ESSAYS ON THE ECONOMICS OF CRIME AND DISCRIMINATION

A Dissertation

by

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Submitted to the Office of Graduate and Professional Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

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May 2020

Major Subject: Economics

ABSTRACT

My dissertation focuses on the decisions making of prosecutors, police officers, and judges. Understanding how these criminal justice actors affect crime and contribute to disparities is particularly important given the high and racially disproportionate costs of the criminal justice system.

First, I consider the effect of prosecutor bias on defendant outcomes in Chapter 1. There is little evidence on the extent to which racial bias exhibited by prosecutors is responsible for racial disparities in the criminal justice system. This paper tests for racial bias in convictions by prosecutors. To identify effects, I leverage as-good-as-random variation in prosecutor race using detailed administrative data on the case assignment process and case outcomes in a major US city. I show that the assignment of an opposite-race prosecutor leads to an increase in conviction for property crimes. I find no evidence of effects for other types of crimes.

Second, I estimate the effect of a new technology, risk assessment scores, on judicial and defendant decisions in Chapter 2. 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 crime. We identify short-term effects by exploiting a large, sudden policy change using a regression discontinuity design. Results show that adopting a risk assessment score leads to increased release on non-financial bond and decreased pretrial detention. Additionally, we find risk assessment scores did not increase violent pretrial crime, however, there is suggestive evidence of increases in non-violent pretrial crime.

Finally, in Chapter 3 I consider how increased risk to officer safety changes police behavior. Residents of high-violence neighborhoods are concerned that police officers are failing to reduce crime, but also that when police officers do engage, their tactics are too severe. This paper examines whether risks to officer safety drive both phenomena. To do so, I exploit variation in unprovoked ambushes on police within and across beats using administrative 911 call data from a large American city. Results show that ambushes lead to a decline in arrests, but no increases in policing severity.

DEDICATION

To my parents, Becky and Jon Sloan.

ACKNOWLEDGMENTS

I am extremely grateful for the support and guidance of many people during the creation of this dissertation.

First, I would also like to thank the members of my dissertation committee, Laura Dague, Jennifer Doleac, Mark Hoekstra, and Jonathan Meer, for their advising and encouragement. I especially want to thank Mark Hoekstra for going above and beyond in supporting all his graduate students. This dissertation would not have been possible without his weekly meetings. Nearly everything I know about research can be attributed to Mark.

Of course, no dissertation is possible without outside perspective and support. I want to extend a special thanks to all the people in my life, especially my friends from college and high school, who I forced to learn a bit of economics. I am also grateful for their willingness to listen to my general worries and new research ideas.

I want to also thank my fellow Aggies for their constant support. Particularly, the honorary fifth member of my committee, Brittany Street, and the other weekly meeting members Meradee Tangvatcharapong, Adam Bestenbostel, and Abigail Peralta. Thank you for fielding all my "easy" questions.

CONTRIBUTORS AND FUNDING SOURCES

Contributors

This work was supervised by a dissertation committee consisting of Professor Mark Hoekstra, and Professors Jennifer Doleac and Jonathan Meer of the Department of Economics and Professor Laura Dague of the Bush School of Government and Public Service.

The data used in Chapter 1 were distributed by the Inter-university Consortium for Political and Social Research (ICPSR 34681) and the data in Chapter 3 were provided by the Indianapolis Police Department. The work in Chapter 2 consists of a collaboration with Dr. George Naufal and Dr. Heather Caspers of the Public Policy Research Institute at Texas A&M.

All other work conducted for the dissertation was completed independently by the student.

Funding Sources

Graduate study was partially supported by fellowships from Texas A&M University and a fellowship from the Private Enterprise Research Center at Texas A&M University.

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1. INTRODUCTION

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2. RACIAL BIAS BY PROSECUTORS: EVIDENCE FROM RANDOM ASSIGNMENT

2.1 Introduction

In the United States, there is widespread recognition of racial disparities in criminal justice outcomes. Black Americans are more than three times as likely to have a criminal record and to have been incarcerated compared to non-blacks (Shannon et al., 2017). Despite public concern that the system is unfair to black and Hispanic Americans (Rasmussen Reports, 2014; Pew Research Center, 2019), little is known about whether these disparities are caused by bias. Recently, legal scholars and judges have hypothesized that prosecutors could play a central role in perpetuating these racial disparities (Foster v. Chatman, 2016; State v. Monday, 2011; Pfaff, 2017; Rehavi and Starr, 2014; Smith and Levinson, 2011). This is because prosecutors have arguably more discretion than any other party when it comes to the handling of alleged crimes (Luna and Wade, 2015; Pfaff, 2017; Sklansky, 2018; Stith, 2008). In particular, prosecutors choose whether and how to dismiss a case, assign charges, offer plea deals, strike potential jury members, and handle a case at trial. However, there is little causal evidence on whether prosecutors exhibit racial bias in making these decisions. The purpose of this paper is to test for prosecutor racial bias in convictions.

The primary difficulty in testing for racial bias by prosecutors is the nonrandom matching of prosecutors and defendants. Nonrandom matching is commonplace, as prosecutors are often allowed to choose their cases during screening. To overcome this endogeneity concern, I exploit the random assignment, conditional on screening date, of defendants to prosecutors in New York County through the Early Case Assessment Bureau. Within each shift at the Early Case Assessment Bureau, cases are assigned on a rotational basis depending on a case's timestamp or when the case was received. The assignment works as follows: when prosecutors arrive at the office, they are given the earliest timestamped case available by the office administrator, who strictly enforces the case-assignment procedure. They cannot screen, or even look at, the case before they begin to work on it. When they finish writing up their case, they return to the office administrator and are again assigned the earliest timestamped case available.

This as-good-as-random assignment of prosecutors implies that a prosecutor's race is uncorrelated with a defendant's underlying guilt. Consequently, some defendants are randomly assigned, conditional on the case-screening dates, to prosecutors of their own race, while others are paired with opposite-race prosecutors. I provide empirical evidence for this random assignment by showing that prosecutor race is uncorrelated with defendant and case characteristics. I use this quasi-random variation in prosecutor race to identify opposite-race effects. Specifically, I estimate opposite-race effects by differencing out the role of defendant and prosecutor race, similar to other studies on racial bias (e.g., Price and Wolfers, 2010; Parsons et al., 2011; Anbarci and Lee, 2014; West, 2018a).

I identify effects using detailed administrative data from the New York County District Attorney's Office, which were collected by the Vera Institute (Kutateladze, 2017). The New York County District Attorney's Office prosecutes all cases originating in New York County (Manhattan). This totals over 100,000 cases per year from a jurisdiction of over 1.6 million, making it the nation's fourteenth largest prosecutor's office (City of New York, 2015). The New York County District Attorney's Office also promotes itself as being especially progressive due to its commitment to criminal justice reform, community partnerships, and reducing bias (Manhattan District Attorney, 2018b). The data collected include information on the case assignment (as well as the dismissal) process and conviction decisions for all cases assigned via the Early Case Assessment Bureau.

My results show significant evidence of prosecutorial bias against opposite-race defendants for property crimes, though not for other crimes. I estimate that being assigned an opposite-race prosecutor increases the probability of guilt by 5 percentage points ($\sim$ 8 percent) for property crimes. This difference represents 50 percent of the black-white gap in conviction rates for property crimes and is robust to multiple inference correction. Additional results indicate that these differences are driven by decreased dismissal of cases by opposite-race prosecutors and defendants with no criminal history.

Although criminology and law studies have long described the power and discretion of prosecutors, lack of data availability, and use of selection-on-observables approaches has left many important questions regarding prosecutor racial bias unanswered.[1] To my knowledge, this is the first paper to use as-good-as-random variation in opposite-race prosecutors to estimate racial bias. This paper primarily contributes to the literature on prosecutors and complements existing work on defense attorneys, in-group bias, and discrimination in the criminal justice system.

In addressing the role of opposite-race prosecutors, this paper contributes to the empirical literature on prosecutor behavior in general (Arora, 2019; Krumholz, 2019; Rehavi and Starr, 2014; Tuttle, 2019; Yang, 2016). Some existing papers focus on how prosecutors respond to financial and electoral pressures. Yang (2016) uses pension eligibility and judge deaths as instruments for judicial vacancies and resources constraints, concluding that these cause more prosecutor case dismissals. Using a regression discontinuity created by close elections, Arora (2019) and Krumholz (2019) find that when a District Attorney is Republican (versus Democratic) defendants receive harsher punishments for felonies. Krumholz (2019) also finds that the election of a nonwhite District Attorney leads to fewer prison admissions by comparing counties with and without a nonwhite District Attorney, over time.

This paper is most closely related to work by Rehavi and Starr (2014) and Tuttle (2019). In a seminal paper, Rehavi and Starr (2014) use a selection-on-observables approach and report that prosecutors may be responsible for racial disparities in federal sentencing. However, this finding can also be explained by the prosecutor selection of cases. Their approach also does not rule out the possibility that unobservable baseline differences between black and white defendants, rather than prosecutor racial bias, explain the sentencing disparities. In this paper, I address these concerns

[1] There are several papers that use a selection-on-observables method to conclude that defendant race may alter prosecutor decision-making at multiple stages such as the initial screening (Albonetti and Hepburn, 1996; Bishop et al., 2010; Franklin, 2010; Freiburger and Jordan, 2011; Kingsnorth et al., 1998; Kingsnorth and Macintosh, 2004; Kutateladze et al., 2014; Leiber and Johnson, 2008; Holleran et al., 2010; Riedel and Boulahanis, 2007; Sorensen and Wallace, 1999; Spears and Spohn, 1997; Spohn and Spears, 1997; Pyrooz et al., 2011; Wooldredge and Thistlethwaite, 2004), pretrial detainment (Demuth and Steffensmeier, 2004; Demuth, 2006; Freiburger and Hilinski, 2010; Kutateladze et al., 2014) dismissal (Franklin, 2010; Kutateladze et al., 2014; Spohn and Homey, 1993; Wooldredge and Thistlethwaite, 2004), guilty pleas (Albonetti, 1992), and sentencing (Hartley et al., 2007; Johnson, 2005; Johnson and Betsinger, 2009; Kutateladze et al., 2014; Shermer and Johnson, 2010; Spohn and Fornango, 2009; Ulmer et al., 2007). These papers are summarized in Kutateladze and Andiloro (2014) and Kutateladze et al. (2012).

by using the randomization of defendants to prosecutors, which allows me to isolate the role of opposite-race bias in prosecutor decisions. The level of detail in my data also allow me to be more thorough and investigate the degree of bias for each prosecutor.

Tuttle (2019) examines abnormal bunching in crack cocaine amounts used for federal sentencing and shows that black and Hispanic defendants receive harsher drug sentences. These results are likely driven by prosecutors shifting drug amounts just over a quantity threshold, triggering mandatory minimum sentences. This study differs from Tuttle (2019) in that I can test for racial bias in multiple prosecutor decisions across different types of cases, where the level of prosecutorial discretion, and thus, the scope for racial bias to matter, can differ. Importantly, this means that I consider the entire cumulative effect of prosecutor bias from case creation until disposition.

More generally, this paper also complements a larger related body of literature documenting the effects of other actors in the criminal justice system. Several of these papers have examined the effects of defense attorneys (e.g., Abrams and Yoon, 2010; Agan et al., 2020; Anderson and Heaton, 2012; Cohen, 2012; Iyengar, 2007; Roach, 2012; Shem-Tov, 2016). In addition, there has been considerable research on racial and gender bias by police officers (e.g., Antonovics and Knight, 2009; Anwar and Fang, 2006; Fryer Jr, 2020; Goncalves and Mello, 2018; Grogger and Ridgeway, 2006; Horrace and Rohlin, 2016; Johnson et al., 2019; Knowles et al., 1999; Pierson et al., 2017; Sanga, 2009; Weisburst, 2019; West, 2018a), judges (e.g., Depew et al., 2017; Eren and Mocan, 2018; Gazal-Ayal and Sulitzeanu-Kenan, 2010; Johnson, 2014; Knepper, 2017; Lim et al., 2016; Schanzenbach, 2005; Shayo and Zussman, 2011; Steffensmeier and Hebert, 1999), and juries (e.g., Anwar et al., 2012, 2018; Flanagan, 2018; Hoekstra and Street, 2018).

The results of this paper have several important implications. First, my results imply that in-group bias can persist despite the widespread focus on equal treatment in prosecutor training. For example, the American Bar Association's standards for prosecutors states, "The prosecutor should not manifest or exercise, by words or conduct, bias or prejudice based upon race" (American Bar Association, 2018). These guidelines even go so far as to advocate active resistance to bias: "A prosecutor's office should be proactive in its efforts to detect, investigate, and eliminate improper biases, with particular attention to historically persistent biases like race, in all of its work" (American Bar Association, 2018). This study's finding of racial bias for property crimes is particularly striking given that I study the New York County District Attorney's Office, which is known for actively trying to combat racial discrepancies in criminal justice outcomes (Manhattan District Attorney, 2018b). For example, in 2010, the New York County District Attorney's Office stopped prosecuting most low-level infractions and began offering a treatment program instead of probation for low-level drug crimes. Both policies are described as being particularly important for communities of color (Cyrus Vance For District Attorney, 2017). The New York County District Attorney's Office also employ a chief diversity officer and diversity committee because they believe a diverse staff can help reduce racial bias (Manhattan District Attorney, 2018a).

Second, as the majority of defendants are black and the majority of prosecutors are white, the consequences of opposite-race bias are disproportionately borne by black Americans. These costs often extend beyond penalties imposed by courts. Perhaps the most significant of these costs is the worsened labor market outcomes attributed to more convictions and thus, criminal records (Holzer et al., 2007; Finlay, 2008; Mueller-Smith and Schnepel, 2017; Pager, 2003; Raphael, 2014). Other long-term ramifications, such as the increased use of welfare programs, decreased mental health, less access to public housing, and some negative impacts on children, are also associated with having a criminal record (e.g., Curtis et al., 2013; Dobbie et al., 2018a; Johnson, 2009; Murray and Farrington, 2012; Wolff and Shi, 2012). Further, my opposite-race bias results are greatest for defendants with no criminal history. This means that for many defendants, prosecutor assignment is particularly important because it could be the difference between a clean record and their first conviction.

Finally, the results of this paper have compelling implications for designing policies aimed at reducing racial disparities. Estimates presented here suggest that fair treatment by prosecutors could reduce the black-white gap in property crime convictions by about one-half. Therefore, targeting prosecutor behavior could be a productive policy tool for reducing disparities. However, it is also important to recognize that for over half (62 percent) of the crimes in my sample, I do not find strong evidence of racial bias. Therefore, I note that many cases and defendants receive fair treatment already. In light of these differences across cases, it may be optimal to target specific crimes or case characteristics to eliminate bias better. This also highlights the need to understand further why bias only occurs in certain contexts.

2.2 Background and Data

2.2.1 Case Assignment and the Prosecutor's Role in New York County

The primary problem in assessing the effect of prosecutor race is the nonrandom matching of cases. To overcome this problem, I chose to study New York County, which gives no discretion in case selection to prosecutors for certain crimes. In New York County, after a defendant is arrested, the police are responsible for recording all arrest charges and prior arrest history during booking. If the case is a less serious offense, such as an infraction, or a violation, the defendant is often given a desk appearance ticket or court summons, and the case is not assigned to a prosecutor. Next, the police fax or email misdemeanor and felony cases to the Early Case Assessment Bureau, where misdemeanors and felonies are assigned to a prosecutor. Felony and misdemeanor cases follow a different assignment procedure. For felonies, a head prosecutor screens each case and assigns it to another prosecutor based on their experience with particular types of cases. Because the assignment of felony cases is not as-good-as random at the Early Case Assessment Bureau, I exclude them from my analysis.

In contrast to felonies, the assignment of misdemeanor cases is as-good-as random. Within each shift at the Early Case Assessment Bureau, cases are assigned on a rotational basis depending on a case's timestamp, which is when the bureau received it. The assignment works as follows: when prosecutors arrive at the office, the office administrator gives them the earliest timestamped case available. The timestamp on the case is essential. During my visit to the Early Case Assessment Bureau, multiple prosecutors and administrators mentioned the importance of handling cases in the order they arrived. To this end, the administration works 24 hours a day to handle arrests that come in outside of typical work hours, to ensure that timestamps are correct. A prosecutor cannot screen, or even look at, a case before she begins working on it. When she is finished writing up her case, she will return to the administrator and is again assigned the earliest timestamped case available. I was able to observe this prosecutor case assignment when I visited the Early Case Assessment Bureau. The as-good-as-random assignment of cases was also confirmed by the researchers who originally collected these data, although they do not consider the effects of opposite-race prosecutors, nor do they solely examine cases with as-good-as-random assignment (Kutateladze and Andiloro, 2014). In short, this assignment procedure means that the prosecutor and defendant pairing is as-good-as random within each screening day.

Nearly all first-year prosecutors will work at the Early Case Assessment Bureau as part of their training. Each month, a group of first-year prosecutors is assigned to work at the Early Case Assessment Bureau to handle misdemeanor cases by a supervisor. Because of these rotations, I can observe the decisions of many different prosecutors. However, they are primarily less experienced. When first-year prosecutors are not working at the Early Case Assessment Bureau, they are also exposed to the many different bureaus and units within the District Attorney of New York County's Office. These bureaus and units specialize in specific types of crimes, as prosecutors tend to believe that decision-making differs enough across case types to require specialization.

After a case is assigned to a prosecutor at the Early Case Assessment Bureau, the prosecutor has multiple opportunities to alter case outcomes. Specifically, the prosecutor can decide to decline to prosecute the case, change a defendant's charges, endorse pretrial detainment, pursue a case dismissal through adjournment in contemplation of dismissal, offer a plea deal, and design the plea deal. All of these decisions may alter a defendant's most crucial case outcome: guilty or not guilty.

The first decision a prosecutor makes is whether to decline to prosecute a case. In contrast to many other settings, prosecutors in New York County decline relatively few cases, likely due to the close relationship between the New York County's District Attorney's Office and the New York City Police Department (Kutateladze and Andiloro, 2014). This outcome is rare because most cases are only declined if the case has a complete lack of evidence or if the defendant was arrested for a crime that the District Attorney's Office has decided not to prosecute anymore.

Next, the prosecutor decides which charges to bring against a defendant at screening. Often this includes the option of increasing or decreasing the severity of charges assigned to a defendant's case.[2] For example, a defendant may be booked for a Class B misdemeanor crime, punishable by up to 90 days in jail, but a prosecutor may increase the crime to a Class A misdemeanor, punishable by up to 1 year in jail, at screening (New York State, 2018). The severity of charges is critical because prosecutors often choose to follow department norms for pretrial detainment, plea deals, and sentencing based on charge severity (Frederick and Stemen, 2012).

The prosecutor also has the option of offering and designing a plea deal for all defendants. A plea deal can include charges that are higher or lower than the initial charges for which a defendant is booked. During plea bargaining, a prosecutor can also recommend a particular sentence to the judge. While a judge must approve of any plea or sentence, prosecutors play a significant role in designing the attributes of the plea deal and sentencing request. If a defendant accepts a plea deal, she will be considered guilty.

Instead of a plea deal, the prosecutor can also offer the defendant a particular type of dismissal, referred to as an adjournment in contemplation of dismissal. This acts as an agreement to dismiss a case in 6 to 12 months if there are no subsequent arrests. In New York, an adjournment in contemplation of dismissal is not a conviction or an admission of guilt.[3] It is also extremely rare that an adjournment in contemplation of dismissal will be reopened, let alone lead to a guilty outcome. During 2010 and 2011 in New York County, 36,411 court events had an adjournment in contemplation of dismissal outcome. Of these events, only 1 percent (384) had a later recalendaring. A recalendaring implies that the case could have been reopened but not that the defendant was tried again and found guilty. Like a plea deal, an adjournment in contemplation of dismissal must be approved by a judge, but it cannot be offered without the approval of the prosecutor.[4]

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[2] Prosecutors in Manhattan are specifically trained to be very careful in assigning screening charges. For example, prosecutors are told not to merely rerecord the arresting charges because the police officer may be unaware of the criminal history of a defendant or the details of the characteristics of a specific charge.

[3] New York Criminal Procedure §170.55

[4] There are also special marijuana adjournment in contemplation of dismissals that can be offered without the approval of the prosecutor. These can only be offered in marijuana drug cases.

Finally, a case can be disposed through a dismissal. A dismissal can be the result of a motion brought by a judge, defendant, or prosecutor. Misdemeanor cases can also be dropped unilaterally by a prosecutor (Kutateladze and Andiloro, 2014). For dismissals, charges against the defendant are immediately dropped. The most common reason for a dismissal in New York County is a lack of speedy prosecution, which makes up 34 percent of dismissals. A prosecutor's decision to prioritize certain cases could influence which cases are dismissed. Specifically, a prosecutor could choose to work on particular cases first, knowing nonprioritized cases are more likely to be dismissed if the evidence is not gathered in time.

Cases may also be disposed through a trial. However, in my sample, which is primarily misdemeanors, only 0.01 percent of cases go to trial. Therefore, I do not separately investigate the probability of guilt through a trial or an acquittal. Cases with these outcomes are included in my measure of guilty or not.

Finally, for most cases, the New York County District Attorney's Office practices vertical prosecution, which means that the same prosecutor remains with the case from screening through disposition. Specifically, for 60 percent of misdemeanor cases, the prosecutor assigned to the case at the Early Case Assessment Bureau is the only one on the case. Other cases are reassigned to another prosecutor after arraignment. Importantly, in regard to those cases, I observe both prosecutors in my data. This allows me to conduct my entire study as an intent-to-treat analysis using the first assigned prosecutor to the case.

2.2.2 Data

I use data from the New York County District Attorney's Office, a large prosecutor's office responsible for prosecuting all crimes in the Manhattan borough of New York City. The dataset was compiled by the Vera Institute and is housed by the National Archive of Criminal Justice Data (Kutateladze, 2017).

I use the New York County District Attorney's Office's detailed administrative data on all misdemeanor cases assigned through the Early Case Assessment Bureau in 2010–2011 for New York County. All data are collected at the case level. I focus my analysis on black defendants, black prosecutors, white defendants, and white prosecutors, the majority of my sample. The third-largest demographic group is Hispanics. Hispanic prosecutors only make up 4 percent of prosecutors at the New York County District Attorney's Office, making subgroup analysis difficult.

Police officers record their perception of defendant race on the New York Police Department's arrest reports. The New York County District Attorney's Office reports prosecutor race. Unfortunately, information on defendant and prosecutor race is missing for 1.63 percent and 1.82 percent of cases, respectively. I also do not observe defendant date of birth for 17 cases and gender for 170 cases. For the remaining analysis, I only show results for the sample of cases where I observe all case and defendant characteristics. Although these missing characteristics are likely the result of clerical mistakes and are not related to the race of the defendant, prosecutor, or case outcomes, I address this minor issue in Section B.1.1. Specifically, I show that my results are robust to the inclusion of cases with missing characteristics and to numerous assumptions about the value of missing characteristics.

Data from the New York County District Attorney's Office include the race of the defendant and prosecutor and other characteristics about the case, defendant, and prosecutor. For each case in the dataset, I observe arrest, screening, and sentencing charges, type of crime, prior arrest history, prior conviction history, prior incarceration history, gender, and age for the defendant. I also have information on the gender and race of the prosecutor, as reported by the New York County District Attorney's Office. Finally, I observe the disposition of every case that originated at the Early Case Assessment Bureau. Potential dispositions include conviction through trial, acquittal through trial, plea deal, decline to prosecute, dismissal, and dismissal through adjournment in contemplation of dismissal. Importantly, I also observe the screening date for each case. Because as-good-as-random variation in prosecutor race only requires I condition on the screening date of a case, I show in Section 2.4.1 that prosecutor race is uncorrelated with other case and defendant characteristics.

My primary outcome of interest is an indicator for whether the defendant was found guilty at the case level. This means that if a defendant is guilty of any charge on her case, she is considered guilty. Importantly, this includes all cases, even the ones dismissed. A defendant can be found guilty in one of two ways: by accepting a plea offer or by conviction through a trial. A defendant is considered not guilty if her case is declined or dismissed or if her trial ends in an acquittal. As mentioned before, the vast majority (99.9 percent) of guilty outcomes come from plea deals.[5]

Next, I also consider other decisions influenced by prosecutors that may determine a defendant's final case outcome (guilty or not guilty) to investigate what mechanism may drive the results. These outcomes include declined prosecution, case dismissal, dismissal through adjournment in contemplation of dismissal, charge increases, and pretrial detention. Declined prosecution means a case was dropped in the Early Case Assessment Bureau by a prosecutor, and case dismissal is a dismissal by a judge or prosecutor. An adjournment in contemplation of dismissal is an agreement to dismiss a case in 6 to 12 months if there are no subsequent arrests. Declined prosecution, case dismissal, and adjournment in contemplation of dismissal all directly lead to a not guilty outcome. Charge increases, meaning a case's charges are changed to a higher severity at any point before disposition, and pretrial detention, may indirectly influence a case outcome. Finally, pretrial detention means being detained after arraignment.

Crime type is defined by the researchers who originally collected the data according to New York law.[6] The three most common types are drug crimes, property crimes, and person crimes. All other crimes are classified as other.[7] Although I do not observe the specific crime type associated with a case, the most common drug misdemeanor in New York County is possession of marijuana (Kutateladze and Andiloro, 2014). Most property misdemeanors are petit larceny (theft of property worth less than $1,000), and the most common person crime is third-degree assault (Kutateladze and Andiloro, 2014; Chauhan et al., 2014). Drug crimes account for 25 percent of all cases, property crimes 38 percent, person crimes 7 percent, and other crimes 31 percent. I am missing the crime type for 1.53 percent of cases. I also address this minor issue in Section B.1.1.

[5]Two hundred and twenty-one cases go to trial, and 127 trial cases end in a conviction.

[6]Kutateladze et al. (2012) defines crime types using the New York Penal Law: person offenses, New York Penal Law §120.00–135.75; property offenses, §140.00–165.74; and drug offenses, §220.00–221.55.

[7]Unfortunately, I do not observe the specific crimes that fall into the other category. I do know that the most common crime types in the “other" category are escape and others relating to custody (PL §205), firearms, and other dangerous weapons (PL §265) and offenses against public order (PL §240).

Table A.1 displays summary statistics. I have a total of 87,461 cases, where the average defendant has been arrested and convicted of a crime more than four times, and nearly half have no prior arrests. On average, 20 percent of cases are dismissed, 20 percent are dismissed through adjournment in contemplation of dismissal (ACD), and 58 percent end with a guilty verdict. As my cases are primarily misdemeanors, 99.9 percent of convictions come from plea deals. The majority, 82 percent, of defendants are male with an average age of 34 years. Across all cases, 41 percent of prosecutors are male.

Black defendants make up 79 percent (68,798 cases) of my sample, and black prosecutors handle 14 percent of cases in my sample (11,937 cases). There are 2,488 (3 percent) cases with white defendants and black prosecutors, 9,449 (11 percent) cases with black prosecutors and black defendants, 59,349 (68 percent) cases with white prosecutors and black defendants, and 16,175 (18 percent) cases with white prosecutors and white defendants. In total, there are 83 black prosecutors and 495 white prosecutors.

2.3 Model

The conditional random assignment of cases to prosecutors provides an ideal setting for investigating the effect of prosecutor race on defendant outcomes. I use a generalized difference-in-differences model to estimate the effect of being assigned an opposite-race prosecutor on conviction. Formally, I estimate the following:

$$\begin{split} Guilty_c = \beta_0 + \beta_1 I(BlackDefendant)_c + \beta_2 I(WhiteProsecutor)_c + \\ \beta_3 I(BlackDefendant * WhiteProsecutor)_c + X_c + ScreeningDate_c + \epsilon_c, \end{split} \tag{2.1}$$

where $Guilty$ is a binary variable equal to one when the defendant is considered guilty for case c and zero for all other case dispositions; $Black\ Defendant$ takes on a value of one when the defendant is black and zero when the defendant is white; β_1 captures differences in the probability of guilt across defendant race; and $White\ Prosecutor$ is equal to one when the prosecutor is white and zero when the prosecutor is black and controls for differences in probability of guilt across prosecutor race. The coefficient of interest, β_3, on $BlackDefendant * WhiteProsecutor$ captures the effect of being assigned an opposite-race prosecutor.[8] X_c includes control variables at the case level. Specifically, X_c contains defendant race, age, date of birth, gender, number of prior arrests, felony arrests, convictions, felony convictions, jail sentences, prison sentences, and non-incarceration sentences; indicators for drug crime, property crime, person crime, arrest zipcode and prosecutor gender. All specifications include $ScreeningDate$ fixed effects.

Robust standard errors are clustered at the prosecutor level to allow error terms to be correlated across cases for a particular prosecutor. As I present results for multiple subgroups of crime, I also correct standard errors for multiple comparisons, as suggested by Anderson (2008a). I compute the FDR q-values using the method proposed by Anderson (2008a), adjusting for four different crime categories (property, drug, person, other). The FDR q-values can be interpreted as adjusted p-values from a two-sided test. The q-values account for the increased probability of estimating extreme coefficients when considering many subgroups.

Additionally, I also compute randomization inference p-values. To do so, I randomly reassigned defendant and prosecutor race assuming 78.6 percent of defendants are black and 86.3 percent of prosecutors are white. Then I estimated the effect of an opposite-race prosecutor (β_3 from Equation (2.1)) for 10,000 replications, the result of which provides an empirical distribution of coefficients observed due to chance. I then calculated the fraction of these 10,000 coefficients that are more extreme than the absolute value of the coefficient from my actual result, which is interpreted as a two-sided p-value.

Intuitively, the difference-in-differences compares differences in the probability of guilt between black defendants and white defendants for black prosecutors and white prosecutors. This model allows for black defendants to be more or less likely to be found guilty than white defendants. Similarly, black prosecutors may have different propensities for earning convictions than

[8]This paper focuses on the effect of opposite-race prosecutors. However, I also estimate the effect of prosecutor race on conviction. Results are shown in Table B.1. Here I regress $Guilty$ on indicators for prosecutor race. Overall, I find evidence that white prosecutors increase the probability of defendant guilt by 2.1 percentage points (3.6 percent) for the entire sample. I also show that being assigned a white prosecutor increases the probability of guilt by 2.7 percentage points (4.5 percent) for property crimes only. This result is robust to the inclusion of controls in column 2.

white prosecutors.

The identifying assumption of this model is that the differences in probability of guilt between black and white defendants across white and black prosecutors would be the same in the absence of opposite-race bias. Identification relies on the random assignment of cases to prosecutors. The identifying assumption could fail if prosecutor race is correlated with other factors that also alter the probability of conviction. For instance, in other settings, black prosecutors may choose to prosecute cases for white defendants only when they have a strong-enough case to ensure a guilty verdict and choose to accept any case with a black defendant. In this case, I would conclude my treatment effect was due to opposite-race bias, when it could actually be attributed, in part, to the initial quality of the case. I avoid this problem by using the random assignment of prosecutors to cases conditional on screening date. I can illustrate empirically that prosecutor race is uncorrelated with many observed defendant and case characteristics that would alter conviction rates.

The identifying assumption could also fail if prosecutors are responding to characteristics about a case that are correlated with defendant race. For example, if black prosecutors always earn more guilty verdicts for drug crimes and white defendants are more likely to commit drug crimes, I would find evidence of opposite-race bias. To address this potential failure, I interact all case and defendant characteristics with prosecutor race. If the inclusion of these interactions altered my estimate of opposite-race bias, then I could conclude the treatment effect could, perhaps in part, be attributed to prosecutors' responses to observed characteristics correlated with defendant race but not necessarily defendant race itself. For example, if white defendants are more likely to commit drug crimes, and black prosecutors always earn more guilty verdicts for drug crimes, some of the opposite-race treatment effect I estimate could be due to black prosecutors' differential treatment of drug crimes but not defendant race.

2.4 Results

2.4.1 Exogeneity of Prosecutor Race

I start this section by showing that prosecutor race is not correlated with confounding factors. While I expect this to be true based on the case assignment process at the New York County District Attorney's Office, I also provide empirical evidence. To begin, I regress defendant and case characteristics (determined before the case is assigned to a prosecutor) on prosecutor race. Each specification includes screening date fixed effects. Specifically, I examine if defendant race, age, date of birth, gender, number of prior arrests, felony arrests, convictions, felony convictions, jail sentences, prison sentences, and non-incarceration sentences are correlated with the race of the prosecutor. I also examine whether a case's number of current arrest charges, number of current arrest counts, misdemeanor type, type of crime—drug, property, person, and other—are correlated with prosecutor race.

Results are reported in Table A.2. Of the 20 coefficients presented, only 1 is statistically significant at conventional levels, which is consistent with random chance. Additionally, the coefficients are also close to zero. For example, compared to white prosecutors, black prosecutors are 0.66 percentage points more likely to be on cases with a black defendant. I conclude that defendant and case characteristics are not correlated with prosecutor race. These results indicate that case and defendant characteristics are orthogonal to prosecutor race and are consistent with the institutional background that cases are as-good-as randomly assigned to prosecutors conditional on screening date.

I also include another test to show that race is not correlated with confounding factors. The intuition behind this test is to show that the underlying probability of guilt for a defendant, as predicted before her case is assigned to a prosecutor, is unrelated to the race of her prosecutor. To do so, I predict the probability of guilt for each defendant using all observable characteristics about the defendant and case except for the race of the prosecutor. Specifically, I predict $Guilty$ (after removing screening date fixed effects) using column 2 controls for controls for defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, and arrest zipcode. Next, I compare the predicted probability of guilt for black and white defendants across white and black prosecutors. If the predicted values are the same for black and white defendants regardless of prosecutor race, then I provide further evidence that the underlying probability of guilty for defendants is not correlated with prosecutor race.

Results for the predicted values test are shown in Figure A.1 for the full sample. The predicted probability of guilt is 50.7 percent for white defendants assigned to white prosecutors and 51.1 percent for white defendants assigned to black prosecutors. These predicted values are not statistically different from each other (p-value = 0.598).[9] Similarly, the predicted probability of guilt for black defendants assigned to white prosecutors and black prosecutors are not statistically different (59.4 percent and 59.9 percent, respectively, p-value = 0.425). Figure A.1 is also replicated for only property crimes in Figure A.2. Again, predicted values are similar for white and black defendants regardless of the race of the prosecutor (p-values = 0.784 and 0.205, respectively). This further suggests that prosecutor race is unrelated to a defendant's predetermined likelihood of guilt, which is consistent with my identifying assumption.

2.4.2 Effect of Opposite-Race Prosecutors on Defendant Guilt

Next, I present results for my entire sample of cases in Table A.3. Each column includes screening date fixed effects along with standard errors clustered at the prosecutor level. The outcome variable for each column is the probability of guilt. $Guilty$ takes on a value of one if the defendant is convicted of a crime in any manner and zero for all other case outcomes.

Column 1 presents the estimate for opposite-race prosecutors for all case types. The coefficient on $BlackDefendant * WhiteProsecutor$ is 0.0252 and is statistically significant at the 5 percent level. This coefficient shows that being assigned an opposite-race prosecutor increases conviction

[9]Formally, I regress the predicted probabilities of guilt for white defendants on an indicator for prosecutor race.

by 2.52 percentage points (4 percent).

Column 2 adds controls for defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, and arrest zipcode and gender of the prosecutor. The coefficient is somewhat smaller (0.0167) and is significant at the 10-percent level.

Along with case-level controls, column 3 adds prosecutor fixed effects, which account for unobserved time-invariant prosecutor characteristics, having little effect on the magnitude of the coefficient. The coefficient of interest remains similar in magnitude—slightly decreasing to 0.0159 and not significant at conventional levels—although it is not statistically different from the estimate in column 1.

Column 4 explicitly addresses a potential threat to identification. If prosecutors are responding to case characteristics that are correlated with defendant race, but not defendant race itself, then I could incorrectly categorize different treatment of case characteristics as opposite-race bias. For example, if black defendants are more likely to commit drug crimes, and white prosecutors are more likely to win guilty verdicts for drug crimes, then I would incorrectly attribute differences in prosecuting drug crimes to opposite-race bias. To directly investigate this threat, I add a separate interaction for each case characteristic and defendant control added in column 2, interacted with prosecutor race; this allows black and white prosecutors to respond differently to case characteristics. The coefficient of interest remains about the same with the inclusion of interactions, slightly increasing from column 3 to 0.259, and is significant at the 5-percent level. Taken together, these columns provide suggestive evidence that opposite-race prosecutors increase the probability of conviction by 1.6–2.6 percentage points (3 percent to 4.5 percent).

Next, I explore effects by crime type, as different types of crimes are also often handled uniquely based on their quality of evidence (Frederick and Stemen, 2012; Ratledge et al., 1982; Spohn and Holleran, 2001; Spohn and Spears, 1997). In general, property crimes also tend to have less physical evidence (Peterson et al., 2010; Schroeder and Elink-Schuurman-Laura, 2017). This means that I might expect greater bias for property crimes, which tend to have less quality evidence and, therefore, have more room for discretion. Further, earlier research suggests that racial disparities may differ by crime type (e.g., Albonetti, 1997; Mustard, 2000; Steffenmeier et al., 2006). In particular, I consider effects for drug, property, person, and other crimes in Table A.4.

In Table A.4, each panel represents a different type of crime. The column layout of Table A.4 is similar to Table A.3. For each crime type, I first present results for the specification with screening date effects only. The second column adds controls, the third column adds prosecutor fixed effects, and the fourth column adds interactions.

I find little evidence of opposite-race bias for drug, person, or other offenses, as shown in panels A, B, and C. Results in panel D present robust and significant opposite-race effects for property crimes. In column 1, the baseline estimate, including screening date fixed effects, of 0.0549 indicates that being assigned an opposite-race prosecutor increases the likelihood of a guilty outcome by 5.5 percentage points (9 percent) for property crimes. Column 2 adds controls for defendant and case characteristics, such as the criminal history of the defendant and indicators for the type of crime committed. Specifically, column 2 adds controls for defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, and arrest zipcode and gender of the prosecutor. Consistent with my identifying assumption, the coefficient remains similar in magnitude (0.049) and is statistically significant at the 1-percent level. Column 3 adds prosecutor fixed effects to the case-level controls, which account for unobserved time-invariant prosecutor characteristics, having little effect on the magnitude of the coefficient. The coefficient of interest remains similar in magnitude—slightly decreasing to 0.0489 and is again significant at the 1 percent level.

Finally, column 4 explicitly addresses a potential threat to identification. If prosecutors are responding to case characteristics that are correlated with defendant race, but not defendant race itself, then I could incorrectly categorize different treatment of case characteristics as opposite-race bias. Here, I add a separate interaction for each case characteristic and defendant control, included in column 2 and interacted with prosecutor race; doing this allows black and white prosecutors to respond differently to case characteristics. The coefficient of interest remains about the same with the inclusion of interactions, slightly increasing from column 3 to 0.0547, and is significant at the 1-percent level. These results indicate that opposite-race prosecutors increase the probability of a guilty outcome by 5 to 5.5 percentage points (8 percent to 9 percent).

Because I report results for multiple types of crimes, I also include false discovery rate (FDR) adjusted q-values for the estimates presented in Table A.4. I compute the FDR q-values using the method proposed by Anderson (2008a), adjusting for four different crime categories. The FDR q-values can be interpreted as adjusted p-values. The FDR q-values for the property crime estimates in panel D are statistically significant at the 1-percent level for each specification.

Finally, I also computed randomization inference p-values. To do so, I randomly reassigned defendant race and estimated the effect of an opposite-race prosecutor (β_3 from Equation (2.1)) for 10,000 replications, the result of which provides an empirical distribution of coefficients observed due to chance. I then calculated the fraction of these 10,000 coefficients that are more extreme than the absolute value of the coefficient from my actual result, which are interpreted as a two-sided p-value. Randomization inference p-values also confirm that the effects for Property Crimes are significant at conventional levels (p-value=0.0037), while there is no significant effect for drug (p-value=0.3553), person (p-value=0.7105), and other crimes (p-value=0.7879).[10] Therefore, I conclude that the effects I find are large enough not to be attributed to chance. In combination, these results show strong opposite-race bias for property crimes only.

2.4.3 Potential Mechanisms

Given how my results show strong evidence of opposite-race bias in the probability of guilt for property crimes, I investigate potential mechanisms through which a prosecutor could affect the disposition of a case. As mentioned previously, there are many ways a prosecutor can alter the

[10] The distribution of estimates from this permutation exercise are reported in Figure B.1.

final outcome of a case. First, a prosecutor could indirectly affect whether a defendant is convicted by altering pretrial detainment or by increasing charges. A prosecutor can also directly affect whether a defendant is guilty or not through declining prosecution, dismissing the case, or offering an adjournment in contemplation of dismissal. To examine the effect of opposite-prosecutors on potential mechanisms, I first estimate equation (2.1) using pretrial detainment, increasing charges, declined prosecution, pretrial detainment, case dismissal, and adjournment in contemplation of dismissal as outcome variables. Results are shown in Table A.5.

Each specification in the table includes screening date fixed effects, case-level controls, and interactions, just as in column 4 of Tables A.3 and A.4. First, I consider pretrial detention in column 1 because prosecutors often have the power to recommend pretrial detention for defendants. Existing literature documents that pretrial detention can lead to increases in convictions for defendants because they are more likely to accept a plea deal while detained (Dobbie et al., 2018a; Heaton et al., 2017; Stevenson, 2018c). However, I only find suggestive evidence of opposite-race bias in pretrial detention for both the entire sample and the subsample of property crimes. As both coefficients are positive, but statistically insignificant, these results suggest opposite-race prosecutors might increase pretrial detention by 6 percent to 7 percent.

Results for charge increases are shown in column 2. A prosecutor's decision to increase the severity of charges may make it more difficult for a defendant to be released pretrial or may make the prosecutor more likely to seek out a guilty plea based on the new higher charges (Frederick and Stemen, 2012). Results indicate that for all cases and property crimes, an opposite-race prosecutor could increase charge severity by 3 percent to 9 percent, although neither coefficient is statistically significant at conventional levels.

I also show results for declined prosecution in column 3. It is possible that prosecutors could exhibit bias by declining to prosecute certain cases for certain same-race defendants. In column 1, I find evidence of opposite-race bias in the decision to decline to prosecute for all cases. When a defendant is assigned an opposite-race prosecutor, they are 32 percent less likely to have their case declined. However, I find no evidence of this bias for property crimes. This finding suggests that prosecutors are more likely to decline to prosecute a case for some cases, although not property ones.

Next, I consider case dismissal as a potential mechanism in column 4. Some misdemeanor dismissals are determined unilaterally by the case's prosecutor. Most dismissals are due to lack of speedy prosecution, which is officially determined by a judge, but a prosecutor's prioritization decisions can alter how long it takes to gather evidence on a case. For example, a prosecutor could decide to first work on cases where the defendant is opposite race versus own race. For all cases and property crimes, an opposite-race prosecutor decreases the chance of a case dismissal by 3 percent to 5 percent, although neither coefficient is statistically significant at conventional levels. This indicates that prosecutors could be altering case outcomes through increased dismissals for opposite-race defendants. However, the small estimates for pretrial detention, charge increases, declined prosecution, and case dismissal suggest these effects are unlikely to be the primary mechanism through which prosecutors exhibit opposite-race bias.

In column 5, I present results for adjournment in contemplation of dismissal, the third most common case outcome (after a guilty plea and case dismissal). For the entire sample, the estimate of opposite-race bias is statistically insignificant, but its magnitude suggests that being assigned an opposite-race prosecutor decreases the likelihood of case dismissal through adjournment in contemplation of dismissal by 0.0975 percentage points or 5 percent. Among defendants who have committed property crimes, being assigned an opposite-race prosecutor decreases the chance of dismissal through adjournment in contemplation of dismissal by 4.5 percentage points or 19 percent. These results suggest a substantial portion of the opposite-race bias I estimate could be attributed to prosecutors not dismissing cases for defendants.

2.4.4 Effect of Opposite-Race Prosecutors by Defendant Criminal History

Last, I consider opposite-race bias separately for defendants with and without a criminal history. This is because the ramifications of a conviction are likely much greater for those facing their first conviction versus subsequent ones. Results for the full sample and property crimes for defendants with and without a criminal record are shown in Table A.6. Specifically, each column in Table A.6 reports the effect of being assigned an opposite-race prosecutor on the probability of defendant guilt from a separate regression. All specifications include case-level controls and interactions similar to column 4 of Table A.4. Columns 1 and 2 only include defendants with no prior arrests or no prior convictions, respectively. For both panels, there is strong evidence of opposite-race bias for defendants with no criminal history. For defendants with no prior arrests, the coefficients in both panels (0.0345 and 0.0793) show that being assigned an opposite-race prosecutor increases the probability of defendant guilt by 8 percent for all crimes and 20 percent for Property Crimes. Column 2 focuses on another definition of no criminal history, defendants with no prior convictions, and indicates similar results. For defendants with no prior convictions, being assigned an opposite-race prosecutor increases the probability of defendant guilt by 6 percent and 17 percent for all crimes and Property Crimes, respectively. All estimates for defendants with criminal histories are also statistically significant at conventional levels. Estimates in columns 3 and 4 show that being assigned an opposite-race prosecutor has a much smaller effect for defendants with a criminal history. For instance, for Property Crimes and defendants with prior arrests, the coefficient (0.005) suggests that being assigned an opposite-race prosecutor increases the probability of defendant guilt by 0.7 percent. Further, none of the estimates in column 3 or 4 are significant at conventional levels.

I also investigate opposite-race bias for adjournment in contemplation of dismissals separately for defendants with and without a criminal history in Table A.7. The layout of Table A.7 is similar to Table A.6, and each specification includes case-level controls and interactions. In panel A, all estimates indicate no strong evidence of opposite-race bias in adjournment in contemplation of dismissal any defendants. However, in panel B, there is strong evidence of opposite-race bias for defendants with no prior arrests and defendants with no prior convictions. Coefficients of -0.0822 and -0.0686 show that opposite-race prosecutors decrease adjournment in contemplation of dismissals by 19 percent and 20 percent, respectively. Both estimates are significant at the one-percent level. Columns 3 and 4 show there is no strong evidence of opposite-race bias for defendants with prior criminal history. These results indicate that opposite-race bias for property crimes is driven, nearly entirely, by defendants with no criminal history.

2.5 Discussion

The results in the previous section show strong evidence of opposite-race bias for property crimes, although not for other crime types. This raises questions as to why prosecutors exhibit bias for only one type of crime. Further, it is natural to wonder if these results matter for overall racial disparities in the criminal justice system if prosecutors are only biased for one specific type of case.

While I cannot definitively conclude why prosecutor bias exists for only property crimes, one important factor may be evidence quality. When there is hard evidence on whether a crime occurred, prosecutors may have less ability to exhibit taste-based bias. Similarly, the availability of hard evidence may reduce the tendency of prosecutors to statistically discriminate.

Prosecutors and scholars agree that evidence quality is important for deciding how to prosecute a case. Based on one survey of two large urban district attorney's offices, researchers conclude "the most important factor considered in determining whether a case will go forward is the strength of the evidence" (Frederick and Stemen, 2012). Other studies also confirm that prosecutors rely heavily on evidence strength when making case decisions (Spohn and Spears, 1997; Ratledge et al., 1982; Spohn and Holleran, 2001).

It is also generally believed that most property crimes have less hard evidence than other types of crimes. For example, physical evidence is considered the most reliable type of evidence by prosecutors, and prosecutors agree that physical evidence in property cases is typically weaker than in drug cases (Frederick and Stemen, 2012; Kutateladze et al., 2016). Using data from five different jurisdictions, Peterson et al. (2010) finds that for randomly selected property crimes (burglary and robbery in their setting), physical evidence is only collected for 9 percent to 17 percent of cases, compared to 22 percent to 83 percent of person crimes cases (homicide, assault, rape) and nearly 100 percent of drug cases. Schroeder and Elink-Schuurman-Laura (2017) also confirms that person crimes, such as homicides and rapes, tend to have higher evidence-collection rates than property crimes.

Without quality evidence on property crimes, prosecutor decisions may rely more on person assessments of the likelihood of conviction, which could be altered by bias. In fact, some scholars have suggested that prosecutors may interpret weak evidence in a more “negative light" for minority defendants (Smith and Levinson, 2011; Kutateladze et al., 2016). Therefore, prosecutors may be able to exercise bias in the decision to dismiss a case through adjournment in contemplation of dismissal, as property cases may have more room for discretion. Suppose that a defendant is arrested for a property crime, but the case lacks solid evidence. In this case, the prosecutor would have more leeway to choose to push for a dismissal or a plea deal than compared to a case where a person is arrested with drugs, as hard evidence on them. In this context, at least, it seems as if prosecutors are more likely to fairly prosecute crimes when they lack room for discretion.

Prosecutors having greater potential for discretion and bias in crimes with less quality evidence, like property crimes, is less concerning if property crimes are uncommon. However, property cases are the most common type of crime in New York County. Further, in 2016 there were 7,919,035 property crime offenses in the nation, and 25 percent of jail inmates were incarcerated for property offenses (FBI: UCR, 2016; Sawyer and Wagner, 2019). Finally, although there are not many sources for nation-wide misdemeanor arrests, the best estimates suggest that over 1.4 million individuals were arrested for property crimes in 2014 (Stevenson and Mayson, 2018). This high number indicates that there are many cases with greater room for discretion.

In addition, in many ways, one might expect effects found in this setting to be a lower bound for racial bias in other prosecutors’ s across the country. This is because the Manhattan District Attorney has actively tried to address racial bias. For example, since 2010, the New York County District Attorney’s Office stopped prosecuting most low-level infractions and started offering a treatment program, instead of probation, for low-level drug crimes. Both policies are described as being particularly important for communities of color (Cyrus Vance For District Attorney, 2017). It also employs a chief diversity officer and diversity committee because it believes a diverse staff can help reduce racial bias (Manhattan District Attorney, 2018a). For this reason, larger effects might be expected elsewhere. Because of the prevalence of property crimes and the progressive nature of the Manhattan District Attorney of New York, finding effects for property offenses is nontrivial.

Finally, the results I find in New York County have important implications for racial disparities in the criminal justice system. Opposite-race bias by prosecutors could account for about 50 percent ($\frac{Estimate of Opposite Race Bias * Pr(White Prosecutor)}{Estimated Black White Disparity} = \frac{0.055*0.86}{0.049+0.055*0.86}$) of the difference in guilt across race for property crimes.[11] Even if prosecutors are acting fairly in other types of cases, the magnitude of the opposite-race bias I estimate should warrant further investigation into prosecutor bias.

2.6 Conclusion

In this paper, I test for opposite-race prosecutor bias in criminal convictions. To overcome potential endogenous case selection by prosecutors, I exploit the as-good-as-random assignment of cases to prosecutors in New York County, under which assignment is random conditional on screening date. The resulting variation in prosecutor race, combined with variation in defendant race, allows me to estimate the extent to which prosecutors are biased against opposite-race defendants.

My results indicate that the assignment of an opposite-race prosecutor leads to a 5 percentage point ($\sim$ 8 percent) increase in the probability of being found guilty for property crimes only. Individuals we expect might be hurt the greatest by a conviction, defendants with no criminal history, drive this opposite-race estimate. In addition, I explore the potential mechanisms through which opposite-race bias affects the probability of guilt. I show that being assigned an opposite-race

[11] 0.86 is the probability of being assigned a white prosecutor for property crimes, 0.055 is my estimate of opposite-race bias for property crimes, and 0.049 is the β_1 I estimate in the same regression (Table A.4). Referring to the model I present in equation (2.1), I estimate that the difference in conviction races between black and white defendants is $[(\beta_0 + \beta_1) * Pr(BlackProsecutor|BlackDefendant) + (\beta_0 + \beta_1 + \beta_2 + \beta_3) * Pr(WhiteProsecutor|BlackDefendant)] - [\beta_0 * Pr(BlackProsecutor|WhiteDefendant) + (\beta_0 + \beta_2) * Pr(WhiteProsecutor|WhiteDefendant)]$. Because cases are randomly assigned, $Pr(BlackProsecutor|WhiteDefendant) = Pr(BlackProsecutor|BlackDefendant)$, and similarly $Pr(WhiteProsecutor|WhiteDefendant) = Pr(WhiteProsecutor|BlackDefendant)$. Further, Pr(Black Prosecutor) = 1-Pr(White Prosecutor). Using this information to simplify, I determine the difference in black and white conviction rates is $\beta_1 + Pr(WhiteProsecutor)\beta_3$, where β_3 is my estimate of opposite-race bias. Therefore $\frac{\beta_3 * Pr(WhiteProsecutor)}{\beta_1 + \beta_3 * Pr(WhiteProsecutor)}$ represents the amount of the black-white gap explained by my estimate of opposite-race bias.

prosecutor decreases the likelihood that a case is dismissed through an adjournment in contemplation of dismissal. I interpret the reason for these findings as likely because prosecutors can more easily exercise discretion for crimes with weaker evidence, although I cannot rule out other interpretations.

The finding of prosecutor bias against opposite-race defendants lends support to recent movements to increase the training of prosecutors and to curb the ability of prosecutors to exercise race-based discretion (U.S. Department of Justice, 2016). Further, these results are striking because the New York County District Attorney's Office promotes itself as being especially progressive, expressed through its commitment to criminal justice reform, community partnerships, and reducing bias. My results add to existing evidence documenting opposite-race bias, though it is important to highlight that I find no evidence of bias in person, other, or drug crimes. However, it is possible that a meaningful portion of the black-white disparity in convictions, 50 percent, could be attributed to prosecutors exhibiting opposite-race bias even if prosecutors do not display bias on all cases.

3. THE EFFECT OF RISK ASSESSMENT SCORES ON JUDICIAL BEHAVIOR AND DEFENDANT OUTCOMES

3.1 Introduction

In the United States (US), the Eighth Amendment and most state constitutions guarantee the right to non-excessive bail. However, 34 percent of all felony defendants are detained until case disposition (Bureau of Justice Statistics, 2013). Further, 90 percent of those pretrial detained are incarcerated 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 pretrial population. A common suggestion is a shift from monetary bail to a risk-based system, where defendants are released according to their risk of pretrial crime instead of their ability to pay bail or secure a bond. 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 income could lead to less pretrial detention, allowing defendants to keep their jobs and imposing near zero costs on the criminal justice system if defendants do not commit new crimes 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 (release on your own recognizance) and risk assessment scores could increase pretrial crime as the expected penalty of future crime is lower, further threatening societal safety and raising costs. It is also possible that risk assessment scores could exacerbate existing racial disparities, as these scores often include components, i.e. employment or criminal history, that are correlated with defendant's race.[1] This paper focuses on the question of whether the use of risk assessment scores can increase

[1] Academics and journalists express varying degrees of concern about potential racial biases in risk assessment scores (e.g. Angwin et al., 2019; Doleac et al., 2017).

non-financial bond, and decrease pretrial detention without increasing societal costs.

Although the use of risk assessment scores is rapidly expanding 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 case disposition, including recording whether they were assessed using a risk assessment score. Second, some jurisdictions only use scores for certain defendant types, often those charged with less serious crimes. 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 in Texas using data from Travis County, home to the state capital, Austin, and 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 publicly. 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 80 percent of defendants booked after arrest. This type of sudden change can be used to identify 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. Put 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 we present results for release outcomes. We show that the use of a risk assessment score increases release on non-financial bail by 4.5-7% and decreases pretrial detention by 7-9.5%. We also provide evidence that results are driven by low-income defendants and do not worsen existing racial disparities. Second, we are able to rule out meaningful increases in violent pretrial crime. These results are robust to multiple inference and several robustness checks. There is some suggestive evidence that non-violent pretrial crime may increase, however these results are not robust. It also appears judges returned to their old release patterns about three months after the policy change.

To our knowledge, this paper provides some of 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; New Jersey Courts, 2018; 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 (DeMichele et al., 2018; Latessa et al., 2010; Turner et al., 2009; Zhang et al., 2014). Others have focused on comparing human decisions with actuarial predictions (Chanenson and Hyatt, 2016; Dressel and Farid, 2018; Grove et al., 2000). Perhaps the most rigorous paper in this field, Kleinberg et al. (2017), used machine learning to determine what 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 were chosen according to their algorithmic risk scores, crime rates would fall.

Conversely, there are few serious independent evaluations of risk assessment score implementation. This paper is most similar in spirit to Stevenson (2018b). She evaluates multiple pretrial risk assessment score policy changes in Kentucky using an event study framework. 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. Further, using data from a large metropolitan area allows us to consider whether risk assessment scores altered existing racial disparities in pretrial detention and non-financial bonds in a more diverse area and specifically for low-income defendants.

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 (Didwania, 2018; Dobbie et al., 2018b; Heaton et al., 2017; Leslie and Pope, 2016; Stevenson, 2018a). 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 at least in the short term. These savings 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 crime.

Second, the use of risk assessment scores is important to defendants because it relieves low-income and minority 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., 2018b; Stevenson, 2018a). To the extent that our results apply in other settings, these findings indicate that risk assessment score policies may be an effective tool for decreasing the income-based disparity in pretrial detention and improving the lives of defendants at least in the short term. Notably, these decreases in pretrial detention are not associated with increases in violent pretrial crime, implying minimal risk and costs to society. However, policy makers must be careful to weigh these potential benefits with the possibility for some increases in non-violent pretrial crime. They should also recognize the benefits of risk assessment scores may be short-lived and are not likely to completely overhaul an existing bail system.

3.2 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 by 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. The form used by Pretrial Services to calculate a defendant's score is presented in Figure A.4. 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 pretrial crime 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 after the hearing with no financial obligation. 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 say definitively 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 if they pay for release[2], 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 committed a new crime. This defendant is then likely returned to jail until the final disposition of their case. For all defendants, we say a defendant is convicted if they are found guilty by trial or accept a plea deal.

3.3 Data

We use individual-level administrative data from Travis County on all criminal cases disposed between 2011 and 2014. 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 information from a second source: data on the disposition of cases and pretrial crime from the Travis County Court System.

We identify five outcomes of interest: release on non-financial bond, pretrial detention, conviction, non-violent pretrial crime and violent pretrial crime.[3] 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 is not related to the

[2] A defendant can either pay their bail in full or a bail bondsman can post bail instead.

[3] It might also be natural to consider failure to appear. Unfortunately, Travis County does not keep accurate data on this outcome. In fact, in most Texas counties, failure to appear is not measured accurately or tracked.

policy change or a particular type of defendant. Even so, we discuss this limitation in greater depth in section 3.5.6.

Non-financial bond is equal to one if a judge assigns a defendant a non-financial bond and zero for those denied bail or those assigned monetary bail. If a defendant is released on non-financial bond, they can leave jail immediately and need only promise to return to court at a later date. Pretrial detention takes on a value of one if a defendant is kept in jail for more than two days before their disposition not including time served after potential subsequent arrests.[4] Conviction is recorded as a one if a defendant is convicted of the crime they were originally arrested for, and zero otherwise. Pretrial crime 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 pretrial crime takes on a value of one for all defendants who, before their trial, are arrested for a new non-violent crime. Violent pretrial crime takes on a value of one for all defendants who are arrested for a new violent pretrial crime.

Table A.8 presents descriptive statistics for all defendants booked in Travis County from 2011 through 2014. Most defendants are minorities (non-white or Hispanic), male, and US citizens: 58 percent, 76, and 89 percent respectively.[5] Eighty eight 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.[6] 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 after January 2013.

[4]Our results are similar in significance and magnitude if we define pretrial detention as being in jail for more than one or three days.

[5]Travis County records the race and ethnicity of each defendant. Defendants are white if they are white and not Hispanic. Minority defendants are either non-white or Hispanic.

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

3.4 Methods

3.4.1 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 80 percent of defendants. This is an ideal setting for applying our regression discontinuity design to estimate the short term 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:

$$Outcome_{it} = \beta_0 + \beta_1 policyenacted_t + f(daysfromcutoff)_t + \lambda_i + \pi_c + \gamma_d + epsilon_{it} \quad (3.1)$$

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, respectively. 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 main 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 calculated as suggested in Calonico et al. (2017).[7]

3.4.2 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 to be 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 A.5 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

[7]Although we do not believe release decisions or pretrial crime 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 A.9. 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 A.9 retain their significance level with clustering.

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 (3.1) 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 A.6 and Appendix Table C.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 20 estimates presented in Appendix Table C.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 characteristics 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 assigned 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 pretrial crime, violent pretrial crime, and conviction) 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 of an outcome 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 attribute any treatment effect we later estimate to the policy change, not underlying differences in defendants booked just before and after the policy change.

Figure A.7 and Table C.2 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.

3.5 Results

3.5.1 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 (3.1) using the assignment of a risk assessment score as the outcome variable. Figure A.8 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 A.8 shows clearly that we estimate a large (about 80 percent) increase in risk assessment score assignment across the policy change threshold.[8] 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 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 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.

3.5.2 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. Next, we consider pretrial detention. If a defendant

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

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 a risk assessment score on non-financial bonds and pretrial detention in Figure A.9. Formally, we estimate equation (3.1) with the probability of release on non-financial bond and pretrial detention as outcome variables. Figure A.9 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, we will discuss possible reasons later in this section.

Table A.9 presents corresponding point estimates, with each column representing a separate regression. Each column includes controls for days since the policy change, case specific controls, and 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).[9] 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 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 of similar magnitudes for non-financial bond and

[9]The primary specification in this paper includes controls. If we recreate our results with no controls our estimates are not meaningfully different. For example our results for non-financial bond range from 0.0271 to 0.0428. Pretrial detention estimates range from -0.0172 to -0.038

pretrial detention.[10]

Our estimates for non-financial bond range from 0.029 to 0.043. 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%). For pretrial detention, our estimates range from -0.025 to -0.033, showing the risk assessment score policy decreases the chance of pretrial detention by about 3 percentage points (7-9.5%).

Because our results include four different outcomes, we also include false discovery rate (FDR)-adjusted q-values for the estimates presented in Table A.9. We compute the FDR-adjusted q-values using the method proposed by Anderson (2008b), adjusting for our five 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 attributable 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 (3.1) 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 C.1 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 C.1 Panels (b), (c), (e), and (f). The dashed lines represent the optimal MSE bandwidth. The estimated coefficients remain 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.

[10]Our results are similar in significance and magnitude if we define pretrial detention as being in jail for more than one or three days.

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. This test also addresses a specific concern about the time-series nature of our data. Specifically, that our errors terms are serially correlated, potentially leading to incorrect standard errors. To do so, we estimate equation (3.1) reassigning the policy threshold to be a day before the true policy change occurred. Because we only have data beginning in 2011, we estimate equation (3.1) 910 times using every possible date that occurred before the true policy change, a linear specification, optimal bandwidth, and the controls included in Table A.9. The distribution of placebo estimates for release on non-financial bond and pretrial detention are shown in Figure C.2. Nearly all placebo coefficients (97.99) are less than the reported estimates in Table A.9 for release on non-financial bond. Our pretrial detention estimate in Table A.9 is less than 95.88 percent of our placebo estimates. These simulations imply p-values of 0.02 and 0.04 for our non-financial bond and pretrial detention estimates, indicating that our results cannot be explained by serial correlation. Further, these simulations imply our estimates are not simply due to chance.

Finally, it is natural to wonder what type of defendants were released after the adoption of a risk assessment score policy. To investigate this question, we estimate the effect of a risk assessment score on multiple subgroups in Figure A.10. Each figure shows the regression discontinuity estimate from a separate regression. Estimates for the optimal bandwidth are reported.

First we show results for defendants with low predicted risk versus high predicted risk. Because defendants booked before the adoption of the risk assessment score are not assigned a score, we use all case observables (race, age, gender, criminal history, indigent status, severity of arrest, mental health status, US citizenship status, and specific court) to predict a risk score for every defendant. Defendants with above average predicted risk are considered high risk and those with below average predicted risk are considered low risk. Overall it appears that defendants with higher predicted risk were more likely to be offered non-financial bond or not detained pretrial than were low risk defendants. Older, felony-committing, minority, and indigent defendants are all more likely to be released on non-financial bond compared to younger, misdemeanor-committing, white, and non-indigent defendants.

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 whether a risk assessment score policy alters pretrial crime and conviction.

3.5.3 Effects of a Risk Assessment Score Policy on Pretrial Crime

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 crime. Results for non-violent and violent pretrial crime are shown in Figure A.11. All graphs in Figure A.11 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 A.11(a) and Figure A.11(b) presents some suggestive evidence of a small increase in non-violent recidivism and no change in violent recidivism. Table A.10 presents the corresponding estimates. Similar to Table A.9, 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, and indigent status, as well as 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 pretrial crime are of similar magnitudes across all six columns. For non-violent pretrial crime, estimates range from 0.0095 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 pretrial crime, our results are not robust to alternative specifications.

Next we consider violent pretrial crime . Across all columns our estimates remain stable, ranging from -0.001 to -0.005. We are also able to rule out any increase in violent pretrial crime when using the larger sample size from twice the optimal bandwidth.[11] We also report FDR q-values for pretrial crime outcomes.[12] The FDR q-values are also not consistently significant for

[11] -.000748 is the top of the 95% confidence interval from this specification.
[12] Again, we correct for 5 categories.

any outcome.

We can also show our pretrial crime results are robust to alternative bandwidths and functional forms. As we did for non-financial bond and pretrial detention, we estimate equation (3.1) using non-violent and violent pretrial crime as outcomes, with the inclusion of the controls and allow the bandwidth to vary from 20 to 660 days in 10-day increments. Figure C.3 shows the coefficients and standard errors from each model for non-violent and violent pretrial crime. 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 C.3 Panels (b), (c), (e), and (f). The dashed lines represent the optimal MSE bandwidth. The estimated coefficients remain consistent across the different bandwidths. Together these results indicate that risk assessment scores do not increase violent pretrial crime. We also find some evidence, although not robust, of increases in nonviolent pretrial crime.

Finally, we consider conviction. Given the existing literature on the effects of pretrial detention on conviction, risk assessment scores could also alter conviction (Dobbie et al., 2018b; Stevenson, 2018a). Figure A.11(c) shows our results for conviction. Here there appears to be some decrease in conviction after the adoption of a risk assessment score. Corresponding estimates are found in Table A.10. All six coefficients are negative and suggest that risk assessment scores could have decreased the odds of conviction for defendants. However, the magnitude and significance of the coefficients are not robust to multiple specifications.[13] Therefore, we do not consider this outcome for subgroup analysis.[14]

3.5.4 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 were 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 in-

[13]Further, Figures C.3(g), C.3(h) and C.3(i) show conviction results for different bandwidths.

[14]For all subgroups (indigent, non-indigent, minority, white) our estimates are not robust to different specifications and bandwidths.

digent defendants compared to non-indigent defendants. Our graphical results are shown in Figure A.12. 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 C.3.

In Table C.3, Panel A presents results for indigent defendants and Panel B shows 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 low-income defendants.

We also explore results by indigent status for pretrial crime. As indigent defendants are the most likely to be released pretrial, it is possible that changes in their pretrial crime behavior are masked in the entire sample results. Results for non-violent and violent pretrial crime are shown in Figure A.13. Panels (a) and (d) repeat the entire sample results for comparison. Indigent results are shown in Panels (b) and (e), while Panels (c) and (f) report results for non-indigent defendants. For non-violent pretrial crime, there is some evidence of a larger increase in pretrial crime for indigent defendants and no increase for non-indigent defendants. For violent pretrial crime, however, there appears to be no increase for indigent or non-indigent defendants.

Table C.4 shows pretrial crime estimates. For non-violent pretrial crime, the coefficients for indigent defendants are larger in magnitude than for non-indigent defendants. For violent pretrial crime there are no meaningful differences in the coefficients for indigent and non-indigent defendants. Further, for each subgroup the coefficient for violent pretrial crime is negative, again suggesting there are no increases in violent pretrial crime 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 pretrial crime may increase for lower income defendants, who are most likely to be released. Violent pretrial crime does not increase for either group.

3.5.5 Minority Defendants

There is growing concern that risk assessment scores may exacerbate existing racial disparities. For example, before the adoption of risk assessment scores, minority defendants were 10 percent less likely to be released on non-financial bond and 40 percent more likely to be pretrial detained than non-minorities.[15] Components of the ORAS-PAT such as employment, prior incarcerations and drug use may be correlated with race. This means that even though the ORAS-PAT does not directly consider race, it could lead to different outcomes for minority versus white defendants. Graphical non-financial bond and pretrial detention results for white and minority defendants are shown in Figure A.14. 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 white defendants, while Panels (c) and (f) show results for minority defendants. For both outcomes the discontinuity for minorities is potentially larger than for white defendants. Corresponding estimates are shown in Table C.5. For non-financial bail, in each specification, the magnitude of our estimate is larger for minority defendants than white defendants.

We also consider pretrial crime for minority and white defendants in Figure A.15. Results for non-violent and violent pretrial crime are shown in Figure A.15. Panels (a) and (d) repeat the entire sample results for comparison. White defendant results are show in Panels (b) and (e), while Panels (c) and (f) report results for minority defendants. For non-violent crime, there is some evidence of a larger increase in pretrial crime for minority defendants and no increase for white defendants. For violent pretrial crime however, there appears to be no increase in pretrial crime for white or

[15] In this setting minority defendants are non-white and Hispanic. White defendants are only white.

minority defendants.

Table C.6 shows pretrial crime estimates. For non-violent pretrial crime, the coefficients for minority defendants are larger in magnitude than for white defendants. For violent pretrial crime, however, there are no meaningful differences in the coefficients for minority and white defendants. Further, for each subgroup the coefficient for violent pretrial crime is negative, again suggesting there are no increases in violent pretrial crime across either group.

Taken together these results show that our results for non-financial bail and pretrial detention are in part driven by minority defendants. There is no evidence that the adoption of risk assessment scores increased pre-existing racial disparities in non-financial bail or pretrial detention. If anything, the magnitude of our coefficients suggests that risk assessment score may have decreased racial disparities.

3.5.6 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 C.4. Here we estimate equation (3.1) 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 C.7. Even columns allow the running variable to vary quadratically and odd columns are linear. Columns (1)-(6) use the optimal bandwidth determined in Table A.9 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 of release on non-financial bond for defendants who are missing this outcome. We then estimate equation (3.1) using predicted probability of release on non-financial bond as the outcome just for defendants who are missing data. Figure C.5 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.

3.5.7 Long Term Effects

We now turn to the reasons 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 related to the timing of when the effects of the policy begin to fade. To do so, we conduct event study analysis with results presented in Figure C.6.[16] Visually, it is clear that the effect of non-financial bonds lasts for only the first 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 risk assessment scores fade with time, so this is not an uncommon pattern. Travis County Pretrial

[16] 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.

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 highlights 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.

3.6 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 led to an 4.5-7% increase in non-financial bonds and a 7-9.5% decrease in pretrial detention. We also find no increases in violent pretrial crime. 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 crime or racial disparities. However, policy makers must be careful to weigh these potential benefits against the chance of increases in non-violent pretrial crime.

4. THE EFFECT OF VIOLENCE AGAINST POLICE ON POLICING BEHAVIOR

4.1 Introduction

Serious violence is very geographically concentrated in the United States. For example, 78 percent of murders occur in only 5 percent of American counties (FBI UCR, 2014). Consequently, policing has the highest expected marginal return in high-violence areas. This is reflected by the fact that 56 percent of residents in low-income, high-violence areas would prefer police to spend more time in their neighborhood (Gallup, 2019). At the same time, civilians in these neighborhoods are the least satisfied with policing. For example, civilians in high-violence neighborhoods are 40 percent less likely to approve of police officers' ability to prevent crime, help victims, and solve problems compared to those who live in low-violence areas (Maxson et al., 2003). Further, movements such as Black Lives Matter and public support for greater surveillance of police officers reflect a growing concern with policing severity (Morin et al., 2017). Low income and minority Americans are also 66 and 76 percent more likely to believe police use lethal force too quickly (Ekins, 2014). This paper examines the extent to which increased risk to officer safety drives both phenomena.

Despite increasing concerns with the effectiveness and severity of policing, there is little evidence on how risks to officer safety could alter policing behavior. An important exception is Legewie (2016). This paper compares similar police stops before and after a civilian shooting of a police officer using a regression discontinuity and matching design. Legewie (2016) finds police officers use force more often during stop and frisk events on black civilians, but not white ones, in the 14 days after a shooting. This paper's approach has several advantages relative to Legewie (2016). First, I can examine long-run effects, thereby testing whether police response is temporary or persistent. Additionally, because I use emergency calls for service, where officers are assigned to incidences by a computer system rather than initiating them, there is no selection in whether an event is recorded. For example, police officers could change their reporting behavior of civilian interactions after a violent event. This means I am able to estimate the effect of increased risk on police arrests without concerns about misreporting. Finally, emergency calls include detailed information on call urgency, severity, and incident descriptions. This is consequential because I can observe important characteristics of police and civilian interactions even when an arrest is not made, or use of force is not recorded.

To estimate the effect of threats to officer safety on police behavior, I use data on ambushes and calls for service from Indianapolis, Indiana. From 2014-2017, Indianapolis experienced seven different police ambushes across four different police beats. Ambushes are defined as a “situation where an officer is assaulted, unexpectedly, as the results of premeditated design by the perpetrator" (FBI LEOKA, 2015). Using police ambushes to measure threats against police officers has multiple benefits. First, police ambushes are perhaps the most severe act of violence an officer can face and are highly salient. In addition, the relative infrequency of the events gives me variation that can be used to identify effects. This contrasts with officer assaults, of which there were nearly 3,500 (averaging 35 per beat per year) over my sample period. Finally, an ambush is an act of violence that, by definition, was not initiated or provoked by officers. As a result, ambushes are the one form of violence against police that can be perceived as only due to the fault of the perpetrator.

Using this variation in ambushes, I compare how police behavior changed in ambushed beats compared to other beats over time. The identifying assumption is that absent an ambush those beats would have experienced similar changes in police behavior. I show empirical support for this assumption, as police officer arrests and use of force change similarly before an ambush. Results show that violence against police, in the case of ambushes, decreases arrest by 8 percent in the three years following an ambush. These results are the strongest in the first six months after an ambush, where I estimate arrests decreased by 10 percent. In contrast, results indicate policing severity – as measured by use of force and civilian complaints – does not increase after an ambush. These results are robust to the inclusion of call level controls, covariate-by-time controls, and beat-specific linear time trends. I also show the results are not driven by changes in the composition of calls coming from ambushed beats after an ambush.

By providing the first evidence that violence against police officers leads to decreases in arrests and no change in use of force or civilian complaints, this paper contributes to related literature on understanding the effects of violence and trauma on decision making. Much of this literature focuses on the relationship between exposure to disruptive events and risk preferences.[1] While this literature forces on measuring changes in risk preferences, I focus on observed changes in high stakes decisions. Second, I provide evidence on how the context surrounding police officers affects behavior. This contrasts with most of the existing literature, which has focused on the potential bias in police decision making (e.g., Fryer Jr, 2020; Weisburst, 2017; West, 2018b). Other papers emphasize how characteristics of a police force such as, degree of monitoring[2], militarization[3] or composition[4] may alter crime. This paper contributes to this literature by demonstrating whether and how police officers change behavior based on the perceived risks. These results suggest that the same qualities that raise the marginal benefit of police officers – a dangerous, high-crime area – are the qualities that can result in the behavioral response of de-policing. On the other hand, results here provide evidence against the hypothesis that increased personal risk to officers – at least of the type studied here – generates increases in use of force and other controversial tactics.

4.2 Institutional Details

4.2.1 Ambushes in Indianapolis

I can estimate the effect of violence against police on police behavior using ambushes. The Indianapolis Police Department defines an ambush according to the Federal Bureau of Investigation's definition from its Law Enforcement Officers Killed and Assaulted data collection guidelines. The Federal Bureau of Investigation defines an ambush as a "situation where an officer is assaulted, unexpectedly, as the result of premeditated design by the perpetrator".[5] Ambushes provide an ideal

[1] Some of this literature focuses on civil conflicts (e.g., Callen et al., 2014; Jakiela and Ozier, 2018; Voors et al., 2012), while another branch centers on natural disasters (e.g., Cameron and Shah, 2015; Cassar et al., 2017; Eckel et al., 2009). There is no consistent evidence on whether events like these increase or decrease risk aversion.

[2] e.g., Ariel et al. (2015); Ater et al. (2014); Cheng and Long (2018); Heaton et al. (2016)

[3] e.g., Bove and Gavrilova (2017); Harris et al. (2017)

[4] e.g., Miller and Segal (2012, 2018)

[5] The Federal Bureau of Investigation defines an assault as "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault is accompanied by the use of a weapon or by a means likely to produce death or great bodily injury".

environment to consider violence against police for multiple reasons. First, police ambushes are one of the most severe acts of violence an officer might face. This concern is reflected by the fact that some police departments adopted programs to provide basic psychological support and design different policing tactics (pairing up police officers on calls etc.) after the high profile Dallas ambush in 2016 (Smith, 2019). Further, a survey of officers before and after the Dallas ambush reported that officers were more concerned about their physical safety after the ambush (Morin et al., 2017). Second, the rare nature of ambushes gives me variation in exposure to locations where extreme violence occurred. In contrast, Indianapolis experienced nearly 3,500 officer assaults from 2014-2017. This means, on average, each police beat was treated 35 times an average year, making it difficult to estimate the effect of less severe violence because each police beat is treated multiple times in a small period. Finally, for ambushes, the officers involved not initiate or incite the perpetrators. The responding officer could have provoked police injury in other settings. Therefore, for ambushes only, other officers on the force will perceive the violence to be the perpetrator's fault alone.[6] During the time I consider in this paper (2014-2017), Indianapolis experienced seven police ambushes in four different police beats. I only take the first ambush in each beat for my analysis.

4.2.2 Police Patrol

One advantage of the approach used in this paper is I examine behavior during responses to 911 calls for service. Using calls for service is important because officers are assigned to these events, meaning that there are no civilian and police interactions that are unreported. This is because every 911 call has at least one police officer assigned to it. No call can be unanswered. Leveraging 911 calls is also consequential because I observe important characteristics of police and civilian interactions even when an arrest is not made or use of force is not recorded. I consider the effects of increased risk to officer safety on policing behavior by using data from Indianapolis,

[6] It could be the case that a police officer is assaulted because they accelerated a situation due to events in their personal life. For example, imagine a scenario where a police officer is going to get a divorce and allows this to spill over into his work life. The potential divorce could cause him to violently engage with a civilian and lead to the officer himself being injured. In this scenario, other officers might perceive the assault to be the fault of the officer, and not a situation that could happen to them.

which includes information on the expected severity of each call (call priority) and a detailed, standardized call description. Each patrol officer is assigned to a beat within the city. There are 33 beats in Indianapolis and 1345 patrol officers in my time periods. Beats in the city center, where the population is more concentrated, are smaller than those closer to the suburbs. A map of police beats is shown in Figure A.16.

After a call for service comes in, the dispatcher will assign the police officer on duty in a beat or the closest officer geographically to respond to the call. Because officers are sometimes assigned based on their geographic locations, during one shift a police officer may respond to calls in many different beats. This variation allows me to include individual police officer fixed effects in all my specifications, thereby ensuring any response to police ambushes is not due to the sorting of officers across regions or call.

When an officer is dispatched to a call, they are given information on the location of the call, the priority of the call, as well as a call description. The priority of a call, a number between 1 and 4, provides the officer with details on the severity and urgency of a call. Lower numbers correspond to higher severity and urgency. For example, a burglary in progress will be assigned a lower priority number than a report of a missing vehicle. The call description is one of 200 different call types. For example, a police officer could be told that a call is for a traffic accident or a burglary in progress. After the police officer responds to the call for service, they are required to record if a call ended in an arrest using the computer in their police car. A call could also end in a use of force. I connect calls for service to use of force using separate police reports on use of force. The use of force files record all incidences of use of force. The most common type of use of force is hands and feet, but gun uses of force do represent 7 percent of all uses of force. I also consider civilian complaints against police officers as an outcome. Unlike arrests and use of force, complaints are not measured at the call level. This is because I am unable to link civilian complaints to specific 911 calls. If a civilian believes they were treated improperly by a police officer or that an officer violated protocol they can submit a complaint to the Citizens Police Complaint Office. Office staff may follow up on the complaint to secure further details. These complaints may be filed on-line, however, more "serious" complaints must be filed as a formal complaint.[7] Formal complaints must be filed in person within 60 days of the incident (City of Indianapolis, 2019). During my time period 16,380 complaints were filed against Indianapolis police officers.

4.3 Data

My data comes from the Indianapolis Police Department and include all 911 calls for service from 2014-2017. I link these data to Indianapolis Police Department data on use of force incidences and records of ambushes. Summary statistics for my data are shown in Table A.11. Panel A shows summary statistics for the call level analysis. In total I have 3.4 million calls for service. Ten percent of calls come from ambushed beats and 90 percent come from beat in which there was not an ambush. The average call priority (a measure of the severity and urgency of a call) is 1.78, where the most severe and urgent calls are assigned a priority of 1. On average ambushed beats have slightly more severe and urgent calls for service, reflected in the lower call priority. My first two outcome variables, arrest and use of force, are measured at the call level. On average 7 percent of calls end with an arrest and 0.6 percent of calls end in use of force. Ambushed beats have slightly higher arrests and use of force per call. I also consider the effect of police ambushes on civilian complaints and number of calls. These outcomes cannot be connected to specific 911 calls, so I perform this analysis at the beat-day-hour level. Summary statistics at the beat-day-hour level are shown in Panel B. Each beat has about 2 civilian complaints and 937 calls for service per week. Ambushed beats are less likely to receive civilian complaints compared to un-ambushed ones and ambushed beats receive more calls for service than un-ambushed ones.

4.4 Methods

I will estimate the effect of increased risk to officer safety on policing behavior using a difference-in-differences approach. This method will compare ambushed and un-ambushed beats, over time. In particular, I will estimate the following generalized difference-in-differences model to determine

[7] The police department does not explicitly define what a more serious complaint is. Rather, the staff reviewing a complaint will make this distinction.

the impact of ambushes on arrests and use of force:

$$Outcome_{ctbo} = \theta_b + \beta(AfterAmbush_t * AmbushedBeat_b) + \alpha_o + \omega Year * Month_t + X_c + \epsilon_{ctbo} \tag{4.1}$$

where $Outcome_{ctbo}$ is an indicator variable for whether a call for service c, in beat b, at time t, attended by officer o, ended in an arrest or use of force. θ_b are beat fixed effects to control for any systematic differences across police beats. For example, some beats may experience higher average arrests or crime than others. $AfterAmbush_t * AmbushedBeat_b$ is the treatment variable that takes of a value of one for ambushed beats, after an ambush. The coefficient on this term, β measures the change in arrests or use of force per call after an ambush, relative to arrests or use of force in un-ambushed beats. Year-by-month fixed effects, ω_t, control for shocks to arresting behavior that are common to all beats in a year-month. X_c can include call-level controls. Specifically, X_c includes controls for call location—latitude and longitude, dispatch time and fixed effects for call description and priority. Finally, I include individual officer fixed effects, α_o, to account for any time-invariant officer characteristics. These individual officer fixed effects also account for any differential sorting of officers after an ambush. In each specification standard errors are clustered the the beat level.

In this approach, the identifying assumption is that absent an ambush, ambushed beats would have experienced changes in arrests and use of force similar to un-ambushed beats. I test this assumption in the following ways. First, I graphically examine whether arrests or use of force began diverging before an ambush. If arrests and use of force were not changing similarly during the time period before an ambush it would suggest that un-ambushed beats are not a valid counterfactual for the change ambushed beats would have experienced absent treatment. Next I include covariate-by-time controls. If my results could be explained by idiosyncratic shocks to areas with specific characteristics, then including these controls would absorb my results. For example, if just areas with very high priority calls received a shock that coincided with treatment, then controlling for priority-by-year-by-month effects could change my estimate. Finally, I also include beat-specific linear time trends, which allow for the possibility that beats followed different trends over time. For instance, if my results can be explained by beats trending differently based on unobserved or observed characteristics over time, then the beat-specific trends should absorb my treatment effect. If my estimates are robust to these specification these tests provide evidence to support the validity of this research design.

I also include an additional test of identification. For this test, I consider whether observed determinants of an arrest exhibit pre-trends or change after treatment. Rather than focus on each covariate separately, I predict arrest for every call using all characteristics about the call except treatment status. This means I predict arrest and use of force using the latitude, longitude, dispatch time, call priority, and call description. Therefore, predicted arrest is a linear combination of all observable characteristics about the call. For example, if call priority is very important for determining if a call ends in an arrest, it will be given more weight in the prediction. If my identifying assumption holds, I would expect no divergence in arrests in the pre-period. Further, if I do not observe a treatment effect for predicted arrests, this provides further evidence that my results can be attributed to ambushes and not other confounding factors.

Another threat to identification is that the number of calls for service could be changing after ambush. For example, it could be the case that after an ambush civilians do not make as many domestic abuse calls and, therefore, officers do not have the opportunity to make arrests or use force on those types of calls. I directly test for threat by estimating Equation (4.1) using the inverse hyperbolic sine of number of calls as the outcome variable and collapsing my data to the beat-day level. If I find that the number of calls does not change after an ambush, this provides evidence that I am identifying the effect of ambushes and not a change in the number of calls.

4.5 Results

4.5.1 The Effect of Ambushes on Police Behavior

I first consider the effect of ambushes on predicted arrests to determine if important characteristics of calls for service are changing before or after treatment. Arrests are predicted using all call characteristics (latitude, longitude, dispatch time, call priority, and call description) aside from treatment status. To determine if ambushed and un-ambushed beats began diverging before treatment, I graph the estimated divergence, over time, between ambushed and un-ambushed beats relative to thirteen or more months before the ambush. Explicitly, the Figures in this section graph a dynamic version of Equation (4.1), where $AfterAmbushed_t$ is replaced by indicators variables for months before/after an ambush. Formally, I estimate the following equation:

$$Outcome_{ctbo} = \theta_b + \sum_{t=-19}^{42+} \beta(MonthsAfterAmbush_t * AmbushedBeat_b) + \alpha_o + \omega Year * Month_t + X_c + \epsilon_{ctbo} \quad (4.2)$$

where I combine $MonthsAfterAmbush$ into three month bins. In Figure A.17, I plot dynamic difference-in-differences estimates for predicted arrest. If my identifying assumption holds, I would expect no divergence in predicted arrests before an ambush and no treatment effect. Figure A.17 shows no divergence in predicted arrest in the pre-period and no evidence of a strong treatment effect after an ambush. This further supports that my research method is identifying the effect of an ambush, rather than any other confounding factor.

Next, I consider the effect of ambushes on arrests. To do so, I compare the changes in arrests in ambushed beats to un-ambushed beats, before and after an ambush. Figure A.18 shows results for observed arrests. Importantly, both ambushed and un-ambushed beats appear to track each other in terms of arrest per call in the periods before an ambush. This suggests that in the absence of an ambush, ambushed and un-ambushed beats would have continued to track each other. Therefore, no evidence of pre-trends in arrests supports the validity of this research design. There is also a sharp decrease in arrests in the six months immediately after an ambush, further supporting that ambushes caused this divergence. The decrease in arrests persists for in the following months as well, although the decrease is not as dramatic.

Estimation results of Equation (4.1) are shown in Table A.12. Each column reports the effect of an ambush on arrests per call. In Column 1, where only beat, year-by-month, and officer fixed effects are included, the estimated effect of police ambushes is a decrease in arrests per call by 8% (0.6 percentage points). This represents a substantial decrease in arrests and is statistically significant at the one percent level. In Column 2, I include call-level controls. Specifically, I control for the latitude, longitude, and dispatch time of a call. I also add call priority and call type fixed effects. The estimate in this specification is -0.00594 (8%) and is statistically significant at the one percent level. In Column 3 I include covariate-by-time (each characteristic from Column 2 interacted with year-x-month) controls. If my results could be explained by idiosyncratic shocks to areas with specific characteristics, then including these controls would absorb my results. For example, if just areas with very high priority calls received a shock that coincided with treatment, then controlling for priority-by-year-by-month effects could change my estimate. But, my estimate in Column 3 remains very similar in magnitude and significance. In Column 4, I add beat-specific linear time trends, which allow each beat to follow a different trend over time. These trends allow for both observable and unobservable beat characteristics to change linearly over time. If results are being driven by ambushed beats being on a different path than un-ambushed beats, then adding a beat-specific linear time trend should absorb the treatment effect. However, this estimate is similar in magnitude and significance to Columns 1, 2, and 3. Finally, given the more dramatic drop in arrests just after an ambush, I separately consider treatment effects for the first six months after an ambush and all the months afterward. In both periods, I estimate statistically significant decreases. The decrease in the first six months after an ambush corresponds to a 10 % decrease in arrests compared to a 8% decrease in later months. Taken together, these results indicate that violence against police (ambushes) can lead to a significant and sustained decreases in arrests for at least three and a half years.

Even though I estimate a decrease in arrests, the severity of policing could increase on each call. For example, now officers engage with civilians less to make arrests, but when they do choose to engage, more force is used. I answer this question directly by estimating Equation (4.2) but replacing use of force as my outcome variable. As before, I first show predicted values in Figure A.19. Again there is no clear trend in use of force leading up to the ambush, supporting my identifying assumption. There is also no break in use of force behavior after the ambush for predicted use of force. This indicates identifying the effects of an ambush on use of force will not be confounded by other factors.

I present my main results for use of force in Figure A.20. Here there is no trend in use of force leading up to an ambush, further validating my research design. After an ambush, there is also no clear change in use of force behavior. Estimation results from Equation (4.1) are in Table A.13. Table A.13 uses the same specifications as Table A.12. Column 1 shows the baseline specifications, Column 2 adds controls, Column 3 allows controls to vary with time, and Column 4 includes a beat-specific linear trend. Across the first four columns, my estimates remain statistically insignificant and close to zero. The magnitude of the coefficients suggests that use of force may have decreased by as much as 1% (Column 3) or increased by up to 2.5% (Column 4). Finally, Column 5 shows results for the first six months after an ambush separately from longer-term effects. The coefficient for the first six months after an ambush suggests a slight decline in use of force after an ambush, but is not statistically significant at conventional levels. Together, these estimate show no evidence of an increase in use of force or policing severity, after an ambush. In fact, I am able to rule out any increases in use of force above 4.5%.[8]

Now, I consider another measure of policing severity: civilian complaints. I investigate civilian complaints because they are not reported by police officers, as use of force is, but rather by the civilians that interact with police officers. Civilian complaints are not measured at the call level because when civilians make complaints it is not possible to link them back to the 911 call that caused the complaint. To consider the effect of ambushes on complaints directly, I estimate Equation (4.2) using the inverse hyperbolic sine of the number of complaints in a beat-day-hour as my outcome variable. Results for civilian complaints are shown in Figure A.21. Again, there is no trend in complaints leading up to an ambush, further validating my research design. After an ambush, there is also no clear change in the number of complaints. Estimation results from Equation (4.2) are in Table A.14. Column 1 includes beat and year-by-month fixed effects. The coefficient is

[8]The top of the 95% confidence interval from the estimate in Column 1 represents a 4.5% increase in use of force.

not significant at conventional levels and is close to zero. This estimate rules out increases in complaints of 1/100 of a percent, or one complaint per beat-year.[9] In Column 2, I estimate the show and longer-term effects of ambushes on complaints. The coefficient for the first six months after an ambush is close to zero and is not statistically significant at conventional levels. The coefficient for longer-term effects suggests a slight decline in the number of arrests and is statistically at the 10 percent level. Jointly, these use of force and civilian complaint results show no strong evidence of increases in policing severity in response to heightened officer risk.

4.5.2 The Effect of Ambushes on Calls for Service

A potential threat to identifying the effects of an ambush is that the number of calls for service from an ambushed beat may change after an ambush. This is problematic if the decrease in calls comes from the type of calls where arrests are often made or force is typically used. For example, if the number of serious calls decreases in ambushed beats then police officers may not have the opportunity to make arrests and use force, as they could in the calls before the ambush. To address this potential threat directly, I estimate Equation (4.2) using the inverse hyperbolic sine of the number of calls as my outcome variable. I also separately consider calls where arrest or use of force are very likely. I define "likely" by only keeping calls types that are in the top quartile of ending in a use of force or arrest.[10]

Results for all calls presented in Figure A.22a. First, it is important to note the there is no divergence between ambushed and un-ambushed beats before treatment. Second, there is no strong evidence of a change in the number of calls following an ambush. Corresponding estimates are presented in Table A.15 Panel A. Column 1 presents the estimate for the entire period after an ambush and Column 2 considers the short and long term effects separately. None of the estimates are statistically significant and are close to zero. My estimate in Column 1 Panel A corresponds to a 2 percent decrease in calls, although it is not statistically significant at conventional levels. Further, in the first six months after an ambush, where I estimate the most dramatic drop in arrests,

[9]The top of the 95% confidence interval for column 1 is 0.0001695.

[10]I also present results for arrests and use of force for arrest likely and use of force likely calls in Figures D.1 and D.2 as well as Tables D.1,D.2, D.3, and D.4.

my estimate represents a 0.04 percent decrease in calls.

Only considering entire sample results may mask heterogeneity in calls that are more likely to experience an arrest or use of force. Therefore, I also consider the effect of an ambush on the number arrest likely calls in Figure A.22b and use of force likely calls in Figure A.23c. In both figures, there is no evidence of a pre-trend in the number of calls or a change in the number of calls after an ambush. Estimates in Table A.15, Panel B, and C, again show no significant change in calls after an ambush. Column 1 presents results for the entire period after an ambush. For both Panel B and C the coefficients are close to zero. For example, in Column 1 Panel B the coefficient suggests a 0.06 percent decrease in arrest likely calls. Column 2 presents results separately for short and longer-term effects. Again, each coefficient is not statistically significant.

These number of call results indicate that there is no meaningful change in the number of calls following an ambush. This suggests that I am identifying the effect of police ambushes rather than the effect of a decline in certain types of calls for service.

4.6 Conclusion

The fundamental question addressed in this paper is whether increased risk to police officers alters police behavior. As police ambushes only occur in some beats and are some of the most severe threats faced by officers, ambushes provide a unique setting to estimate the effect of violence on policing. Using this variation in ambushes, I can compare how police behavior changed in ambushed beats compared to un-ambushed beats, over time. I find that police ambushes lead to an 8 percent decline in arrests. This fall in arrests persists for at least three and a half years after an ambush. I also show that police severity, measured as use of force and civilian complaints, does not increase after an ambush. This is consistent with police officers de-policing after violent incidences.

5. CONCLUSIONS

My dissertation focuses on the decisions making of prosecutors, police officers, and judges. Understanding how these criminal justice actors affect crime and contribute to disparities is particularly important given the high and racially disproportionate costs of the criminal justice system.

In Chapter 1, I test for opposite-race prosecutor bias in criminal convictions. To overcome potential endogenous case selection by prosecutors, I exploit the as-good-as-random assignment of cases to prosecutors in New York County, under which assignment is random conditional on screening date. The resulting variation in prosecutor race, combined with variation in defendant race, allows me to estimate the extent to which prosecutors are biased against opposite-race defendants.

My results indicate that the assignment of an opposite-race prosecutor leads to a 5 percentage point ($\sim$ 8 percent) increase in the probability of being found guilty for property crimes only. Individuals we expect might be hurt the greatest by a conviction, defendants with no criminal history, drive this opposite-race estimate. In addition, I explore the potential mechanisms through which opposite-race bias affects the probability of guilt. I show that being assigned an opposite-race prosecutor decreases the likelihood that a case is dismissed through an adjournment in contemplation of dismissal. I interpret the reason for these findings as likely because prosecutors can more easily exercise discretion for crimes with weaker evidence, although I cannot rule out other interpretations.

The finding of prosecutor bias against opposite-race defendants lends support to recent movements to increase the training of prosecutors and to curb the ability of prosecutors to exercise race-based discretion (U.S. Department of Justice, 2016). Further, these results are striking because the New York County District Attorney's Office promotes itself as being especially progressive, expressed through its commitment to criminal justice reform, community partnerships, and reducing bias. My results add to existing evidence documenting opposite-race bias, though it is important to highlight that I find no evidence of bias in person, other, or drug crimes. However, it is possible that a meaningful portion of the black-white disparity in convictions, 50 percent, could be attributed to prosecutors exhibiting opposite-race bias even if prosecutors do not display bias on all cases.

Chapter 2 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 led to an 4.5-7% increase in non-financial bonds and a 7-9.5% decrease in pretrial detention. We also find no increases in violent pretrial crime. 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 crime or racial disparities. However, policy makers must be careful to weigh these potential benefits against the chance of increases in non-violent pretrial crime.

The fundamental question addressed in Chapter 4 is whether increased risk to police officers alters police behavior. As police ambushes only occur in some beats and are some of the most severe threats faced by officers, ambushes provide a unique setting to estimate the effect of violence on policing. Using this variation in ambushes, I can compare how police behavior changed in ambushed beats compared to un-ambushed beats, over time. I find that police ambushes lead to an 8 percent decline in arrests. This fall in arrests persists for at least three and a half years after an ambush. I also show that police severity, measured as use of force and civilian complaints, does not increase after an ambush. This is consistent with police officers de-policing after violent incidences.

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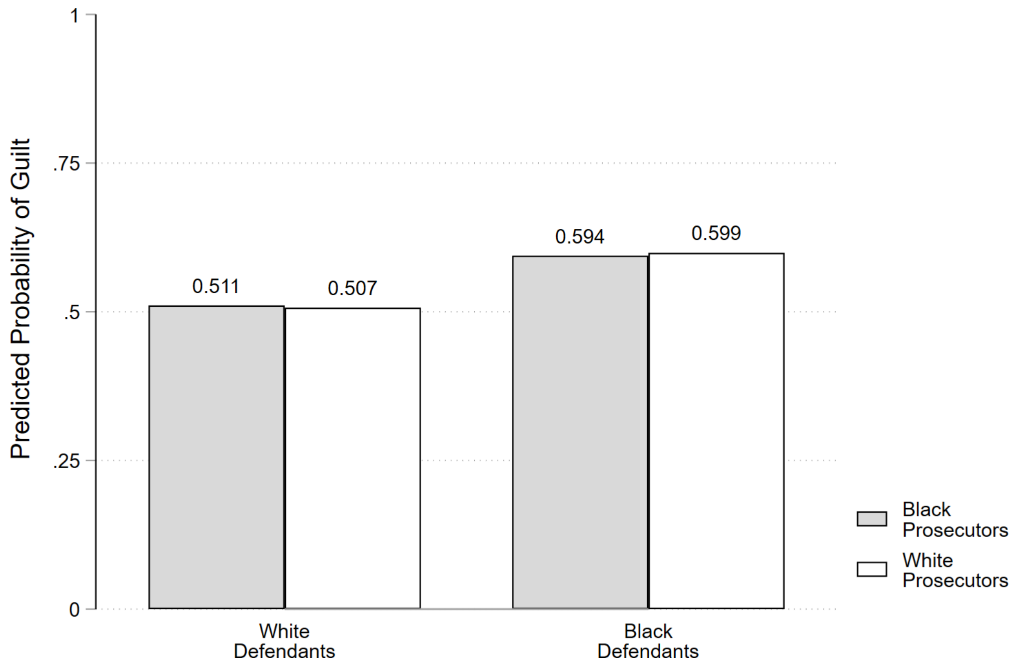
APPENDIX A

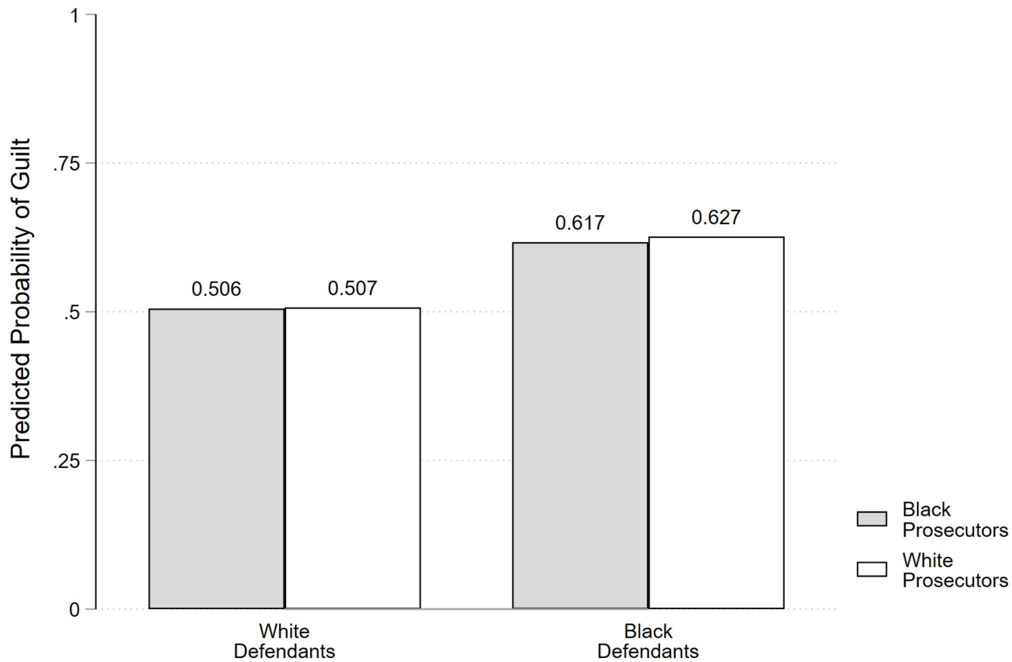
FIGURES AND TABLES

Figure A.1: Predicted Values of Guilt



Notes: This figure reports predicted guilt for black and white defendants assigned to black and white prosecutors. The predicted value is calculated by regressing $Guilty$ on all observable characteristics (except for prosecutor race) about the defendant and case that were determined before the case was assigned to the prosecutor. Specifically, $Guilty$ is predicted (after removing screening date fixed effects) using defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, and arrest zipcode. There is no statistical difference in predicted guilt for white defendants assigned to white or black prosecutors. The same is true for black defendants.

Figure A.2: Predicted Values of Guilt for Property Crimes



Notes: This figure reports predicted guilt for black and white defendants assigned to black and white prosecutors. The predicted value is calculated by regressing $Guilty$ on all observable characteristics (except for prosecutor race) about the defendant and case that were determined before the case was assigned to the prosecutor. Specifically, $Guilty$ is predicted (after removing screening date fixed effects) using defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, and arrest zipcode. There is no statistical difference in predicted guilt for white defendants assigned to white or black prosecutors. The same is true for black defendants.

Table A.1: Summary Statistics

	(1) All	(2) Black Defendants	(3) White Defendants	(4) Drug Crimes	(5) Property Crimes	(6) Person Crimes	(7) Other Crimes
Panel A: Outcomes							
Pretrial Detention	0.0892	0.100	0.0487	0.0858	0.0862	0.209	0.0689
Charges Increased	0.0501	0.0522	0.0423	0.0225	0.0738	0.0802	0.0366
Decline to Prosecute	0.0189	0.0203	0.0139	0	0.00865	0	0.0513
Case Dismissed	0.204	0.208	0.189	0.196	0.140	0.594	0.203
ACD	0.196	0.169	0.298	0.261	0.243	0.0984	0.108
Guilty	0.579	0.601	0.497	0.543	0.607	0.303	0.636
Panel B: Case Characteristics							
Black Defendant	0.787	1	0	0.796	0.797	0.770	0.770
Defendant Age	34.01	34.02	33.98	34.12	33.12	32.92	35.28
	(12.84)	(12.97)	(12.34)	(12.69)	(13.32)	(12.32)	(12.35)
Defendant Male	0.819	0.825	0.800	0.874	0.756	0.780	0.861
	(0.385)	(0.380)	(0.400)	(0.331)	(0.429)	(0.414)	(0.346)
No Prior Arrests	0.498	0.430	0.750	0.393	0.480	0.583	0.587
No Prior Convictions	0.555	0.492	0.786	0.475	0.533	0.653	0.626
Prior Arrests	4.013	4.645	1.685	5.172	4.511	2.118	2.877
	(9.282)	(9.894)	(6.001)	(10.45)	(9.865)	(5.216)	(7.957)
Prior Felony Arrests	0.792	0.933	0.270	1.070	0.847	0.561	0.548
	(1.985)	(2.149)	(1.047)	(2.282)	(2.109)	(1.451)	(1.599)
Prior Convictions	4.152	4.804	1.747	5.280	4.888	1.732	2.865
	(9.873)	(10.54)	(6.305)	(11.10)	(10.42)	(5.017)	(8.612)
Prior Felony	0.214	0.253	0.0710	0.269	0.238	0.159	0.152
Convictions	(0.584)	(0.626)	(0.354)	(0.636)	(0.624)	(0.493)	(0.492)
Prior Jail Sentences	1.851	2.150	0.747	2.448	2.377	0.594	0.997
	(5.654)	(6.094)	(3.369)	(6.693)	(6.210)	(2.596)	(4.159)
Prior Incarcerations	0.135	0.160	0.0406	0.171	0.159	0.0882	0.0862
	(0.473)	(0.511)	(0.270)	(0.519)	(0.521)	(0.371)	(0.376)
Prior	2.146	2.470	0.952	2.637	2.337	1.036	1.759
Non-Incarceration Sentences	(4.899)	(5.188)	(3.384)	(5.105)	(4.884)	(2.703)	(5.059)
Black Prosecutor	0.136	0.137	0.133	0.142	0.136	0.133	0.133
Prosecutor Male	0.405	0.407	0.398	0.388	0.412	0.404	0.411
Observations	87461	68798	18663	21798	32959	5984	26720

mean coefficients; sd in parentheses

Notes: ACD stands for Adjournment in Contemplation of Dismissal.

Table A.2: Correlation Between Case Characteristics and Prosecutor Race

Panel A: Defendant Characteristics	Black Defendant	Defendant Age	Defendant Date of Birth	Male Defendant	Number Prior Arrests	Number Prior Felony Arrests	Number Prior Convictions	Number Prior Felony Convictions	Number Prior Jail Sentences	Number Prior Prison Sentences	Number Prior Non-Incarceration Sentences
White Prosecutor	-0.00659	0.0709	-25.50	0.00189	0.0979	0.00465	0.162	0.00232	0.0966	0.00107	0.0642
	(0.00499)	(0.203)	(74.01)	(0.00533)	(0.154)	(0.0316)	(0.162)	(0.00674)	(0.0811)	(0.00480)	(0.0828)
Observations	87461	87461	87461	87461	87461	87461	87461	87461	87461	87461	87461
Outcome Mean	0.787	34.01	5645.7	0.819	4.013	0.792	4.152	0.214	1.851	0.135	2.146

Panel B: Case Characteristics	Number Arrest Charges	Number Arrest Counts	Class A Misdemeanor	Class B Misdemeanor	Class U Misdemeanor	Drug Crime	Property Crime	Person Crime	Other Crime
White Prosecutor	0.0227*	0.0202	0.00596	-0.00712	0.00116	-0.00779	-0.00701	0.00459	0.0102
	(0.0123)	(0.0143)	(0.00805)	(0.00772)	(0.00501)	(0.0112)	(0.0148)	(0.00568)	(0.00973)
Observations	87461	87461	87461	87461	87461	87461	87461	87461	87461
Outcome Mean	1.671	1.735	0.618	0.231	0.151	0.249	0.377	0.0684	0.306

Standard errors in parentheses
$^{*}\ p < .1,^{**}\ p < .05,^{***}\ p < .01$

Notes: This table reports the coefficient on $White\ Prosecutor$ from separate regressions of case and defendant characteristics on a binary variable representing prosecutor race. Each regression includes screening date fixed effects. Standard errors are clustered at the prosecutor level.

Table A.3: Estimates of Opposite-Race Bias for Defendant Guilt

	(1)	(2)	(3)	(4)
Outcome: Guilty				
Black Defendant*White Prosecutor	0.0252**	0.0167*	0.0159	0.0259**
	(0.0115)	(0.00981)	(0.00975)	(0.0110)
Observations	87,461	87,461	87,461	87,461
Outcome Mean	0.579	0.579	0.579	0.579
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	N	Y	Y	Y
Prosecutor FE	N	N	Y	N
Interactions	N	N	N	Y

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficient on the interaction of $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Guilty$ on indicators for prosecutor race, defendant race, and the interaction term. Each specification includes screening date fixed effects. Column 2 adds controls for defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, arrest zipcode, and gender of the prosecutor. Column 3 includes the same controls as column 2, with the exception of prosecutor gender, and adds individual prosecutor fixed effects. Column 4 adds interactions for every case and defendant control added in column 2, interacted with prosecutor race. Standard errors are clustered at the prosecutor level.

Table A.4: Estimates of Opposite-Race Bias in Defendant Guilt by Crime Type

	(1)	(2)	(3)	(4)
Panel A: Drug Crimes				
Outcome: Guilty				
Black Defendant*White Prosecutor	0.0211	0.0149	0.0128	0.0129
	(0.0257)	(0.0228)	(0.0243)	(0.0274)
Observations	21,798	21,798	21,798	21,798
Outcome Mean	0.543	0.543	0.543	0.543
FDR q-value	0.825	0.633	0.824	0.948
Panel B: Person Crimes				
Outcome: Guilty				
Black Defendant*White Prosecutor	-0.0125	-0.0264	0.00211	-0.0130
	(0.0562)	(0.0552)	(0.0560)	(0.0585)
Observations	5,984	5,984	5,984	5,984
Outcome Mean	0.303	0.303	0.303	0.303
FDR q-value	0.825	0.633	0.824	0.97
Panel C: Other Crimes				
Outcome: Guilty				
Black Defendant*White Prosecutor	-0.00740	-0.0238	-0.0278	-0.0101
	(0.0199)	(0.0184)	(0.0191)	(0.0207)
Observations	26,720	26,720	26,720	26,720
Outcome Mean	0.636	0.636	0.636	0.636
FDR q-value	0.825	0.393	0.294	0.948
Panel D: Property Crimes				
Outcome: Guilty				
Black Defendant*White Prosecutor	0.0549***	0.0490***	0.0489***	0.0547***
	(0.0159)	(0.0133)	(0.0136)	(0.0144)
Observations	32,959	32,959	32,959	32,959
Outcome Mean	0.607	0.607	0.607	0.607
FDR q-value	0.002	0.001	0.003	0.002
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	N	Y	Y	Y
Prosecutor FE	N	N	Y	N
Interactions	N	N	N	Y

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficient on the interaction of $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Guilty$ on indicators for prosecutor race, defendant race, and the interaction term. Each specification includes screening date fixed effects. Column 2 adds controls for defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, arrest zipcode, and gender of the prosecutor. Column 3 includes the same controls as column 2, with the exception of prosecutor gender, and adds individual prosecutor fixed effects. Column 4 adds interactions for every case and defendant control added in column 2, interacted with prosecutor race. Robust standard errors are clustered at the prosecutor level. False discovery rate (FDR) q-values are adjusted for multiple inference given the four categories of crime examined. FDR q-values are estimated using the method proposed by Anderson (2008a) and are interpreted as two-sided p-values.

Table A.5: Mechanism

	(1)	(2)	(3)	(4)	(5)
	Pretrial Detention	Charges Increased	Declined Prosecution	Case Dismissed	Adjournment in Contemplation of Dismissal
Panel A: Entire Sample					
Black Defendant*White Prosecutor	0.00646	0.00148	-0.00597*	-0.00652	-0.00975
	(0.00665)	(0.00560)	(0.00353)	(0.00792)	(0.0120)
Observations	87,461	87,461	87,461	87,461	87,461
Outcome Mean	0.0892	0.0501	0.0189	0.204	0.196
Panel B: Property Crimes					
Black Defendant*White Prosecutor	0.00529	0.00681	0.000923	-0.00631	-0.0450***
	(0.0120)	(0.00727)	(0.00328)	(0.0104)	(0.0162)
Observations	32,959	32,959	32,959	32,959	32,959
Outcome Mean	0.0862	0.0738	0.00865	0.140	0.243
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y	Y
Prosecutor FE	N	N	N	N	N
Interactions	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficient on the interaction of $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Pretrial\ Detention$, $Charges\ Increased$, $Declined\ Prosecution$, $Case\ Dismissed$, and $Adjournment\ in\ Contemplation\ of\ Dismissal$ on indicators for prosecutor race, defendant race, and the interaction term. All specifications include screening date fixed effects, controls, and prosecutor race interactions. Robust standard errors are clustered at the prosecutor level.

Table A.6: Estimates of Opposite-Race Bias in Defendant Guilt by Criminal History

	(1) Defendants with No Prior Arrests	(2) Defendants with No Prior Convictions	(3) Defendants with Prior Arrests	(4) Defendants with Prior Convictions
Panel A: All Crimes				
Outcome: Guilty				
Black Def.*White Prosecutor	0.0345***	0.0264**	0.00502	0.0113
	(0.0117)	(0.0117)	(0.0179)	(0.0194)
Observations	43575	48532	43886	38929
Outcome Mean	0.447	0.421	0.710	0.777
Panel B: Property Crimes				
Outcome: Guilty				
Black Def.*White Prosecutor	0.0793***	0.0676***	0.00538	0.0150
	(0.0252)	(0.0242)	(0.0211)	(0.0214)
Observations	15828	17559	17131	15400
Outcome Mean	0.401	0.388	0.798	0.858
Prosecutor & Def. Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y
Prosecutor FE	N	N	N	N
Interactions	Y	Y	Y	Y

Standard errors in parentheses

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

Notes: This table reports the coefficient on the interaction of $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Guilty$ on indicators for prosecutor race, defendant race, and the interaction term. All specifications include screening date fixed effects, controls, and prosecutor race interactions. Robust standard errors are clustered at the prosecutor level.

Table A.7: Estimates of Opposite-Race Bias in Adjournment in Contemplation of Dismissal by Criminal History

	(1) Defendants with No Prior Arrests	(2) Defendants with No Prior Convictions	(3) Defendants with Prior Arrests	(4) Defendants with Prior Convictions
Panel A: All Crimes				
Outcome: Adjournment in Contemplation of Dismissal				
Black Def.*White Prosecutor	-0.0180	-0.0115	0.0155	0.000771
	(0.0129)	(0.0133)	(0.0154)	(0.0119)
Observations	43575	48532	43886	38929
Outcome Mean	0.308	0.320	0.0860	0.0424
Panel B: Property Crimes				
Outcome: Adjournment in Contemplation of Dismissal				
Black Def.*White Prosecutor	-0.0822***	-0.0686***	0.0127	-0.00319
	(0.0272)	(0.0228)	(0.0170)	(0.0147)
Observations	15828	17559	17131	15400
Outcome Mean	0.424	0.428	0.0763	0.0329
Prosecutor & Def. Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y
Prosecutor FE	N	N	N	N
Interactions	Y	Y	Y	Y

Standard errors in parentheses

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

Notes: This table reports the coefficient on the interaction of $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Adjournment\ in\ Contemplation\ of\ Dismissal$ on indicators for prosecutor race, defendant race, and the interaction term. All specifications include screening date fixed effects, controls, and prosecutor race interactions. Robust standard errors are clustered at the prosecutor level.

Figure A.4: Ohio Risk Assessment Score in Travis County

OHIO RISK ASSESSMENT SYSTEM: PRETRIAL ASSESSMENT TOOL (ORAS-PAT)

Name: ______________________________ Date of Assessment: ______________________________

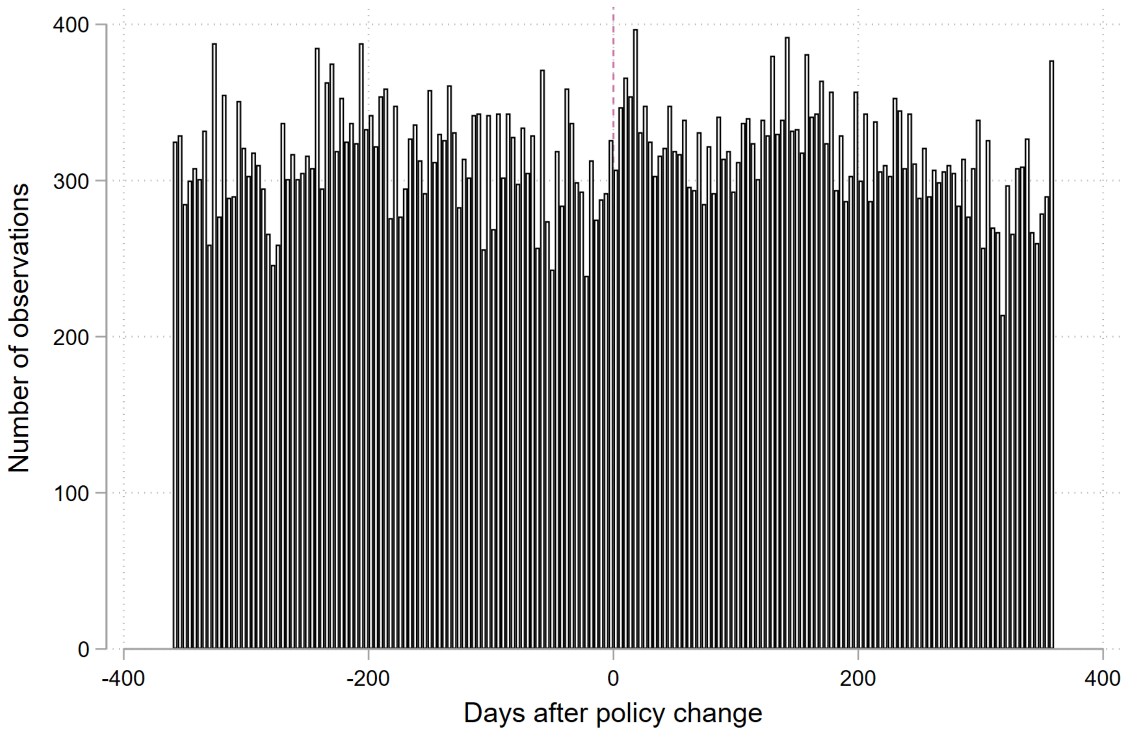
Case #: ______________________________ Name of Assessor: ______________________________

Pretrial Items		**Verified**
1.1 Age at First Arrest 0 = 33 or Older 1 = Under 33		
1.2 Number of Failure-to-Appear Warrants Past 24 Months 0 = None 1 = One Warrant for FTA 2 = Two or more FTA Warrants		
1.3 Three or more Prior Jail Incarcerations 0 = No 1 = Yes		
1.4 Employed at the Time of Arrest 0 = Yes, Full-time 1 = Yes, Part-time 2 = Not employed		
1.5 Residential Stability 0 = Lived at Current Residence Past Six Months 1 = Not Lived at Same Residence		
1.6 Illegal Drug Use during Past Six Months 0 = No 1 = Yes		
1.7 Severe Drug Use Problem 0 = No 1 = Yes		
Total Score:		

Scores	**Rating**	**% of Failures**	**% of Failure to Appear**	**% of New Arrest**
0-2	Low	5%	5%	0%
3-5	Moderate	18%	12%	7%
6+	High	29%	15%	17%

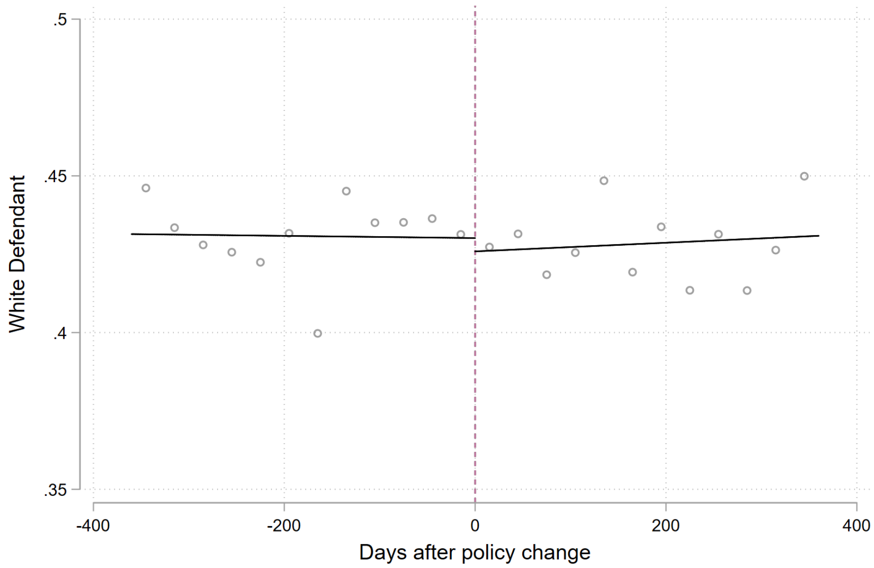
Notes: This figure shows the risk assessment tool used in Travis County, Texas.

Figure A.5: Frequency of Running Variable

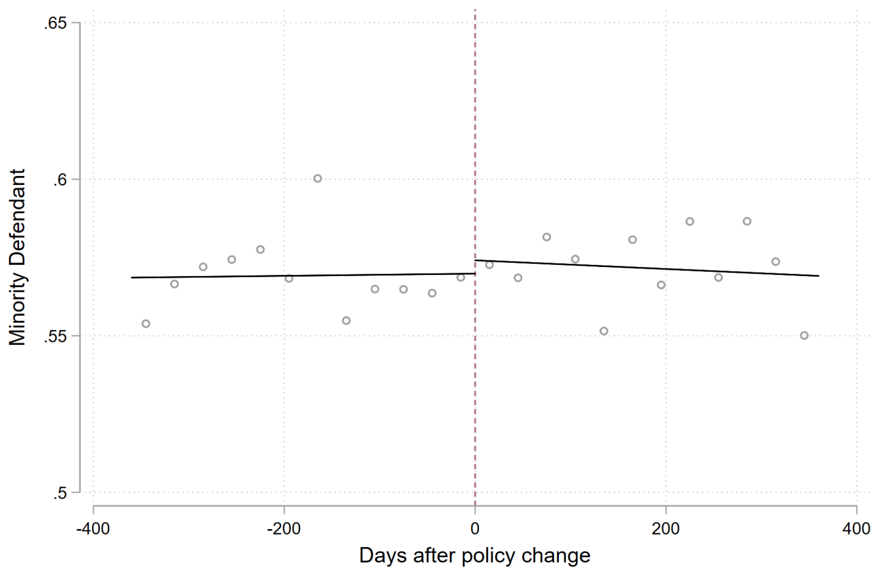


Notes: This figure shows the distribution of running variable observations near the adoption of risk assessment scores. Each bin is 2 days. The dashed line marks the day of the policy change.

Figure A.6: Smoothness of Baseline Covariates

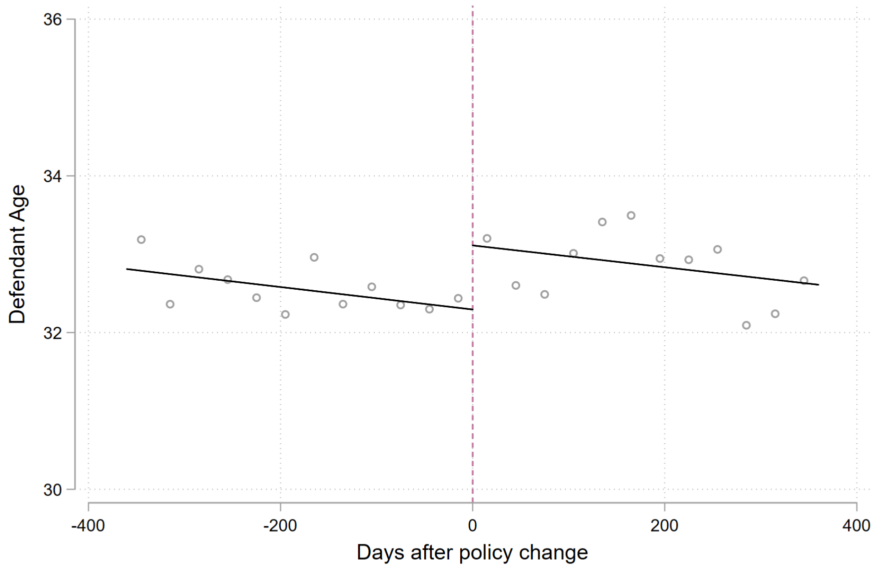


(a) White Defendant

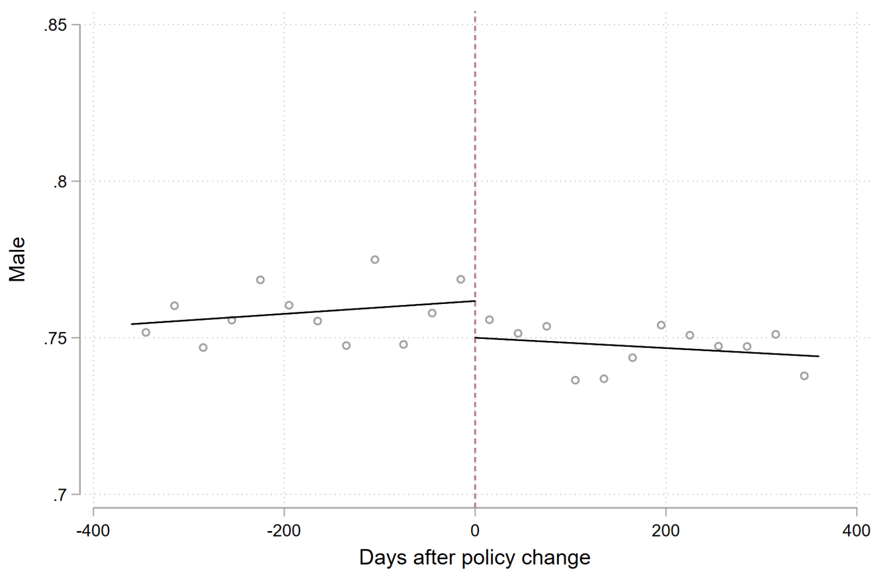


(b) Minority Defendant

Figure A.6: Continued

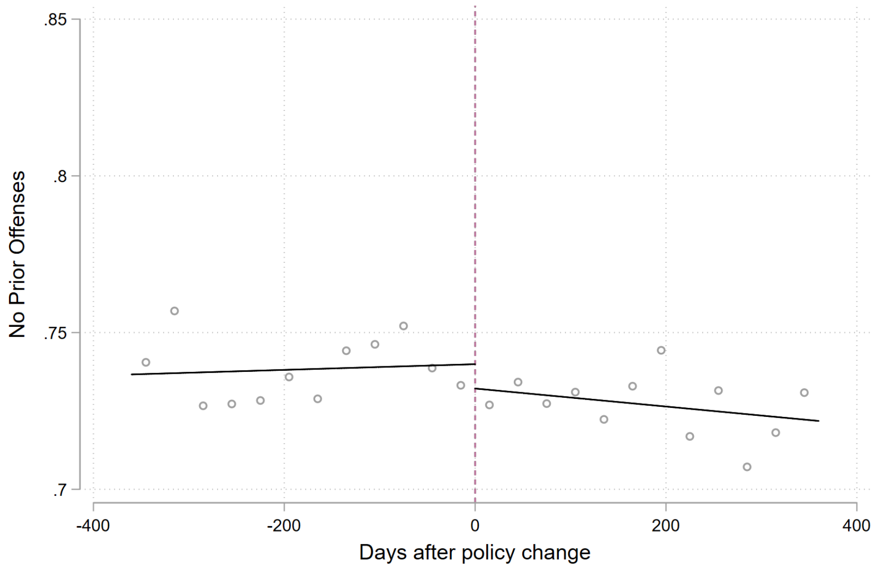


(c) Defendant Age

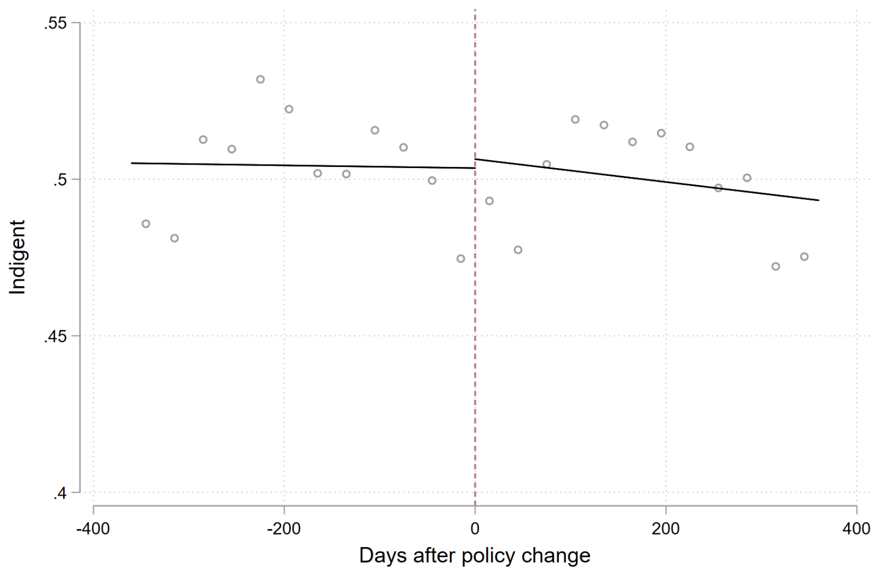


(d) Defendant Gender

Figure A.6: Continued

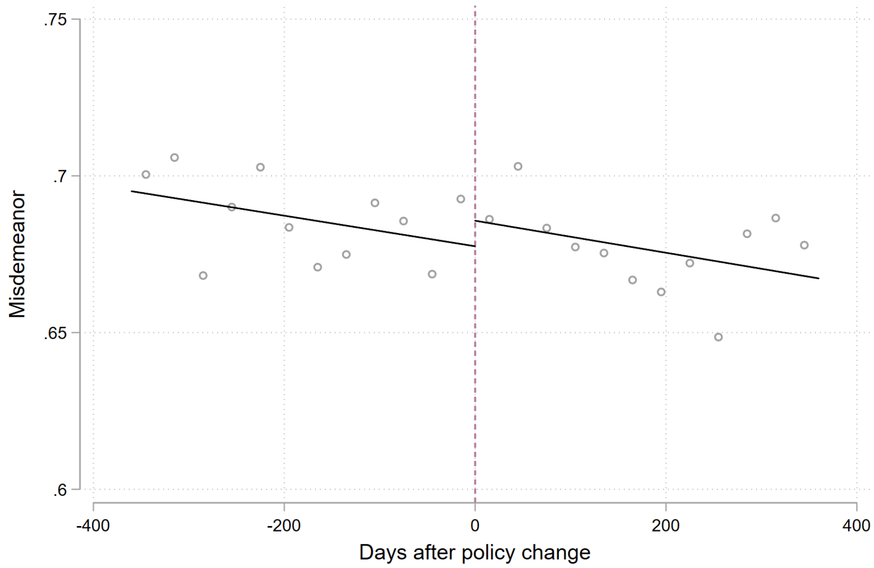


(e) No Priors

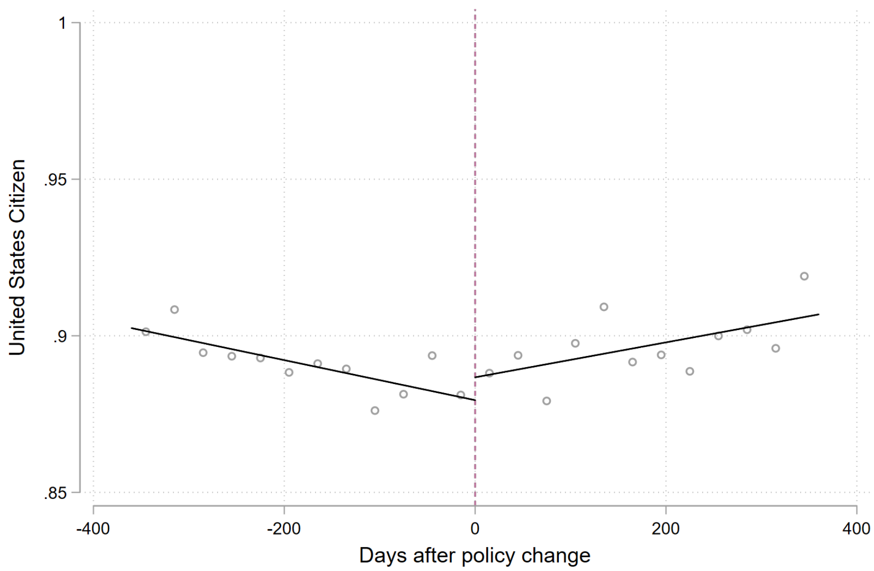


(f) Indigent Status

Figure A.6: Continued

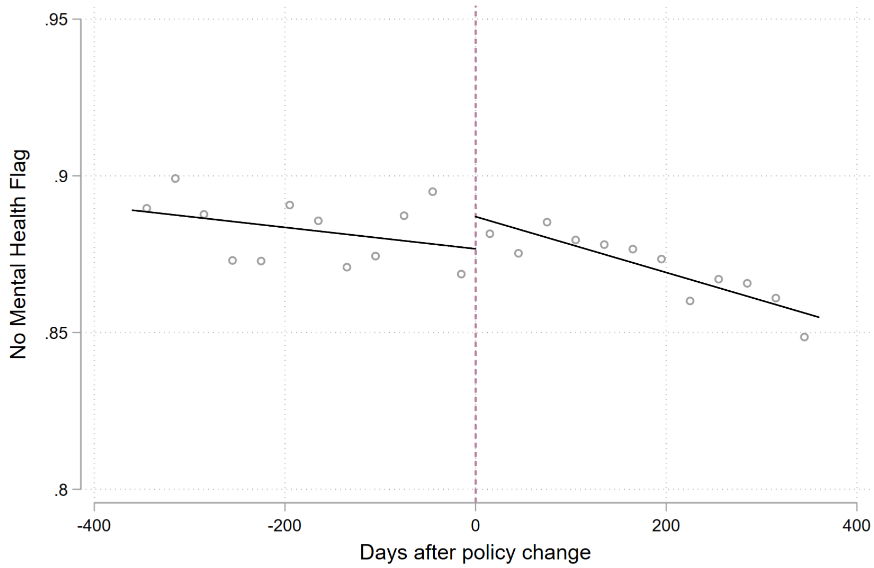


(g) Misdemeanor or Felony Case



(h) United States Citizen

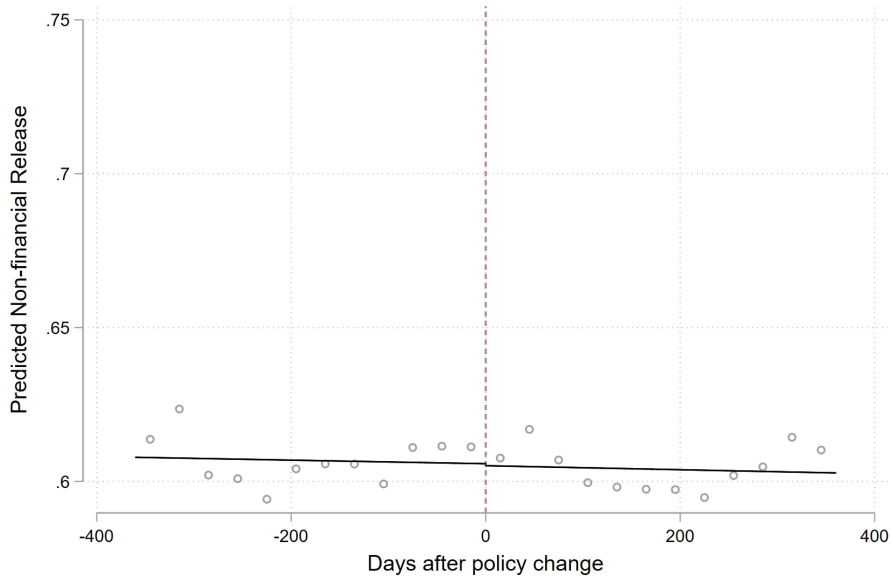
Figure A.6: Continued



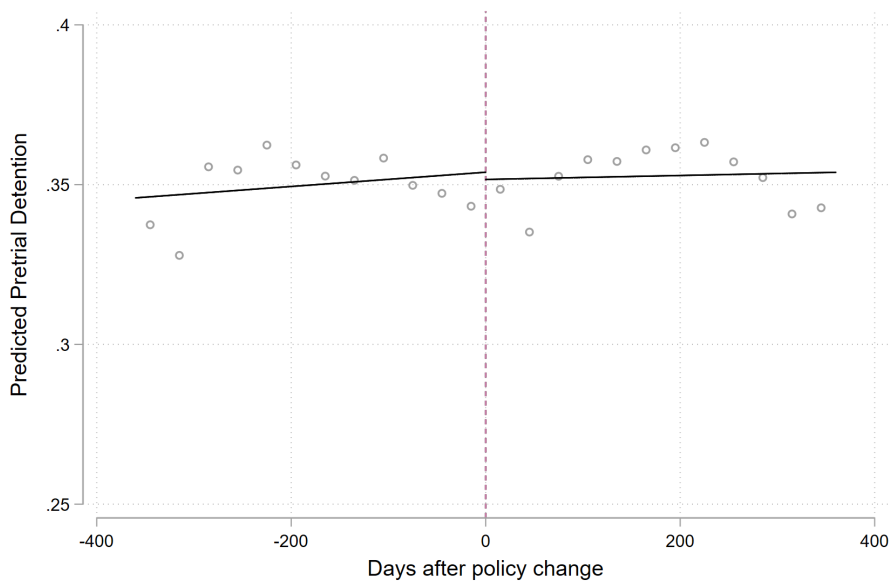
(i) Mental Health Flag

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 A.7: Regression Discontinuity Results for Predicted Values

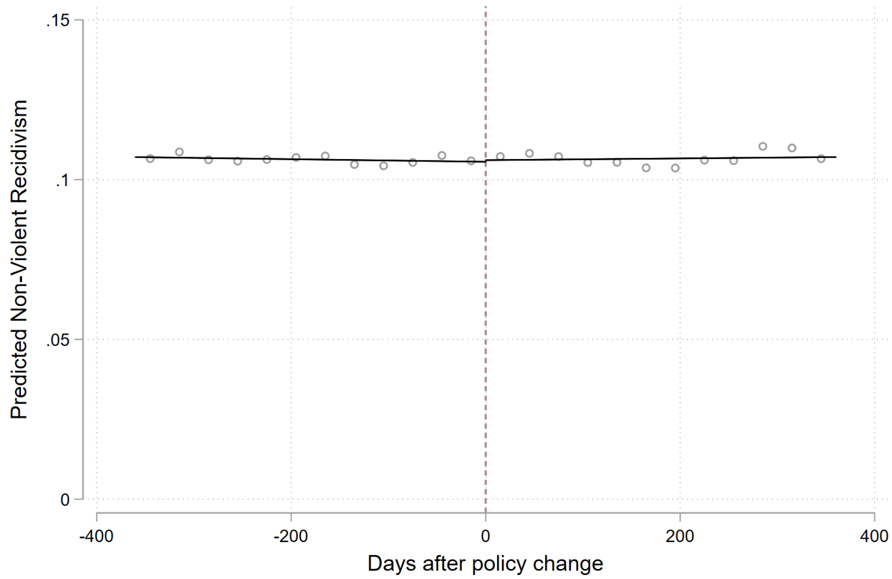


(a) Predicted Probability of Release on Non-financial Bond

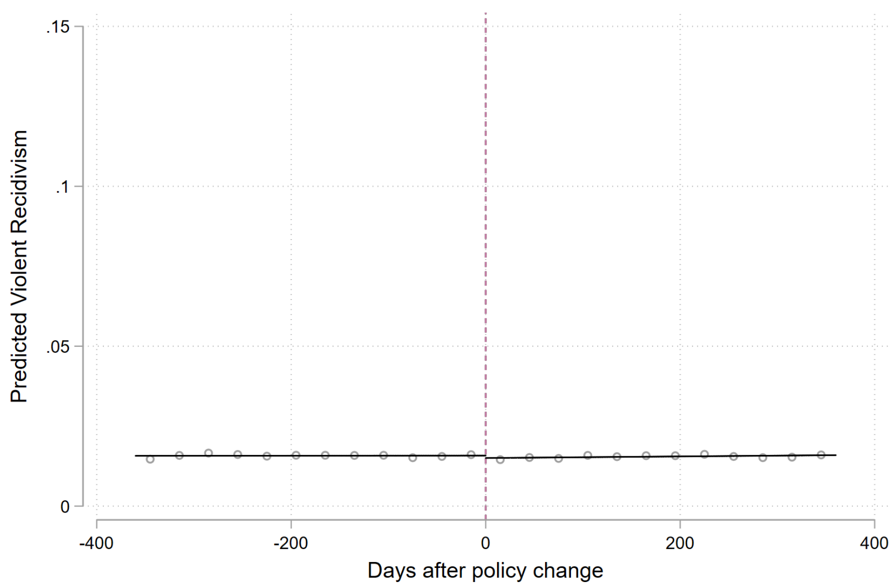


(b) Predicted Probability of Pretrial Detention

Figure A.7: Continued

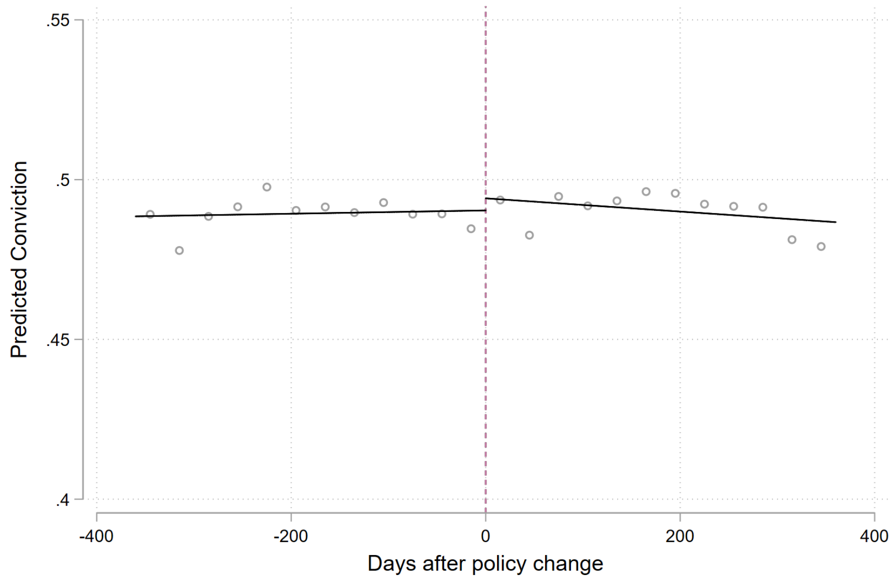


(c) Predicted Probability of Non-Violent Recidivism



(d) Predicted Probability of Violent Recidivism

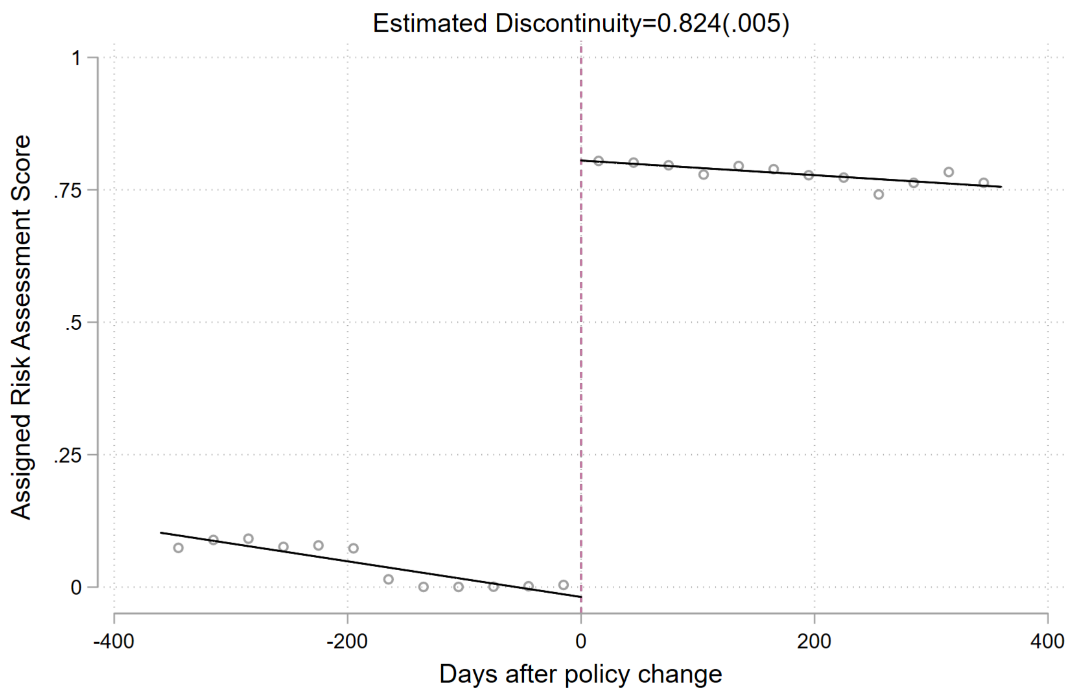
Figure A.7: Continued



(e) Predicted Probability of Conviction

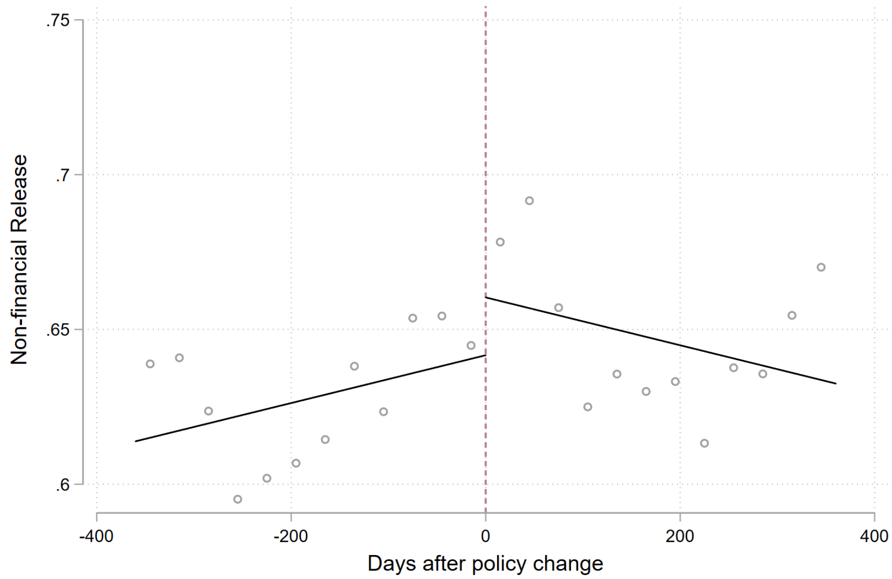
Notes: These figures plot tests of the regression discontinuity design. Each figure plots linear fits of the outcome listed and means of the outcome variable in 30 day bins. Outcome variables are predicted using observable case and defendant characteristics. Specifically, we use race, age, gender, criminal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

Figure A.8: Regression Discontinuity Results for the Probability of Receiving a Risk Assessment Score

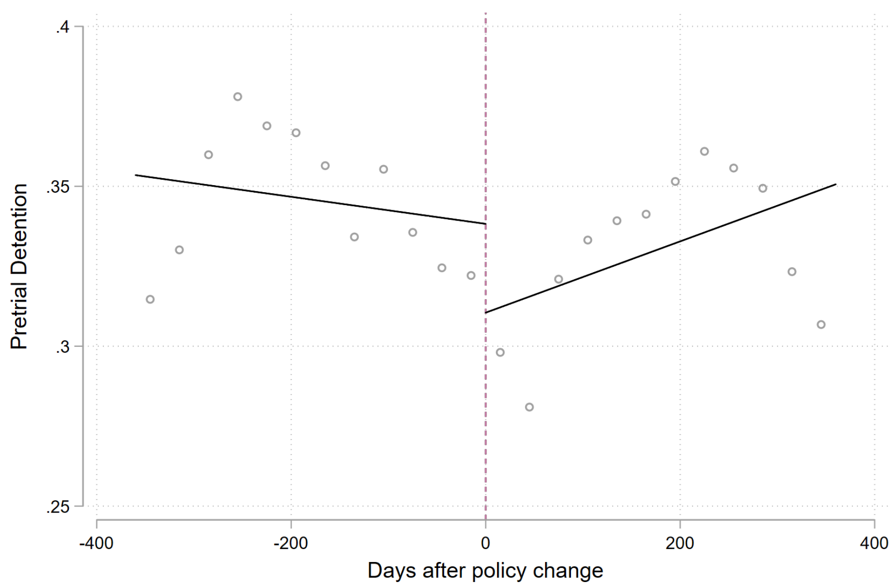


Notes: This figure shows the regression discontinuity estimate of the effect of implementing a risk assessment score policy on score usage by plotting the mean of risk assessment score take-up in 30 day bins with linear fits. The outcome variable takes on a value of one if a defendant has a risk assessment score and zero if she does not. A bandwidth of 360 days is shown. There was a small pilot study run about a year before the policy change.

Figure A.9: Regression Discontinuity Results for Non-financial Bond and Pretrial Detention



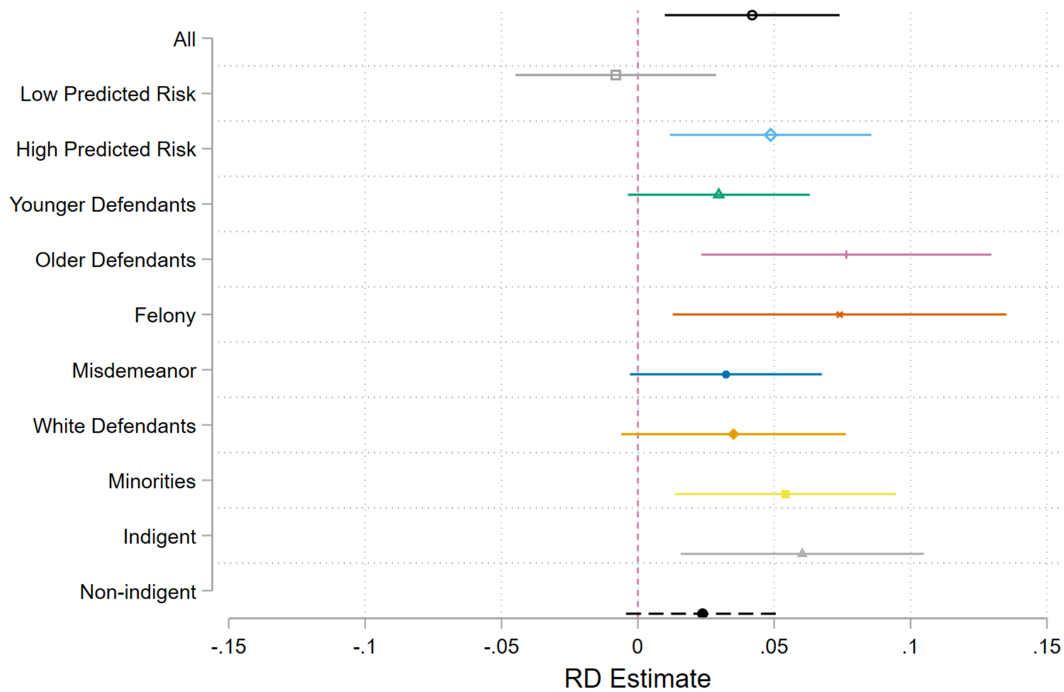
(a) Non-financial Bond



(b) Pretrial Detention

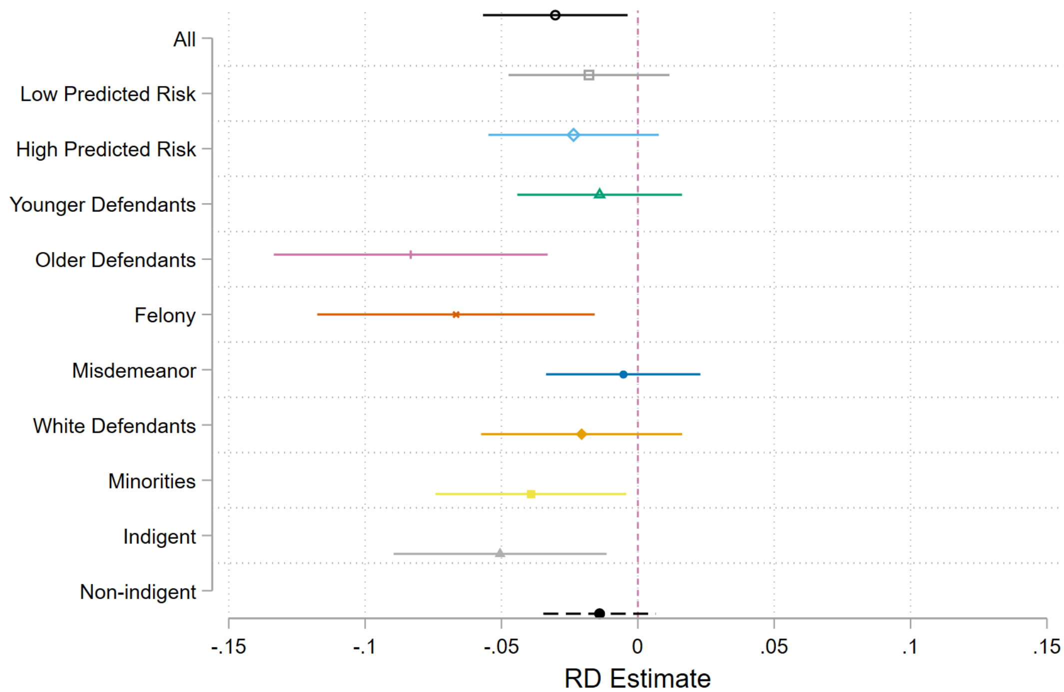
Notes: This figure shows the regression discontinuity estimate of the effect of implementing a risk assessment score policy on non-financial bond or pretrial detention by plotting the mean non-financial bond or pretrial detention in 30 day bins with linear fits. A bandwidth of 360 days is shown.

Figure A.10: The Effect of Risk Assessment Scores on Non-financial Release by Subgroup



(a) Non-financial bond

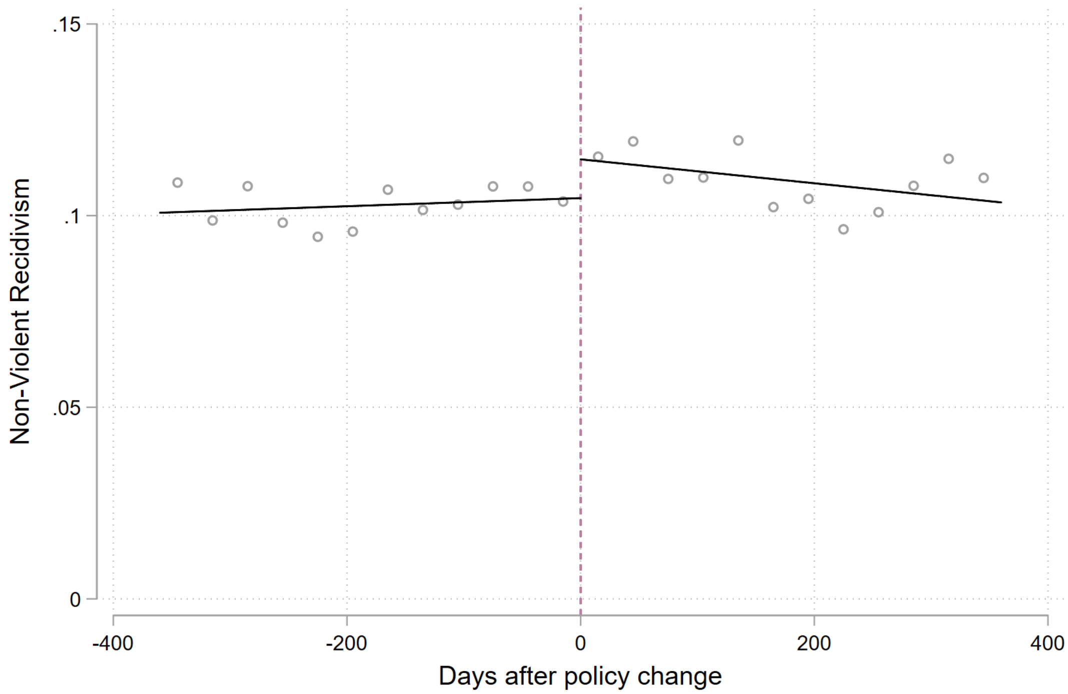
Figure A.10: Continued



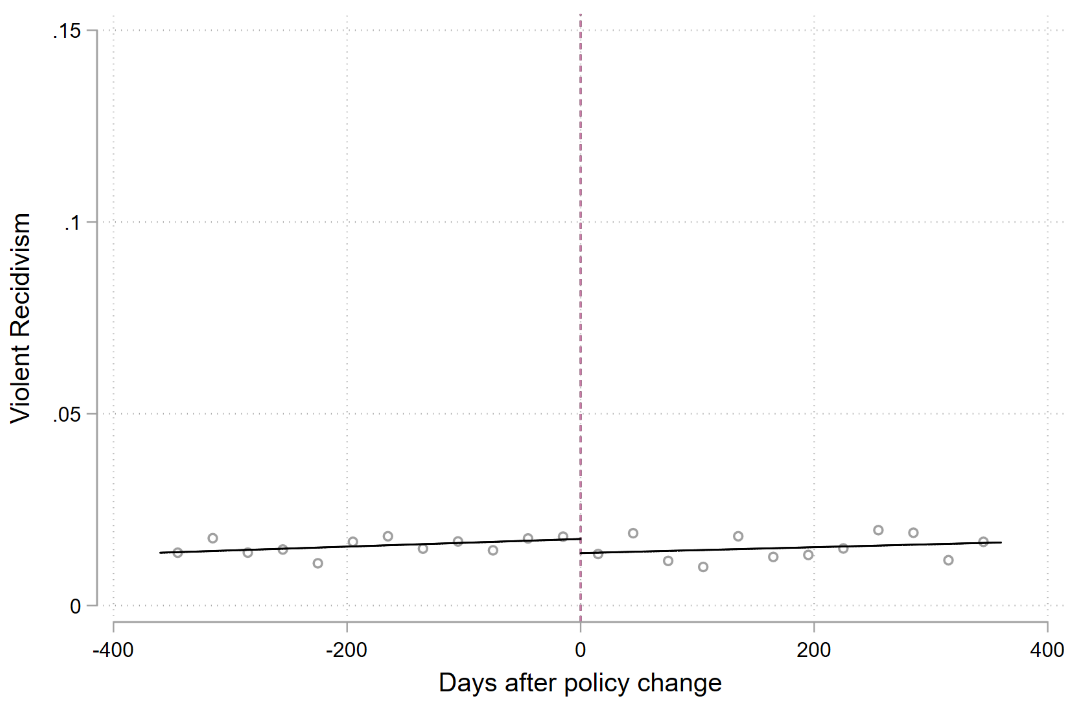
(b) Pretrial Detention

Each figure presents regression discontinuity results from eleven separate regressions. Each estimate is calcuated using the MSE optimal bandwith. Risk is predicted using using observable case and defendant characteristics (race, indigent status, age, criminal history, gender, mental health, citizenship, misdemeanor, day of the week and court). Young defendants are younger than the average age (33 years). Older defendants are older than the average age. Non-minorities are only white defendants. Minorities are non-white or Hispanic.

Figure A.11: Regression Discontinuity Results for Conviction and Pretrial Crime

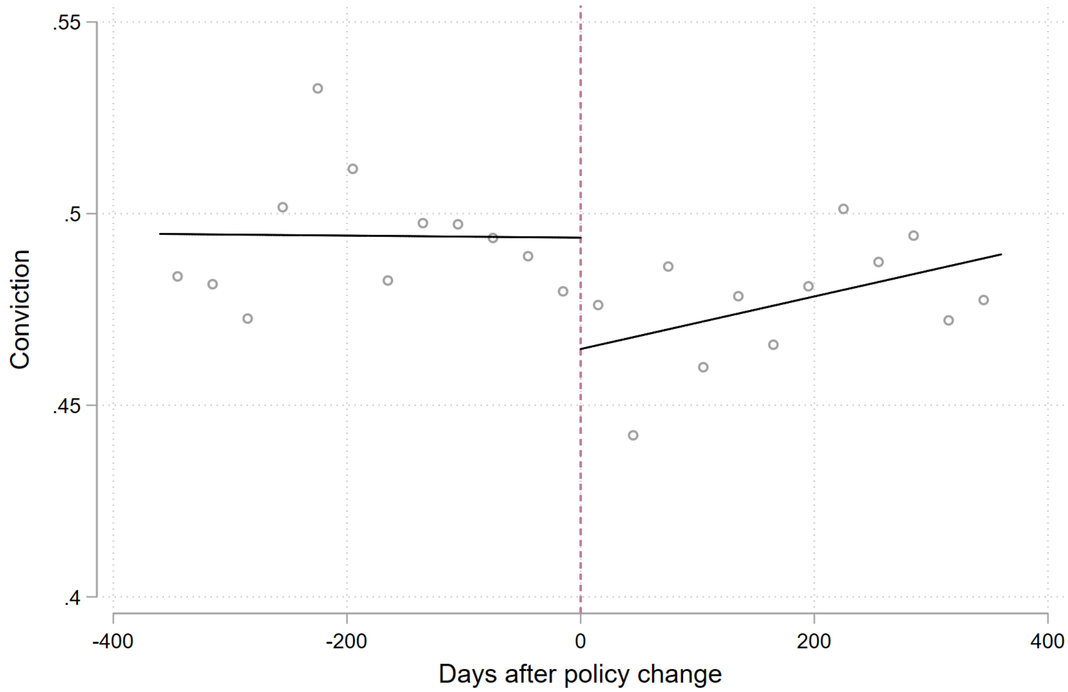


(a) Probability of Non-Violent Pretrial Crime



(b) Probability of Violent Pretrial Crime

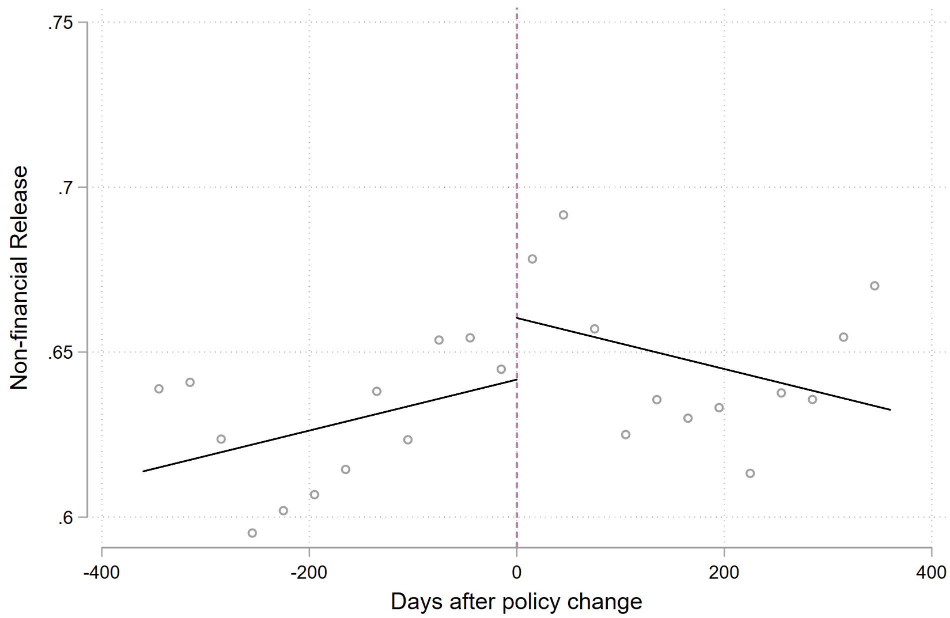
Figure A.11: Continued



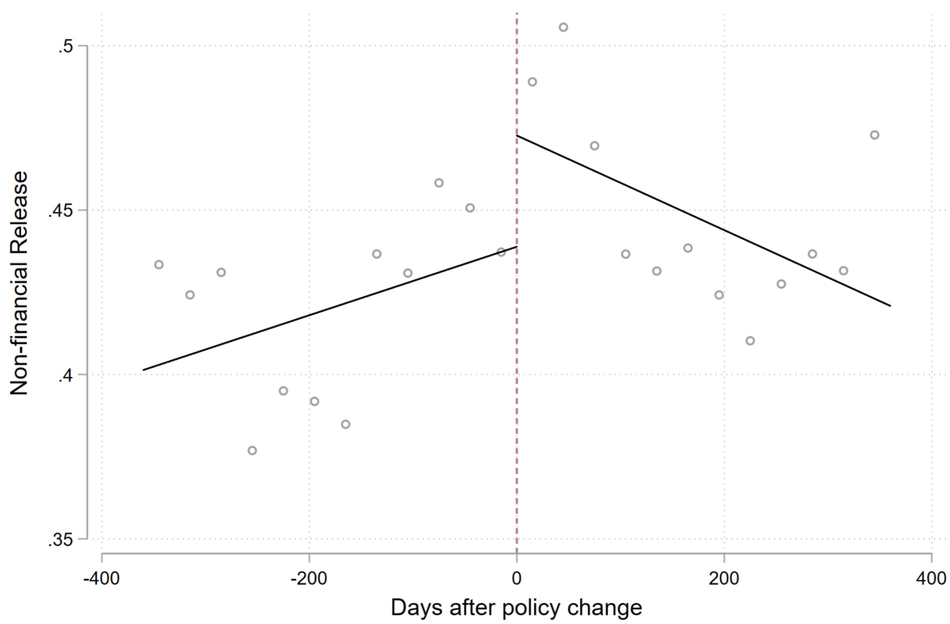
(c) Probability of Conviction

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

Figure A.12: Indigent Regression Discontinuity Results for Non-financial Bond and Pretrial Detention

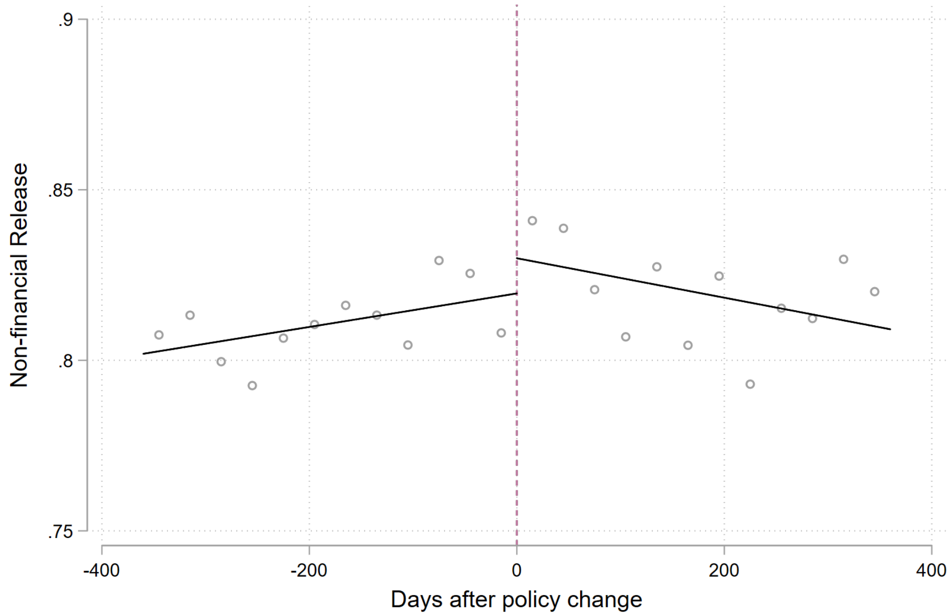


(a) Non-financial bond - Entire Sample

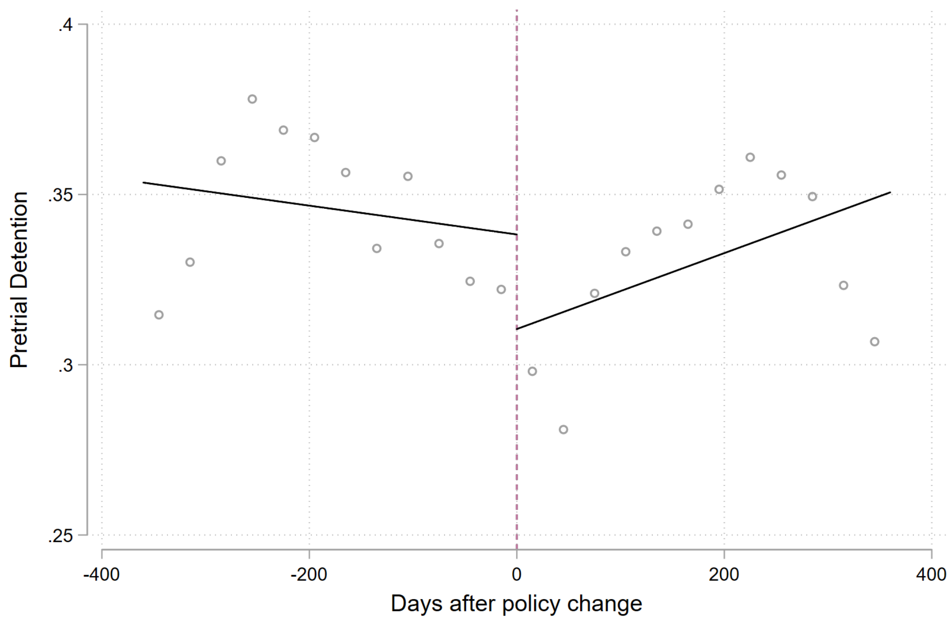


(b) Non-financial bond - Indigent

Figure A.12: Continued

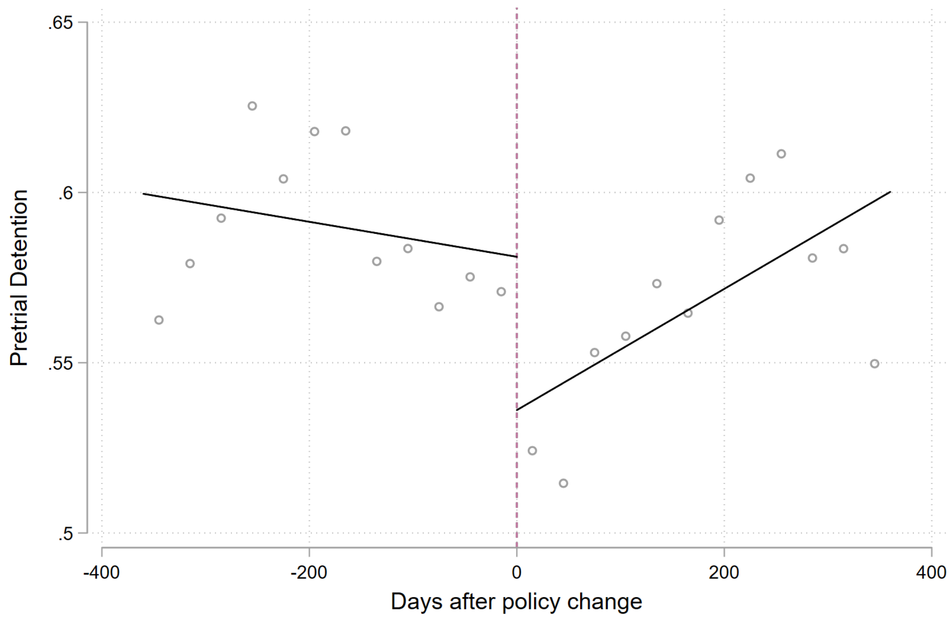


(c) Non-financial bond - Non-Indigent

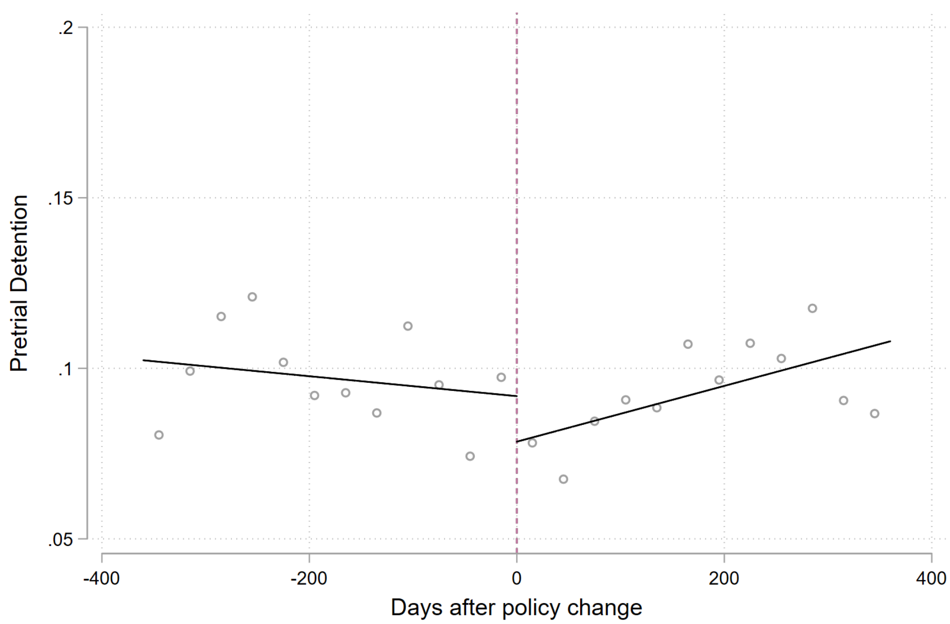


(d) Pretrial Detention - Entire Sample

Figure A.12: Continued



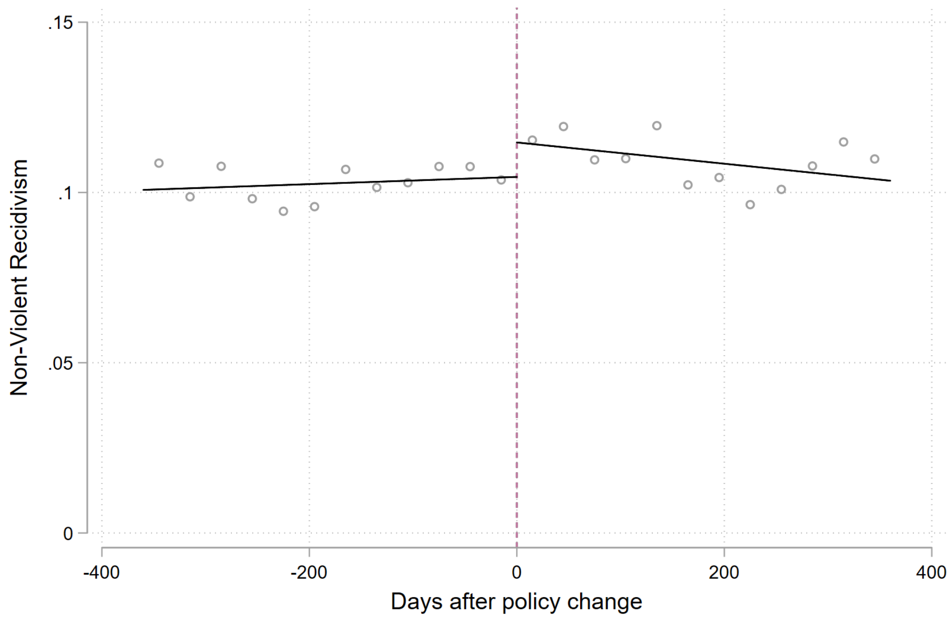
(e) Pretrial Detention - Indigent



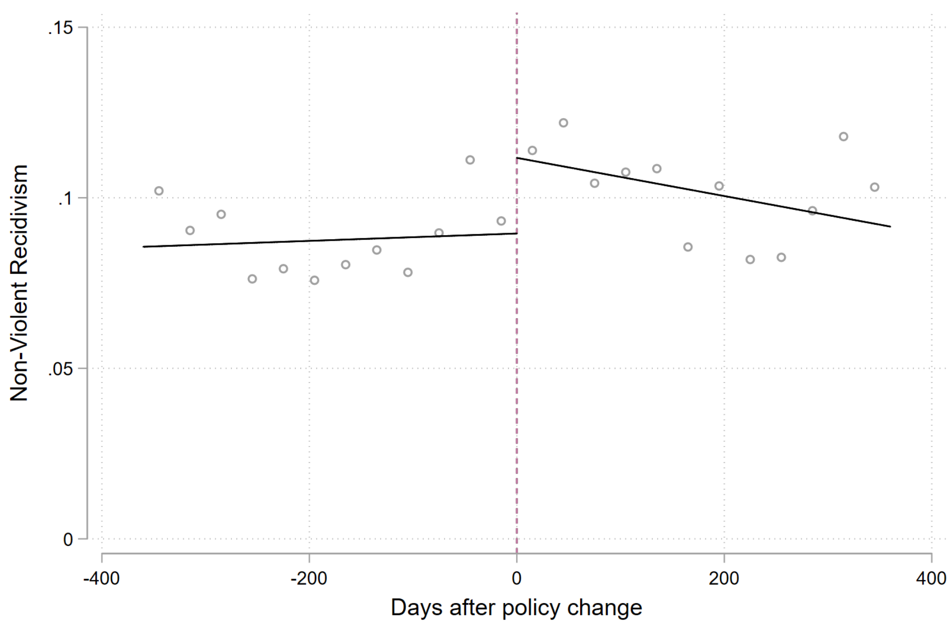
(f) Pretrial Detention - Non-Indigent

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

Figure A.13: Indigent Regression Discontinuity Results for Pretrial Crime

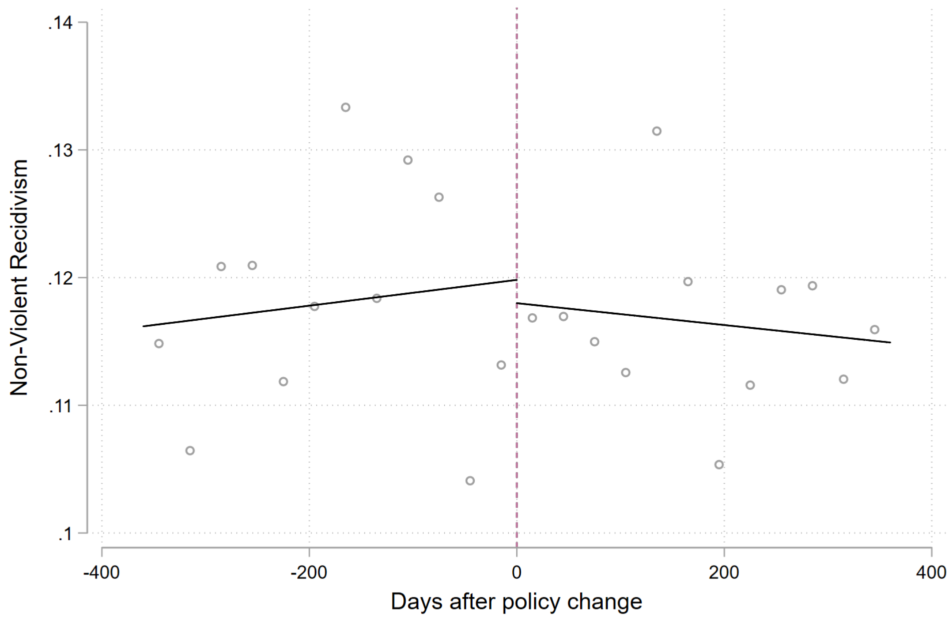


(a) Non-violent Pretrial Crime - Entire Sample

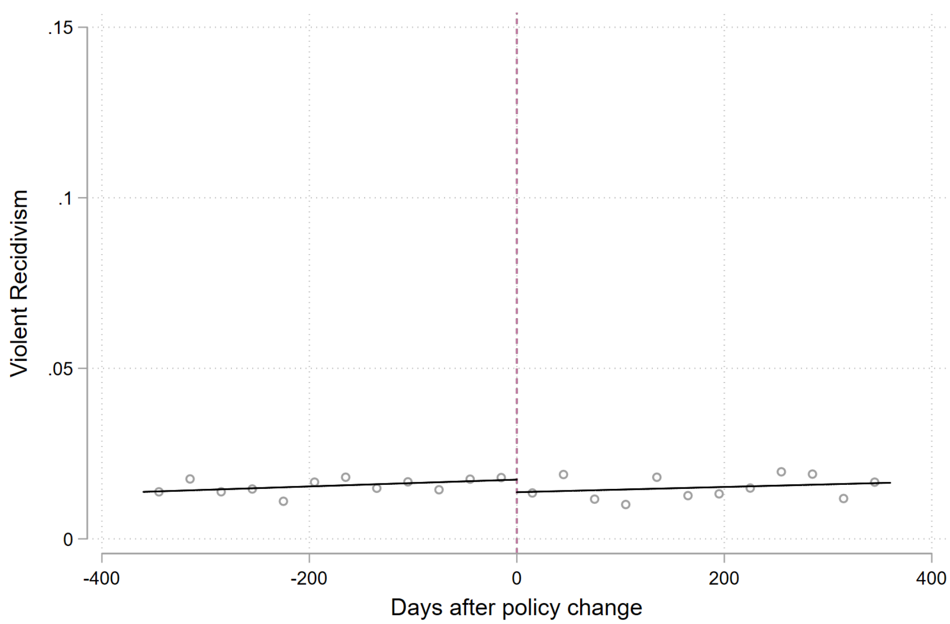


(b) Non-violent Pretrial Crime - Indigent

Figure A.13: Continued

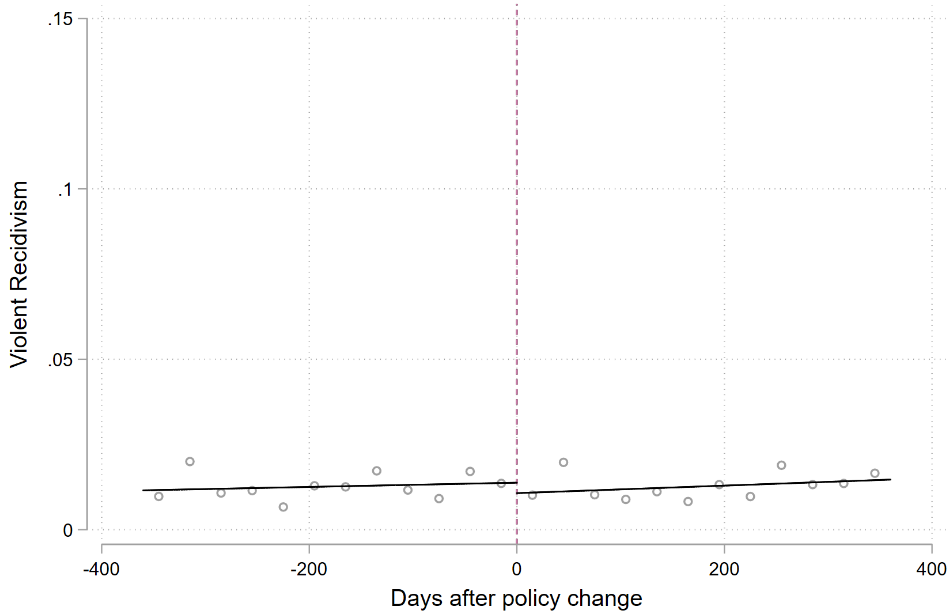


(c) Non-violent Pretrial Crime - Non-Indigent

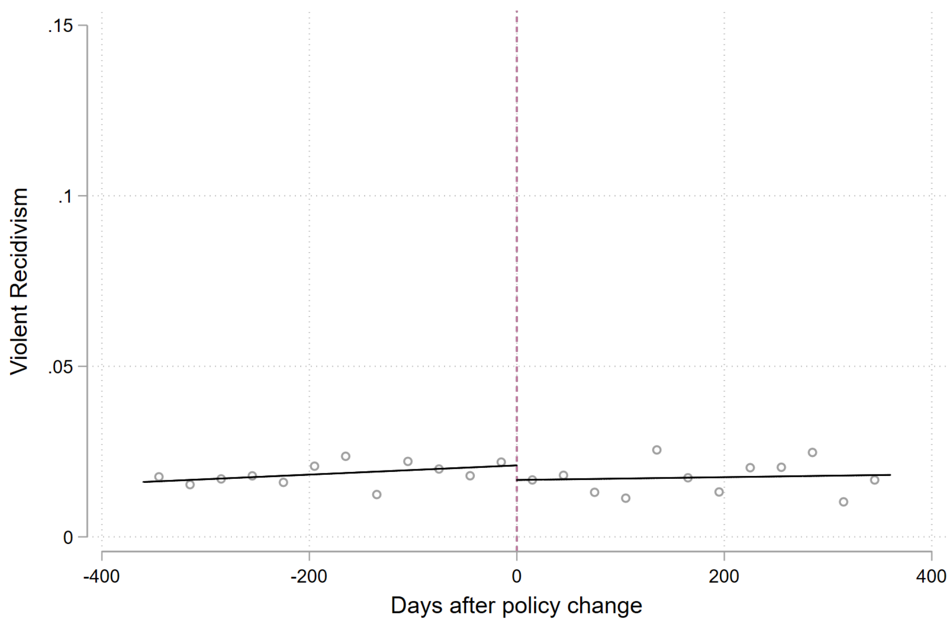


(d) Violent Pretrial Crime - Entire Sample

Figure A.13: Continued



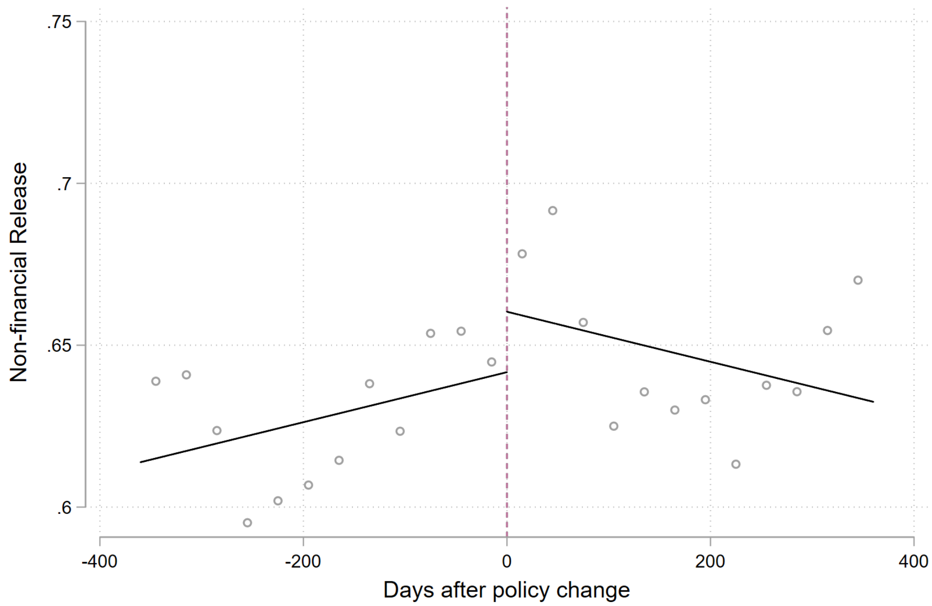
(e) Violent Pretrial Crime - Indigent



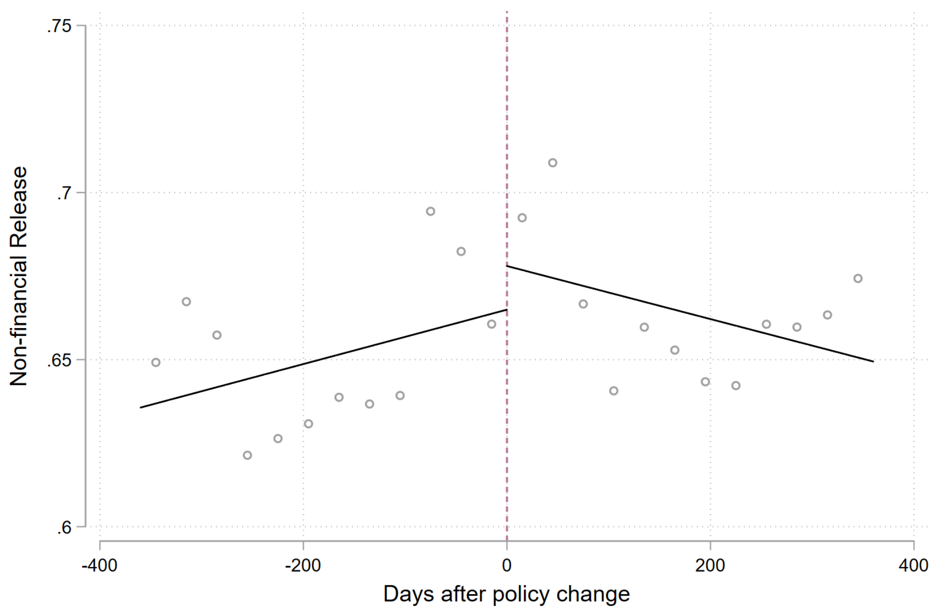
(f) Violent Pretrial Crime - Non-Indigent

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

Figure A.14: Regression Discontinuity Results for Non-financial Bond and Pretrial Detention by Race

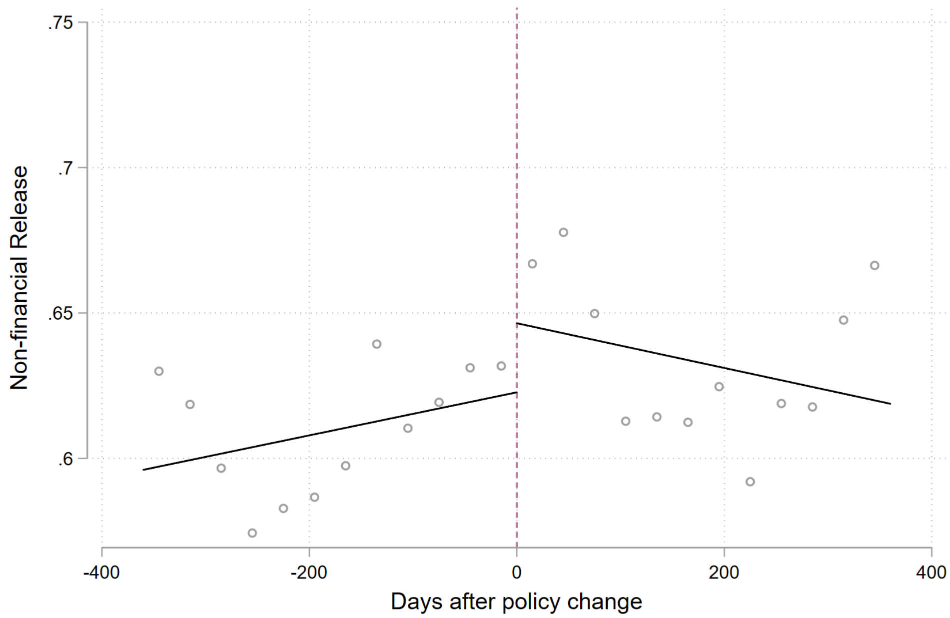


(a) Non-financial Bond - Entire Sample

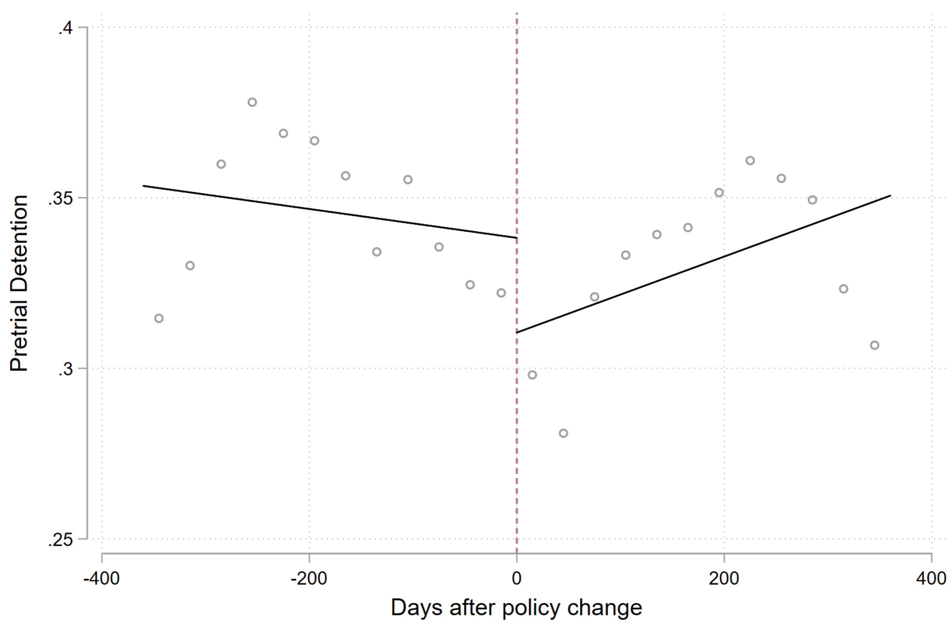


(b) Non-financial Bond - White

Figure A.14: Continued

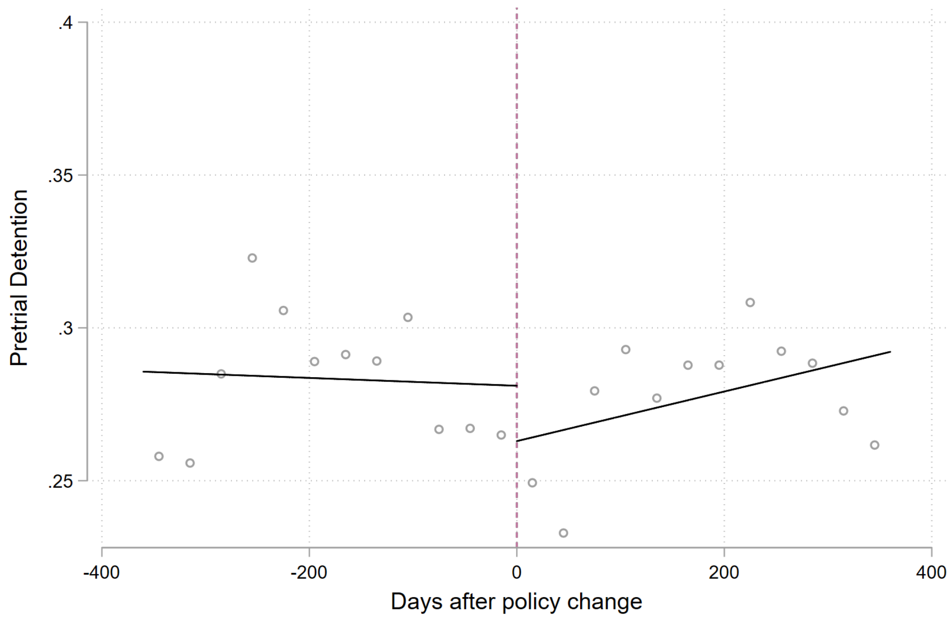


(c) Non-financial Bond - Minority

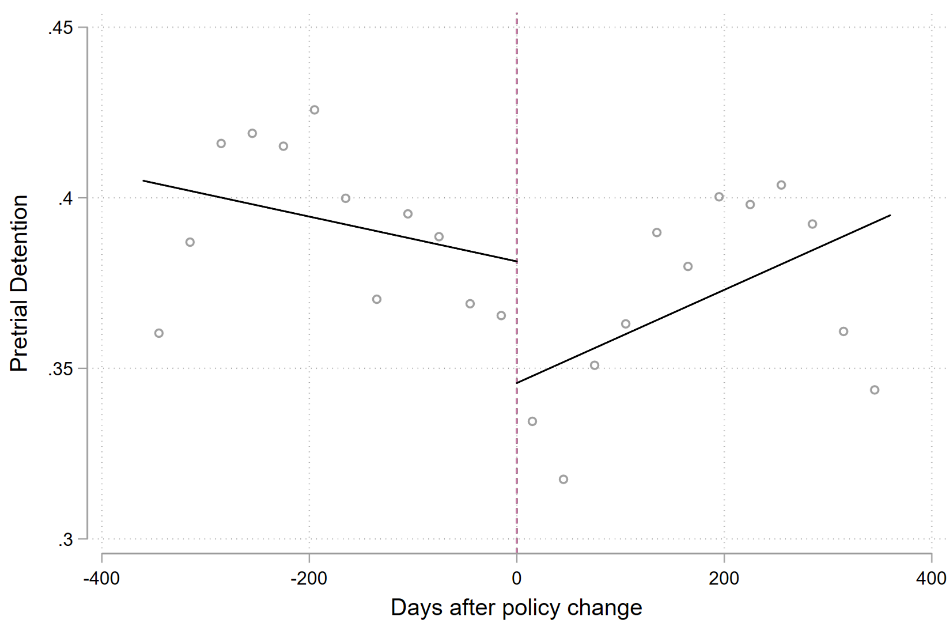


(d) Pretrial Detention- Entire Sample

Figure A.14: Continued



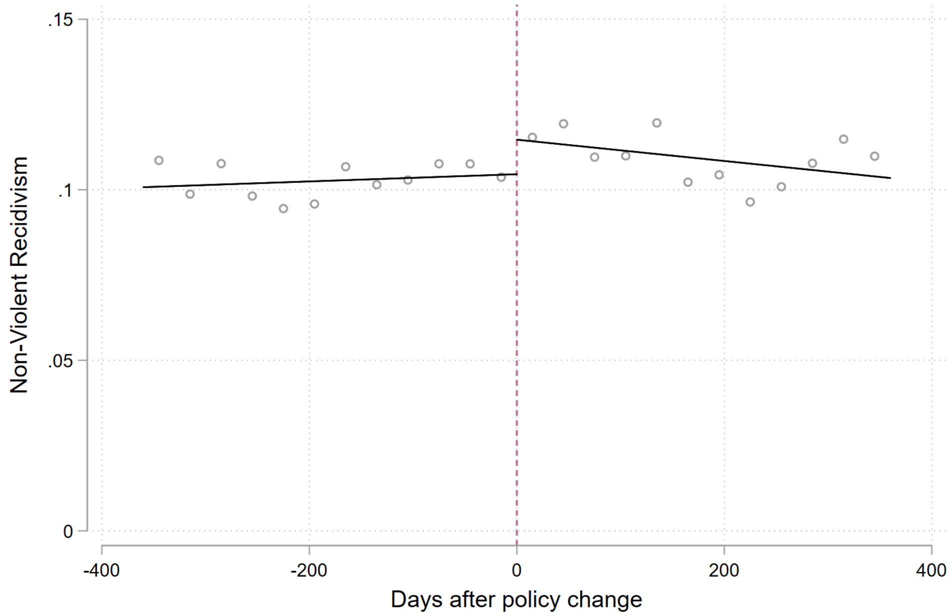
(e) Pretrial Detention- White



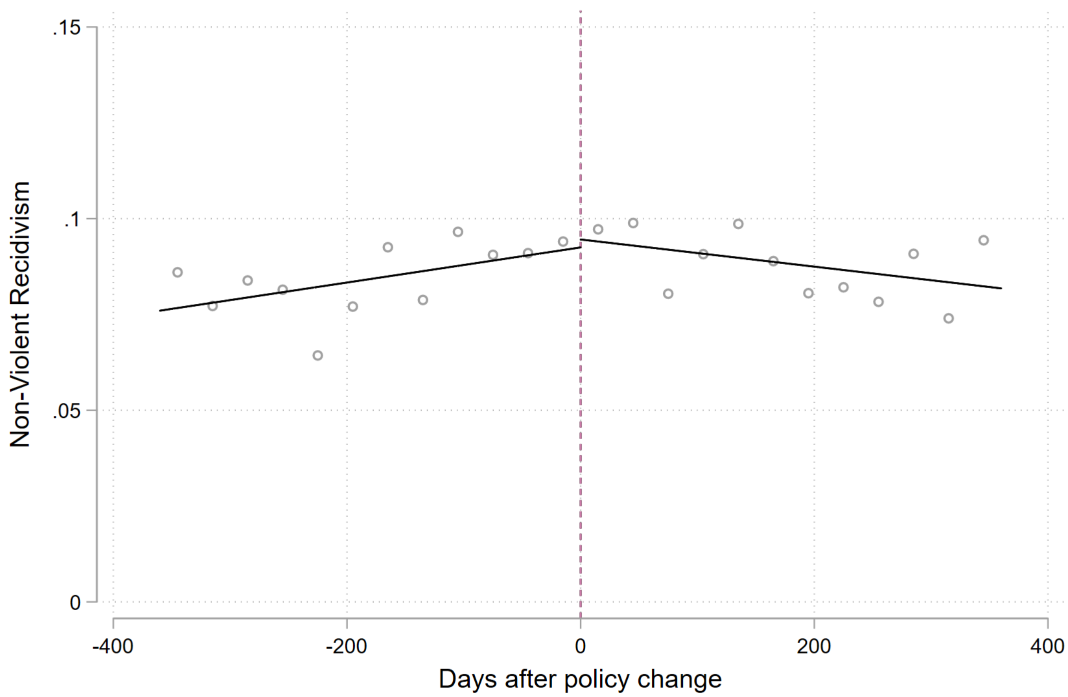
(f) Pretrial Detention- Minority

Notes: This figure shows the regression discontinuity estimate of the effect of implementing a risk assessment score policy on the non-financial bond or pretrial detention by plotting the mean non-financial bond or pretrial detention in 30 day bins with linear fits. A bandwidth of 360 days is shown. White defendants are only white. Minority defendants are Hispanic or non-white.

Figure A.15: Regression Discontinuity Results for Pretrial Crime by Race

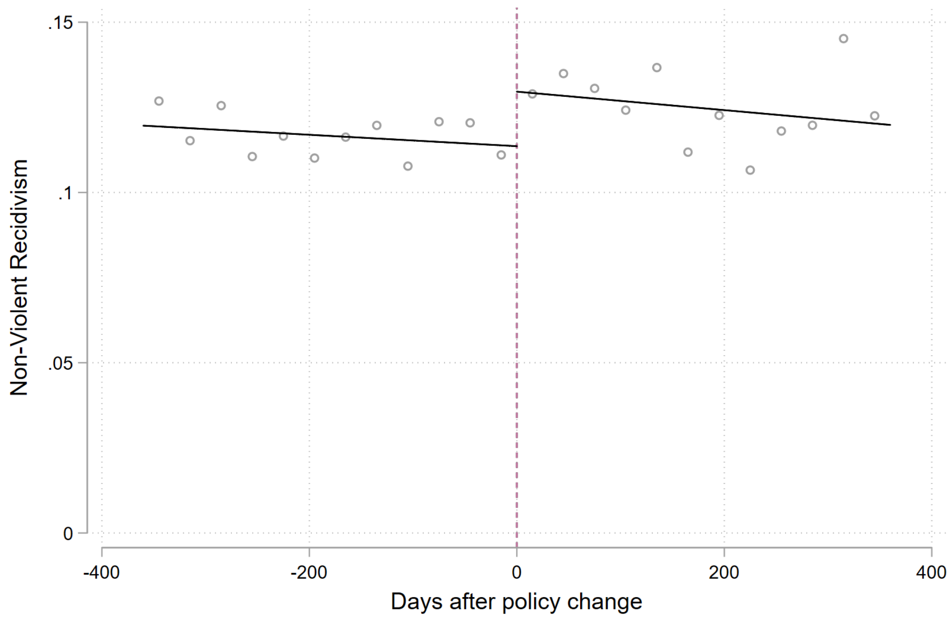


(a) Non-violent Pretrial Crime - Entire Sample

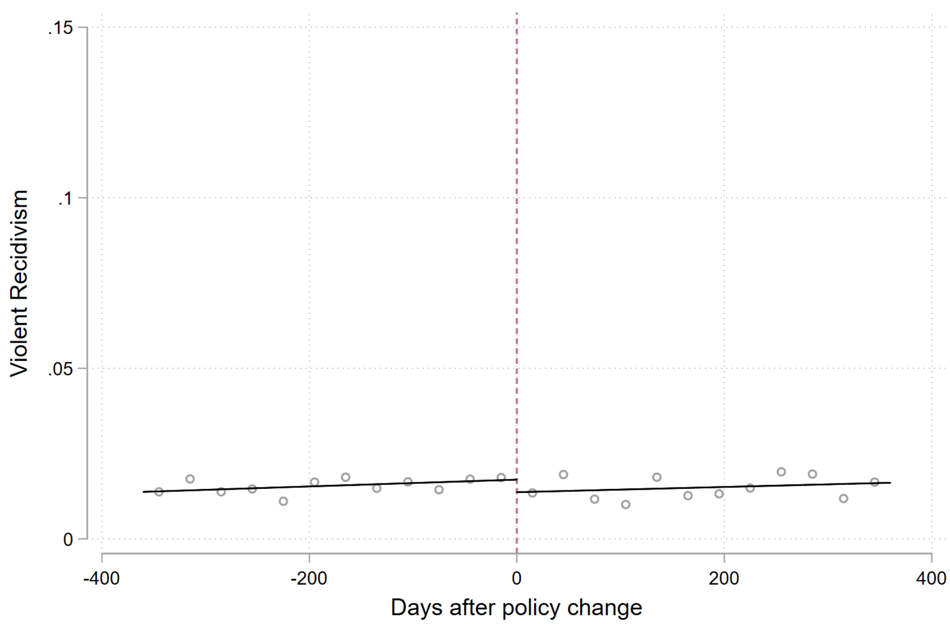


(b) Non-violent Pretrial Crime - White

Figure A.15: Continued

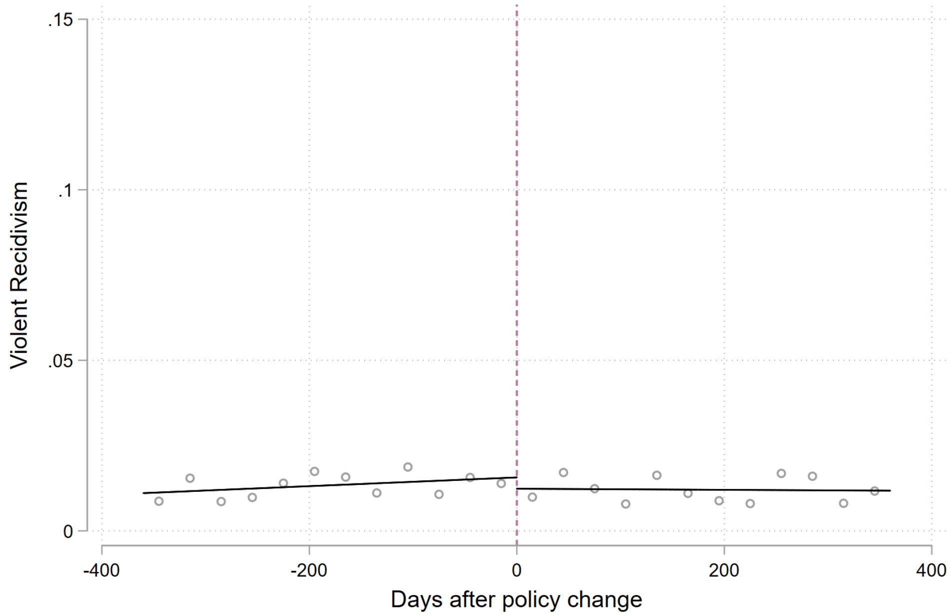


(c) Non-violent Pretrial Crime - Minority

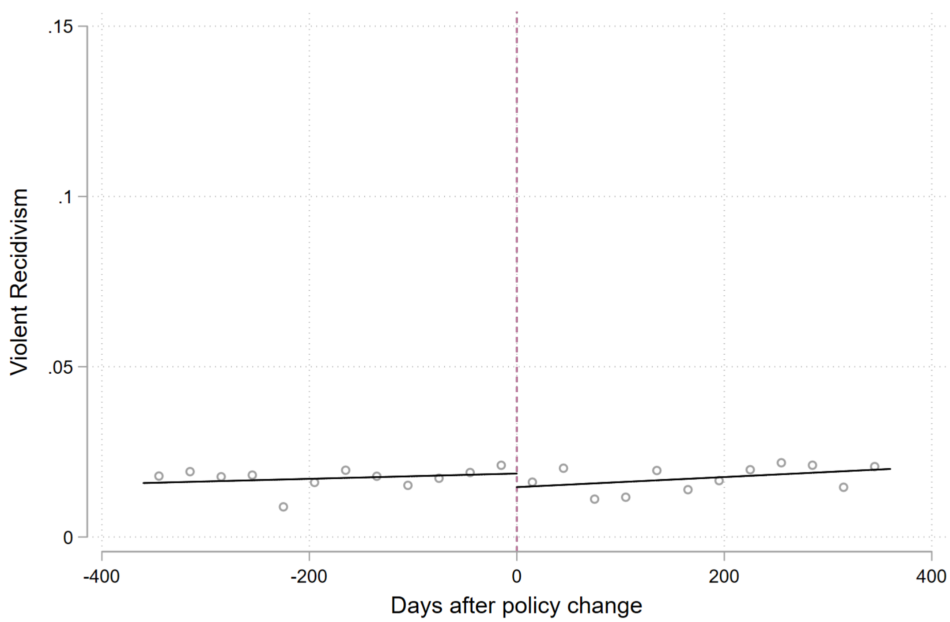


(d) Violent Pretrial Crime - Entire Sample

Figure A.15: Continued



(e) Violent Pretrial Crime - White



(f) Violent Pretrial Crime - Minority

Notes: This figure shows the regression discontinuity estimate of the effect of implementing a risk assessment score policy on pretrial crime by plotting the mean non-financial bond or pretrial detention in 30 day bins with linear fits. A bandwidth of 360 days is shown. White defendants are only white. Minority defendants are Hispanic or non-white.

Table A.8: Summary Statistics

	Mean	Standard Deviation	Number of Observations
Case Characteristics			
White Defendant	0.42	0.49	143,092
Minority Defendant	0.58	0.49	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
Outcomes			
Non-financial Release	0.6253	0.484	127,904
Pretrial Detention	0.3532	0.478	143,092
Violent Pretrial Crime	0.0154	0.123	143,092
Non-Violent Pretrial Crime	0.1056	0.307	143,092
Conviction	0.4904	0.500	143,092

Notes: Each observation is a separate case. Data are from Travis County Courts and Travis County Pretrial Services for the years 2011-2014. Travis County records the race and ethnicity of each defendant. Defendants are white if they are white and not Hispanic. Minority defendants are either non-white or Hispanic.

Table A.9: Release Regression Discontinuity Results

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome: Non-financial Bail						
RD_Estimate	0.0285***	0.0320***	0.0406***	0.0433***	0.0411**	0.0423***
	(0.0101)	(0.0113)	(0.0117)	(0.0130)	(0.0160)	(0.0159)
Observations	25742	47484	19136	36052	12572	23440
FDR q-value	0.009	0.013	0.002	0.005	0.04	0.04
Bandwidth	185.0	337.6	138.8	253.2	92.51	168.8
Outcome: Pretrial Detention						
RD_Estimate	-0.0292***	-0.0322***	-0.0307***	-0.0332***	-0.0310**	-0.0255*
	(0.00731)	(0.0102)	(0.00844)	(0.0117)	(0.0129)	(0.0144)
Observations	39354	45418	28734	34086	18862	22300
FDR q-value	0.001	0.01	0.001	0.013	0.04	0.19
Bandwidth	247.5	288.1	185.6	216.0	123.7	144.0
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. We compute the FDR-adjusted q-values using the method proposed by Anderson (2008b), adjusting for our five different outcomes.

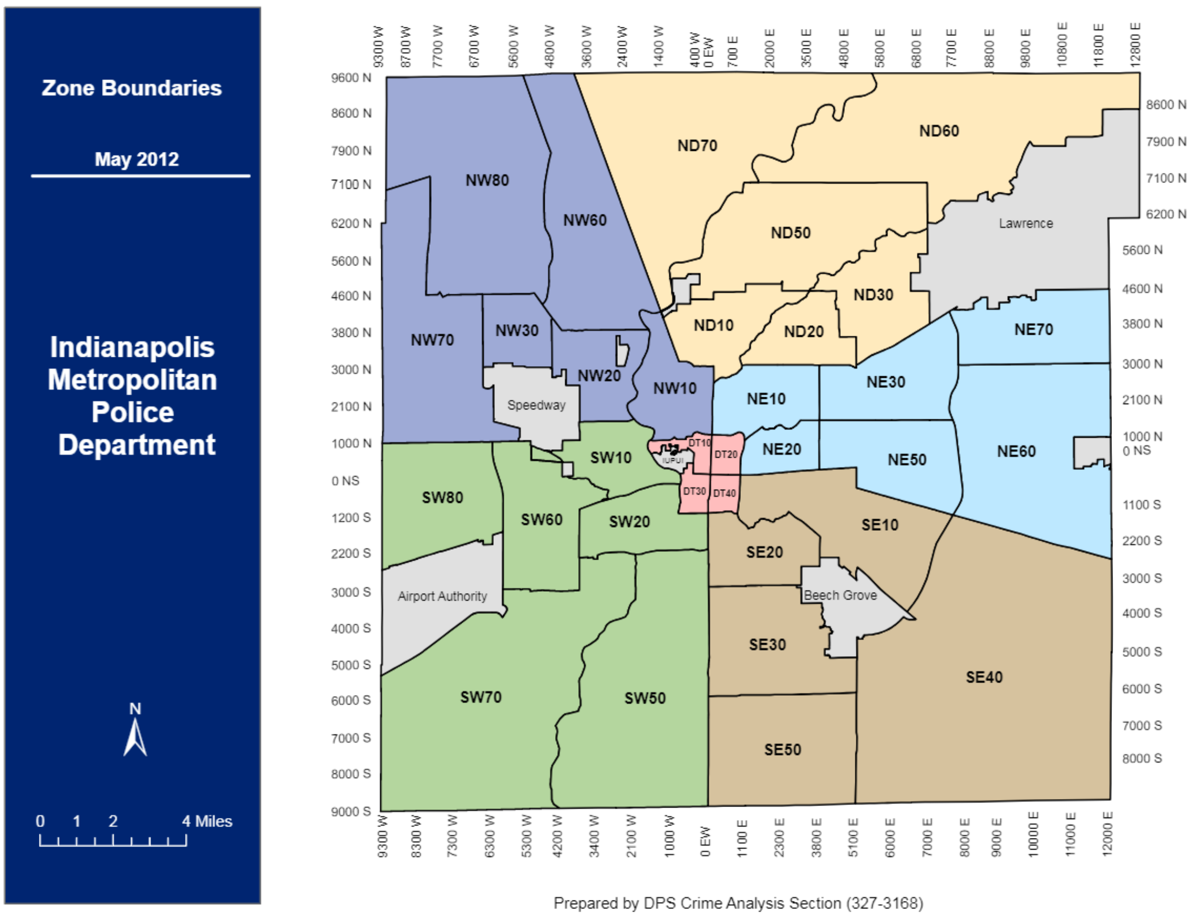
Table A.10: Pretrial Crime and Conviction Regression Discontinuity Results

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome: Non-Violent Pretrial Crime						
RD_Estimate	0.00957*	0.0116*	0.00979*	0.0101	0.0108	0.00985
	(0.00507)	(0.00677)	(0.00586)	(0.00782)	(0.00727)	(0.00951)
Observations	58646	73300	44142	55922	28924	37706
FDR q-value	0.059	0.109	0.119	0.324	0.17	0.502
Bandwidth	372.1	474.6	279.1	356.0	186.0	237.3
Outcome: Violent Pretrial Crime						
RD_Estimate	-0.00474**	-0.00399	-0.00239	-0.00327	-0.00319	-0.000657
	(0.00224)	(0.00273)	(0.00259)	(0.00316)	(0.00321)	(0.00386)
Observations	47280	71312	35572	54348	23258	36462
FDR q-value	0.043	0.143	0.355	0.367	0.322	0.865
Bandwidth	300.6	460.4	225.4	345.3	150.3	230.2
Outcome: Conviction						
RD_Estimate	-0.0322***	-0.0232**	-0.0289***	-0.0112	-0.0202*	-0.0105
	(0.00761)	(0.0108)	(0.00879)	(0.0124)	(0.0113)	(0.0152)
Observations	59880	66434	44996	50492	29616	33590
FDR q-value	0.001	0.054	0.002	0.367	0.125	0.61
Bandwidth	381.0	427.8	285.8	320.9	190.5	213.9
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
* $p < .1$, ** $p < .05$, *** $p < .01$

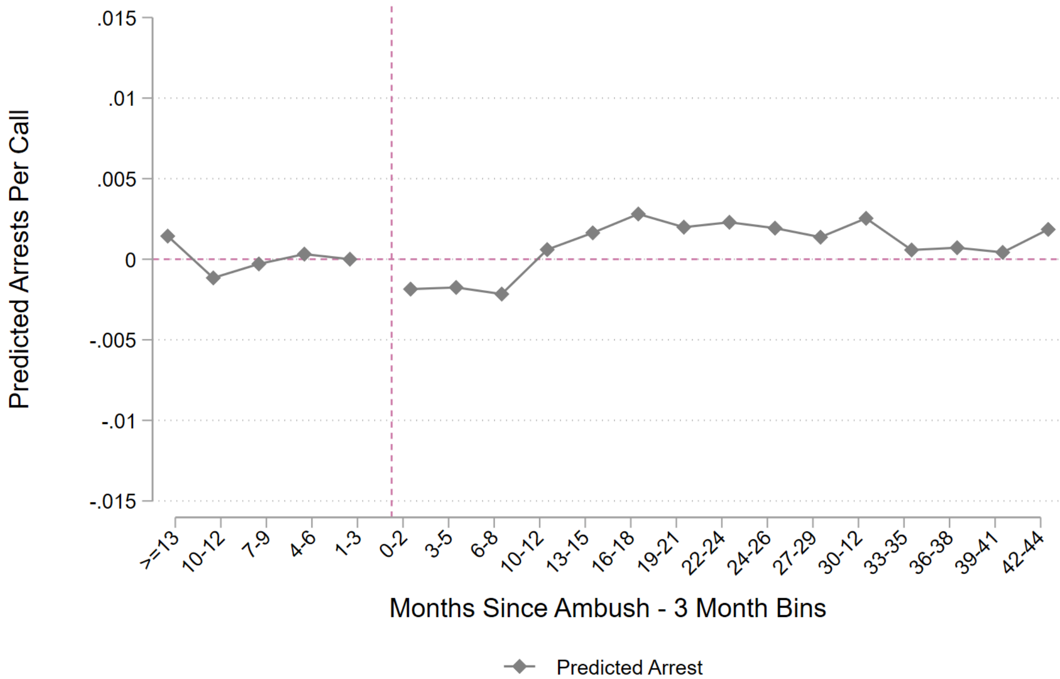
Notes: Each cell shows results for a separate regression. Each Panel shows results for a different dependent variable and the key independent variable is an indicator for policy enactment. Robust standard errors are in parentheses. All specifications control for the distance from policy enactment. The optimal (MSE) bandwidth is used to determine the sample for each separate regression. We compute the FDR-adjusted q-values using the method proposed by Anderson (2008b), adjusting for our five different outcomes.

Figure A.16: Indianapolis Police Beats



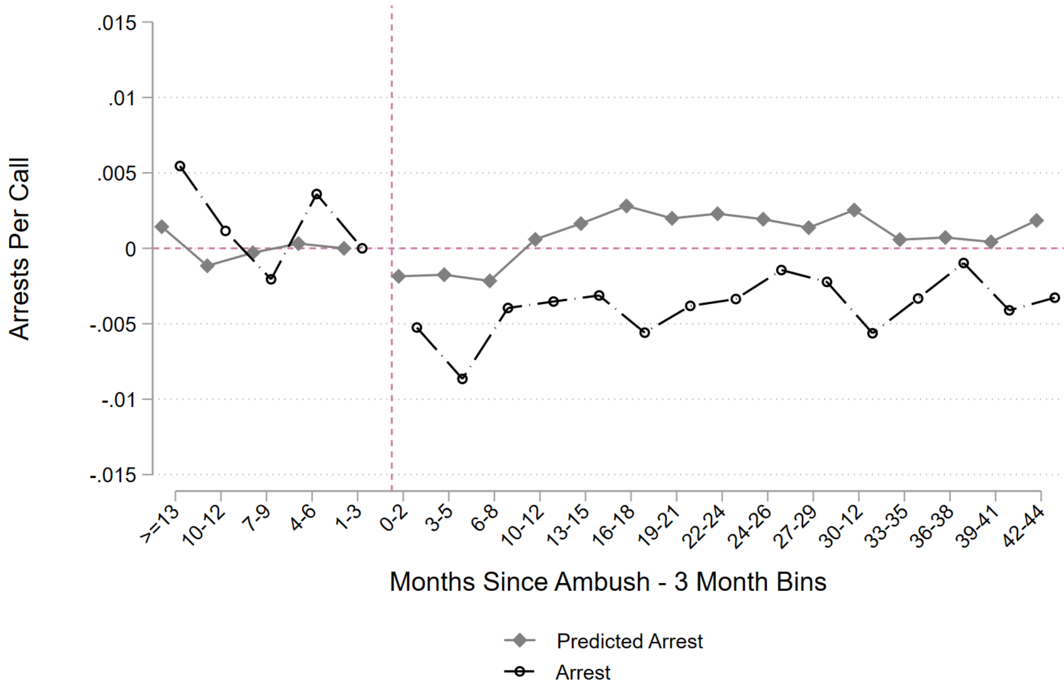
Notes: This figure shows a map of police beats (zones) in Indianapolis and was created by the Indianapolis Police Department.

Figure A.17: The Effect of Ambushes on Predicted Arrests



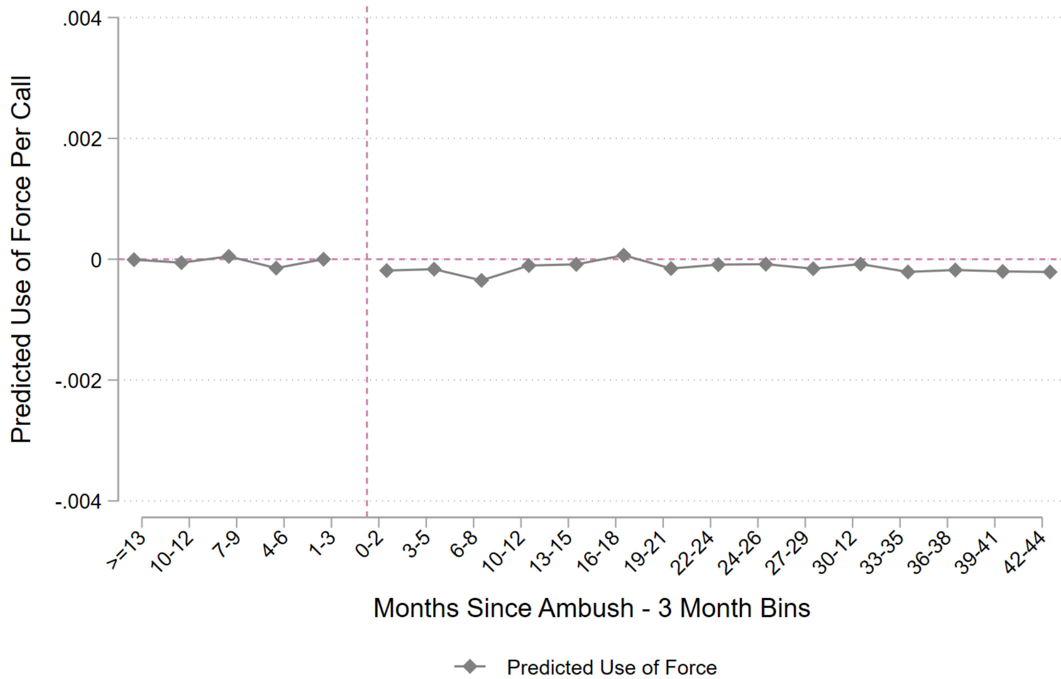
Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes individual police officer, year-x-month, and beat fixed effects. Arrest is predicted using observable call characteristics (latitude, longitude, time dispatched, call priority and call descriptions). Predicted arrest is measured at the call level.

Figure A.18: The Effect of Ambushes on Predicted and Real Arrests



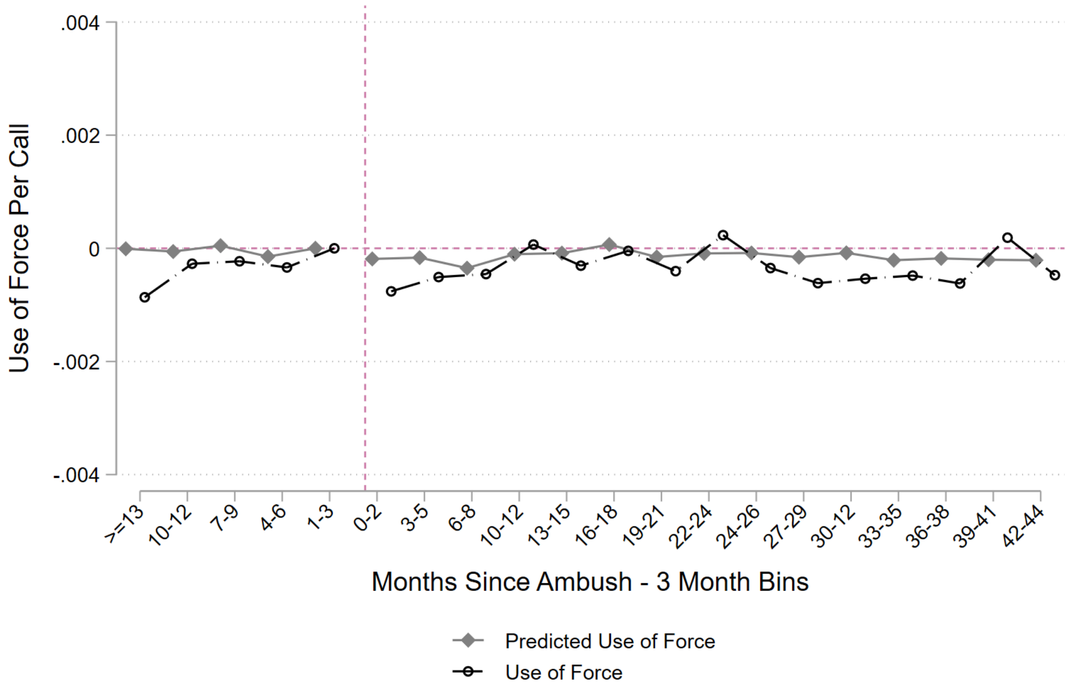
Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes police officer, year-x-month, and beat fixed effects. Results for predicted arrest and observed arrest are shown. Arrest and predicted arrest are measured at the call level.

Figure A.19: The Effect of Ambushes on Predicted Use of Force



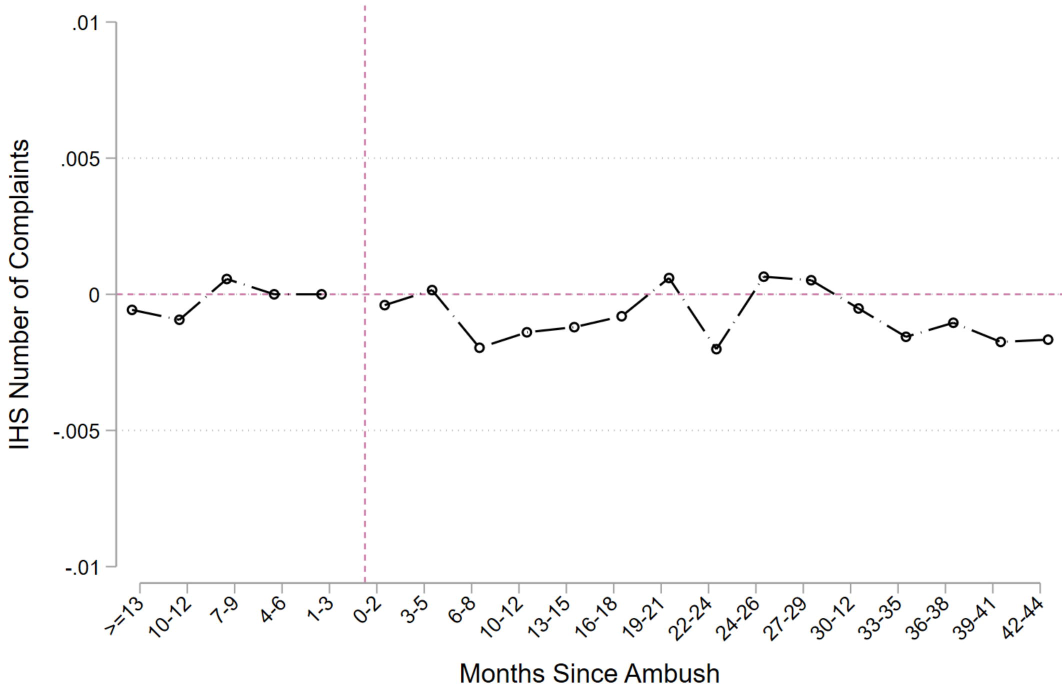
Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes individual, year-x-month, and beat fixed effects. Use of Force is predicted using observable call characteristics (latitude, longitude, time dispatched, call priority and call descriptions). Predicted use of force is measured at the call level.

Figure A.20: The Effect of Ambushes on Predicted and Real Use of Force



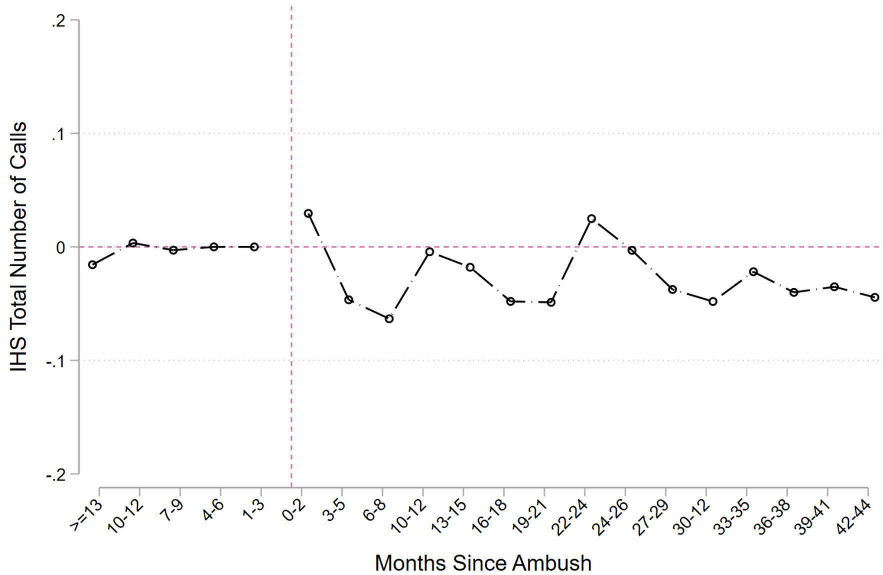
Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes police officer, year-x-month, and beat fixed effects. Results for predicted use of force and observed use of force are shown. Use of Force and predicted use of force are measured at the call level.

Figure A.21: The Effect of Ambushes on Civilian Complaints

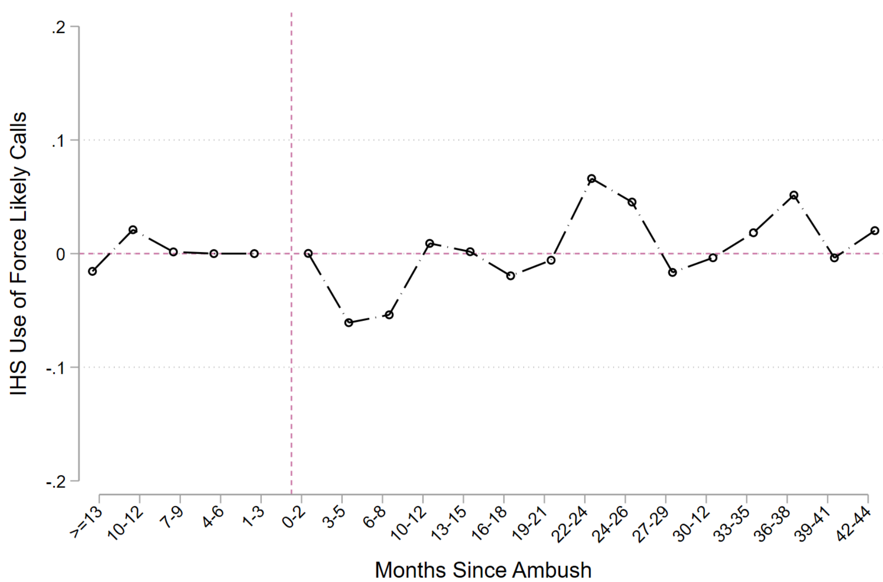


Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes year-x-month and beat fixed effects. The inverse hyperbolic sine of the number of civilian complaints is measured at the beat-day-hour level. The average number of complaints per beat-day-hour is 0.01 (or 2.3 complaints per beat per week).

Figure A.22: The Effect of Ambushes on Number of Calls

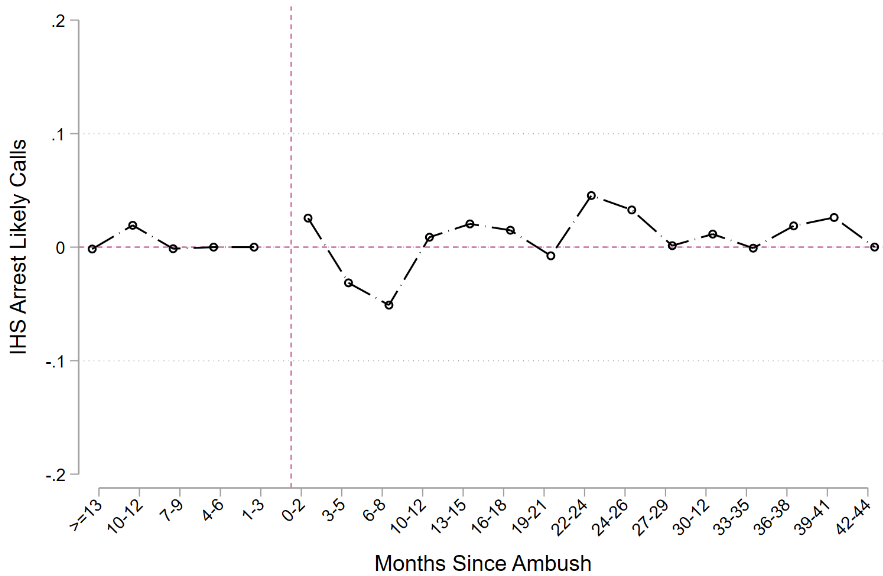


(a) Total Number of Calls



(b) Arrest Likely Calls

Figure A.22: Continued



(c) Use of Force Likely Calls

Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes year-x-month and beat fixed effects. The inverse hyperbolic sine of the number of calls is measured at the beat-day-hour level. The average number of calls per beat-day-hour is 6 (or 978 calls per beat per week). Arrest or Use of Force likely calls are calls with calls types that are in the top quartile of arrest or use of force likelihood, respectively.

Table A.11: Summary Statistics

	Full Sample	Ambushed Beats	Un-ambushed Beats
Panel A: Call Level			
Arrest	0.0707	0.0760	0.0697
Use of Force	0.0056	0.0070	0.0053
X-Coordinate	365.3054	-86.0840	452.1991
Y-Coordinate	3900.7045	39.8056	4643.9372
Priority	1.7777	1.6385	1.8045
Observations	3435382	554500	2880882
Panel B: Beat-by-Day-by-Hour Level			
Civilian Complaints	0.0133	0.0090	0.0140
Calls	5.8483	7.1335	5.6581
Observations	1092870	140847	952023

Notes: Data are from Indianapolis calls for service from 2014-2017.

Table A.12: The Effect of Ambushes on Arrests

	Arrest	Arrest	Arrest	Arrest	Arrest
After Ambush	-0.00587***	-0.00594***	-0.00588***	-0.00549**	
	(0.00123)	(0.00108)	(0.00104)	(0.00201)	
0-5 Months After Ambush					-0.00669***
					(0.00169)
>5 Months After Ambush					-0.00569***
					(0.00101)
Observations	3415078	3415078	3415078	3415078	3415078
Outcome Mean	0.0707	0.0707	0.0707	0.0707	0.0707
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

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

Notes: This table presents results from the regression of $Arrest$ on beat specific indicators and an indicator treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level.

Table A.13: The Effect of Ambushes on Use of Force

	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
After Ambush	-0.0000544	0.0000453	-0.0000707	0.00000112	
	(0.000155)	(0.000158)	(0.000135)	(0.000260)	
0-5 Months After Ambush					-0.000375
					(0.000238)
>5 Months After Ambush					-0.00000116
					(0.000126)
Observations	3415078	3415078	3415078	3415078	3415078
Outcome Mean	0.00559	0.00559	0.00559	0.00559	0.00559
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

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

Notes: This table presents results from the regression of $Use\ of\ Force$ on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level.

Table A.14: The Effect of Ambushes on Civilian Complaints

	IHS Number of Complaints	IHS Number of Complaints
After Ambush	-0.000541	
	(0.000362)	
0-5 Months After Ambush		0.0000896
		(0.000737)
>5 Months After Ambush		-0.000676*
		(0.000355)
Observations	1073724	1073724

Standard errors in parentheses

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

Notes: This table presents results from the regression of the inverse hyperbolic sine of the number of complaints on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes year-x-month fixed effects. Column 2 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. The average number of complaints per beat-day-hour is 0.01 (or 2.3 complaints per beat per week).

Table A.15: The Effect of Ambushes on Number of Calls

	IHS Number of Calls	IHS Number of Calls
Panel A: All Calls		
After Ambush	-0.0208	
	(0.0200)	
0-5 Months After Ambush		-0.00353
		(0.0199)
>5 Months After Ambush		-0.0245
		(0.0215)
Observations	1073724	1073724
Panel B: Arrest Likely Calls		
After Ambush	-0.000660	
	(0.0185)	
0-5 Months After Ambush		-0.0303
		(0.0189)
>5 Months After Ambush		0.00572
		(0.0233)
Observations	1073724	1073724
Panel C: Use of Force Likely Calls		
After Ambush	0.00376	
	(0.0181)	
0-5 Months After Ambush		-0.00541
		(0.0175)
>5 Months After Ambush		0.00574
		(0.0201)
Observations	1073724	1073724
Beat FE, Year-x-Month FE	Y	Y

Standard errors in parentheses

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

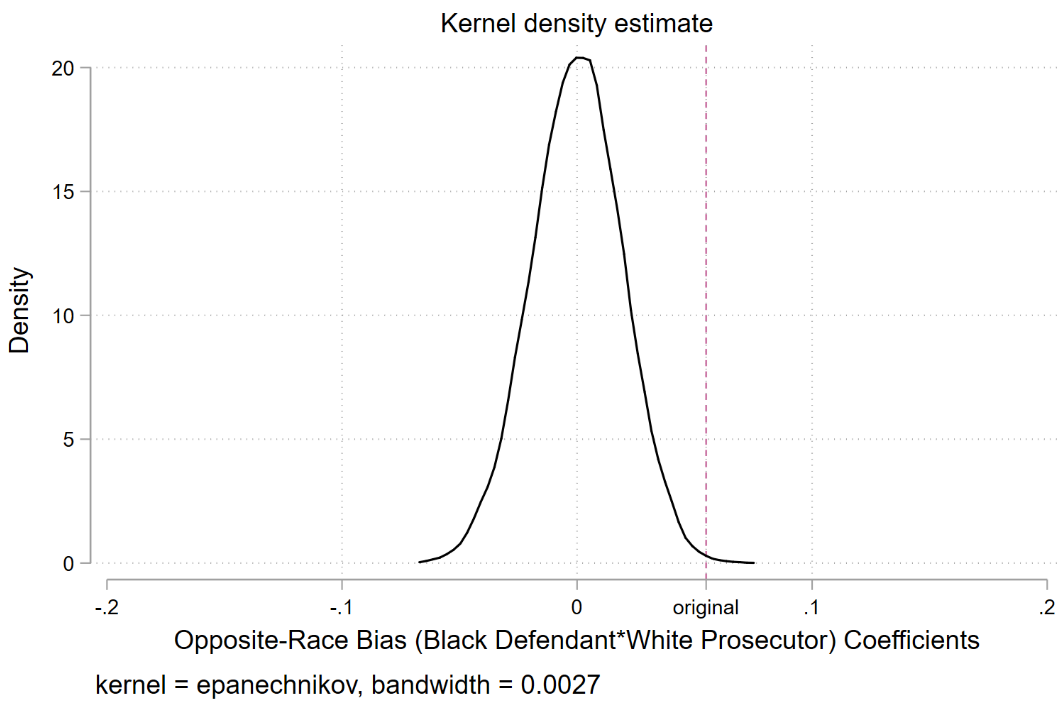
Notes: This table presents results from the regression of the inverse hyperbolic sine of the number of calls on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes year-x-month fixed effects. Column 2 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. The average number of calls per beat-day-hour is 6 (or 978 calls per beat per week).

APPENDIX B

APPENDIX CHAPTER 1

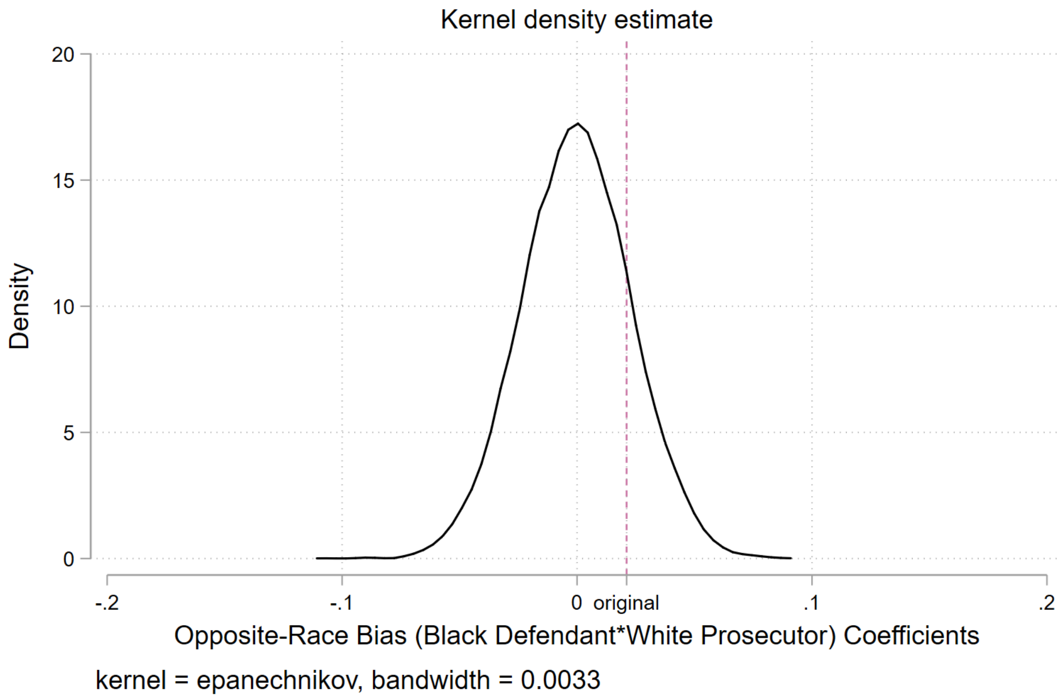
B.1 Figures and Tables

Figure B.1: Permutation Results for Opposite-Race Bias by Crime Type

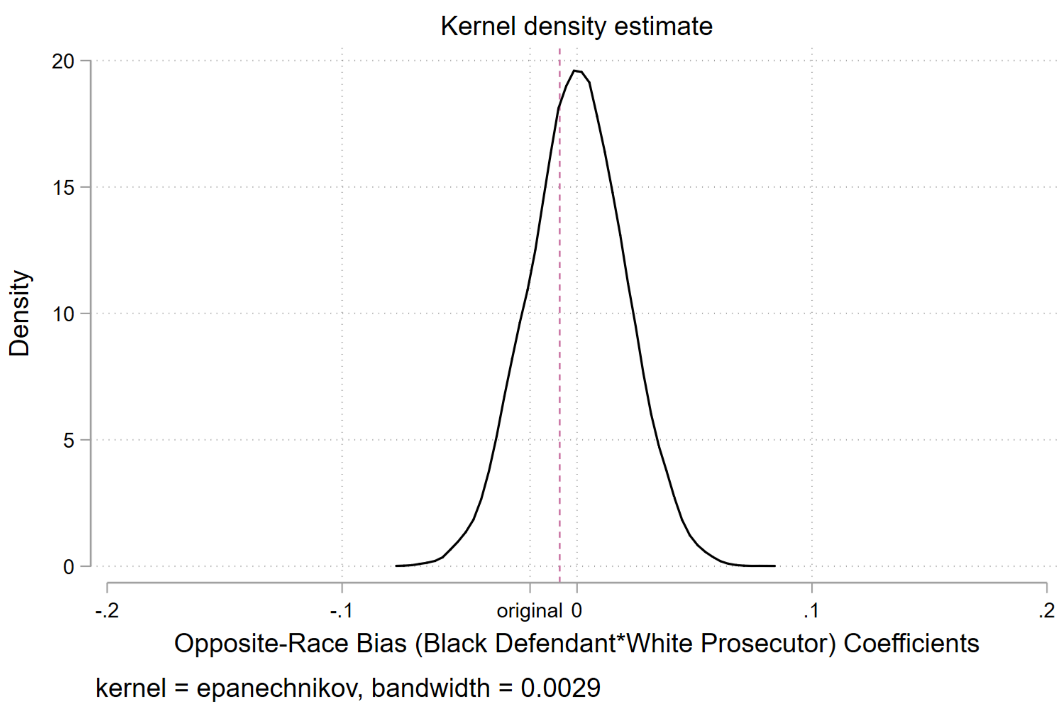


(a) Property Crimes

Figure B.1: Continued

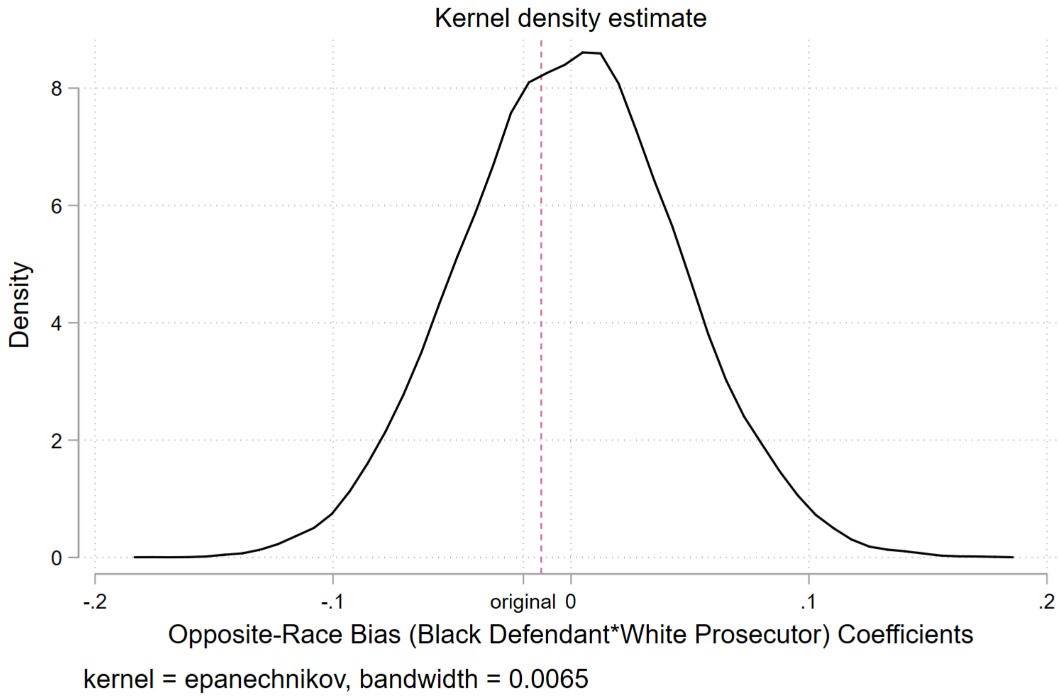


(b) Drug Crimes



(c) Other Crimes

Figure B.1: Continued



(d) Person Crimes

Notes: This figure presents the results from a permutation exercise where I randomly reassigned defendant race and estimated the effect of an opposite-race prosecutor (β_3 from Equation (2.1)) for 10,000 replications. The distribution of β_3 coefficients for each crime type are presented. The dashed line denotes the original estimate from column 1 of Table A.4.

Table B.1: The Effect of Prosecutor Race on Defendant Guilt

	(1) Guilty	(2) Guilty	(3) Guilty	(4) Guilty
Panel A: Entire Sample				
Black Defendant	0.0933***	0.0694***	0.0693***	0.0688***
	(0.00513)	(0.00472)	(0.00472)	(0.00473)
White Prosecutor	0.0208*	0.0176*		
	(0.0118)	(0.0102)		
Observations	87,461	87,461	87,461	87,461
Outcome Mean	0.579	0.579	0.579	0.579
Panel B: Property Crimes				
Black Defendant	0.122***	0.0969***	0.0968***	0.0966***
	(0.00641)	(0.00632)	(0.00631)	(0.00623)
White Prosecutor	0.0273**	0.0187*		
	(0.0125)	(0.0103)		
Observations	32,959	32,959	32,959	32,959
Outcome Mean	0.607	0.607	0.607	0.607
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	N	Y	Y	Y
Prosecutor FE	N	N	N	Y
Interactions	N	N	Y	N

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficient on $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Guilty$ on an indicator for prosecutor race and defendant race. Each specification includes screening date fixed effects. Column 2 adds controls for defendant race, age, date of birth, gender, number of arrest charges, number of arrest counts, number of prior arrests, number of prior felony arrests, number of prior convictions, number of prior felony convictions, number of prior jail sentences, number of prior incarcerations, number of prior non-incarceration sentences, misdemeanor type, drug crime, property crime, person crime, arrest zipcode, and gender of the prosecutor. Robust standard errors are clustered at the prosecutor level.

Table B.2: Missing Values for Property Crimes

	Original Estimate	Missing Controls	Missing Crime Type		Missing Prosecutor Race		Missing Defendant Race					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
				Average							Average	
				[95% CI]							[95% CI]	
Outcome: Guilty												
Black Def*White Pros.	0.0547***	0.0545***	0.0592***	0.0590	0.0542***	0.0542***	0.0569***	0.0509***	0.0549***	0.0564***	0.0554	0.0549
	(0.0144)	(0.0144)	(0.0155)	[0.0547,0.0598]	(0.0145)	(0.0145)	(0.0147)	(0.0141)	(0.0145)	(0.0146)	[0.0501,0.0605]	[0.0498,0.0598]
Observations	32,959	32,991	34,330	34,330	32,959	32,959	33,385	33,385	33,385	33,385	32,959	32,959
Outcome Mean	0.607	0.607	0.583	0.583	0.607	0.607	0.606	0.606	0.606	0.606	0.607	0.607
Pros. & Def. Race Ind	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Prosecutor FE	N	N	N	N	N	N	N	N	N	N	N	N
Interactions	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Missing Control Indicators	N	Y	N	Y	N	N	N	N	N	N	N	N
Missing Defendant Race	-	-	-	-	-	-	Black	White	0.5 Black	0.79 Black	-	-
Missing Prosecutor Race	-	-	-	-	White	Black	-	-	-	-	White	White

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficient on the interaction of $Black\ Defendant$ and $White\ Prosecutor$ from the regression of $Guilty$ on indicators for prosecutor race, defendant race, and the interaction term. All specifications include screening date fixed effects. Each specification also includes controls, and interactions, similar to column 4 in Table A.3 and A.4. Column 1 repeats the estimate for Table A.4 panel D, column 4. Standard errors are clustered at the prosecutor level. Column 2 includes indicators for missing defendant characteristics. Column 3 replaces all missing crime types as property crimes, and column 4 presents the average and 95 percent confidence interval from 10,000 iterations of randomly replacing crime type. Columns 5–6 replace missing prosecutor race as white or black respectively. Columns 7–10 replace missing defendant race as black, white, 0.5 black or 0.79 black (sample mean). Columns 11–12 present the average and 95 percent confidence intervals from 10,000 iterations of randomly replacing defendant race.

B.1.1 Missing Values

As described earlier, one limitation of the data is that I do not observe certain covariates for every case. In particular, defendant age, gender, and race; crime type; and prosecutor race are missing for some observations in my sample. In this section, I show that these minor data limitations do not alter the results of this paper.

First, I show that including cases with missing information on defendant age and gender does not change my estimates for Property Crimes. Defendant age and gender are missing for 0.02 percent and 0.2 percent of cases, respectively (17 and 170 cases). Results are shown in Table B.2. Each specification in the table includes screening date fixed effects, case-level controls, and interactions, just as in column 4 of earlier result tables. Column 1 repeats the result for property crimes in Table A.4 for comparison. In column 2, I include dummy variables for missing defendant age and missing defendant gender and interact each of these dummies with prosecutor race. I also replace the values of defendant gender and defendant age with zeros for observations where I do not observe true gender or age. My coefficient on $Black\ Defendant * White\ Prosecutor$ is almost identical in magnitude and is significant at the 1-percent level. This coefficients indicates that missing information for defendant age and gender does not alter my results.

Next, I consider missing crime types. In column 3, I assume all missing crime types are Property Crimes. In column 4, I randomly assign case type based on the probability of property crime in my data (38 percent of cases are property crimes). Then I estimate my result using screening date fixed effects, case-level controls, and interactions. I then repeat this exercise 10,000 times. I present the average coefficient for these iterations and the 2.5th and 97.5th percentiles (95-percent confidence interval). In both columns, my estimate is similar in magnitude. I can also rule out zero in my confidence interval.

Third, I consider missing values of defendant and prosecutor race. Defendant race is missing for 1.6 percent of the sample (887 defendants), and prosecutor race is missing for 1.8 percent of the sample (3 prosecutors and 780 cases). Next, I show my results are robust to various assumptions about missing prosecutor and defendant race. First, I address missing values for prosecutors. Because 777 (99 percent) of the cases with missing values have the same prosecutor, I simply replace prosecutor race with either white or black. In column 5, I replace missing prosecutor race as white and reestimate my results. In column 6, I replace missing prosecutor race as black. Both estimates (0.0542 and 0.0542, respectively) are very similar in magnitude to the original estimate and are statistically significant at the 1-percent level.

In columns 7–12, I make various reasonable assumptions about the race of defendants whose race is missing. In columns 7 and 8, I replace all missing defendant races as black and white, respectively. Next, I replace defendant race as 0.5 black and 0.79 black, the sample average, in columns 9 and 10. These results are, again, very similar in magnitude to my original estimate and are statistically significant at the 1-percent level.

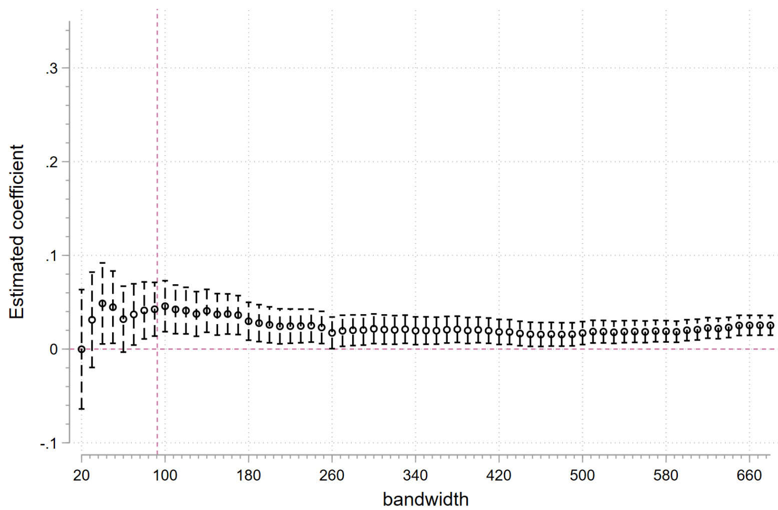
Of course, there are many different combinations of defendant race that could occur beyond the results presented so far in Table B.2. To address these possible scenarios, I conduct a simulation where I randomly replace defendant race based on the distribution of defendant race I observe in my data (79 percent of defendants are black). Specifically, I randomly assign defendant race and estimate my result using arrest category and prior arrest fixed effects, case-level controls, and interactions. I then repeat this exercise 10,000 times. I present the average coefficient for these iterations and the 2.5th and 97.25th percentiles (95-percent confidence interval) in column 11 and 12. I also assume all missing prosecutors are white in column 11 and black in column 12. The average coefficient for both columns (0.0554 and 0.0549 is close to the original estimate, and both confidence intervals do not include zero). These results show that, under reasonable assumptions about which cases have opposite-race pairings of prosecutors and defendants, there is still strong evidence of opposite-race bias for property crimes.

APPENDIX C

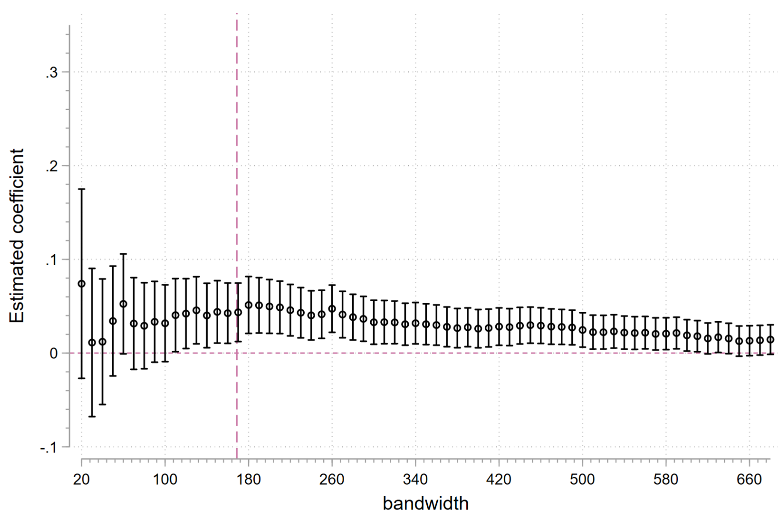
APPENDIX CHAPTER 2

C.1 Figures and Tables

Figure C.1: Non-financial and Pretrial detention Robustness

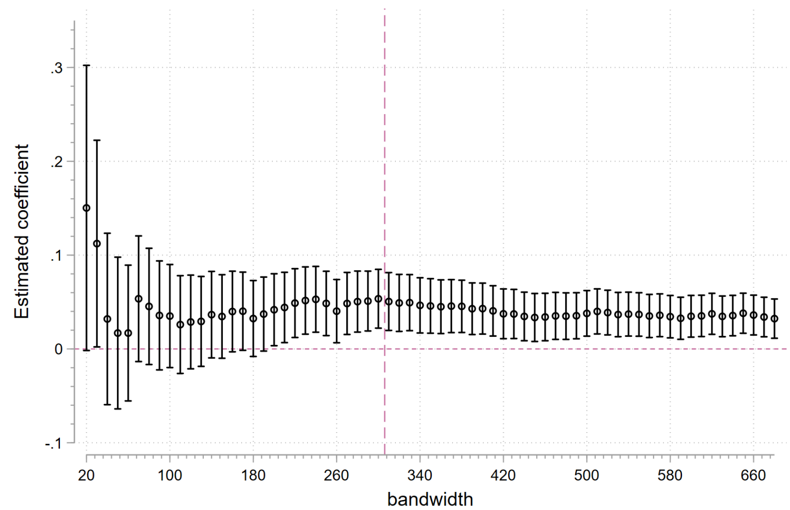


(a) Linear - Non-financial Bond

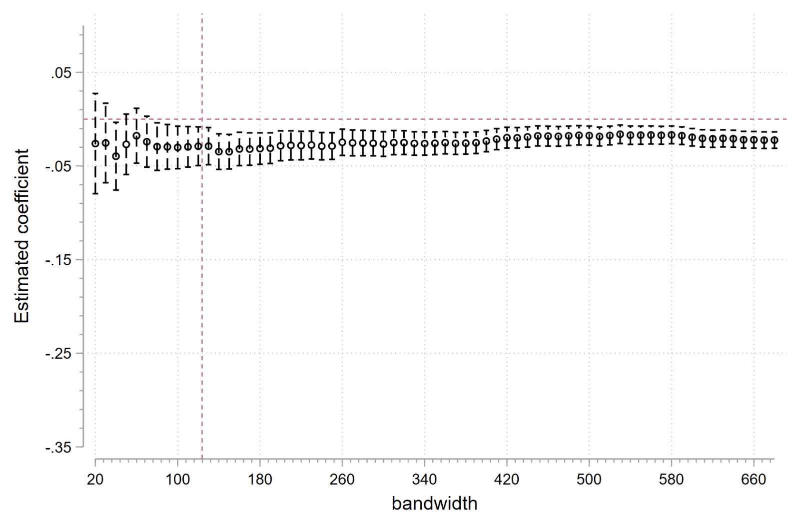


(b) Quadratic - Non-financial bond

Figure C.1: Continued

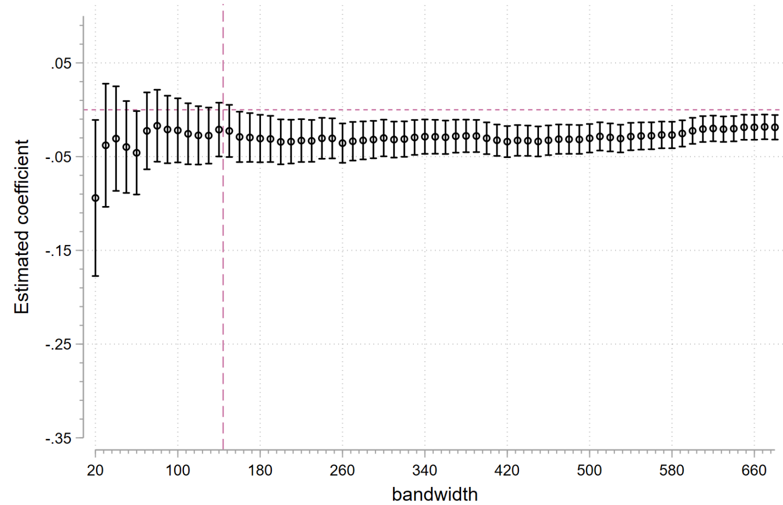


(c) Cubic - Non-financial bond

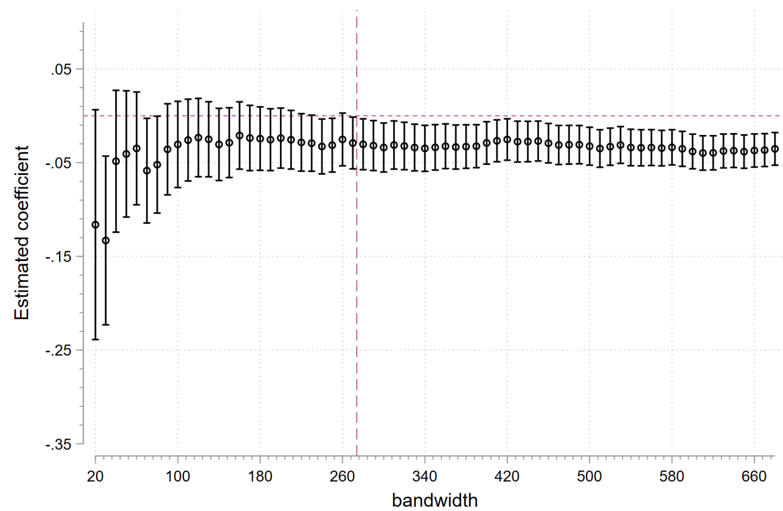


(d) Linear - Pretrial Detention

Figure C.1: Continued



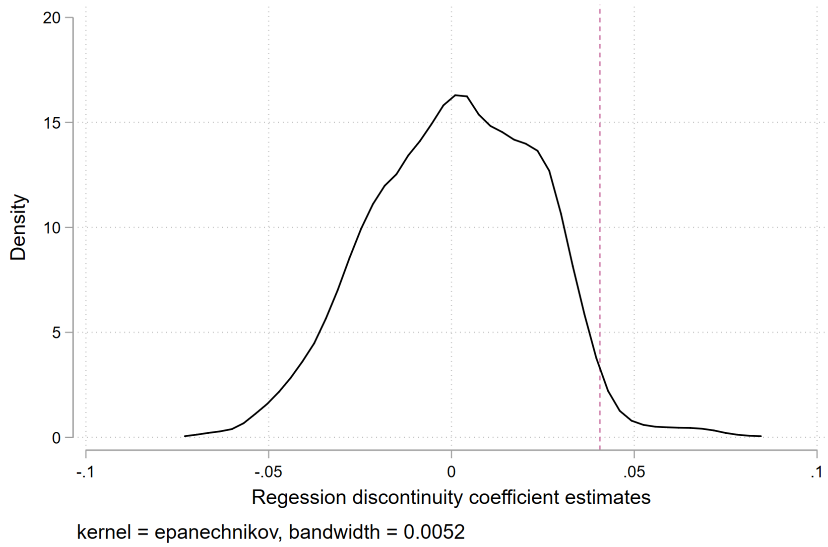
(e) Quadratic - Pretrial Detention



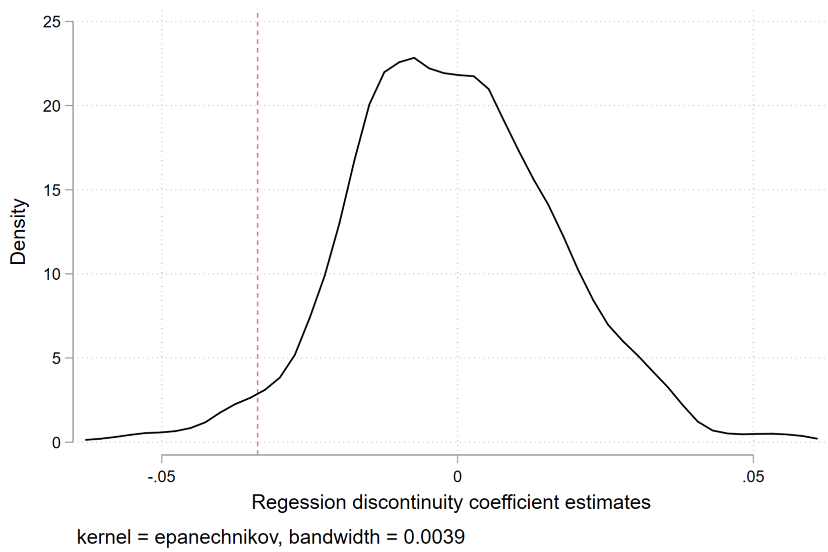
(f) Cubic - Pretrial Detention

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 specification is marked with the dashed line.

Figure C.2: Reassigning Treatment Date



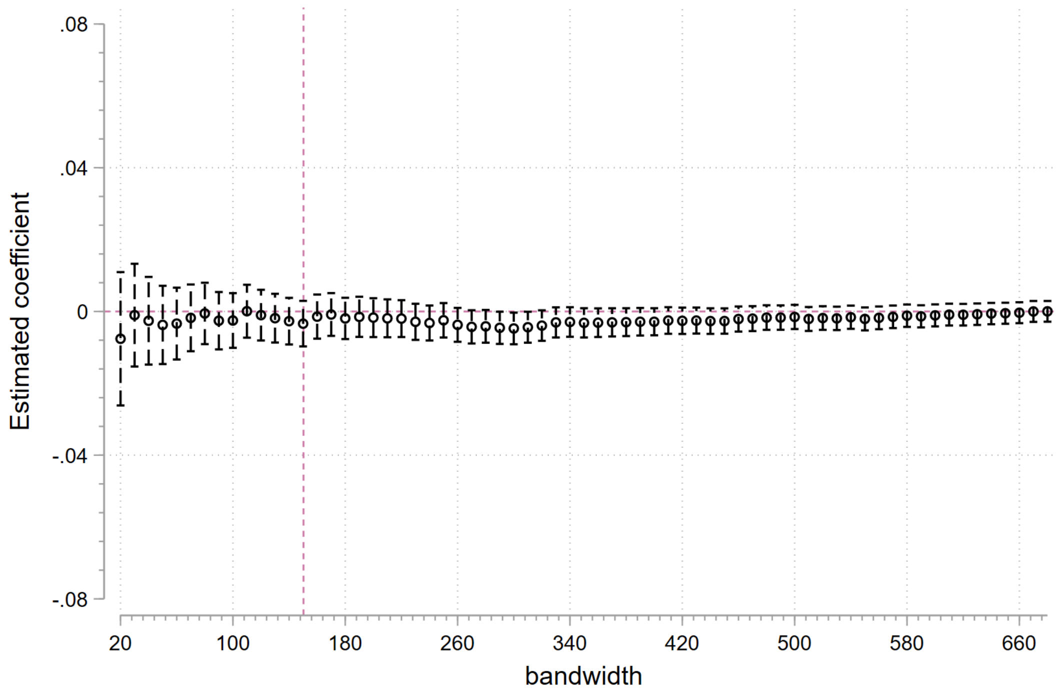
(a) Outcome: Release on Non-financial Bond



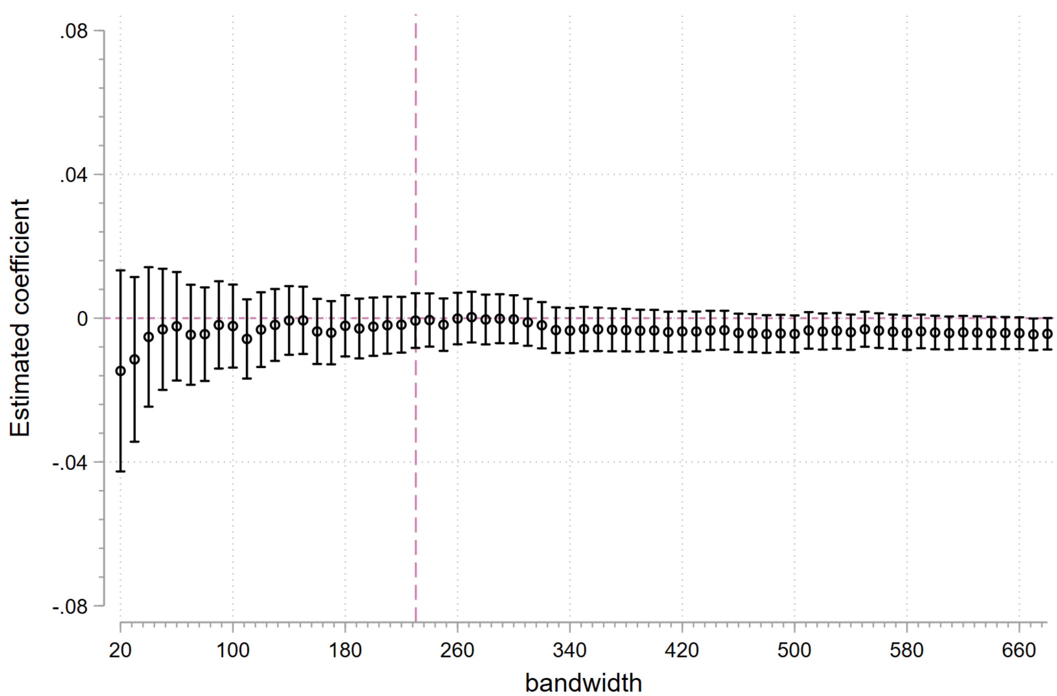
(b) Outcome: Pretrial Detention

Notes: This figure plots the distribution of 910 regression discontinuity coefficients from equation (3.1) using pre-treatment data. Dashed lines are treatment effects from Table C.2. For the probability of release on non-financial bond, our estimate reported in Table C.2 is greater than 97.99 percent of all placebo estimates. For pretrial detention, our estimate reported in Table C.2 is less than 95.88 percent of all placebo estimates.

Figure C.3: Pretrial Crime and Conviction Robustness

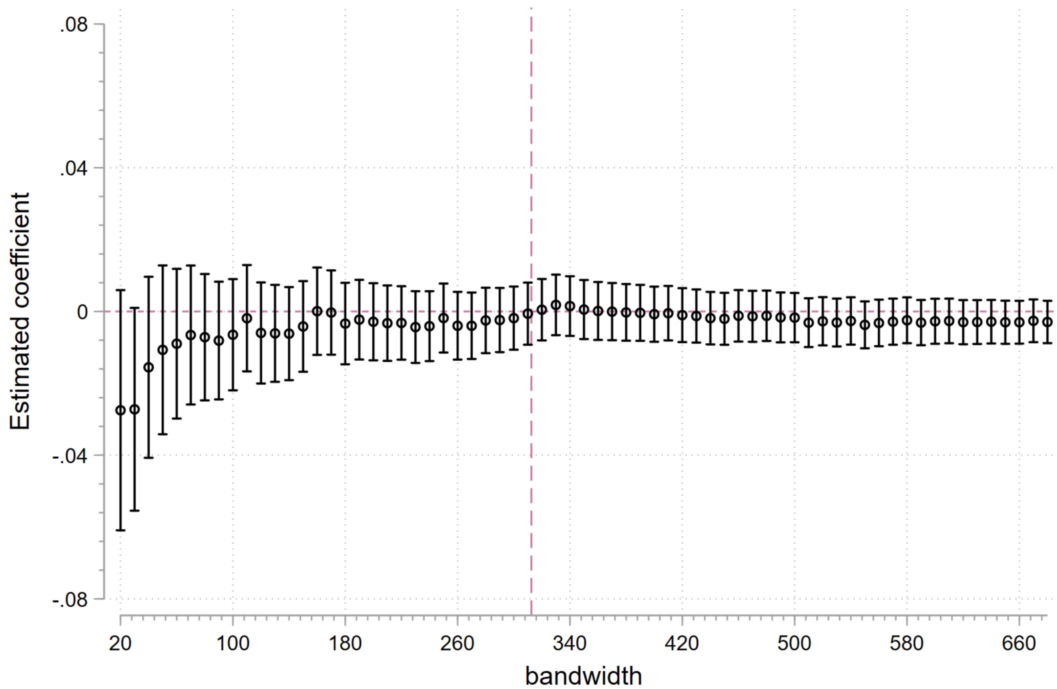


(a) Linear - Violent Pretrial Crime

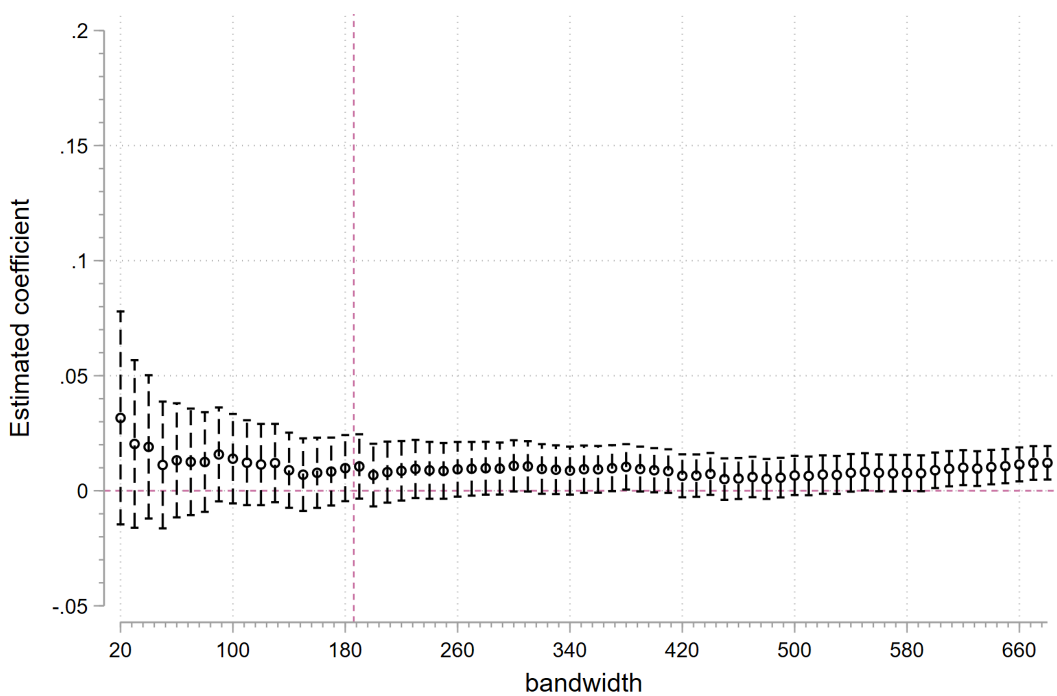


(b) Quadratic - Violent Pretrial Crime

Figure C.3: Continued

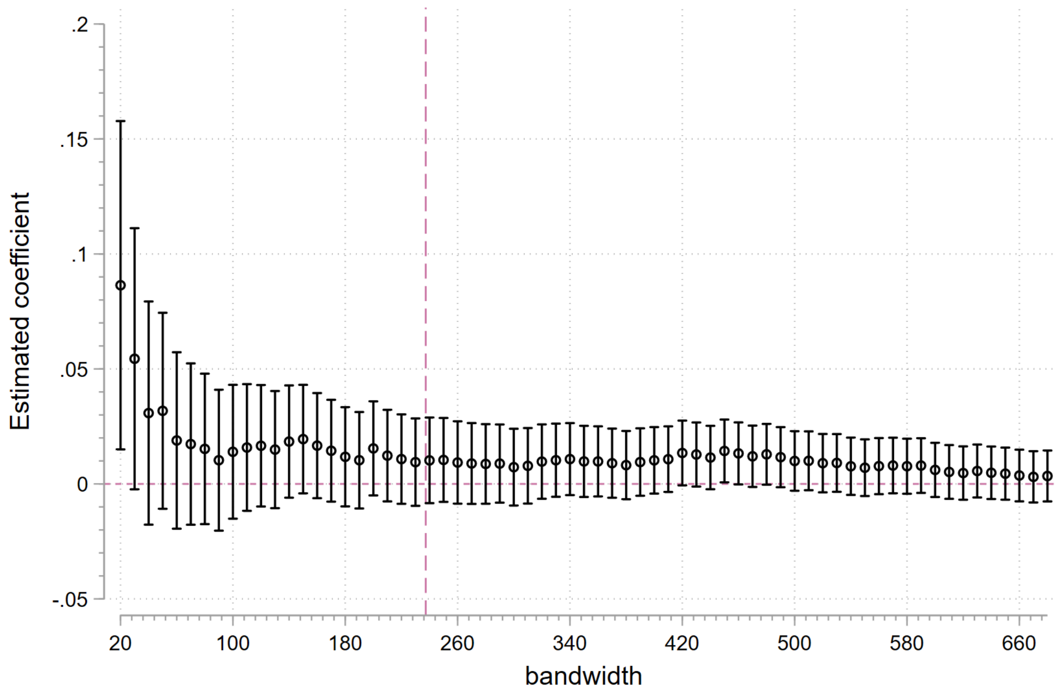


(c) Cubic - Violent Pretrial Crime

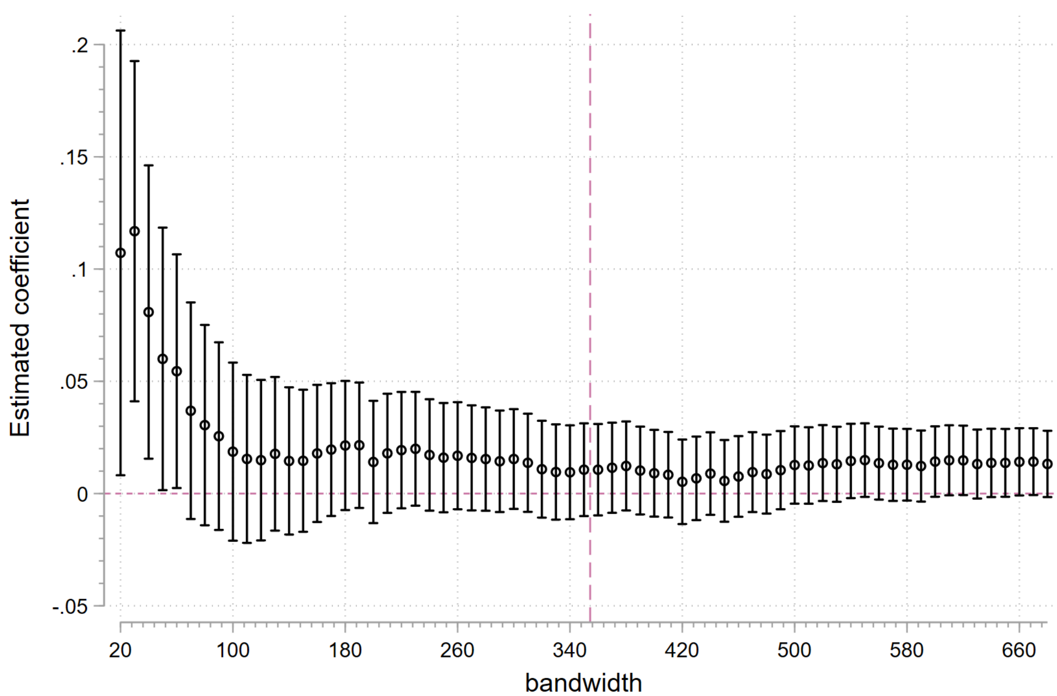


(d) Linear - Non-Violent Pretrial Crime

Figure C.3: Continued

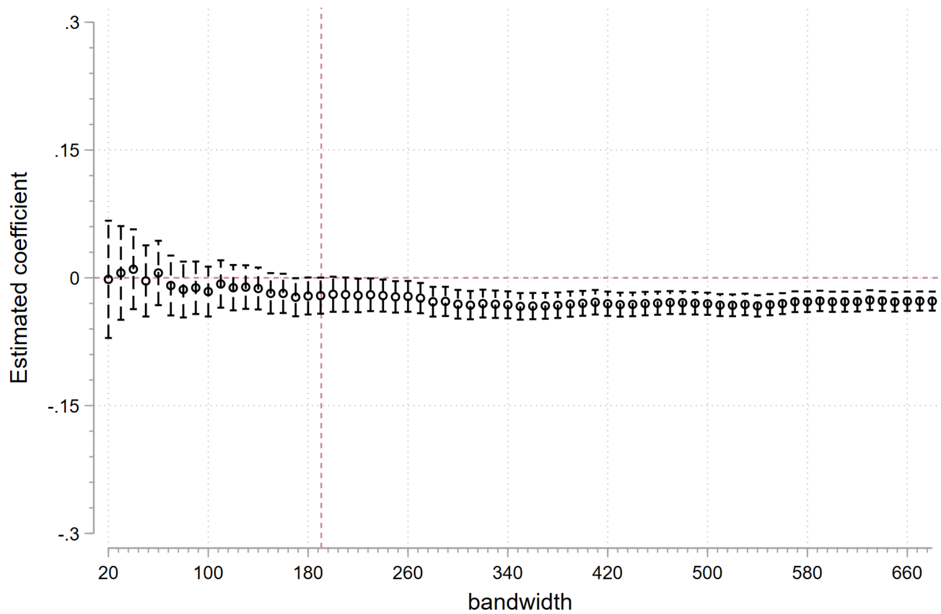


(e) Quadratic - Non-Violent Pretrial Crime

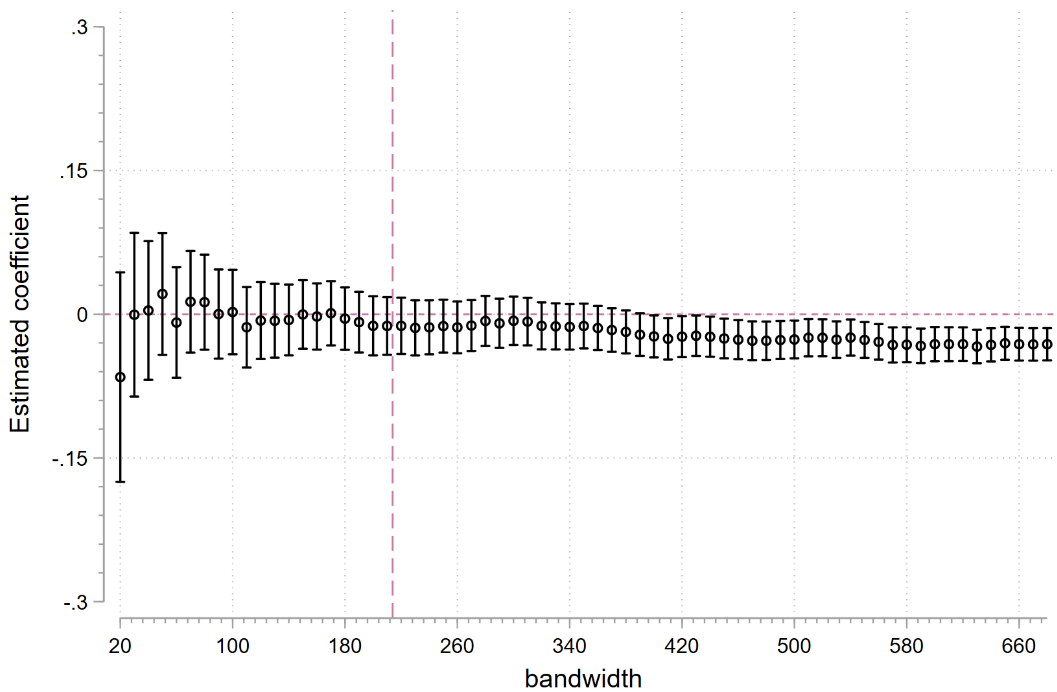


(f) Cubic - Non-Violent Pretrial Crime

Figure C.3: Continued

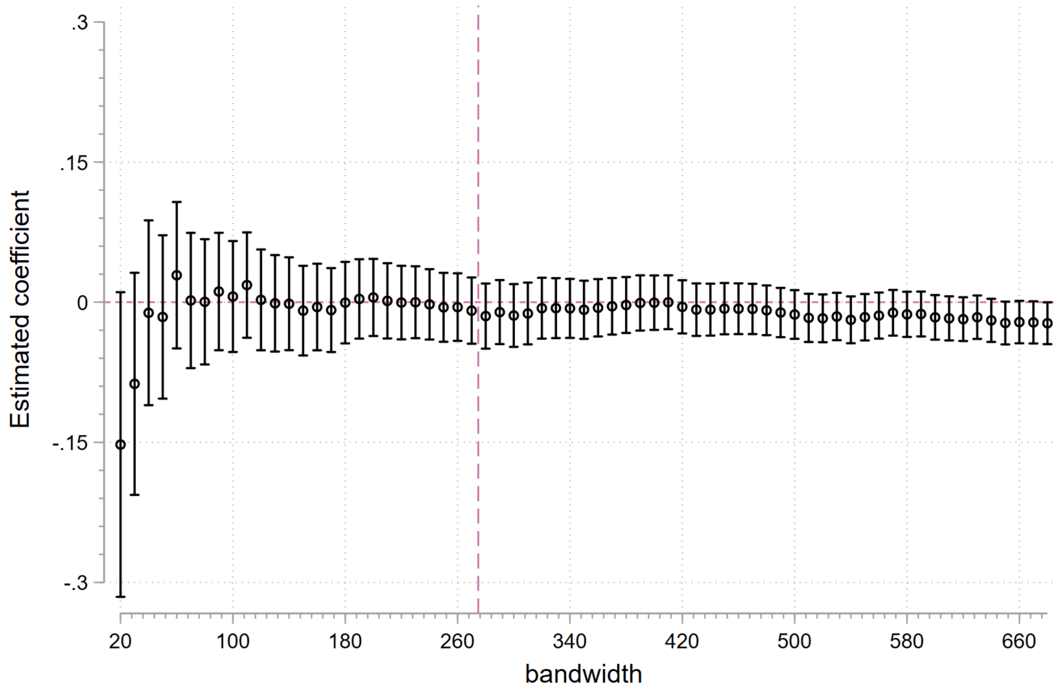


(g) Linear - Convicted



(h) Quadratic - Convicted

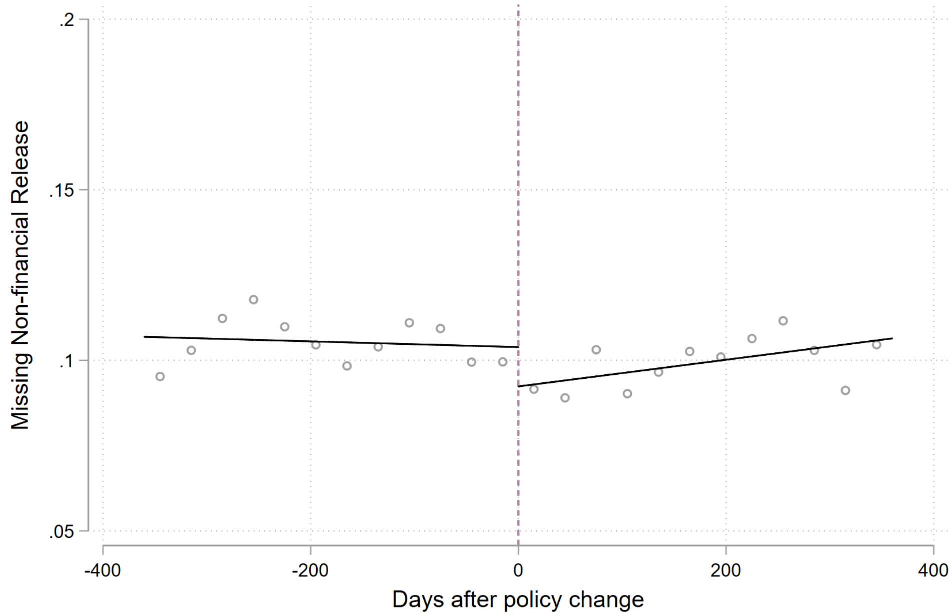
Figure C.3: Continued



(i) Cubic - Convicted

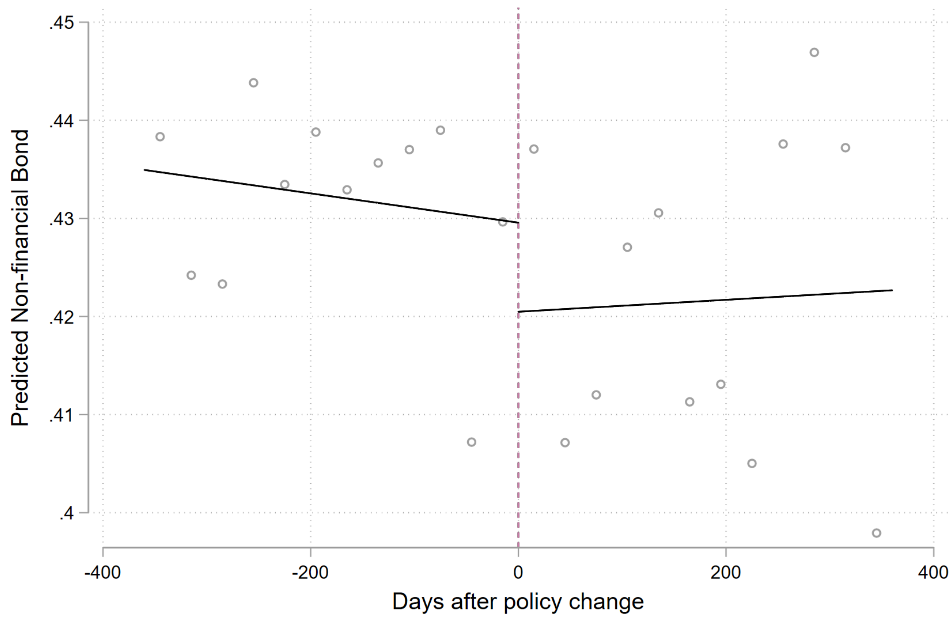
Notes: Each figure plots coefficients from 64 different regressions using different bandwidths. 95 percent confidence intervals are also presented. The optimal MSE bandwidth for each specification is marked with the dash line.

Figure C.4: Regression Discontinuity Results for the Probability of Missing Outcome Data



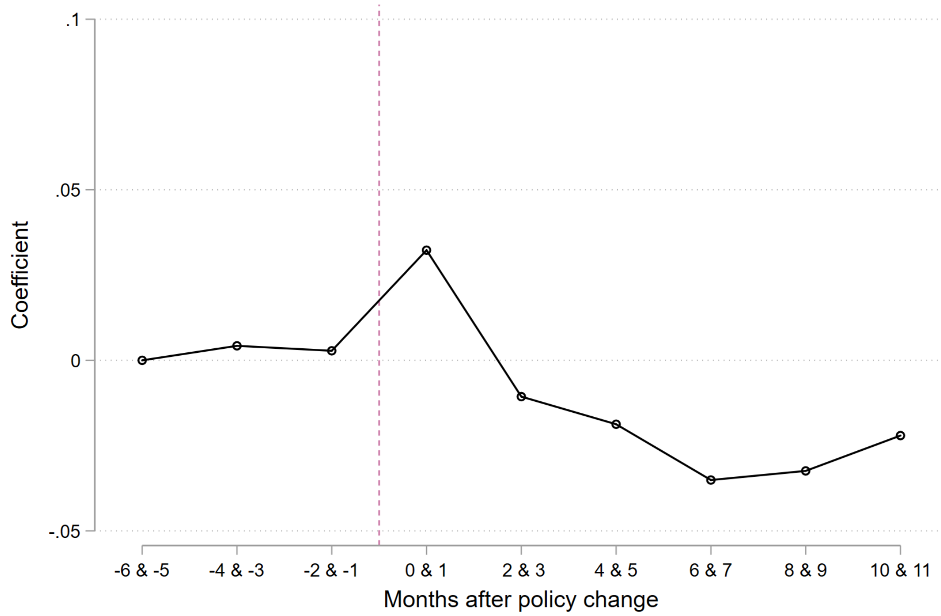
Notes: This figure shows the regression discontinuity estimate of the effect of implementing a risk assessment score policy on the likelihood of missing data by plotting the mean of the probability of missing in 30 day bins with linear fits. A bandwidth of 360 days is shown.

Figure C.5: Regression Discontinuity Results for Predicted Probability of Release on non-financial bond for Defendants with Missing Outcome Data



Notes: This figure plots an additional test of the regression discontinuity design. This graph includes linear fits of 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 C.6: 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 are used. A court-specific time trend is also included.

Table C.1: Tests of the identifying assumption of the RD analysis

	Court 0	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*
	(0.00771)	(0.00697)	(0.00361)	(0.00442)	(0.00418)	(0.00762)	(0.00358)	(0.00411)	(0.00744)	(0.00380)	(0.00823)
Observations	30638	37860	39698	23258	28568	31322	38320	28734	33238	35572	22300
Bandwidth	196.6	238.4	250.0	150.1	184.1	200.7	241.5	185.5	211.4	225.3	144.5
Run. Var. Control	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

	White Defendants	Minority Defendant	Misdemeanor	Def. Age	US Citizen	Male	Indigent	No Priors	Mental Health
RD_Estimate	-0.0114	0.0114	0.0155	0.704**	0.00114	-0.00909	0.00138	-0.00992	0.00425
	(0.0123)	(0.0123)	(0.0116)	(0.336)	(0.00776)	(0.00990)	(0.0141)	(0.0109)	(0.00698)
Observations	25162	25162	24712	16818	25162	29286	19190	25708	34362
Bandwidth	162.4	162.4	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

Standard errors in parentheses

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

Notes: Each cell represents results for separate regressions. 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. Some courts (1,2,13) do not occur enough times in our sample to estimate effects.

Table C.2: Regression Discontinuity Results for Predicted Outcomes

	Optimal Bandwidth	
	(1)	(2)
Outcome: Predicted Non-financial Bail		
RD_Estimate	-0.00153	0.00357
	(0.00673)	(0.00753)
Observations	19604	36288
Bandwidth	128.3	229.3
Outcome: Predicted Pretrial Detention		
RD_Estimate	-0.000287	-0.00345
	(0.00839)	(0.00928)
Observations	19604	37202
Bandwidth	128.2	234.9
Outcome: Predicted Non-Violent Pretrial Crime		
RD_Estimate	0.00113	0.00130
	(0.00152)	(0.00171)
Observations	20440	37556
Bandwidth	133.7	236.5
Outcome: Predicted Violent Pretrial Crime		
RD_Estimate	-0.00101	-0.00117
	(0.000630)	(0.000722)
Observations	23070	41080
Bandwidth	149.7	258.1
Outcome: Predicted Conviction		
RD_Estimate	0.00376	0.00627
	(0.00487)	(0.00578)
Observations	20266	33238
Bandwidth	132.9	211.3
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 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, and 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 C.3: Release 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	0.0313*	0.0400*	0.0538***	0.0544**	0.0604**	0.0640**
	(0.0172)	(0.0206)	(0.0199)	(0.0237)	(0.0245)	(0.0292)
Observations	12794	20578	9438	15534	6120	9932
Bandwidth	195.1	309.7	146.3	232.3	97.54	154.9
Outcome: Pretrial Detention						
RD_Estimate	-0.0428***	-0.0418**	-0.0383**	-0.0480**	-0.0466**	-0.0514*
	(0.0136)	(0.0191)	(0.0158)	(0.0220)	(0.0194)	(0.0271)
Observations	20802	23886	15106	17812	9844	11568
Bandwidth	257.2	298.2	192.9	223.6	128.6	149.1
Panel B: Non-Indigent						
Outcome: Non-financial Bail						
RD_Estimate	0.0153	0.0110	0.0202*	0.0198	0.0262*	0.0368**
	(0.0104)	(0.0113)	(0.0118)	(0.0130)	(0.0144)	(0.0158)
Observations	20254	37668	15112	28618	9940	19164
Bandwidth	270.4	511.2	202.8	383.4	135.2	255.6
Outcome: Pretrial Detention						
RD_Estimate	-0.0145*	-0.0206**	-0.0257***	-0.0223**	-0.0145	-0.0165
	(0.00755)	(0.00896)	(0.00863)	(0.0103)	(0.0105)	(0.0126)
Observations	20392	31924	15118	24010	9896	16062
Bandwidth	261.2	412.6	195.9	309.5	130.6	206.3
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 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 C.4: Pretrial Crime 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 Pretrial Crime						
RD_Estimate	0.0202**	0.0217**	0.0142	0.0144	0.0152	0.0182
	(0.00827)	(0.0100)	(0.00962)	(0.0116)	(0.0119)	(0.0141)
Observations	22040	33760	16220	25796	10550	17114
Bandwidth	273.4	431.4	205.1	323.5	136.7	215.7
Outcome: Violent Pretrial Crime						
RD_Estimate	-0.00216	-0.00298	-0.00341	-0.00217	0.000587	-0.00151
	(0.00249)	(0.00297)	(0.00293)	(0.00349)	(0.00360)	(0.00431)
Observations	31534	52318	24042	38294	15784	26038
Bandwidth	400.3	653.9	300.2	490.4	200.2	326.9
Panel B: Non-Indigent						
Outcome: Non-Violent Pretrial Crime						
RD_Estimate	-0.00154	-0.00166	0.000579	0.00599	0.00379	0.00561
	(0.00767)	(0.00900)	(0.00883)	(0.0104)	(0.0107)	(0.0127)
Observations	27180	43276	20392	32626	13534	21884
Bandwidth	348.3	564.4	261.2	423.3	174.1	282.2
Outcome: Violent Pretrial Crime						
RD_Estimate	-0.00632*	-0.00571	-0.00428	-0.00501	-0.00475	-0.00386
	(0.00326)	(0.00411)	(0.00376)	(0.00474)	(0.00466)	(0.00584)
Observations	24540	34648	18580	26308	12300	17586
Bandwidth	316.1	450.9	237.1	338.2	158.0	225.5
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 C.5: Release Regression Discontinuity Results for White and Minority Defendants

	2x Optimal Bandwidth		1.5x Optimal Bandwidth		Optimal Bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: White Defendants						
Outcome: Non-financial Bail						
RD_Estimate	0.0181	0.0254	0.0145	0.0191	0.0286	0.0254
	(0.0126)	(0.0202)	(0.0146)	(0.0164)	(0.0178)	(0.0202)
Observations	15946	13968	11694	20898	7818	13968
Bandwidth	250.5	221.0	187.8	331.5	125.2	221.0
Outcome: Pretrial Detention						
RD_Estimate	-0.0202**	-0.0242*	-0.0234**	-0.0252*	-0.0259*	-0.0291
	(0.00981)	(0.0123)	(0.0112)	(0.0143)	(0.0138)	(0.0177)
Observations	18118	25624	13384	19128	8878	12656
Bandwidth	266.5	378.4	199.8	283.8	133.2	189.2
Panel B: Minority Defendants						
Outcome: Non-financial Bail						
RD_Estimate	0.0316**	0.0409***	0.0540***	0.0609***	0.0526***	0.0470**
	(0.0128)	(0.0154)	(0.0147)	(0.0178)	(0.0181)	(0.0217)
Observations	17240	26618	12610	20178	8240	13130
Bandwidth	221.3	342.6	165.9	257.0	110.6	171.3
Outcome: Pretrial Detention						
RD_Estimate	-0.0336***	-0.0362**	-0.0354***	-0.0389**	-0.0306**	-0.0293
	(0.0102)	(0.0142)	(0.0118)	(0.0163)	(0.0144)	(0.0199)
Observations	22662	26158	16476	19470	10746	12646
Bandwidth	249.2	290.7	186.9	218.0	124.6	145.3
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 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 white and minority defendants respectively.

Table C.6: Pretrial Crime Regression Discontinuity Results for White and Minority Defendants

	2x Optimal Bandwidth		*1.5x Optimal Bandwidth*		*Optimal Bandwidth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: White Defendants						
Outcome: Non-Violent Pretrial Crime						
RD_Estimate	0.00315	-0.00201	0.000673	0.000465	0.00195	-0.00196
	(0.00597)	(0.00864)	(0.00694)	(0.0100)	(0.00856)	(0.0124)
Observations	35164	38926	26790	29422	18052	19820
Bandwidth	530.6	587.5	398.0	440.6	265.3	293.7
Outcome: Violent Pretrial Crime						
RD_Estimate	-0.00362	-0.00231	0.000665	0.0000628	-0.00116	0.000121
	(0.00307)	(0.00367)	(0.00354)	(0.00419)	(0.00431)	(0.00513)
Observations	20734	31998	15640	24604	10390	16460
Bandwidth	306.7	483.0	230.0	362.2	153.4	241.5
Panel B: Minority Defendants						
Outcome: Non-Violent Pretrial Crime						
RD_Estimate	0.0135*	0.0209**	0.0134	0.0196*	0.0160	0.0163
	(0.00737)	(0.00965)	(0.00845)	(0.0111)	(0.0103)	(0.0135)
Observations	30596	39450	23212	30148	15026	20108
Bandwidth	340.2	447.4	255.1	335.6	170.1	223.7
Outcome: Violent Pretrial Crime						
RD_Estimate	-0.00527	-0.00537	-0.00441	-0.00511	-0.00556	-0.00266
	(0.00323)	(0.00344)	(0.00372)	(0.00402)	(0.00466)	(0.00496)
Observations	25950	50834	19398	37754	12526	25630
Bandwidth	288.5	568.3	216.4	426.2	144.3	284.2
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 white and minority defendants respectively.

Table C.7: 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	-0.00533	0.000386	-0.00693	-0.00718	-0.000794	-0.00467	-0.00557	0.00268
	(0.00527)	(0.00839)	(0.00614)	(0.00681)	(0.00703)	(0.00785)	(0.00955)	(0.00957)
Observations	40600	34528	28734	53140	21372	40298	14016	26176
Bandwidth	255.1	219.5	185.0	337.6	138.8	253.2	92.51	168.8
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

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

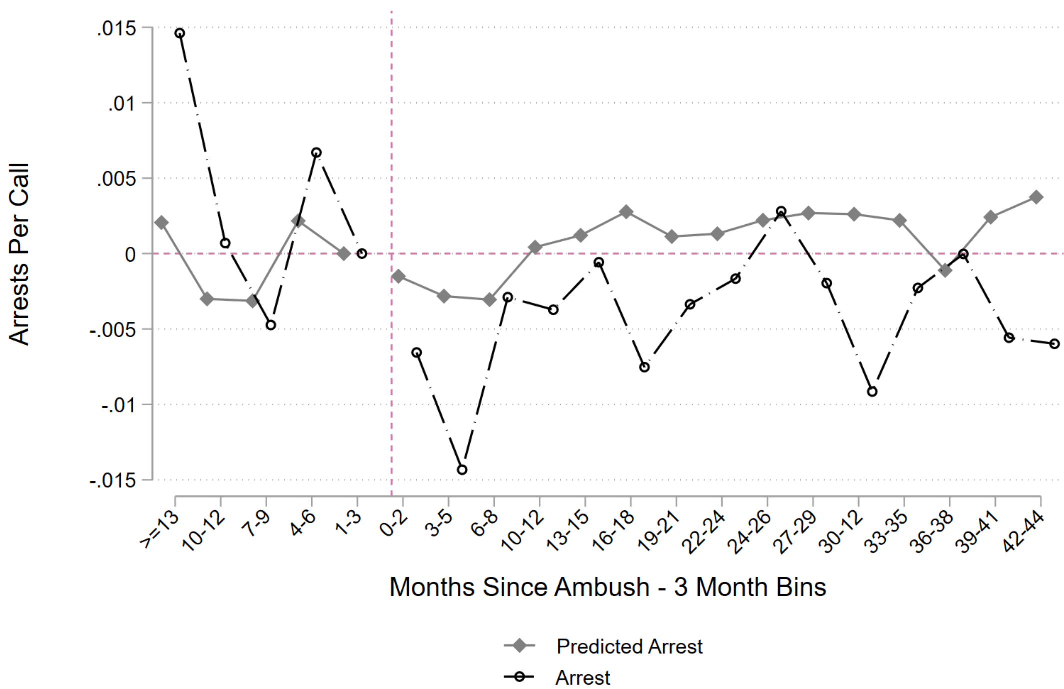
Notes: Each cell represents results for separate regression. Each column presents results for the probability of missing data for the outcome variable and the key independent variable is an indicator for policy enactment. Robust standard errors are in parentheses. All specifications control for a linear 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 A.9. Columns (7)-(8) use the optimal bandwidth for the Probability of Missing Data.

APPENDIX D

APPENDIX CHAPTER 3

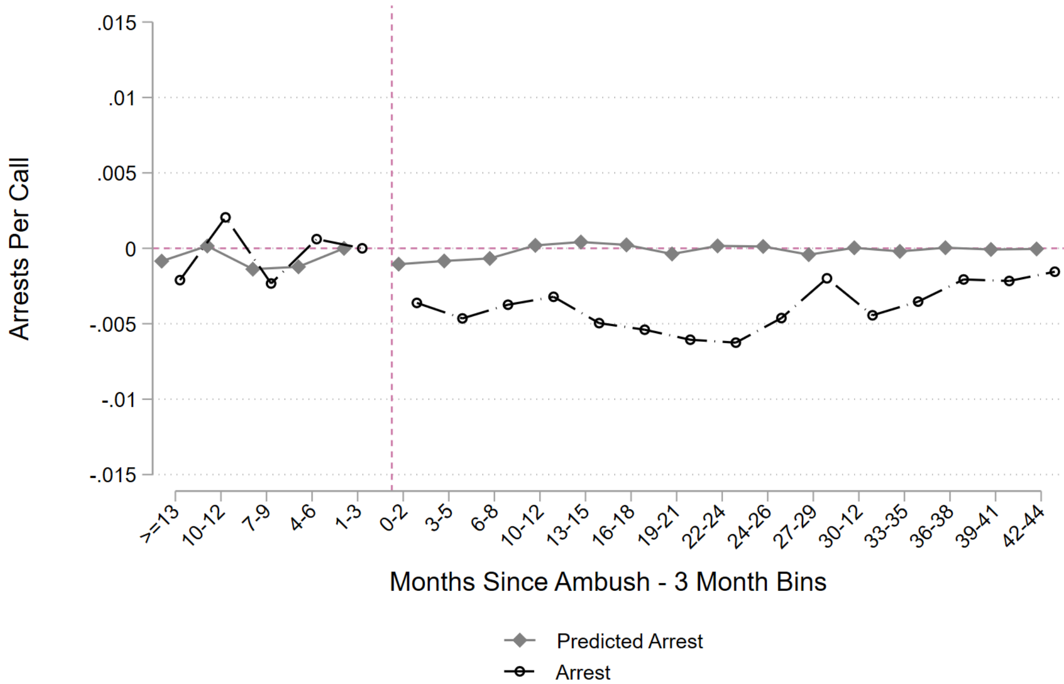
D.1 Figures and Tables

Figure D.1: The Effect of Ambushes on Predicted and Real Arrests



(a) Arrest Likely Calls

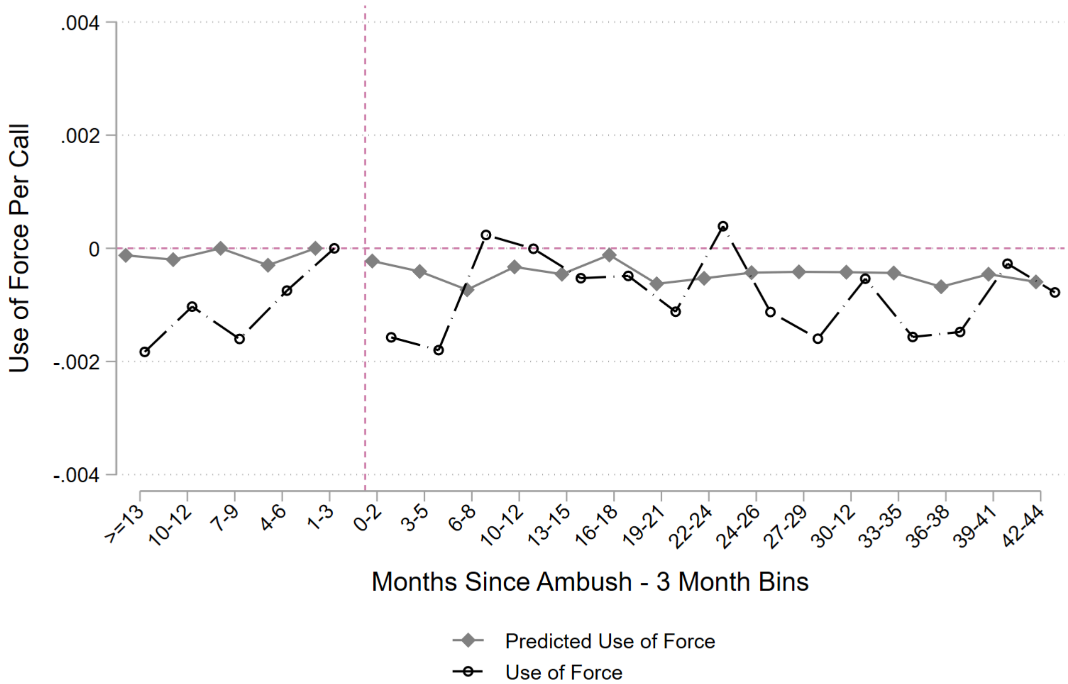
Figure D.1: Continued



(b) Arrest Unlikely Calls

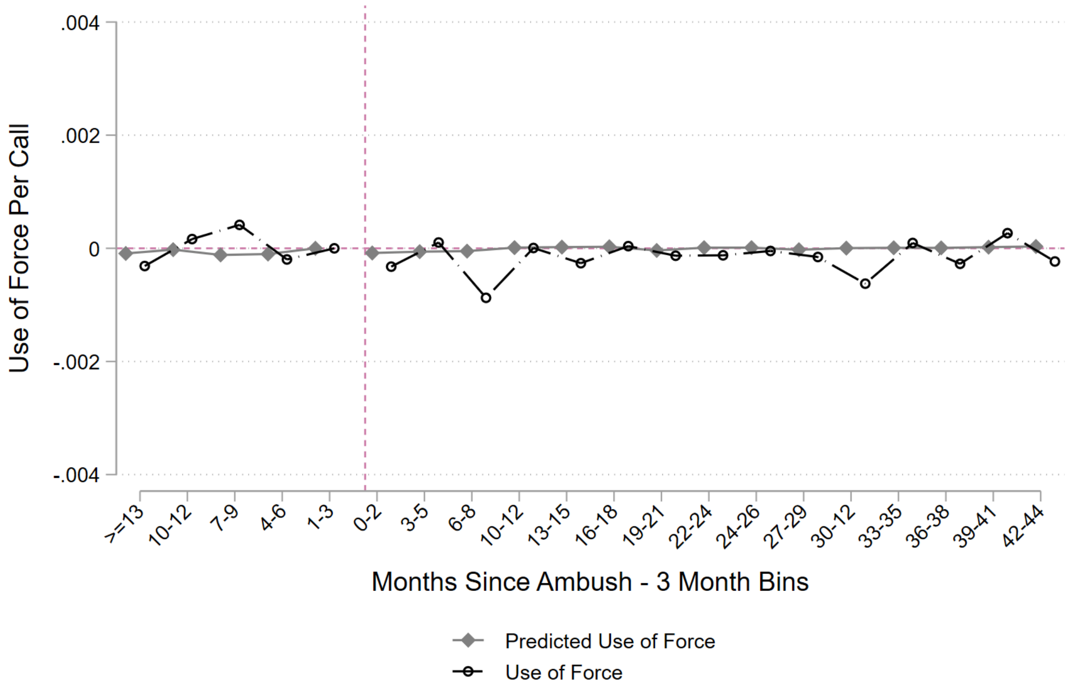
Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes police officer, year-x-month, and beat fixed effects. Results for predicted arrest and observed arrest are shown. Arrest and predicted arrest are measured at the call level. Arrest likely calls are calls with calls types that are in the top quartile of arrest likelihood.

Figure D.2: The Effect of Ambushes on Predicted and Real Use of Force



(a) Use of Force Likely Calls

Figure D.2: Continued



(b) Use of Force Unlikely Calls

Notes: This figure shows dynamic difference-in-difference estimates from Equation (4.2) and includes police officer, year-x-month, and beat fixed effects. Results for predicted use of force and observed use of force are shown. Use of Force and predicted use of force are measured at the call level. Use of Force likely calls are calls with calls types that are in the top quartile of use of force likelihood

Table D.1: The Effect of Ambushes on Arrests for Arrest Likely Calls

	Arrest	Arrest	Arrest	Arrest	Arrest
After Ambush	-0.00806***	-0.00774***	-0.00785***	-0.00750**	
	(0.00246)	(0.00212)	(0.00217)	(0.00277)	
0-5 Months After Ambush					-0.0117***
					(0.00257)
>5 Months After Ambush					-0.00696***
					(0.00236)
Observations	1310451	1310451	1310451	1310451	1310451
Outcome Mean	0.133	0.133	0.133	0.133	0.133
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

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

Notes: This table presents results from the regression of $Arrest$ on beat specific indicators and an indicator treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Arrest likely calls are calls with calls types that are in the top quartile of arrest likelihood.

Table D.2: The Effect of Ambushes on Arrests for Arrest Unlikely Calls

	Arrest	Arrest	Arrest	Arrest	Arrest
After Ambush	-0.00395***	-0.00417***	-0.00414***	-0.00375*	
	(0.000804)	(0.000694)	(0.000584)	(0.00208)	
0-5 Months After Ambush					-0.00334**
					(0.00133)
>5 Months After Ambush					-0.00432***
					(0.000499)
Observations	2104627	2104627	2104627	2104627	2104627
Outcome Mean	0.0320	0.0320	0.0320	0.0320	0.0320
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

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

Notes: This table presents results from the regression of $Arrest$ on beat specific indicators and an indicator treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Arrest unlikely calls are calls with calls types that are not in the top quartile of arrest likelihood.

Table D.3: The Effect of Ambushes on Use of Force for Use of Force Likely Calls

	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
After Ambush	0.0000123	0.000266	0.000233	0.000247	
	(0.000328)	(0.000334)	(0.000260)	(0.000474)	
0-5 Months After Ambush					-0.000679
					(0.000687)
>5 Months After Ambush					0.000448
					(0.000271)
Observations	1162941	1162941	1162941	1162941	1162941
Outcome Mean	0.0113	0.0113	0.0113	0.0113	0.0113
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

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

Notes: This table presents results from the regression of $Use\ of\ Force$ on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Use of Force likely calls are calls with calls types that are in the top quartile of use of force likelihood.

Table D.4: The Effect of Ambushes on Use of Force for Use of Force Unlikely Calls

	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
After Ambush	-0.000154	-0.000166*	-0.000205*	-0.000116	
	(0.0000965)	(0.0000960)	(0.000106)	(0.000211)	
0-5 Months After Ambush					-0.000172
					(0.000220)
>5 Months After Ambush					-0.000213
					(0.000136)
Observations	2252137	2252137	2252137	2252137	2252137
Outcome Mean	0.00267	0.00267	0.00267	0.00267	0.00267
Beat FE, Year-x-Month FE	Y	Y	Y	Y	Y
Individual Officer FE	N	Y	Y	Y	Y
Call Controls	N	Y	Y	Y	Y
Time Varying Controls	N	N	Y	N	N
Beat Linear Time Trend	N	N	Y	Y	N

Standard errors in parentheses

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

Notes: This table presents results from the regression of $Use\ of\ Force$ on beat specific indicators and an indicator for treatment (after an ambush in an ambushed beat). Column 1 includes individual officer and year-x-month fixed effects. Column 2 adds call level controls. Specifically, Column 2 adds controls for the x-coordinate, y-coordinate, and dispatch time of the call. Fixed effects for call priority and call type are also included. Column 3 adds covariate-by-time controls (each characteristics from Column 2 interacted with year-x-month). Column 4 adds a beat specific linear time trend. Column 5 separately estimates short term (0-5 Months) and long term (>5 Months) effects. Standard errors are clustered at the beat level. Use of Force unlikely calls are calls with calls types that are not in the top quartile of use of force likelihood.