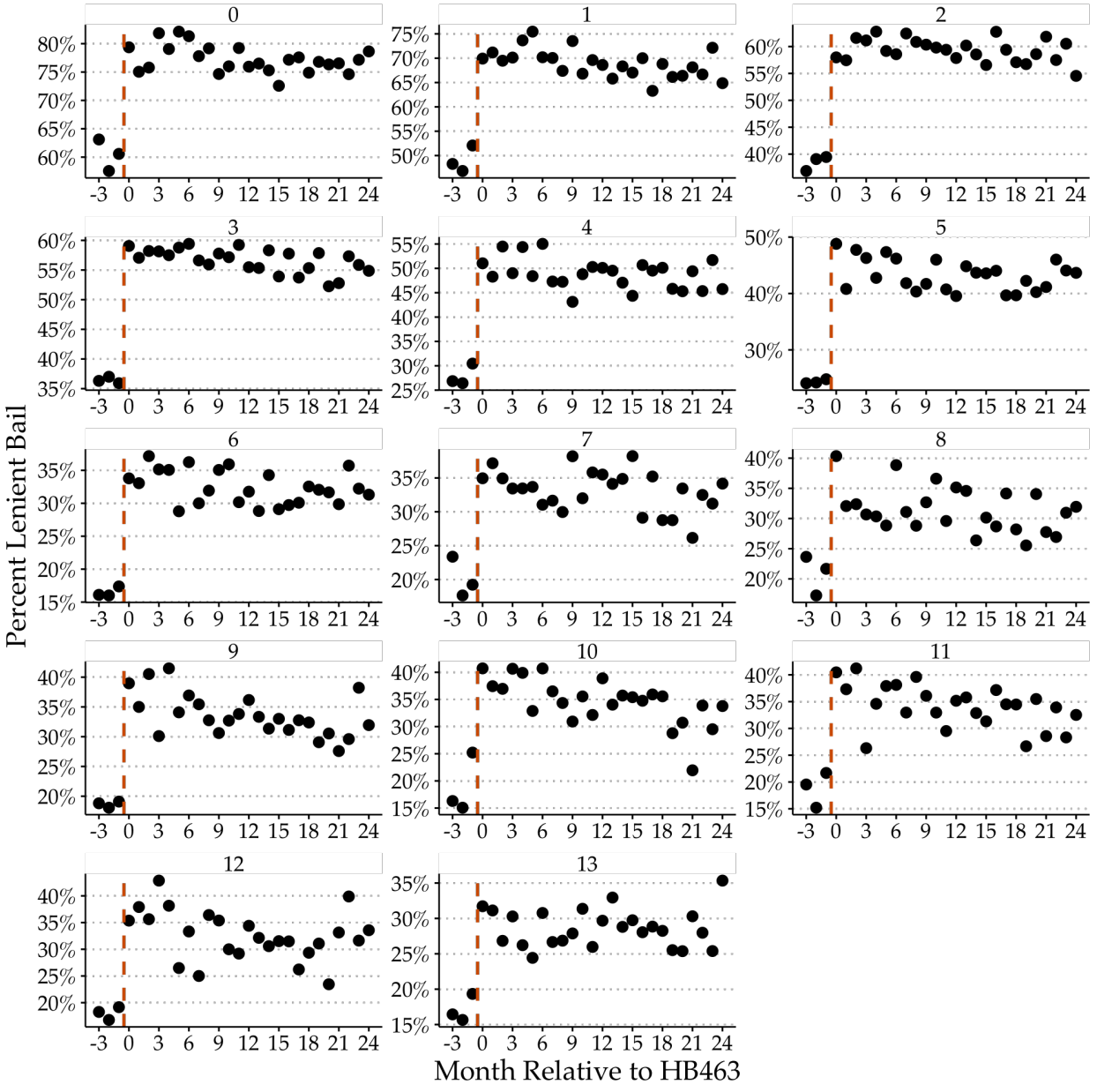
Notes: This histogram demonstrates the number of cases across the full risk score distribution. The dashed lines indicate the cut-offs between risk levels. The orange line is the threshold between "moderate" and "high" risk, and the gray line is the threshold between "low" and "moderate" risk. Scores of 0-5 are low risk, scores of 6-13 are moderate risk, and scores of 14 and above are high risk.

Figure A.2: Dynamic Differences-in-Differences Estimates across Specifications



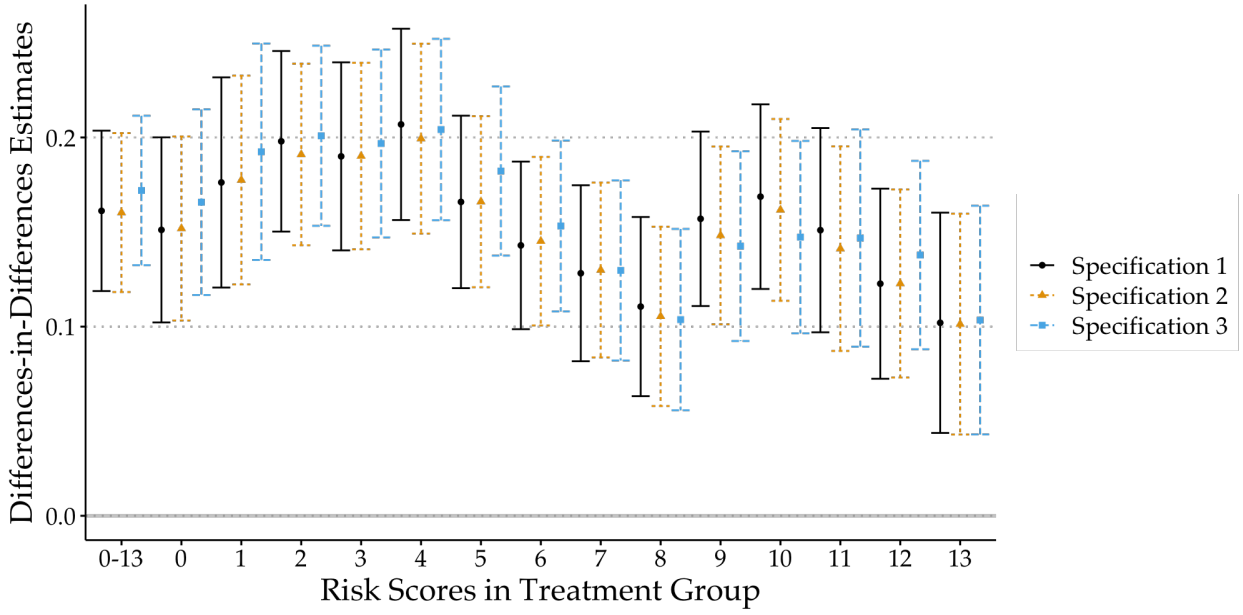
Notes: This figure shows the difference-in-differences coefficients for months relative to recommendation introduction across specifications. The control group consists of cases with high risk scores, and the treated group consists of cases with low or moderate risk scores. The orange dashed line denotes the omitted period of the month before the recommendation introduction. Specification 1 (black circles and error bars) is the main specification, also shown in Figure 3, which includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, county, and all risk score components listed in Table A.1 except for verified address and support. Specification 2 (orange triangles and dotted error bars) includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, and county. Finally, Specification 3 (blue squares and dashed error bars) includes controls only for day of week, month-year, and exact risk score. All error bars denote 95% confidence intervals.

Figure A.3: Lenient Bail Rates by Risk Score over Time



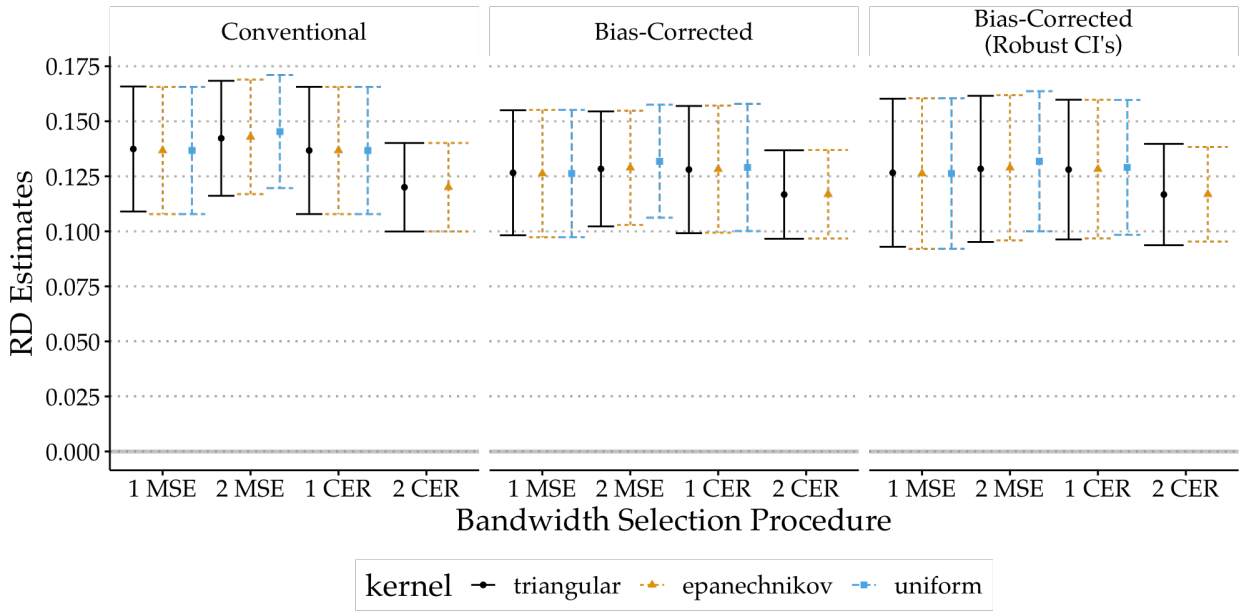
Notes: This figure shows the rate of lenient bail over months by risk scores for low and moderate risk cases. Months are indexed relative to the introduction of algorithmic recommendations. The orange dotted line shows when HB463 went into effect. Each plot is for a different discrete value of risk score between 0 and 13.

Figure A.4: Pooled Differences-in-Differences Estimates across Risk Score Values and Specifications



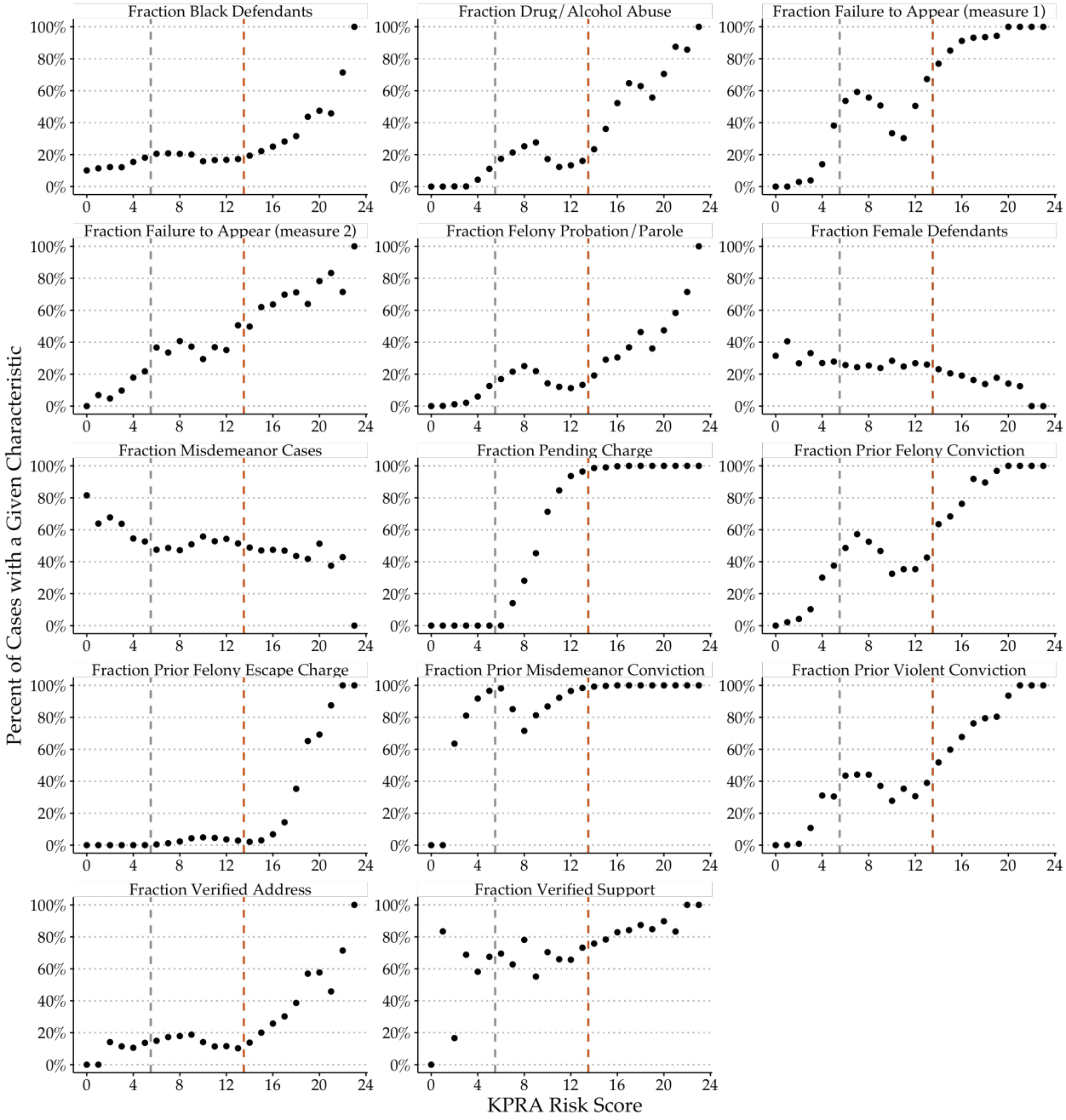
Notes: This figure shows the pooled difference-in-differences coefficients across different treatment groups based on risk scores and across different specifications. The control group consists of cases with high risk scores, and the treated group consists of cases with low or moderate risk scores (varying from 0 to 13). Specification 1 (black circles and error bars) is the main specification and includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, county, and all risk score components listed in Table A.1 except for verified address and support. Specification 2 (orange triangles and dotted error bars) includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, and county. Finally, Specification 3 (blue squares and dashed error bars) includes controls only for day of week, month-year, and exact risk score. All error bars denote 95% confidence intervals.

Figure A.5: RD Robustness across Specification Choices (Post-Period, Moderate-High Threshold)



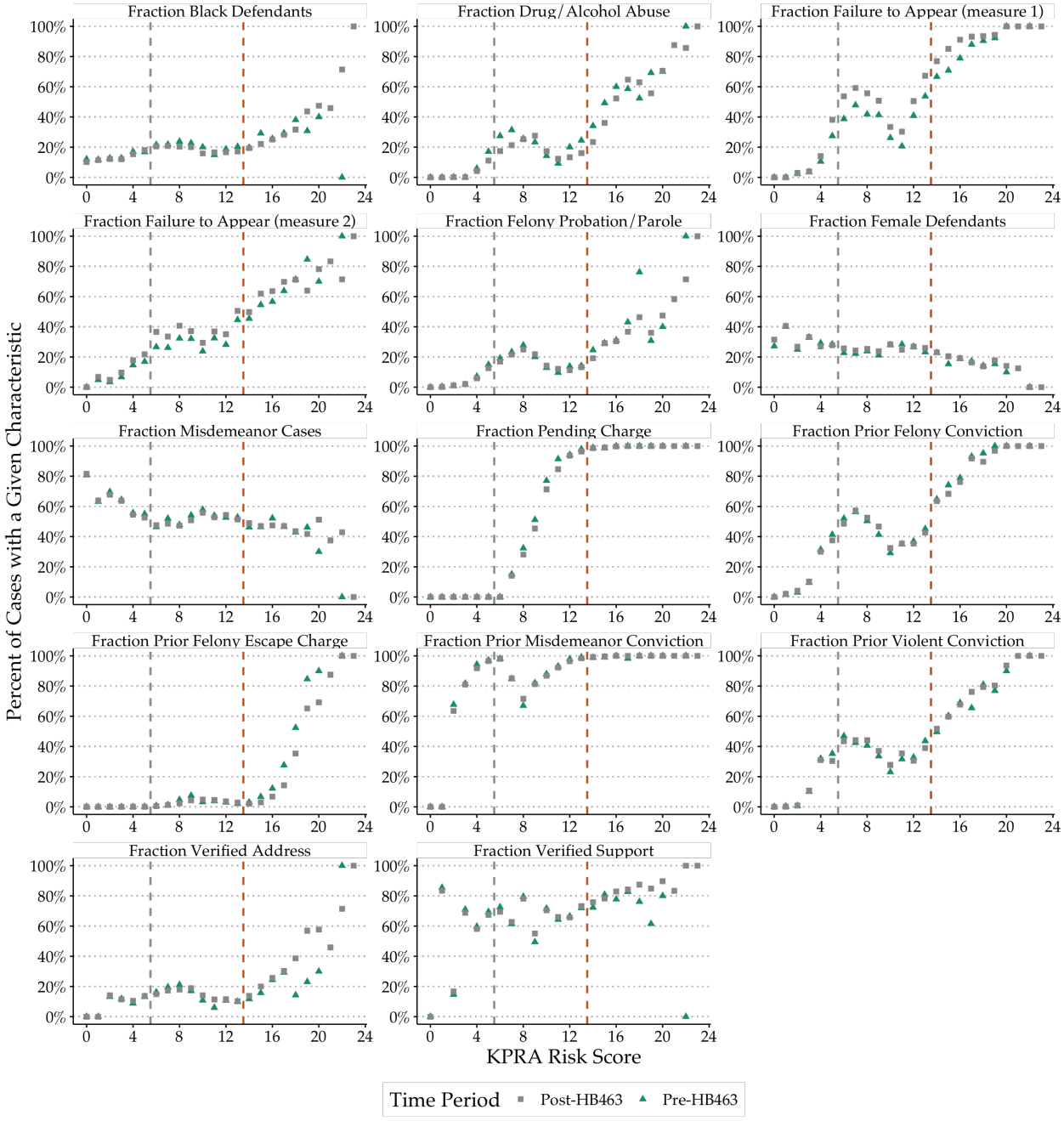
Notes: This figure plots the regression discontinuity estimates using post-period data around the moderate-high cut-off across a range of bandwidth selection procedures, kernels, and estimation adjustments. The shaded gray line at 0 shows the estimate of 0 percentage points. I show estimates with 95% confidence intervals. The plots are grouped based on whether I use conventional estimation, bias-corrected estimation, or bias-corrected estimation with robust bias-corrected confidence intervals. I show different kernel choices with distinct colors, shapes, and line types. The x-axis differentiates between the bandwidth selection procedures (1 MSE = one common Mean Square Error-optimal bandwidth selector; 2 MSE = two different Mean Square Error-optimal bandwidth selectors [below and above the cut-off]; 1 CER = one common Coverage Error Rate-optimal bandwidth selector; 2 CER = two different Coverage Error Rate-optimal bandwidth selectors [below and above the cut-off].

Figure A.6: Defendant and Case Covariates over Risk Score Distribution (Post-Period)



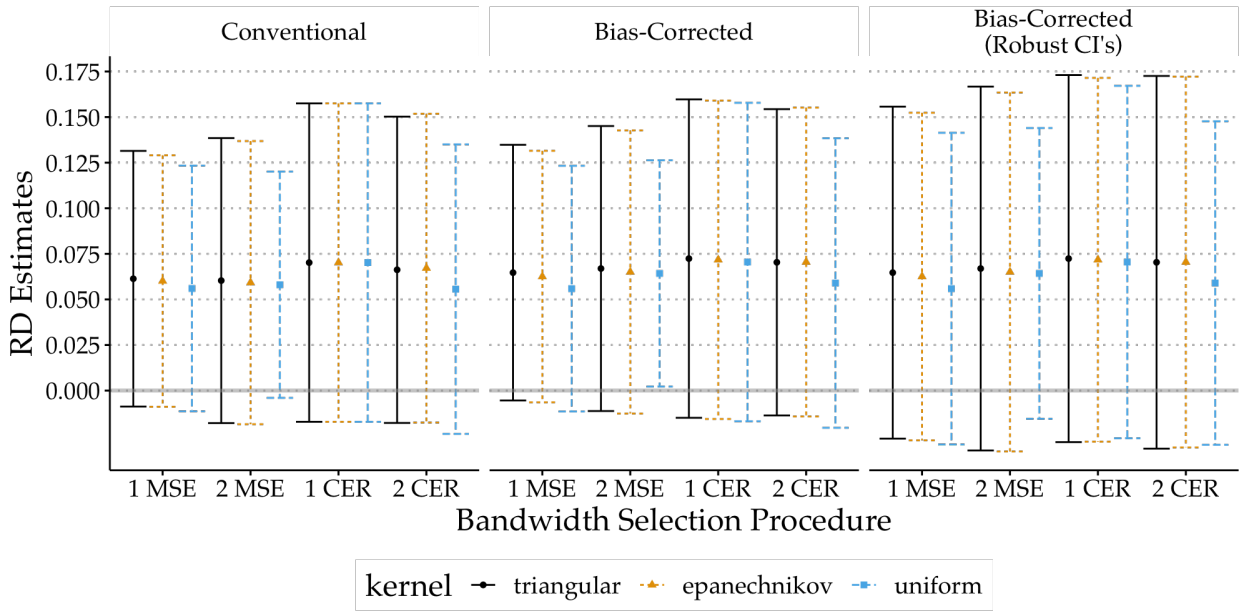
Notes: This figure shows the average defendant and case covariates for each discrete case risk score using data from the post-period. The dashed lines indicate the cut-offs between risk levels. The orange line is the threshold between "moderate" and "high" risk, and the gray line is the threshold between "low" and "moderate" risk. Scores of 0-5 are low risk, scores of 6-13 are moderate risk, and scores of 14 or over are high risk.

Figure A.7: Defendant and Case Covariates over Risk Score Distribution and Time Periods



Notes: This figure shows each discrete case risk score's average defendant and case covariates. The gray rectangles show the averages before HB463, while the green triangles show the averages after HB463. The dashed lines indicate the cut-offs between risk levels. The orange line is the threshold between "moderate" and "high" risk, and the gray line is the threshold between "low" and "moderate" risk. Scores of 0-5 are low risk, scores of 6-13 are moderate risk, and scores of 14 or over are high risk.

Figure A.8: RD Robustness across Specification Choices (Pre-Period, Moderate-High Threshold)



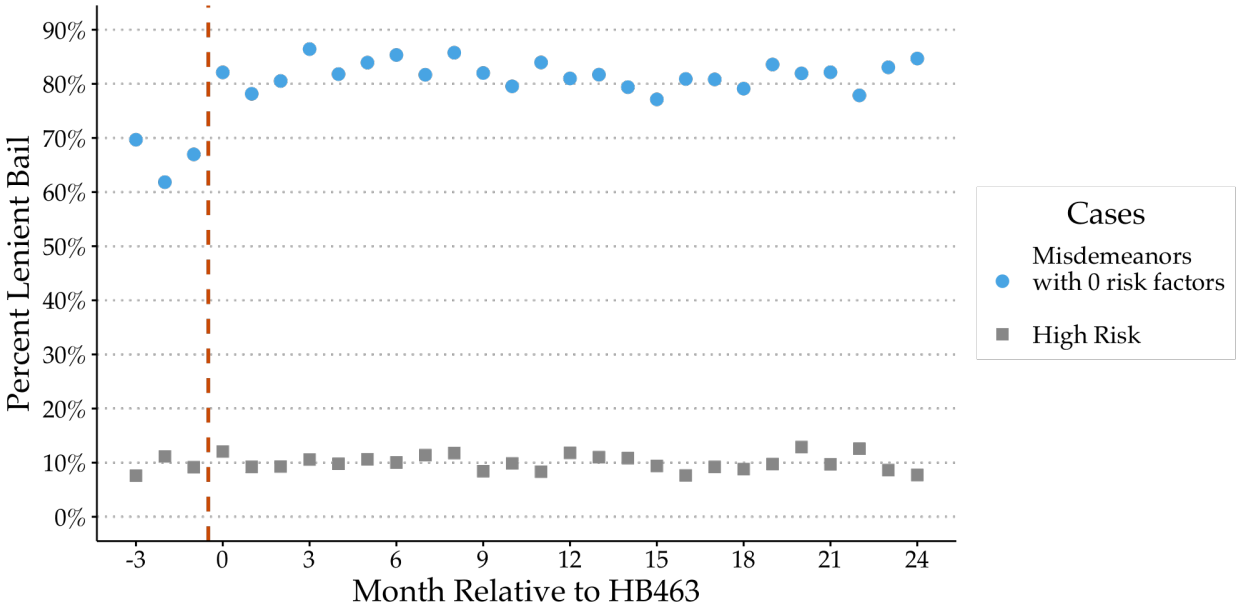
Notes: This figure plots the regression discontinuity estimates using pre-period data around the moderate-high cut-off across a range of bandwidth selection procedures, kernels, and estimation adjustments. The shaded gray line at 0 shows the estimate of 0 percentage points. I show estimates with 95% confidence intervals. The plots are grouped based on whether I use conventional estimation, bias-corrected estimation, or bias-corrected estimation with robust bias-corrected confidence intervals. I show kernel choices with distinct colors, shapes, and line types. The x-axis differentiates between the bandwidth selection procedures (1 MSE = one common Mean Square Error-optimal bandwidth selector; 2 MSE = two different Mean Square Error-optimal bandwidth selectors [below and above the cut-off]; 1 CER = one common Coverage Error Rate-optimal bandwidth selector; 2 CER = two different Coverage Error Rate-optimal bandwidth selectors [below and above the cut-off].

Table A.3: Differences-in-Differences Results across Specifications

	Dependent variable: I(lenient bail)		
I(score<14) x Post	0.161***	0.160***	0.172***
	(0.022)	(0.021)	(0.020)
Pre-Mean Score<14	0.310	0.310	0.310
Time/Score FEs	Y	Y	Y
Charge/judge/county/demographic controls	Y	Y	N
Risk component controls	Y	N	N
Observations	142,466	142,466	142,466
R^2	0.270	0.264	0.133
Adjusted R^2	0.266	0.261	0.132
Note:			*p<0.1; **p<0.05; ***p<0.01

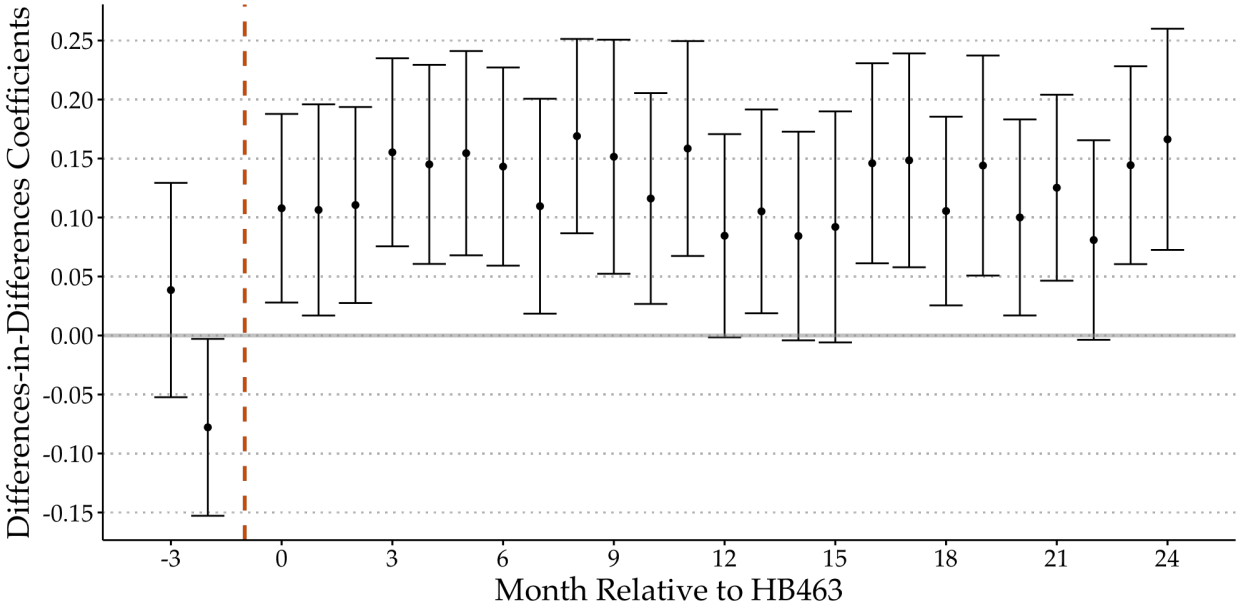
Notes: This table displays estimated differences-in-difference coefficients in specifications with lenient bail as the dependent variable. The control group consists of cases with high risk levels, and the treated group consists of cases with low or moderate risk levels. The table shows results across different specifications. The complete set of controls includes fixed effects for month-year, day of week, exact risk score, top charge level and class, judge, county, defendant gender, and defendant race, and all the characteristics that factor into risk score, listed in Table A.1. Standard errors are always clustered at the judge-level. *p<0.1; **p<0.05; ***p<0.01.

Figure A.9: Lenient Bail Rates by Case Type over Time



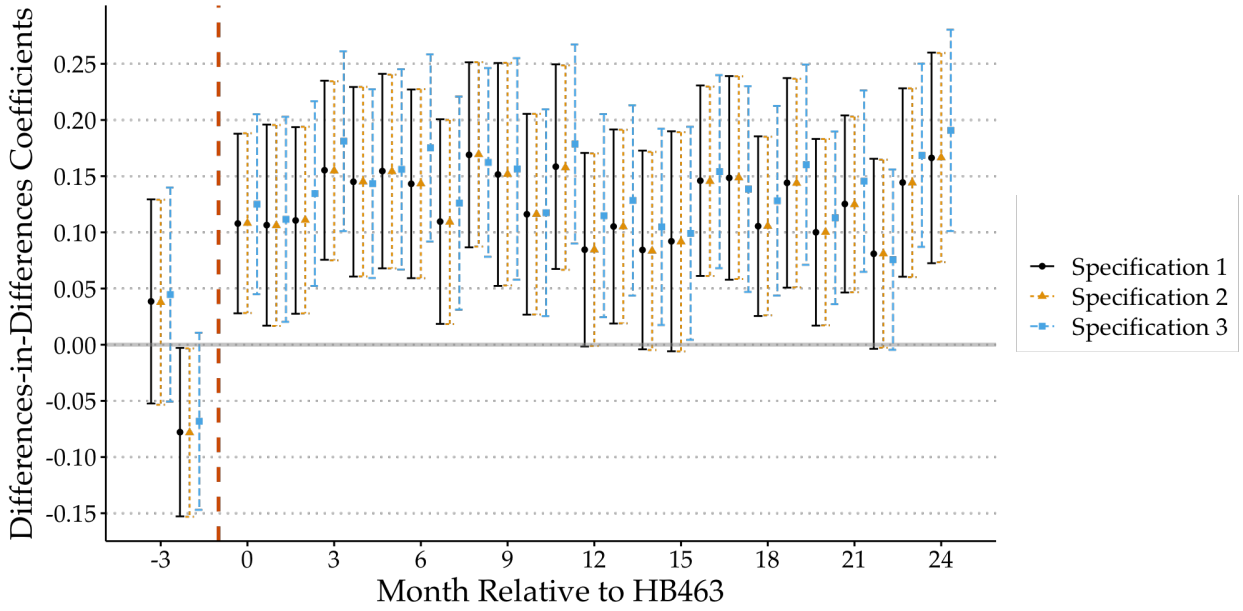
Notes: This figure shows the rate of lenient bail over months by risk score groups. Months are indexed relative to the introduction of algorithmic recommendations. Misdemeanor cases with risk scores of 0 are shown as blue circles, while cases with high risk scores are shown as gray squares. The orange dotted line shows when HB463 went into effect.

Figure A.10: Dynamic Differences-in-Differences Estimates (Treated Group: Lowest Risk Cases)



Notes: This figure shows the difference-in-differences coefficients for months relative to the recommendation introduction. The control group consists of cases with high risk scores, and the treated group consists of cases with risk scores of 0 and misdemeanor offenses. The orange dashed line denotes the omitted period of the month before the recommendation introduction. All error bars denote 95% confidence intervals.

Figure A.11: Dynamic Differences-in-Differences Estimates across Specifications (Treated Group: Lowest Risk Cases)



Notes: This figure shows the difference-in-differences coefficients for months relative to recommendation introduction across specifications. The control group consists of cases with high risk scores, and the treated group consists of cases with risk scores of 0 and misdemeanor offenses. The orange dashed line denotes the omitted period of the month before the recommendation introduction. Specification 1 (black circles and error bars) is the main specification, also shown in Figure 3, which includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, county, and all risk score components listed in Table A.1 except for verified address and support. Specification 2 (orange triangles and dotted error bars) includes controls for day of week, month-year, exact risk score, top charge level and class, defendant demographics (race, gender), judge, and county. Finally, Specification 3 (blue squares and dashed error bars) includes controls only for day of week, month-year, and exact risk score. All error bars denote 95% confidence intervals.