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ABSTRACT

The Effect of Risk Assessment Scores on Judicial Behavior and Defendant Outcomes*

The use of risk assessment scores as a means of decreasing pretrial detention for low-risk, primarily poor defendants is increasing rapidly across the United States. Despite this, there is little evidence on how risk assessment scores alter criminal outcomes. Using administrative data from a large county in Texas, we estimate the effect of a risk assessment score policy on judge bond decisions, defendant pretrial detention, and pretrial recidivism. We identify effects by exploiting a large, sudden policy change using a regression discontinuity design. This approach effectively compares defendants booked just before and after the policy change. Results show that adopting a risk assessment score leads to increased release on non-financial bond and decreased pretrial detention. These results appear to be driven by poor defendants. We also find risk assessment scores did not increase violent pretrial recidivism, however there is some suggestive evidence of small increases in non-violent pretrial recidivism.

JEL Classification: D81, K14, K42, L88

Keywords: pretrial detention, bail, risk assessment, recidivism, regression, discontinuity

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1 Introduction

In the United States (US), individuals are guaranteed by the Eighth Amendment the right to reasonable bail and, therefore, the potential for release before trial. However, 34 percent of all felony defendants, 90 percent of those pretrial detained, are not released because of their inability to post monetary bail (Bureau of Justice Statistics, 2013). In response to the overcrowding of prisons and a perception that the existing bail system disproportionately harms the poor and those with low-risk, many jurisdictions are beginning to look for ways to reduce their inmate population. Often suggested is a shift from monetary bail to a risk-based system, where defendants are released according to their risk of recidivism instead of their financial status. Assessing defendant risk is not a new idea in criminal justice, but in recent years risk assessment has taken on the additional meaning of using more technical and actuarial methods of predicting the likelihood of future crimes or failure to appear. Supporters of risk assessment scores argue that assessing individuals based on their risk rather than their income could lead to less pretrial detention, allowing defendants to keep their jobs and imposing nearly zero costs on the criminal justice system if defendants do not recidivate pretrial. These policies are also most likely to benefit low-income defendants who cannot post bail. Opponents claim that increasing pretrial release through the use of non-financial bond and risk assessment scores could increase pretrial crime, further threatening societal safety and raising costs. This paper focuses on the question of whether the use of risk assessment scores can increase non-financial bond, and decrease pretrial detention without increasing societal costs.

Although the use of risk assessment scores is rapidly increasing across the United States, there is little to no research on their causal effects on release patterns and defendant outcomes. There are two primary difficulties with estimating the effect of risk assessment scores. First, most jurisdictions do not keep detailed records on defendants from arrest until disposition, including recording whether they were assessed using a risk assessment score. Second, some jurisdictions only evaluate certain defendant types, often those with less serious crimes, using a risk assessment score. Any resulting cross-sectional comparisons would be biased as those with scores are observably, and likely unobservably, different across many attributes.

We estimate the effect of risk assessment using data from Travis County, Texas, a large county with a population of over 1.2 million (United States Census Bureau, 2017). On January 14th, 2013, Travis County abruptly changed from not using a research-based risk assessment score at all to assigning one to nearly every inmate. Importantly, Travis County's implementation of the risk assessment score policy was immediate and the exact policy change date was not announced. There was no slow roll-out of the policy—one day the county assigned no risk assessment scores, and the following day it assigned scores to over 75 percent of defendants booked after arrest. This type of sudden change is ideal for identifying local effects through a regression discontinuity design. Using the timing of the policy change, we are able to compare defendants booked just before and after the policy change. The identifying assumption is that all determinants of defendant outcomes aside from the policy change vary smoothly through the policy change. Said another way, we assume that defendants who choose to commit crimes on January 12th and 13th versus January 14th and 15th are not meaningfully different except that those on the later dates received a risk assessment score. We also show empirical tests using exogenous covariates that support this assumption.

First, results show that the use of a risk assessment score increases release on non-financial bail by 4.5%-7.5% and decreases pretrial detention by 7%-10%. We also provide evidence that results are driven by low-income defendants. Second, we are able to rule out meaningful increases in violent pretrial recidivism. These results are robust multiple inference and several robustness checks. There is also some suggestive evidence that non-violent pretrial recidivism may increase, however these results are not robust.

To our knowledge, this paper provides the first causal evidence on the effects of pretrial risk assessment scores. As a result, our work contributes to multiple important existing literatures. First, we contribute to a small, but growing, literature on risk assessment scores in general. The majority of this literature has focused on validity of risk assessment instruments rather than a policy's overall effect (Almond et al., 2017; Flores et al., 2017; Meredith et al., 2007; Schmidt et al., 2017). For example, many states have validated their risk assessment score use by documenting that higher scores are correlated with higher recidivism (Zhang et al., 2014; DeMichele et al., 2018; Latessa et al., 2010; Turner et al., 2009). Others have focused on comparing human decisions with actuarial predictions (Chanenson and Hyatt, 2016; Grove et al., 2000; Dressel and Farid, 2018). Perhaps the most rigorous paper in this field, Kleinberg et al. (2017), used machine learning to determine crime rates would have been if release decisions were made solely based on a risk assessment algorithm. They found that if the same number of inmates were released, but according to their algorithmic risk scores, crime rates would fall.

In contrast, there are few serious independent evaluations of risk assessment score implementation. This paper is most similar in spirit to Stevenson (2018b). She evaluated multiple pretrial risk assessment score policy changes in Kentucky using an event study framework. In contrast to our causal estimates, Stevenson (2018b) provided rigorous pre- and post-comparisons, concluding that the use of risk assessment scores alters bail-setting behavior and leads to increases in failures-to-appear and pretrial crime.

Our paper also relates to a number of papers on the effects of pretrial detention on defendant outcomes. In general, these papers have found that pretrial detention leads to an increased likelihood of conviction (Dobbie et al., 2018; Stevenson, 2018a; Leslie and Pope, 2016; Didwania, 2018; Heaton et al., 2017). Others have considered the effect of nonmonetary bail on outcomes, finding that nonmonetary bail decreases conviction rates (Gupta et al., 2016).

The results of this paper have important implications for criminal justice actors and defendants. First, our finding that risk assessment scores increase non-financial bail and decrease pretrial detention suggests that this policy can be used to lower costs. These costs could be substantial as the estimated annual cost of pretrial detention in the US is $13.4 billion (Wagner and Rabuy, 2017). Significantly, we also show that this reduction in pretrial detention and increase in non-financial bail releases could be possible without increases in violent pretrial recidivism.

Second, the use of risk assessment scores is important to defendants because it relieves lower-income defendants of the potentially disproportionate burden of financial bail and therefore pretrial detention. Perhaps most importantly, decreases in pretrial detention are also associated with greater job stability, less reliance on government assistance, lower probability of conviction and less separation from family (Dobbie et al., 2018). At least to the extent that our results apply in other settings, these findings indicate that risk assessment score polices may be an effective tool for decreasing the income-based disparity in pretrial detention and improving the lives of defendants. Notably, these increases in pretrial detention are not associated with increases in violent recidivism, implying minimal risk and costs to society. However, policy makers must be careful to weigh these potential benefits with the potential for some increases in non-violent recidivism.

2 Overview of the Travis County System

With a population of over 1.2 million, Travis County is one of the largest and fastest-growing counties in the nation (United States Census Bureau, 2018). It is also known as one of the first Texas counties to focus on reducing pretrial detention (Craver, 2017; Smith, 2012). In early 2013, research-based risk assessment scores were implemented into Travis County Pretrial Services for the first time. Travis County chose to implement the Ohio Risk Assessment System-Pretrial Assessment Tool (ORAS-PAT) for its risk assessment scoring. The ORAS-PAT is a relatively new risk assessment tool, developed in 2009 and validated by the University of Cincinnati.

After a defendant is arrested and booked in Travis County, they are interviewed by a pretrial services officer. Relying on information collected during the pretrial interview and facts from a defendant's criminal history, the pretrial services officer calculates a defendant's risk assessment score. Specifically, the ORAS-PAT considers age at arrest, number of past failures to appear, prior jail incarcerations, employment status at arrest, residential stability, and drug abuse as inputs. Next, the pretrial services officer adds up the points assigned to each input, yielding a risk score. This score is used to group a defendant into one of three different categories of recidivism risk: low, moderate, or high. Pretrial officers often also make a recommendation to release or detain defendants pretrial based on the risk assessment score and category assigned to the defendant. If pretrial services recommends release, the recommendation is passed onto a judge.

After considering the recommendation, judges have three options at a bail hearing. First, they can award a non-financial bond, meaning the defendant is not detained pretrial and is free to return home with no financial obligation after the hearing. Second, the judge can award a financial bond, in which case the defendant must post bail (pay the amount of bail in its entirety) or pay a portion of the bail amount upfront to a bail bondsman in order to be released pretrial. In the case of financial bail, the judge does not directly determine the pretrial detention status for the defendant. Third, the judge can deny non-financial bond or financial bond, forcing the defendant to be detained pretrial.

Importantly, because judicial approval is still required for pretrial release (i.e., a defendant's bail and release decisions do not rely entirely on the recommendation from their risk assessment score), it is natural to wonder if judges even utilize risk assessment scores. While it is impossible to definitively say that all judges seriously consider risk assessment scores, 55 percent of Texas pretrial judges surveyed in Carmichael et al. (2017) stated that lack of validated risk assessment tools are a barrier to informed release decisions. Moreover, 80 percent of Texas pretrial professionals and 70 percent of judges support or do not oppose adopting pretrial risk assessment scores. Finally, according to Carmichael et al. (2017), the

ORAS-PAT is considered an important source for determining non-financial bond.

After the judge's decision, a defendant is released from jail if they are awarded non-financial bail or pays for release, but they are expected to show up for all future court proceedings. If a defendant is arrested for a new crime, we say that this defendant has recidivated. This defendant is then likely returned to jail until the final disposition of their case.

3 Data

We use individual-level administrative data from Travis County on all criminal cases disposed between 2011 and 2015. Our data come from two different sources within Travis County. First, Travis County Pretrial Services provides data on defendant characteristics, booking, risk assessment score interviews, and bond outcomes. Importantly, these also include the exact booking date for a defendant, which is essential to determining a defendant's treatment status. We combine these data with the second data source: information on the disposition of cases and recidivism from the Travis County Court System.

We identify four outcomes of interest: release on non-financial bond, pretrial detention, non-violent and violent pretrial recidivism. Non-financial bond takes on a value of one if a defendant is awarded a non-financial bond, meaning they are released on their own recognizance before trial (not detained) and do not have to pay a financial bond. This outcome is zero for all other potential bond outcomes. Unfortunately, information on non-financial bonds is missing for roughly 11 percent (15,188) of defendants. Travis County Pretrial Services believes the missing data to be the result of recording oversights and not related to the policy change or a particular type of defendant. Even so, we discuss this limitation in greater depth in section 5.5. Pretrial detention takes on a value of one if a defendant is kept is jail for greater than two days before their disposition not including time served after potential subsequent arrests. Pretrial recidivism is measured for all defendants, regardless of their pretrial bond or detention status. Severity of crime is defined by the Texas Office of Court Administration. Non-violent recidivism takes on a value of one for all defendants who, before their trial, are arrested for a new non-violent crime. Violent recidivism takes on a value of one for all defendants who are arrested for a new violent crime pretrial.

Table 1 presents descriptive statistics for all defendants booked in Travis County from 2011 through 2015. Most defendants are white, male, and US citizens: 75 percent, 76, and 89 percent respectively. Eighty seven percent of defendants are not flagged by the mental health assessment at booking. Just over half the defendants (51 percent) are also categorized as indigent. Defendants are considered indigent if they have low income, rely on certain forms of government assistance, or reside in a public mental health facility.[1] For the entire time period, 33 percent of defendants have a risk assessment score recorded, although 76 percent of defendants have a risk assessment score post January 2013.

[1] This is the definition of indigence from Travis County Criminal Courts (2012).

4 Methods

4.1 Identification Strategy

For this paper, we exploit a sharp policy change that occurred in Travis County on January 14, 2013. On this date, the county fully implemented a new risk assessment score practice, shifting from not using risk assessment scores for defendants to calculating a risk assessment score for over 75 percent of defendants. This is an ideal setting for applying our regression discontinuity design to estimate the causal effect of a risk assessment score policy on defendant outcomes. The identifying assumption is that all determinants of defendant outcomes vary smoothly through the policy change threshold. Intuitively, we compare defendants booked just before and just after the policy change, assuming that the timing of their booking around the policy change threshold is as good as random. Given the institutional details of the policy change, it is difficult to believe that precise manipulation of the time of a crime is feasible. For manipulation to occur, a defendant must have been aware of the exact start date of the policy—which was not readily advertised to the public—and have shifted the timing of their crime accordingly. Because treatment is also determined by the defendant's booking date and not the bail hearing date, it is unlikely that a judge would be able to alter the treatment status of a defendant.

Formally, we estimate the following individual-level OLS model:

$$Outcome_{it} = \beta_0 + \beta_1 policyenacted_t + f(daysfromcutoff)_t + \lambda_i + \pi_c + \gamma_d + epsilon_{it}$$

Here, i indexes individual defendants and t the date of booking. $Policyenacted_t$ takes on a value of one if a defendant was booked on or after the day of the policy enactment and is zero otherwise. The running variable, $daysfromcutoff_t$, is defined as days from the date of policy enactment, or $dateofbooking_t - policyenactmentdate_t$, and the function $f(.)$ captures the underlying relationship between the outcome variable and the running variable. By interacting $f(.)$ with $policyenacted_t$, we allow the slopes of our fitted lines to differ on either side of the policy change. Our coefficient of interest, β_1, captures the intent-to-treat effect of the risk assessment score policy. λ_i contains individual-level controls that could alter the precision of our estimates, but should not drastically change our estimates of β_1 if our identifying assumption holds. π_c is court-specific fixed effects, and γ_d is day-of-week fixed effects, which capture any time-invariant court tendencies or differences across days of the week. Finally, the error term, ϵ_{it}, measures any unobservable factors that could also alter outcomes.

Our preferred specification employs the mean square error (MSE) optimal bandwidth suggested by Calonico et al. (2017). As is standard in the regression discontinuity literature, we report results for various other bandwidths and show that our results are not sensitive to bandwidth choice. In our results, we also control for the running variable, $daysfromcutoff_t$, in many ways by allowing $f(.)$ to take on different forms. Our preferred specification defines $f(.)$ as a linear function because it enforces the least functional form assumptions on the data. Finally, we report robust standard errors.[2]

[2]Although we do not believe release decisions or recidivism should be correlated for defendants booked on the same day, we also estimate our results clustering on booking date. For non-financial bond and pretrial detention our results have similar significance. Specifically, for non-financial bond the significance level remains the same for 5 of the 6 estimates presented in Table 2. One estimate is significant at the 5% level instead of the 1% due to clustering. For pretrial detention, all six of the estimates in Table 2 retain their significance level with clustering.

4.2 Tests of Identification

Given the nature of the policy change noted before and the late implementation of the policy (i.e., not on January 1), we believe it unlikely that defendants or judges could have manipulated the assignment of treatment in a manner that would discredit our research design. Even so, we provide empirical evidence that our identifying assumption is valid by demonstrating that the number of defendants booked, as well as observable defendant and case characteristics, do not vary discontinuously through the policy change threshold. Figure 1 shows the distribution of the running variable, days from the cutoff. If manipulation were possible, we would expect to see a spike or fall in the number of defendants booked, but this is not the case.

Next, we investigate if specific case and defendant characteristics are smooth through the policy change threshold. If our identifying assumption is valid, defendant and case characteristics will vary similarly on both sides of the policy change threshold. If defendants or judges could have exactly manipulated the timing of booking, we would expect to find differences in case and defendant characteristics through the policy enactment threshold. To test this threat to identification, we estimate equation (1) using race, age, gender, criminal history, indigent status, severity of arrest (misdemeanor or felony), mental health status, US citizenship status, and specific court separately as outcome variables. Figure 2 and Appendix Table 1 show the results for this test. There is only one small visible jump in defendant and case characteristics in the graphs presented (Defendant Age). Of the 21 estimates presented in Appendix Table 1, only two are statistically significant at conventional levels, although with coefficients close to zero, which is consistent with findings due to chance. These results indicate that case and defendant are not discontinuous through the policy change threshold.

We also present another test of the identifying assumption using all the covariates we observe about a defendant and case that are determined before defendants are offered a risk assessment score. Instead of considering the covariates individually, we use them in combination along with a court and day-of-week fixed effect to predict the likelihood of each potential outcome (release on non-financial bond, pretrial detention, non-violent and violent recidivism) for every defendant. This allows us to create a weighted average where the characteristics that contribute more to a specific outcome are considered with greater weight. Here we can estimate the underlying probability that a defendant will be released on non-financial bond, be detained pretrial, or recidviate using everything we know about them except the use of a risk assessment score. If each predicted outcome is smooth through the policy change threshold, then we can contribute any treatment effect we later estimate to the policy change, not underlying differences in defendants booked just before and after the policy change.

Figure 3 and Table A2 show the results for the predicted outcomes. The regression discontinuity estimates for each predicted outcome are statistically insignificant and are close to zero. This further indicates little evidence of underlying differences in defendants across the policy change threshold—proving further that our identifying assumption holds.

5 Results

5.1 Effects of Risk Assessment Score Policy on Score Usage

To determine the effects of a risk assessment score policy, we first need to document that Travis County's enactment of its risk assessment score policy led to a sudden and dramatic increase in the number of defendants assessed and assigned a risk assessment score. To do so, we estimate equation (1) using the assignment of a risk assessment score as the outcome variable. Figure 4 presents our graphical results. This graph and the graphs that follow plot the mean of the outcome variable in 30-day bins and a linear fit of the outcome, which is allowed to vary on each side of the policy change threshold. In all figures, the running variable is normalized to zero (the date of policy enactment is zero days after the policy change).

Figure 4 shows clearly that we estimate a large (about 80 percent) increase in risk assessment score assignment across the policy change threshold.[3] This indicates that about 80 percent of defendants booked after the policy enactment were assigned a risk assessment score. We note that this is not a sharp discontinuity (i.e., 100 percent take-up), which motivates our use of intent-to-treat estimates used throughout the rest of the paper. There are multiple reasons why a defendant may not have been recorded with a risk assessment score. First, the Pretrial Services data we use are not perfect. It may be the case that some scores simply were not recorded. Furthermore, some defendants are much less likely to receive a risk assessment score, such as those who have an active defense attorney to convince Pretrial

[3]Risk assessment usage was greater than zero for a few months before January 2013 because Travis County elected to run a pilot study.

Services not to conduct a pretrial risk assessment score or those with a parole violation. Regardless, our intent-to-treat effects allow us to estimate the unbiased intent-to-treat effect of the risk assessment score policy.

5.2 Effects of a Risk Assessment Score Policy on Non-financial Bond and Pretrial Release

The primary intent of the risk assessment adoption was to increase the number of defendants released on non-financial bond. This decision is made by judges with access to risk assessment scores, so this is the first outcome we consider. We also consider pretrial detention. If a defendant is released on non-financial bond, they are not detained pretrial; but if a judge offers financial bond to a defendant, their pretrial detention status is determined by their ability to pay the bond. Therefore, it is of separate interest to determine the effects of a risk assessment score on pretrial detention.

We first show the effects of risk assessment score on non-financial bonds and pretrial detention in Figure 5. Formally, we estimate equation (1) with the probability of release on non-financial bond and pretrial detention as outcome variables. Figure 5 shows the mean of release on non-financial bond and pretrial detention in 30-day bins and a linear fit of the outcome, which is allowed to vary on each side of the policy change threshold. This figure provides visual evidence that implementing risk assessment scores increases the likelihood of release on non-financial bond and decreases pretrial detention. It also appears these effects fade with time. While it is challenging to determine exactly why our results decrease with time, though we will discuss possible reasons later in this section.

Table 2 presents corresponding point estimates, with each column representing a separate regression. Each column includes controls for days since the policy change, case specific controls, along with fixed effects for the court and day of booking. Specifically each column has case-level controls for defendant race, age, gender, citizenship, mental health flag and indigent status, along with controls for the severity of the crime (misdemeanor or not). As with every specification of a regression discontinuity, the polynomials are allowed to differ on each side of the policy change cutoff. Even numbered columns allow the running variable, days to the cutoff, to vary quadratically and odd numbered columns present linear results. Columns (1)-(2) present results for the double the MSE optimal bandwidth, columns (3)-(4) 1.5 times the MSE optimal bandwidth, and columns (5)-(6) the optimal bandwidth. If our identifying assumption holds, we would expect that our coefficient of interest would remain similar in magnitude. Across all eight columns, our estimates remain statistically significant at conventional levels and are similar magnitudes for non-financial bond and pretrial detention.

Our estimates for non-financial bond range from 0.029 to 0.047. These results indicate that the implementation of a risk assessment score policy increases the likelihood of release on non-financial bond by about 3-5 percentage points (4.5%-7.5%). For pretrial detention, our estimates range from -0.025 to -0.035, showing the risk assessment score policy decreases the chance of pretrial detention by about 3 percentage points (7%-10%).

Because our results include four different outcomes, we also include false discovery rate (FDR)–adjusted q-values for the estimates presented in Table 2. We compute the FDR-adjusted q-values using the method proposed by Anderson (2008), adjusting for our four different outcomes. The FDR q-values can be interpreted as adjusted p-values. The FDR q-values for each outcome are statistically significant at least the five percent level for all but one of the specifications. Therefore, we conclude that the effects we find are large enough not to be attributed to chance.

Now we will demonstrate that our results for pretrial detainment and non-financial bond are robust to various specifications. A standard concern with regression discontinuity estimates is that results are valid only for a specific bandwidth selection or are the result of misfitting the data. To address these concerns, we present several specifications and show that our results are robust to bandwidth and functional form selections. First, we estimate equation (1) with inclusion of the controls and allow the bandwidth to vary from 20 to 660 days in 10-day increments using a linear specification. Figure A1 Panels (a) and (d) show the coefficients and standard errors from each model for non-financial bond and pretrial detention. We also complete the same exercise, but with allowing the running variable to vary quadratically and cubically. The results for these models are shown in Figure A1 Panels (b), (c), (e), and (f). The dashed lines represent the optimal MSE bandwidth. The estimated coefficients remains consistent across the different bandwidths. Estimates for non-financial bond and pretrial detention are also statistically significant for the vast majority of estimates, illustrating that our results are robust to alternative specifications of bandwidth and functional form.

We also conduct a permutation test in the spirit of Abadie et al. (2010) to support our claim that Travis County's risk assessment score policy drives our results. To do so, we estimate equation (1) reassigning the policy threshold to be a day before the true policy change occurred. Because we only have data beginning in 2011, we are able to estimate equation (1) 910 times using every possible date that occurred before the true policy change, a linear specification, optimal bandwidth, and the controls included in Tables 2 and 3. The distribution of placebo estimates for release on non-financial bond and pretrial detention is shown in Figure A2. Nearly all placebo coefficients (97.99 percent) are less than the reported estimates in Table 2 for release on non-financial bond. Our pretrial detention estimate in Table 3 is less than 95.88 percent of our placebo estimates. These results provide further evidence that our results are not simply due to chance.

Together these results show the adoption of a risk assessment scores policy caused increased offers of non-financial bonds by judges which appears to lead to meaningful decreases in pretrial detention. Since we find results for pretrial detention, we also consider if a risk assessment score policy alters pretrial recidivism.

5.3 Effects of a Risk Assessment Score Policy on Recidivism

If it is the case that the new type of individuals released pretrial through non-financial bond disproportionately commit crimes before their trial, there would be an increase in pretrial recidivism. Results for non-violent and violent recidivism are shown in Figure 6. Both graphs in Figure 6 show the mean of the outcome variable in 30-day bins and a linear fit of the outcome, which is allowed to vary on each side of the policy change threshold. Figure 6 presents some suggestive evidence of a small increase in non-violent recidivism and no change in non-violent recidivism. Table 3 presents the corresponding estimates. Similar to Table 2, even columns allow the the running variable to vary quadratically and odd columns are linear. Each column controls for defendant race, age, gender, citizenship, mental health status, indigent status, and the severity of the crime (misdemeanor or not). Fixed effects for the assigned court and booking day of the week are also included. Importantly, our estimates for non-violent and violent estimates are of similar magnitudes across all six columns. For non-violent recidivism, estimates range from 0.009 to 0.01 across the table. Only three estimates are significant at the ten percent level. Although there appears to be some evidence of small increases in non-violent recidivism, our results are not robust to alternative specifications.

Next we consider violent recidivism. Across all columns our estimates remain stable, ranging from -0.001 to -0.004. We are also able to rule out increases in violent recidivism greater than 2 percent (.037 percentage points) when using the larger sample size from twice the optimal bandwidth.[4] We also report FDR q-values for recidivism outcomes. The FDR q-values are also not consistently significant for either outcome.

We can also show our recidivism results are robust to alternative bandwidths and functional forms. As we did for non-financial bond and pretrial detention, we estimate equation (1) using non-violent and violent recidivism as outcomes, with the inclusion of the controls and allow the bandwidth to vary from 20 to 660 days in 10-day increments. Figure A3 shows the coefficients and standard errors from each model for non-violent and violent recidivism. We also complete the same exercise, but with allowing the running variable to vary quadratically and cubically. The results for these models are shown in Figure A3 Panels (b), (c), (e), and (f). The dashed lines represent the optimal MSE bandwidth. The estimated coefficients remains consistent across the different bandwidths. Together these results indicate that risk

[4] 0.00037 is the top of the 95% confidence interval from this specification.

assessment scores do not increase violent-recidivism. We also find some evidence, although not robust, of increases in nonviolent recidivism.

5.4 Indigent Defendants

Since one stated aim of the risk assessment score policy was to improve outcomes for low-income defendants, we also present results for indigent versus non-indigent defendants. As indigent defendants are more likely to be unable to post their bond before the policy change, we would expect effects for release on non-financial bond and pretrial detention to be stronger for indigent defendants compared to non-indigent defendants. Our graphical results are shown in Figure 7. Entire sample results are replicated in Panel (a) and (d) for release on non-financial bond and pretrial detention. Panels (b) and (e) present results for indigent defendants, while Panels (c) and (f) show results for non-indigent defendants. For both outcomes the discontinuity for indigent defendants is visibly larger than for non-indigent defendants. There is also some evidence of an increase in release on non-financial bond and a small decrease in pretrial detention for non-indigent defendants. Corresponding estimates are shown in Table A3.

In Table A3, Panel A presents results for indigent defendants and Panel B show results for non-indigent defendants. Similar to earlier result tables, each specification includes all case controls. Even columns allow the running variable to vary quadratically and odd columns are linear. Across each specification the coefficient for non-financial bond and pretrial detention for indigent defendants has a greater magnitude, roughly two to three times larger, than for non-indigent defendants, although we cannot rule out that the estimates are statistically equivalent. These subgroup results suggest that our release on non-financial bond and pretrial detention results are likely driven by lower-income defendants.

We also explore results by indigent status for recidivism. As indigent defendants are the most likely to be released pretrial it is possible that changes in their recidivism behavior are masked in the entire sample results. Results for non-violent and violent recidivism are shown in Figure 8. Panels (a) and (d) repeat the entire sample results for comparison. Indigent results are show in Panels (b) and (e), while Panels (c) and (f) report results for non-indigent defendants. For non-violent recidivism, there is some evidence of a larger increase in recidivism for indigent defendants and no increase for non-indent defendants. For violent recidivism however, there appears to be no increase in recidivism for indigent or non-indigent defendants.

Table A4 shows recidivism estimates. For non-violent recidivism, the coefficients for indigent defendants are larger in magnitude than for non-indigent defendants. For violent recidivism, however, there are no meaningful differences in the coefficients for indigent and non-indigent defendants. Further, for each subgroup the coefficient for violent recidivism is negative, again suggesting there are no increases in violent recidivism across either group.

In summary, our results for indigent versus non-indigent defendants show that lower income defendants are the most likely to be awarded non-financial bond and released pretrial. We also find some suggestive evidence that non-violent recidivism may increase for lower income defendants, who are most likely to be released. Violent recidivism does not increase for either group.

5.5 Missing Values

One limitation of this study is that we are missing one outcome variable (non-financial bond) for 10 percent of our defendants. Although our institutional details, namely that Travis County Pretrial Services believes that some records are simply missing by chance, indicate that missing outcomes is not correlated with treatment, we also provide empirical evidence that the likelihood of missing the probability of release on non-financial bond is not discontinuous through the threshold. Results of this test are shown in Figure A4. Here we estimate equation (1) using the probability of missing data for release on non-financial bond as the outcome variable. There is no striking visual evidence that the probability of missing data changes through the policy change threshold.

We provide corresponding point estimates in Table A5. Even columns allow the running variable to vary quadratically and odd columns are linear. Columns (1)-(6) use the optimal bandwidth determined in Table 2 for release on non-financial bond. Columns (7)-(8) use the optimal bandwidth for the probability of missing data. Across all eight columns, the coefficient remains statistically insignificant and close to zero. Together these results indicate that the probability of missing data does not vary with treatment.

One might remain concerned that there are changes in the composition of defendants missing data that coincide with treatment. For example, it could be the case that we are missing data for defendants who are likely to be released on non-financial bond to the left of the threshold and are missing data for defendants who are not likely to be released on non-financial bond to the right of the threshold. Although we cannot assess this directly, we can use the case and defendant information we do observe about all defendants to predict the likelihood or release on non-financial bond for defendants who are missing this outcome. We then estimate equation (1) using predicted probability of release on non-financial bond as the outcome just for defendants who are missing data. Figure A5 shows these results. There is no visual evidence of underlying differences in defendants who are missing data across the threshold. Taken together, these results indicate that it is unlikely that the defendants with outcomes missing from our dataset are sufficiently different to alter our results for release on non-financial bond.

5.6 Long Term Effects

We now turn to why we only observe short-term effects that fade over time. Because our regression discontinuity estimates only allow us to obtain local average treatment effects—or, in other words, we can only establish the causal effects of the risk assessment score policy just around the time of the policy change—we cannot credibly identify long-term effects of the policy change. However, we can provide suggestive evidence on when the effects of the policy begin to fade. To do so, we conduct event study analysis with results presented in Figure A6.[5] Visually, it is clear that the effect of non-financial bonds lasts for only two months after the policy change and that the rate of release on non-financial bonds returns to non-financial bond just afterward. It is natural to wonder why we see such a short-lived effect from the risk assessment scores.

First, we note that Stevenson (2018b) also provided some evidence that the effects of

[5] Formally we regress probability of release on non-financial bond on indicators months before and after the policy change in two-month bins. Our regression also controls for race, age, gender, citizenship, mental health status, and indigent status of the defendant, along with controls for the severity of the crime (misdemeanor or not) and fixed effects for the assigned court and booking day of the week. Finally, we add a court-specific time trend.

risk assessment scores fade with time, so this is not an uncommon pattern. Travis County Pretrial Services also noted that judges and pretrial service employees did receive training on the ORAS-PAT near its implementation, and that potential enthusiasm surrounding the policy could have led to short term effects. For example, judges could have paid closer attention to the scores right after the training, but stopped as time passed. It is also possible that judges began to disregard the scores after the novelty of the policy change wore off.

Regardless of why the results diminish with time, the short term-nature of effects highlight an important aspect of risk assessment scores. In practice most risk assessment scores are implemented within a pre-existing pretrial system and judges are not required to adhere to their recommendation. Inherently, any effect risk assessment scores could have on outcomes depends on how judges and pretrial services use them in their decision making process. Policy-makers must be careful to consider not only if they want to implement risk assessment scores, but also how they will be used in practice.

6 Conclusion

This paper estimates the effects of a risk assessment score policy by using a regression discontinuity design. We compare defendants booked barely before and after a policy change in a large county in Texas. Our results indicate that implementing risk assessment scores leads to an increased likelihood of release on non-financial bond and a decreased probability of pretrial detention. Precisely, we estimate that the implementation of risk assessment scores in the county led to an 4.5%-7.5% increase in non-financial bonds and a 7%-10% decrease in pretrial detention. We also find no increases in violent recidivism. We recognize that our results are only for one county in Texas and that the extent to which they apply to other contexts outside of Texas, where existing pretrial systems may be different, is unknown. Further, it is possible that effects are only short-lived. Even with this qualification, we believe that this study is an important contribution to nearly nonexistent literature on risk assessment scores in practice. Our results indicate that risk assessment scores have the potential to decrease costs to society and the disproportionate burden of financial bail for low-income defendants, while not increasing violent pretrial recidivism. However, policy makers must be careful to weigh these potential benefits with the potential for some increases in non-violent recidivism.

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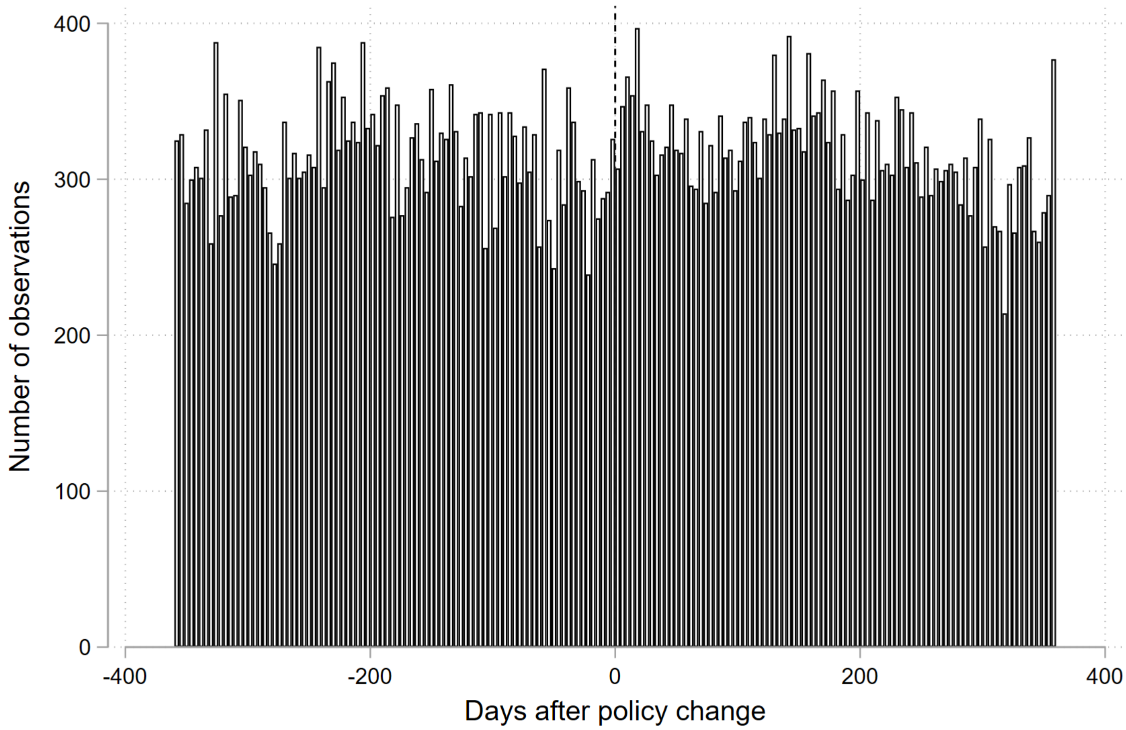
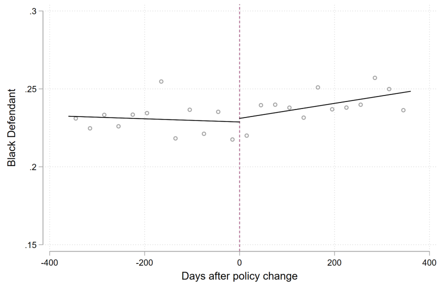
Figures and Tables

Figure 1: Frequency of Running Variable

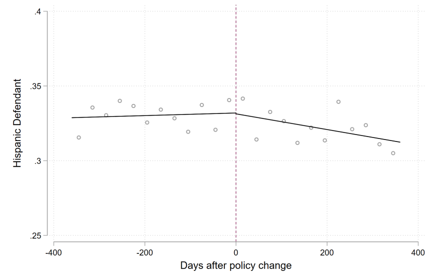


Notes: This figure shows the distribution of running variable observations near the adoption of risk assessment scores. Each bin is 2 days. The dashed line marks the day of the policy change.

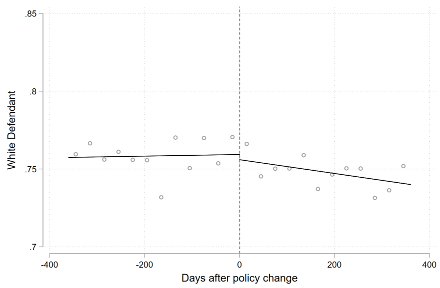
Figure 2: Smoothness of Baseline Covariates



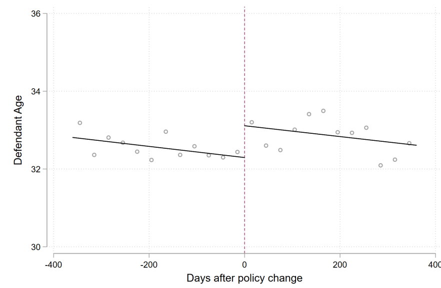
(a) Black Defendant



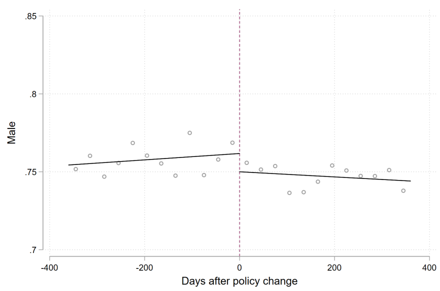
(b) Hispanic Defendant



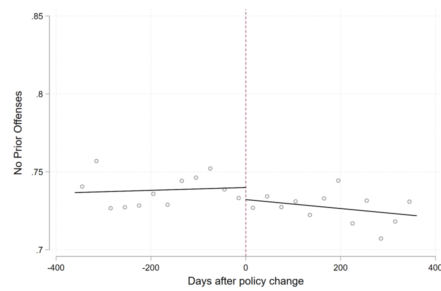
(c) White Defendant



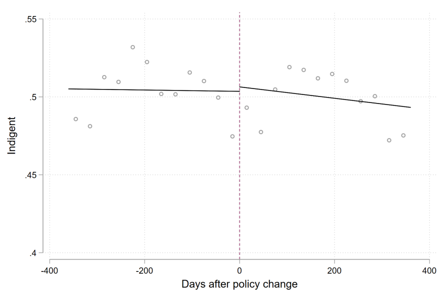
(d) Defendant Age



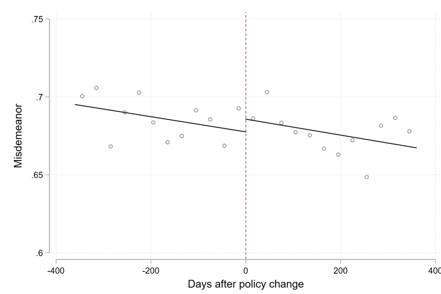
(e) Defendant Gender



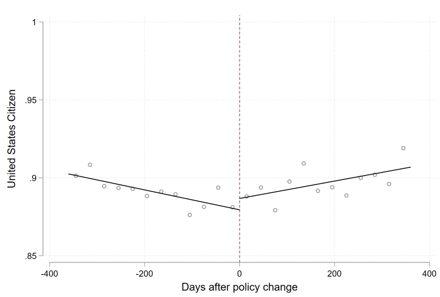
(f) No Priors



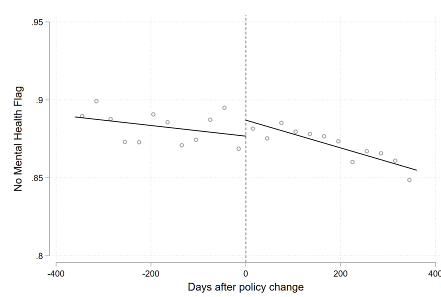
(g) Indigent Status



(h) Misdemeanor or Felony Case



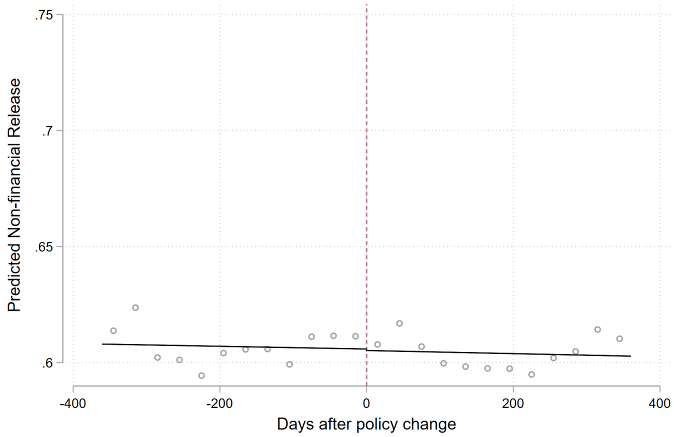
(i) United States Citizen



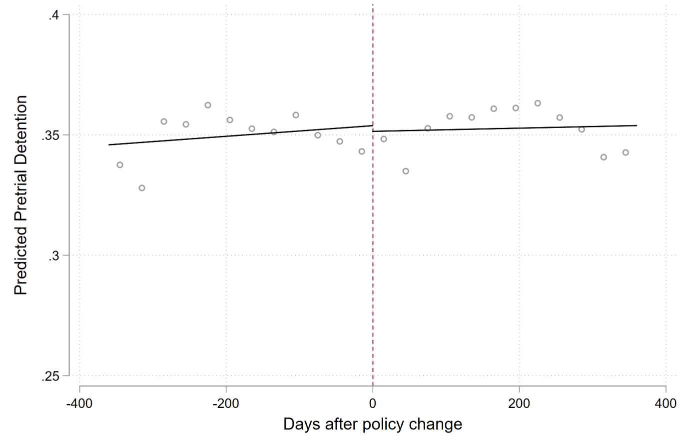
(j) Mental Health Flag

Notes: These figures plot tests of the regression discontinuity design. Each figure plots linear fits of the outcome listed and means of the outcome variable in 30 day bins.

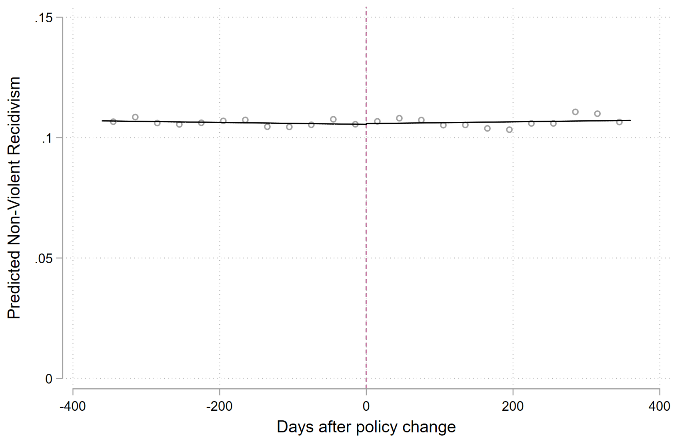
Figure 3: Regression Discontinuity Results for Predicted Values



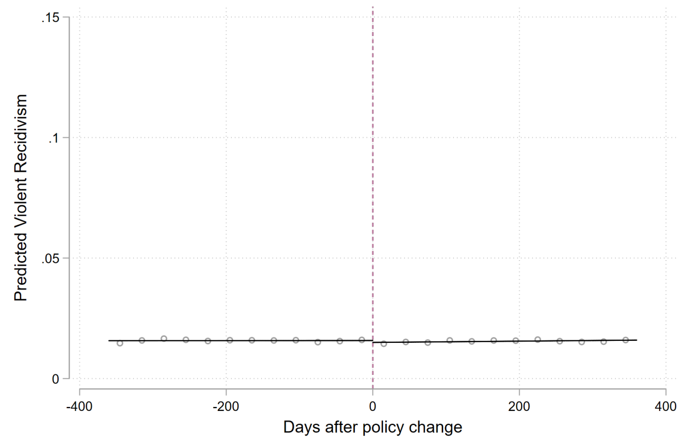
(a) Predicted Probability of Release on Non-financial Bond



(b) Predicted Probability Pretrial Detention



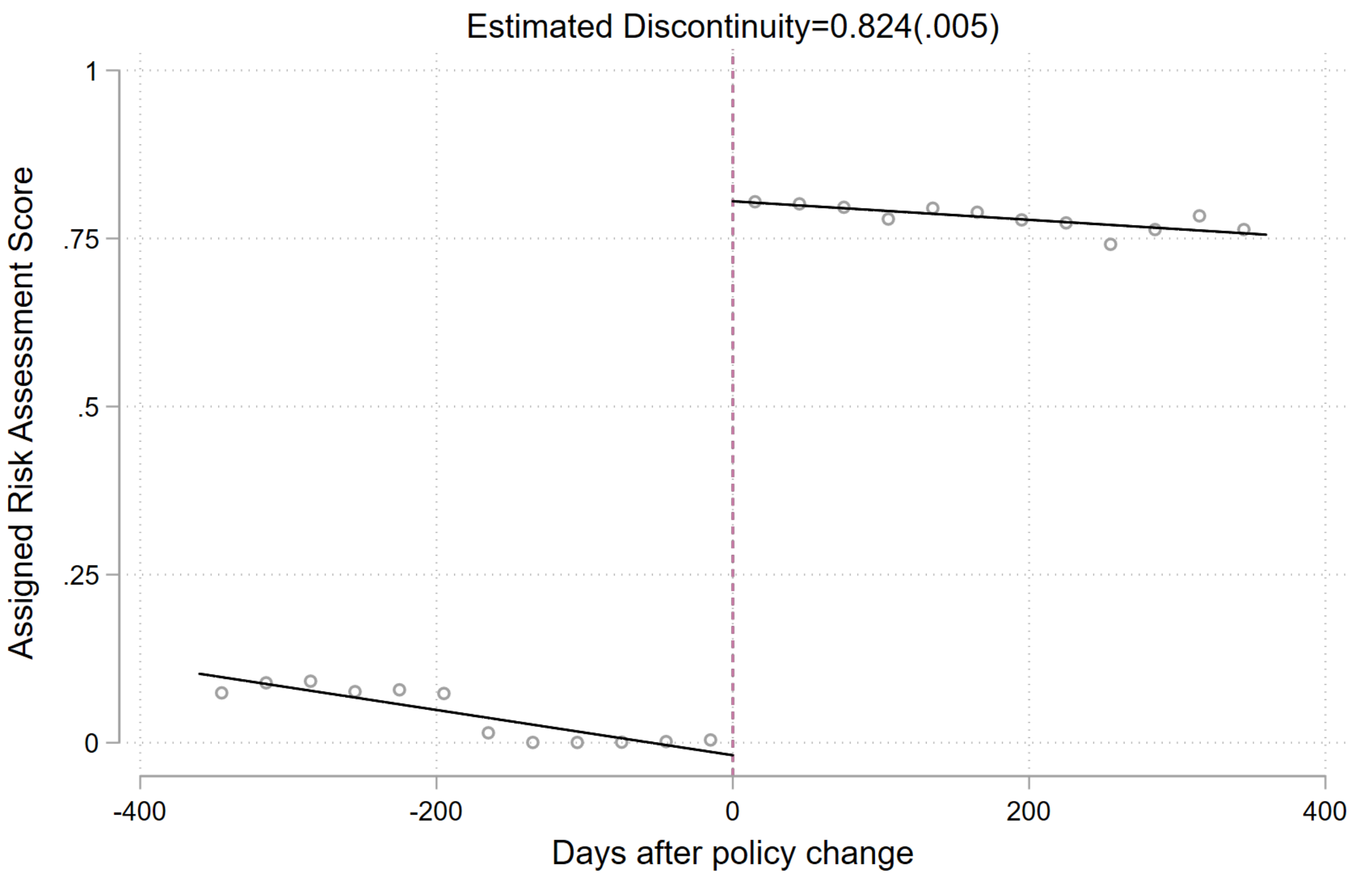
(c) Predicted Probability of Non-Violent Recidivism



(d) Predicted Probability of Violent Recidivism

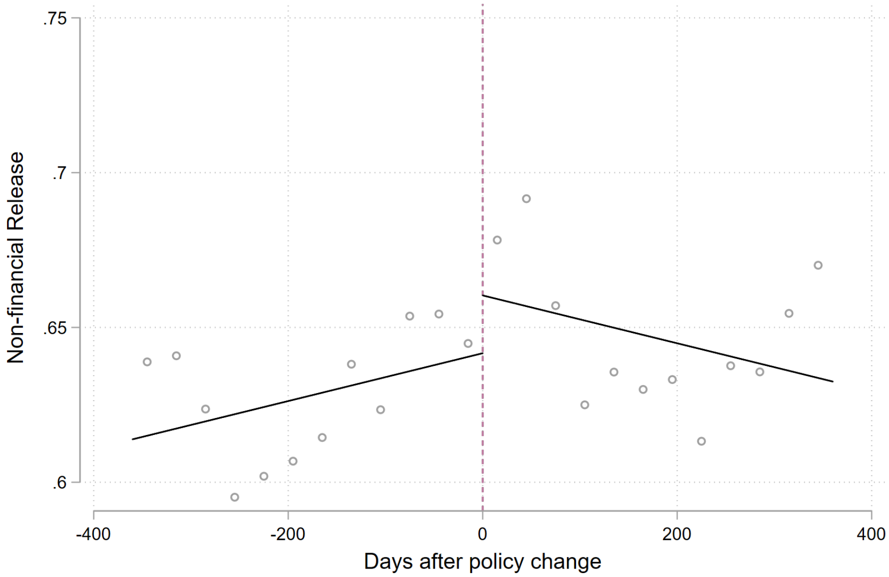
Notes: These figures plot tests of the regression discontinuity design. Each figure plots linear fits of the outcome listed and means of the outcome variable in 30 day bins. Outcome variables are predicted using observable case and defendant characteristics. Specifically we use race, age, gender, criminal history, indigent status, severity of arrest, mental health status, US citizenship status, along with a court and day-of-week fixed effects. A bandwidth of 360 days is shown.

Figure 4: Regression Discontinuity Results for the Probability of Receiving a Risk Assessment Score

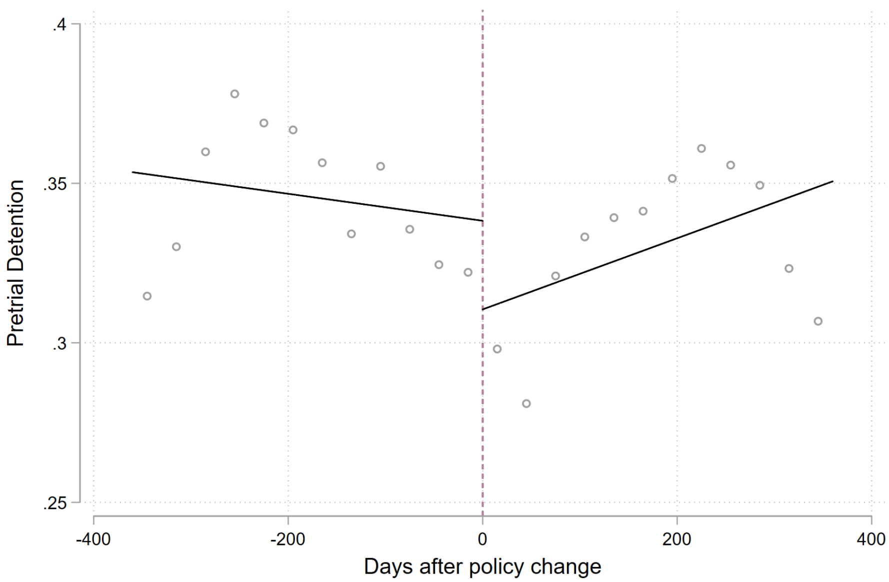


Notes: This figure shows the regression discountinuity estimate of the effect of implementing a risk assessment score policy on useage of risk assessment scores by ploting the mean of risk assessment score take-up in 30 day bins with linear fits. The outcome variable takes on a value of one if a defendant has a risk assemssment score and zero if she does not. A bandwidth of 360 days is shown.

Figure 5: Regression Discontinuity Results for Non-financial Bond and Pretrial Detention



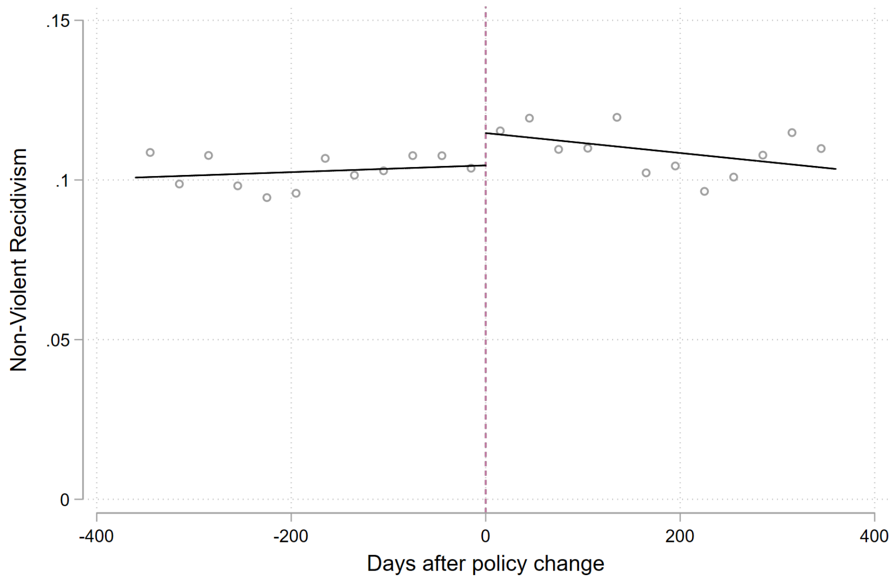
(a) Non-financial Bond



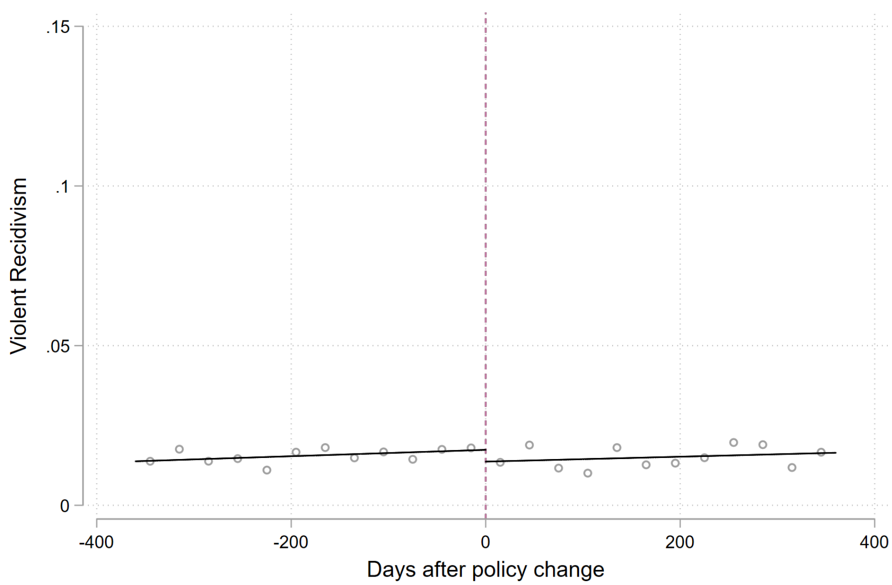
(b) Pretrial Detention

Notes: This figure shows the regression discountinuity estimate of the effect of implementing a risk assessment score policy on the non-financial bond or pretrial detention by ploting the mean non-financial bond or pretrial detention in 30 day bins with linear fits. A bandwidth of 360 days is shown.

Figure 6: Regression Discontinuity Results for Recidivism



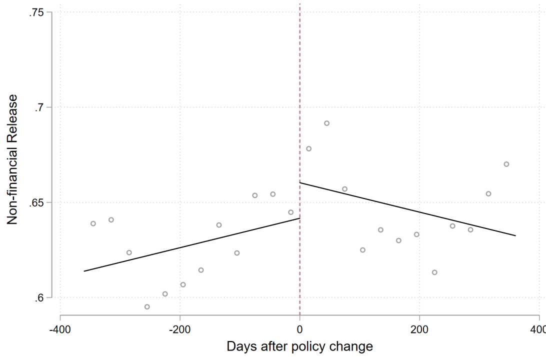
(a) Probability of Non-Violent Recidivism



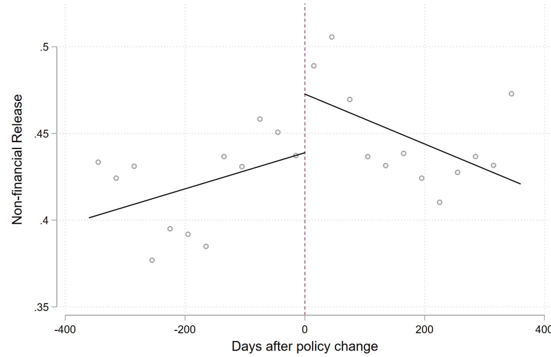
(b) Probability of Violent Recidivism

Notes: This figure shows the regression discountinuity estimate of the effect of implementing a risk assessment score policy on the non-violent and violent recidivism by ploting the mean of the outcome variable in 30 day bins with linear fits. A bandwidth of 360 days is shown.

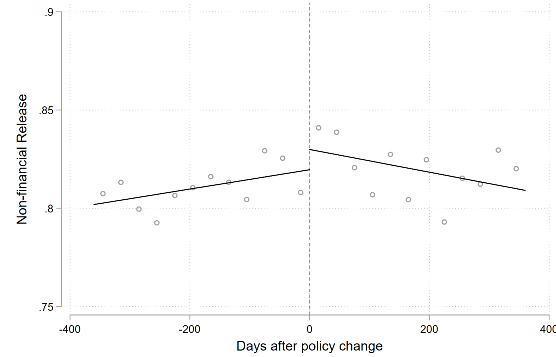
Figure 7: Indigent Regression Discontinuity Results for Non-financial Bond and Pretrial Detention



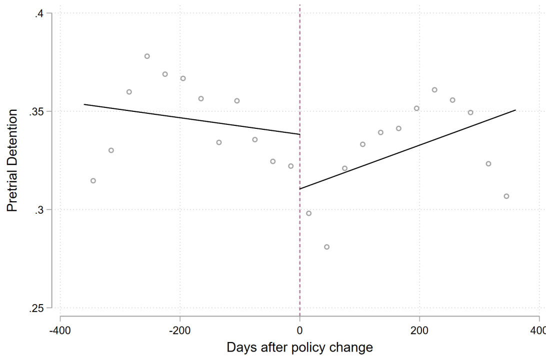
(a) Non-financial bond - Entire Sample



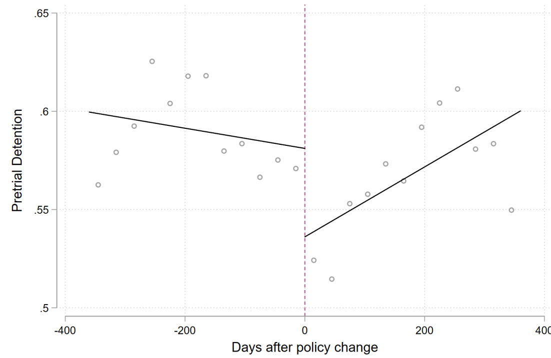
(b) Non-financial bond - Indigent



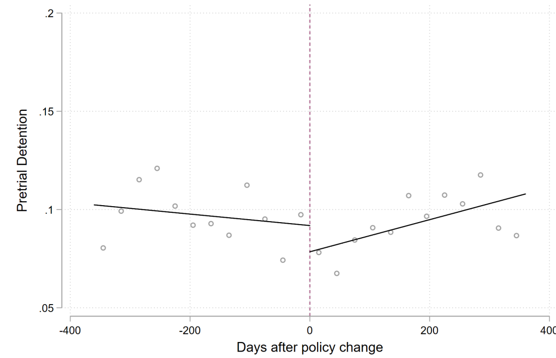
(c) Non-financial bond - Non-Indigent



(d) Pretrial Detention - Entire Sample



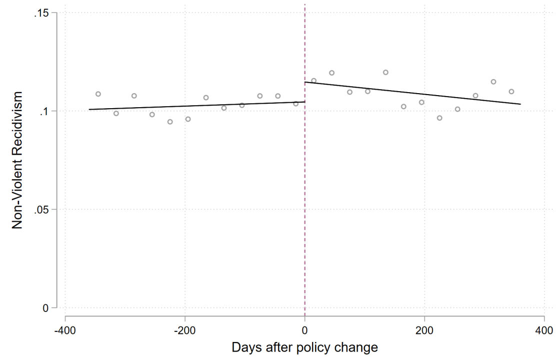
(e) Pretrial Detention - Indigent



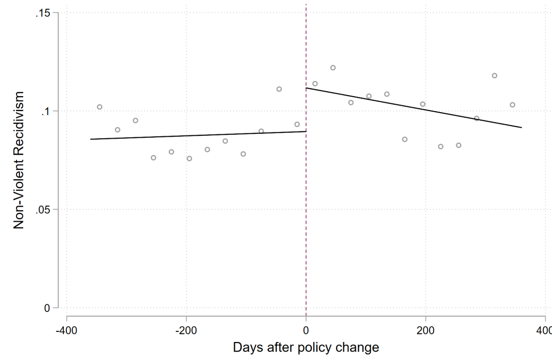
(f) Pretrial Detention - Non-Indigent

Notes: This figure shows the regression discountinuity estimate of the effect of implementing a risk assessment score policy on the non-financial bond or pretrial detention by ploting the mean non-financial bond or pretrial detention in 30 day bins with linear fits. A bandwidth of 360 days is shown.

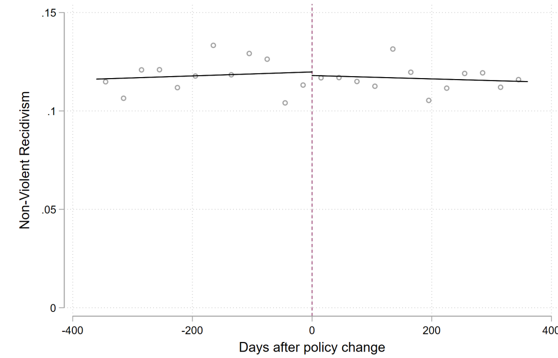
Figure 8: Indigent Regression Discontinuity Results for Recidivism



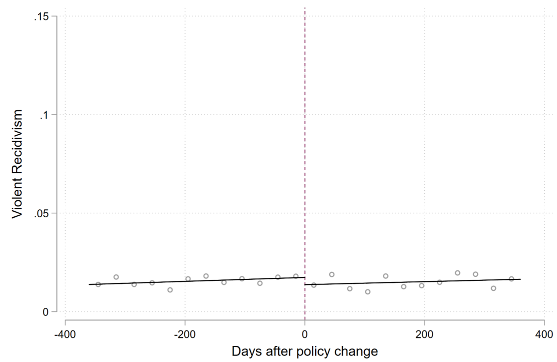
(a) Non-violent Recidivism - Entire Sample



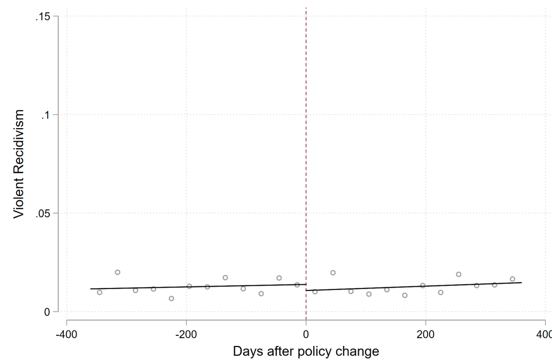
(b) Non-violent Recidivism - Indigent



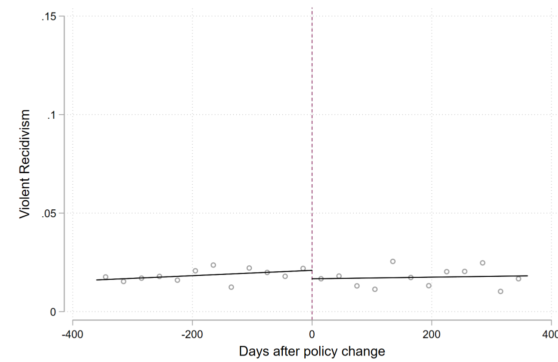
(c) Non-violent Recidivism - Non-Indigent



(d) Violent Recidivism - Entire Sample



(e) Violent Recidivism - Indigent



(f) Violent Recidivism - Non-Indigent

Notes: This figure shows the regression discountinuity estimate of the effect of implementing a risk assessment score policy on the non-financial bond or pretrial detention by ploting the mean non-financial bond or pretrial detention in 30 day bins with linear fits. A bandwidth of 360 days is shown.

Table 1: Summary Statistics

	Mean	Standard Deviation	Number of Observations
Black Defendant	0.24	0.43	143,092
Hispanic Defendant	0.3272	0.469	143,092
White Defendant	0.7480	0.434	143,092
Misdemeanor	0.6772	0.468	143,092
Defendant Age	32.5302	11.220	143,089
United States Citizen	0.8941	0.308	143,092
Male	0.7578	0.428	143,077
Indigent	0.5124	0.500	143,092
No Prior Offenses	0.7638	0.425	143,092
No Mental Health Flag	0.8762	0.329	143,092
Non-financial Release	0.6253	0.484	127,904
Pretrial Detention	0.3532	0.478	143,092
Violent Recidivism	0.0154	0.123	143,092
Non-Violent Recidivism	0.1056	0.307	143,092

Notes: Each observation is a separate case. Data are from Travis County Courts and Travis County Pretrial Services for the years 2011-2015.

Table 2: Release Regression Discontinuity Results

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome: Non-financial Bond						
RD_Estimate	0.0291***	0.0296***	0.0421***	0.0467***	0.0406**	0.0429***
	(0.0103)	(0.0112)	(0.0118)	(0.0129)	(0.0162)	(0.0158)
Observations	25112	48352	18598	36612	12330	23840
FDR q-value	0.01	0.016	0.001	0.001	0.024	0.028
Bandwidth	180.2	343.2	135.1	257.4	90.09	171.6
Outcome: Pretrial Detention						
RD_Estimate	-0.0286***	-0.0315***	-0.0308***	-0.0348***	-0.0338***	-0.0254*
	(0.00729)	(0.0102)	(0.00841)	(0.0118)	(0.0128)	(0.0145)
Observations	39532	44828	28924	33590	19038	21918
FDR q-value	0.001	0.008	0.001	0.006	0.024	0.16
Bandwidth	248.2	284.6	186.1	213.4	124.1	142.3
Controls	Y	Y	Y	Y	Y	Y
Quadratic	N	Y	N	Y	N	Y
Running Variable Control	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Each cell shows results for a separate regression. Each panel shows results for a different dependent variable and the key independent variable is an indicator for policy enactment. Robust standard errors are in parentheses. All specifications control for the distance from policy enactment. The optimal (MSE) bandwidth is used to determine the sample for each separate regression.

Table 3: Recidivism Regression Discontinuity Results

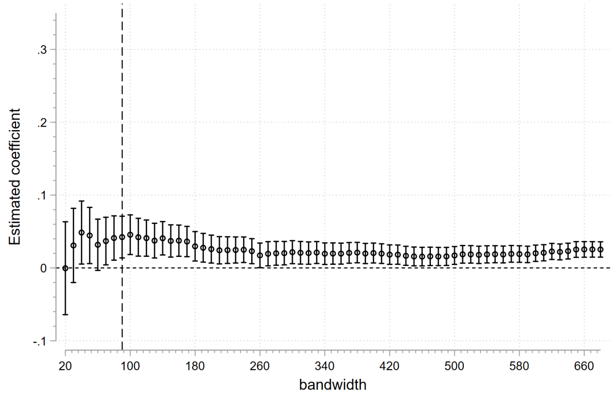
	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome: Non-Violent Recidivism						
RD_Estimate	0.00963*	0.0132*	0.0105*	0.0101	0.0105	0.0106
	(0.00508)	(0.00674)	(0.00586)	(0.00778)	(0.00730)	(0.00948)
Observations	58532	73926	44064	56370	28734	37998
FDR q-value	0.058	0.067	0.098	0.256	0.199	0.351
Bandwidth	371.6	478.7	278.7	359.0	185.8	239.3
Outcome: Violent Recidivism						
RD_Estimate	-0.00439*	-0.00375	-0.00222	-0.00302	-0.00396	-0.00138
	(0.00225)	(0.00276)	(0.00260)	(0.00320)	(0.00323)	(0.00392)
Observations	46830	69076	35278	52680	22924	35104
FDR q-value	0.058	0.174	0.393	0.346	0.221	0.724
Bandwidth	297.5	445.4	223.1	334.1	148.8	222.7
Controls	Y	Y	Y	Y	Y	Y
Quadratic	N	Y	N	Y	N	Y
Running Variable Control	Y	Y	Y	Y	Y	Y

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

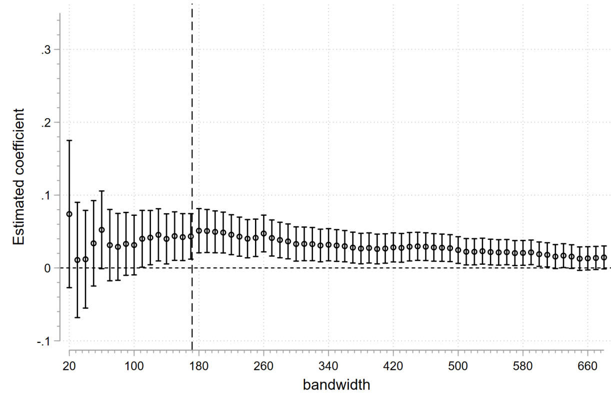
Notes: Each cell shows results for a separate regression. Each Panel shows results for a different dependent variable and the key independent variable is an indicator for policy enactment. Robust standard errors are in parentheses. All specifications control for the distance from policy enactment. The optimal (MSE) bandwidth is used to determine the sample for each separate regression.

A Appendix

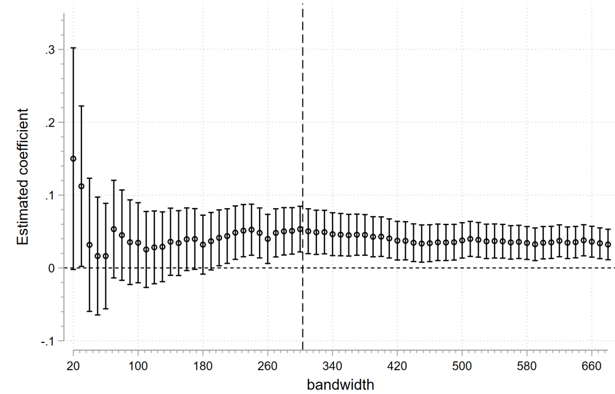
Figure A1: Non-financial and Pretrial detention Robustness



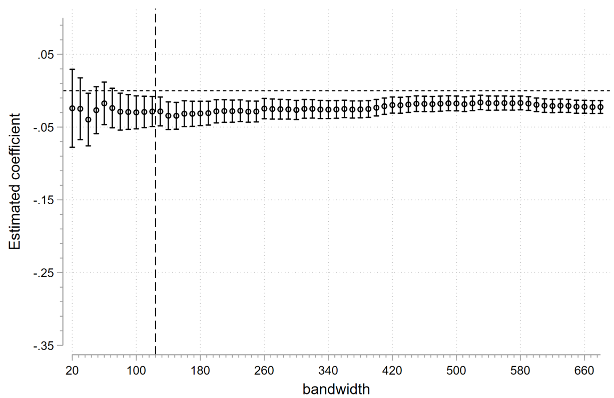
(a) Linear - Non-financial Bond



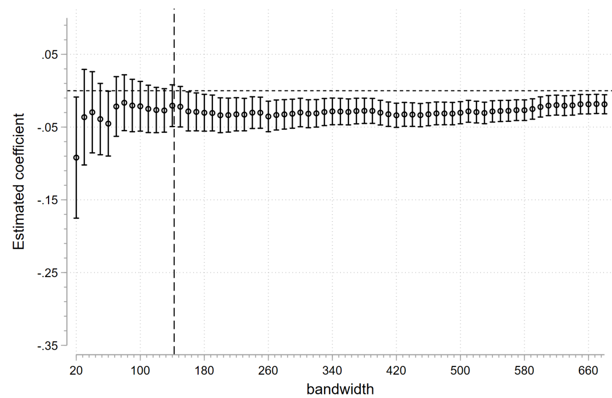
(b) Quadratic - Non-financial bond



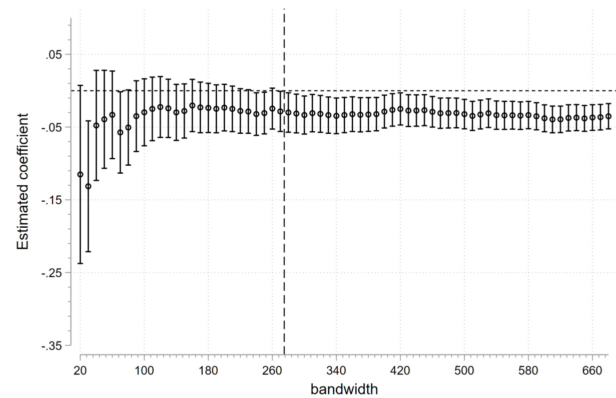
(c) Cubic - Non-financial bond



(d) Linear - Pretrial Detention



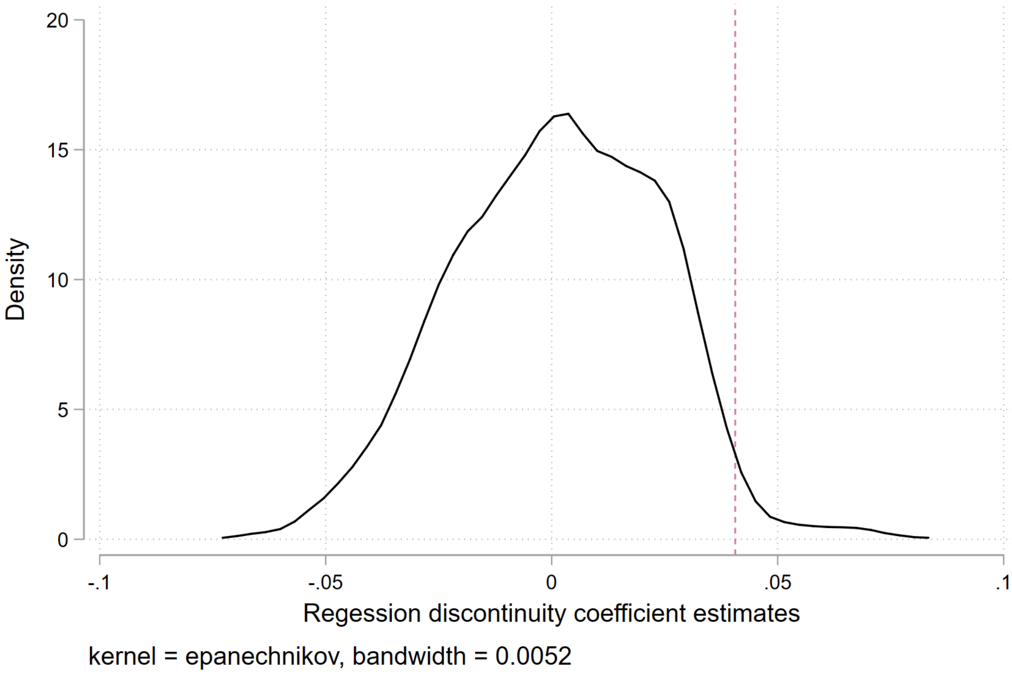
(e) Quadratic - Pretrial Detention



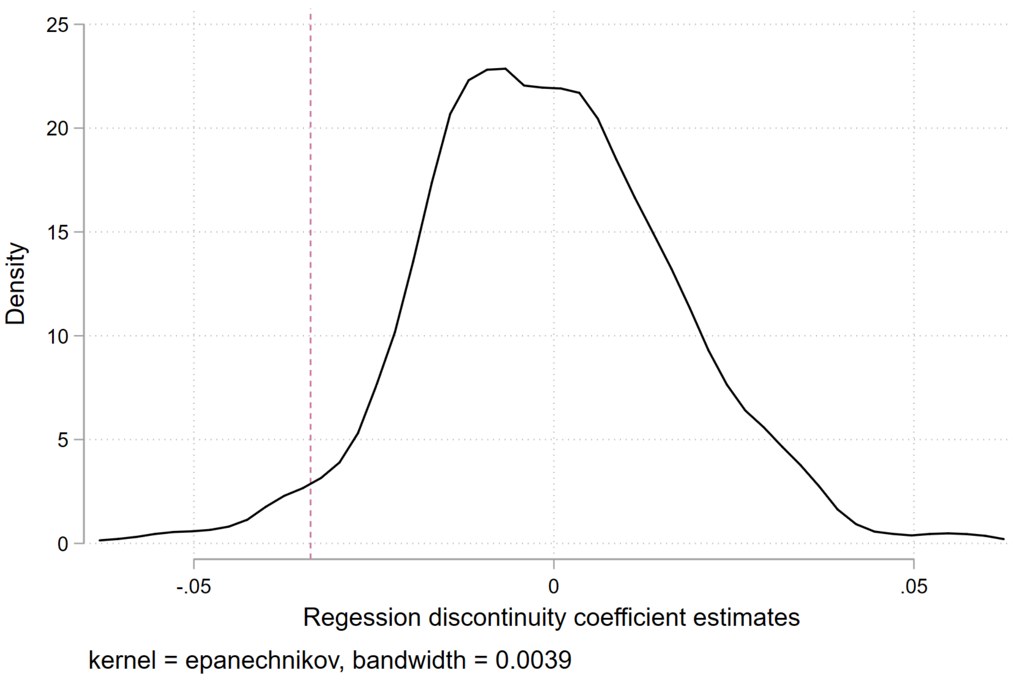
(f) Cubic - Pretrial Detention

Notes: Each figure plots coefficients from 64 different regressions using different bandwidths. Ninety-five percent confidence intervals are also presented. The optimal MSE bandwidth for each specificiation is marked with the dash line.

Figure A2: Reassigning Treatment Date



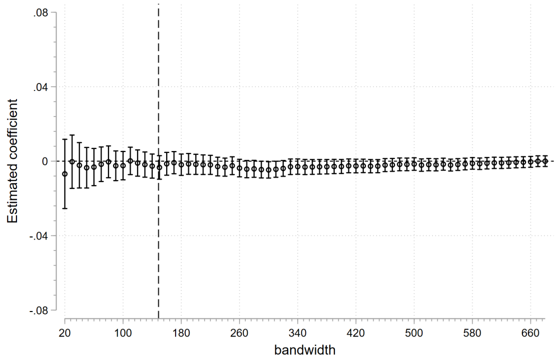
(a) Outcome: Release on Non-financial Bond



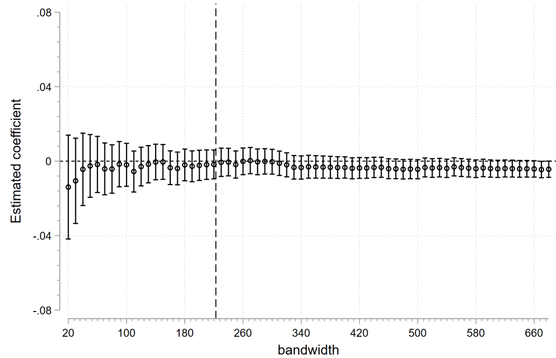
(b) Outcome: Pretrial Detention

Notes: This figure plots the distribution of 910 regession discontiunity coefficients from equation (1) using pre-treatment data. For the probability of release on non-financial bond our estimate reported in Table 2 is greater than 97.99 percent of all placebo estimates. For pretrial detention our estimate reported in Table 3 is less than 95.88 percent of all placebo estimates.

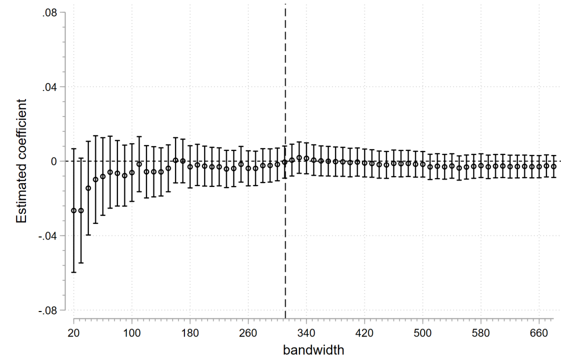
Figure A3: Recidivism Robustness



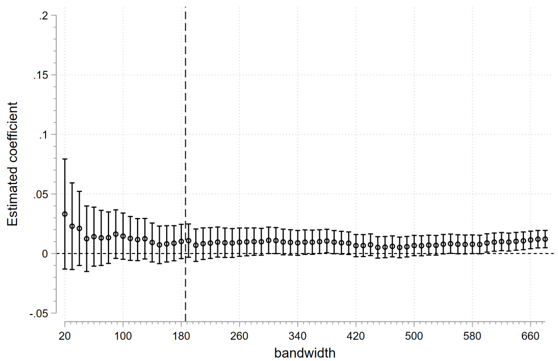
(a) Linear - Violent Recidivism



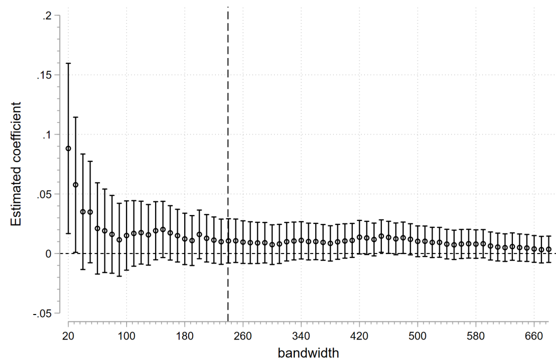
(b) Quadratic - Violent Recidivism



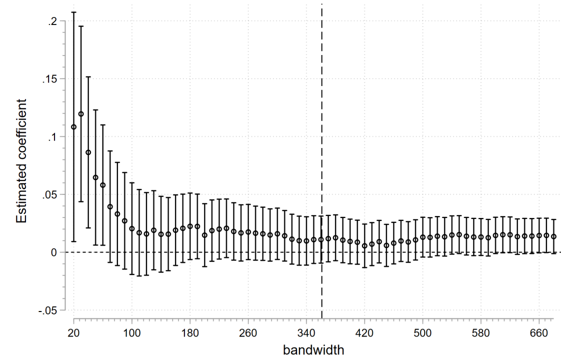
(c) Cubic - Violent Recidivism



(d) Linear - Non-Violent Recidivism



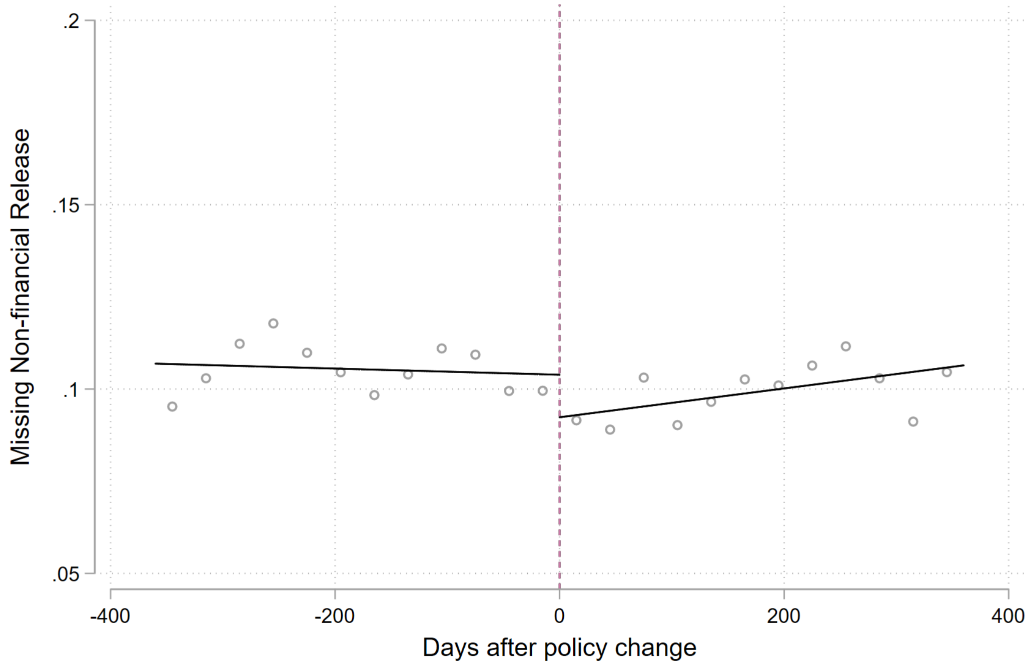
(e) Quadratic - Non-Violent Recidivism



(f) Cubic - Non-Violent Recidivism

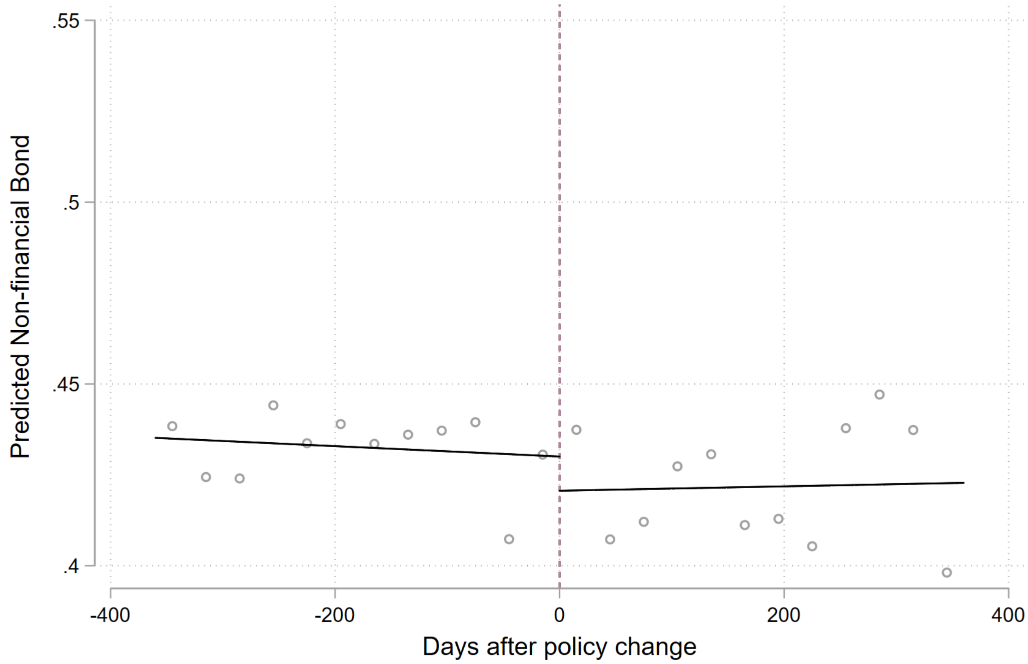
Notes: Each figure plots coefficients from 64 different regressions using different bandwidths. Ninety-five percent confidence intervals are also presented. The optimal MSE bandwidth for each specificiation is marked with the dash line.

Figure A4: Regression Discontinuity Results for Probability of Missing Outcome Data



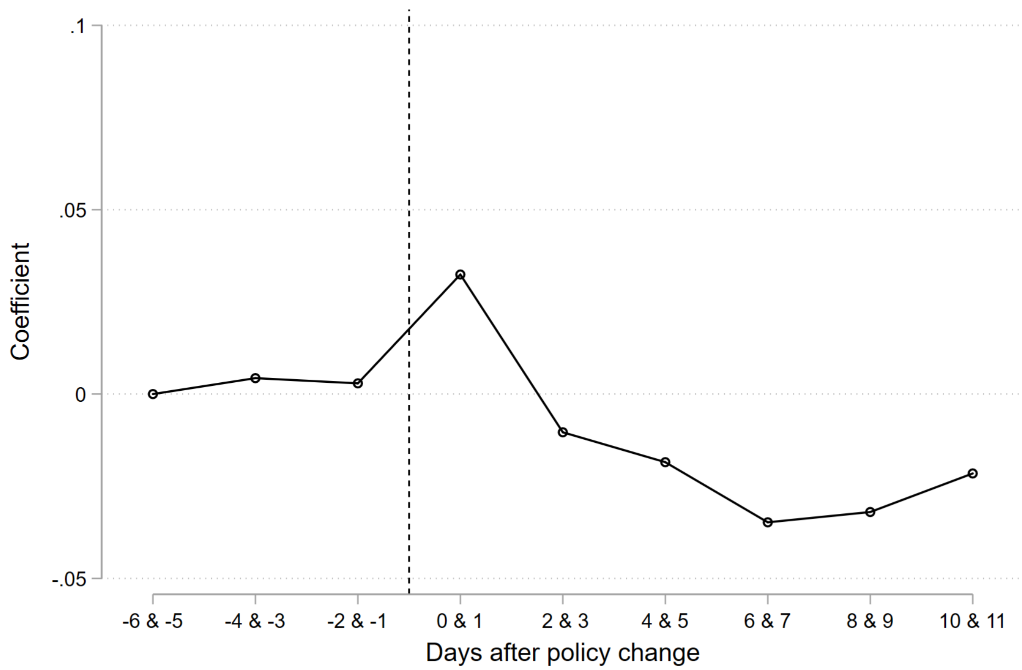
Notes: This figure shows the regression discountinuity estimate of the effect of implementing a risk assessment score policy on the likelihood of missing data on *probability of release on non-financial bond* by ploting the mean of the probability of missing in 30 day bins with linear fits. A bandwidth of 360 days is shown.

Figure A5: Regression Discontinuity Results for Predicted Probability of Release on non-financial bond for Defendants with Missing Outcome Data



Notes: This figure plots and additional test of the regression discontinuity design. This graph includes linear fits of the the predicted probability of release on non-financial bond and means of the predicted probability of release on non-financial bond in 30 day bins. Outcome variables are predicted using observable case and defendant characteristics. A bandwidth of 360 days is shown. The RD is calculated only using observations from defendants who are missing data on non-financial bond.

Figure A6: Dynamic Effects of Risk Assessment Scores



Notes: This figure plots the coefficients from the regression of non-financial bond on indicators for months before or after risk assessment adoption. Individual level controls for race, age, gender, citizenship and indigent status of the defendant along with controls for the severity of the crime (misdemeanor or not) as well as fixed effects for the court assigned and day-of-week of booking. A court-specific time trend is also included.

Table A1: Tests of the identifying assumption of the RD analysis

	Court 1	Court 2	Court 3	Court 4	Court 5	Court 6	Court 7	Court 8	Court 9	Court 10	Court 11	Court 12
RD_Estimate	-0.00720	0.00723	0.000311	-0.00361	-0.000724	0.00118	0.000806	0.000363	0.00454	0.00174	-0.0137*	0.00258
	(0.00771)	(0.00697)	(0.00361)	(0.00442)	(0.00418)	(0.00762)	(0.00358)	(0.00411)	(0.00744)	(0.00380)	(0.00823)	(0.00815)
Observations	30638	37860	39698	23258	28568	31322	38320	28734	33238	35572	22300	27188
Bandwidth	196.6	238.4	250.0	150.1	184.1	200.7	241.5	185.5	211.4	225.3	144.5	175.4
Run. Var. Control	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

	Black Def.	Hispanic Def.	White Def.	Misdemeanor	Def. Age	US Citizen	Male	Indigent	No Priors	Mental Health
RD_Estimate	0.00412	-0.00148	-0.00857	0.0155	0.704**	0.00114	-0.00909	0.00138	-0.00992	0.00425
	(0.0103)	(0.0101)	(0.0104)	(0.0116)	(0.336)	(0.00776)	(0.00990)	(0.0141)	(0.0109)	(0.00698)
Observations	25708	34086	26032	24712	16818	25162	29286	19190	25708	34362
Bandwidth	165.6	216.9	167.9	159.5	110.8	162.4	188.6	125.0	165.2	218.5
Run. Var. Control	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents results for separate regression. Robust standard errors are in parentheses. All specifications control for a linear function of distance from policy enactment in which the slope is allowed to vary on either side of the cutoff. The optimal (MSE) bandwidth is used to determine the sample for each separate regression.

Table A2: Regression Discontinuity Results for Predicted Outcomes

	Optimal Bandwidth	
	(1)	(2)
Outcome: Predicted Non-financial Bail		
RD_Estimate	0.000200	0.000128
	(0.00800)	(0.00872)
Observations	13744	26600
Bandwidth	90.09	171.6
Outcome: Predicted Non-Violent Recidivism		
RD_Estimate	-0.00501	0.00526
	(0.00852)	(0.0120)
Observations	19038	21918
Bandwidth	124.1	142.3
Outcome: Predicted Non-Violent Recidivism		
RD_Estimate	0.00212	0.000900
	(0.00131)	(0.00172)
Observations	28734	37998
Bandwidth	185.8	239.3
Outcome: Predicted Violent Recidivism		
RD_Estimate	-0.000994	-0.00126
	(0.000633)	(0.000778)
Observations	22924	35104
Bandwidth	148.8	222.7
Controls	N	N
Quadratic	N	Y
Running Variable Control	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents results for a separate regression where the key independent variable is an indicator of policy enactment. Robust standard errors are in parentheses. All specifications control for the distance from policy enactment. The optimal (MSE) bandwidth from Table 2 and Table 3 (the optimal bandwidth for the actual outcome variable) is used to determine the sample for each separate regression. Outcome variables are predicted using observable case and defendant characteristics. Specifically we use race, age, gender, criminal history, indigent status, severity of arrest, mental health status, US citizenship status, along with a court and day-of-week fixed effect. Column (1) presents a linear functional form and column (2) is quadratic.

Table A3: Regression Discontinuity Results for Indigent and Non-Indigent Defendants

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Indigent						
Outcome: Non-financial Bail						
RD_Estimate	0.0317**	0.0420**	0.0576***	0.0553**	0.0685***	0.0673**
	(0.0159)	(0.0189)	(0.0184)	(0.0218)	(0.0226)	(0.0268)
Observations	12870	20904	9494	15848	6168	10116
Bandwidth	196.8	315.4	147.6	236.5	98.40	157.7
Outcome: Pretrial Detention						
RD_Estimate	-0.0323***	-0.0301*	-0.0278**	-0.0406**	-0.0390**	-0.0417*
	(0.0121)	(0.0172)	(0.0141)	(0.0198)	(0.0173)	(0.0244)
Observations	20630	23494	14994	17418	9784	11376
Bandwidth	256.0	293.1	192.0	219.8	128.0	146.5
Panel B: Non-Indigent						
Outcome: Non-financial Bail						
RD_Estimate	0.0117	0.00952	0.0182	0.0148	0.0201	0.0302**
	(0.0104)	(0.0110)	(0.0119)	(0.0126)	(0.0144)	(0.0154)
Observations	19808	39162	14744	29684	9708	19956
Bandwidth	264.3	532.4	198.2	399.3	132.1	266.2
Outcome: Pretrial Detention						
RD_Estimate	-0.0153**	-0.0217**	-0.0275***	-0.0266***	-0.0146	-0.0180
	(0.00725)	(0.00859)	(0.00828)	(0.00986)	(0.0101)	(0.0121)
Observations	20392	32202	15194	24270	9896	16262
Bandwidth	261.4	417.2	196.1	312.9	130.7	208.6
Controls	Y	Y	Y	Y	Y	Y
Quadratic	N	Y	N	Y	N	Y
Running Variable Control	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents results for a separate regression where the key independent variable is an indicator of policy enactment. Robust standard errors are in parentheses. All specifications control for the distance from policy enactment. The optimal (MSE) bandwidth is used to determine the sample for each separate regression. Panel A and B present results for indigent and non-indigent subgroups respectively.

Table A4: Regression Discontinuity Results for Indigent and Non-Indigent Defendants

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Indigent Defendants						
Outcome: Non-Violent Recidivism						
RD_Estimate	0.0186**	0.0200**	0.0120	0.0116	0.0153	0.0191
	(0.00818)	(0.00990)	(0.00953)	(0.0115)	(0.0118)	(0.0140)
Observations	21968	33662	16118	25718	10550	17026
Bandwidth	272.4	429.6	204.3	322.2	136.2	214.8
Outcome: Violent Recidivism						
RD_Estimate	-0.00214	-0.00277	-0.00357	-0.00195	0.000384	-0.00220
	(0.00247)	(0.00296)	(0.00292)	(0.00350)	(0.00358)	(0.00431)
Observations	31692	52178	24098	38120	15852	25950
Bandwidth	402.5	651.4	301.9	488.5	201.3	325.7
Panel B: Non-Indigent						
Outcome: Non-Violent Recidivism						
RD_Estimate	-0.000524	-0.000778	0.000119	0.00222	0.00324	0.00759
	(0.00759)	(0.00921)	(0.00873)	(0.0106)	(0.0106)	(0.0130)
Observations	26838	40158	20190	30382	13406	20392
Bandwidth	344.3	523.6	258.2	392.7	172.1	261.8
Outcome: Violent Recidivism						
RD_Estimate	-0.00600*	-0.00548	-0.00400	-0.00445	-0.00427	-0.00436
	(0.00325)	(0.00414)	(0.00375)	(0.00480)	(0.00465)	(0.00588)
Observations	24540	33780	18580	25666	12300	17110
Bandwidth	316.7	439.1	237.6	329.3	158.4	219.5
Controls	Y	Y	Y	Y	Y	Y
Quadratic	N	Y	N	Y	N	Y
Running Variable Control	Y	Y	Y	Y	Y	Y

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Each cell shows results for a separate regression. Each Panel shows results for a different dependent variable and the key independent variable is an indicator for policy enactment. Robust standard errors are in parentheses. All specifications control for the distance from policy enactment. The optimal (MSE) bandwidth is used to determine the sample for each separate regression. Panel A and B present results for indigent and non-indigent subgroups respectively.

Table A5: Regression Discontinuity Results for the Probability of Missing Data

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*		*Optimal Bandwidth for Pr(Missing)*	
Outcome: Missing Data								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-0.00527	0.000807	-0.00659	-0.00599	0.0000473	-0.00623	-0.00635	0.00380
	(0.00527)	(0.00843)	(0.00620)	(0.00675)	(0.00712)	(0.00778)	(0.00966)	(0.00946)
Observations	40600	34086	28016	54084	20778	40934	13744	26600
Bandwidth	255.3	216.8	180.2	343.2	135.1	257.4	90.09	171.6
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Quadratic	N	Y	N	Y	N	Y	N	Y
Run. Var. Control	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents results for separate regression. Each column presents results for the probability of missing data for the outcome variable *probability of non-financial release* and the key independent variable is an indicator for policy enactment. Robust standard errors are in parentheses. All specifications control for a linear function of distance from policy enactment in which the slope is allowed to vary on either side of the cutoff. The optimal (MSE) bandwidth is used to determine the sample for each separate regression in the first three columns. Columns (1)-(6) use the optimal bandwidth determined in Table 2. Columns (7)-(8) use the optimal bandwidth for the Probability of Missing Data.