Does discretion by elected judges benefit defendants, or judges? Evidence from public defense case assignment

Suhyeon Oh*

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Abstract

There is significant concern regarding whether publicly-funded defense attorneys provide adequate legal assistance, as indigent defendants often face worse case outcomes than those with retained counsel. One potential solution is to allow judges discretion in assigning cases to court-appointed attorneys to incentivize better legal assistance and improve attorney-defendant match quality. On the other hand, such assignments can raise concerns regarding *quid pro quo* in settings where judges are elected and where attorneys, who are paid per case, may donate to judges to secure case assignments. This paper examines this question using data from a large county that began prohibiting discretionary case assignments in 2015. I find that prohibiting discretionary case assignments reduces donations from defense attorneys to judges by 21 percent and lowers guilty conviction rates by 9-13 percent. These results indicate that discretionary assignments create corruption risks while worsening outcomes for low-income defendants.

Keywords: Discretion, criminal justice, law and economics, indigent defense, legal representation, campaign finance, *quid pro quo*

JEL codes: H76, J15, K14, K42

*Texas A&M University; suhyeon.oh@tamu.edu

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1 Introduction

Political and economic institutions face a fundamental tradeoff when deciding how to make decisions and allocate resources. On the one hand, granting decision-makers discretion enables expert judgment and flexibility by those who have better information (Aghion and Tirole, 1997), and it can also be used to incentivize good performance (Bénabou and Tirole, 2003; Suvorov and Van de Ven, 2009). On the other hand, discretion also creates opportunities for bias and corruption (Becker and Stigler, 1974; Rose-Ackerman, 1978). The latter concern is especially important when those affected by discretionary decisions can reciprocate through various channels. While some of this reciprocation may be legal, all of it raises at least the appearance of bias and corruption, which can undermine institutional integrity. These concerns have been expressed across a variety of settings, including for procurement officers selecting contractors (e.g., Decarolis et al. (2025)), politicians who make spending decisions and solicit campaign contributions (e.g., Rose-Ackerman (1999)), and regulators who may subsequently accept positions in the sectors they are regulating (e.g., Blanes i Vidal et al. (2012)). However, despite the pervasiveness of these concerns, there is little evidence on whether the observed correlations are in fact due to reciprocity. For example, even when we observe flows between government (e.g., via contracts) and politicians (via campaign donations), these patterns do not establish that one behavior caused the other. Rather, donations might reflect ideological alignment or attempts to influence decisions.[1] Similarly, there is little evidence on whether, and to what extent, outcomes of those impacted by discretion are improved or worsened by the discretion.

The fundamental challenge in assessing these questions is that researchers rarely observe exogenous variation in discretion that allows for a credible test of whether conferring ben-

[1]Similarly, in the context of financial regulation, one interpretation of the "revolving door" of government regulators who later accept positions at the firms they regulate is that those individuals gain knowledge and experience that is valued by those firms (Blanes i Vidal et al., 2012). In this case, the behavior that has the appearance of *quid pro quo* is economically efficient, rather than corrupt.

efits actually affects beneficiaries' behavior. Without such variation, observed correlations between discretionary decisions and reciprocal benefits remain merely suggestive. Similarly, without exogenous variation in discretion, it is not possible to understand the impact of discretion on institutional effectiveness.

This paper overcomes these empirical challenges in a setting that features an exogenous elimination of discretion, measurable reciprocal benefits to the decision-maker, and a measure of whether discretion improves or worsens outcomes of those whom it is supposed to serve. It does so by exploiting the fact that prior to 2015 in Travis County, Texas, judges possessed discretion to override the system of randomly assigning publicly-funded private defense attorneys to indigent defendants. This discretion could benefit defendants in meaningful ways. For example, judges could use their knowledge and discretion to better match defendants with attorneys who would likely be effective in that type of case, or during that time period. Similarly, judges could also simply reward particularly effective defense attorneys with additional appointments, which would help solve the principal-agent problem in the attorney-client relationship. On the other hand, there was also potential for at least the appearance, if not realization, of *quid pro quo*, given that attorneys are paid per case and judges, as elected officials, would benefit from campaign contributions from those attorneys.[2] In fact, the 2015 reform that required all subsequent case assignments to be made randomly via a rotation system was rooted in concerns regarding the appearance of *quid pro quo*.[3] I

[2]In this paper, campaign contributions made by court-appointed attorneys with the expectation of receiving favorable treatment are treated as the appearance of *quid pro quo*, even in the absence of explicit agreements between judges and attorneys. This is supported by established rulings from higher courts. In Evans v. United States (1992), the U.S. Supreme Court held that an explicit promise is not required to establish *quid pro quo* under the Hobbs Act. The Court stated that "The [public] official and the payor need not state the *quid pro quo* in express terms, for otherwise the law's effect could be frustrated by knowing winks and nods." Justice Kennedy, in his concurrence, further emphasized that "an explicit agreement [between donor and donee] may be implied from [the official's] words and actions." More recently, in Benjamin v. United States (2024), the Second Circuit reaffirmed this view, stating that "An explicit *quid pro quo* under McCormick need not be expressly stated but may be inferred from the official's and the payor's words and actions." While legal interpretations may vary, these rulings support the view that campaign contributions made with the intent to influence official action, such as attorney case assignments, can constitute *quid pro quo*.

[3]The preference for random assignment is consistent with the American Bar Association's Ten Principles

use this exogenous elimination of discretion, along with data on campaign donations and the outcomes of defendants' cases, to test whether receiving extra cases through discretion *caused* attorneys to donate to those judges, and whether the removal of discretion benefited or harmed defendants.

The criminal justice system is a particularly important setting in which to examine these issues. The reason is simple: over 80 percent of criminal defendants in the U.S. are appointed a publicly-funded attorney because they cannot provide one for themselves (Harlow, 2001; Justice, 2019). There is widespread concern regarding the effectiveness of publicly-funded attorneys in general,[4] and court-appointed private attorneys in particular.[5] Moreover, the indigent defense system is uniquely characterized by a double principal–agent problem: defendants rely on court-appointed attorneys whose incentives may not align with theirs, while judges, in turn, may control attorney assignments under their own political incentives. This structure provides an ideal setting to study how discretion interacts with both layers of agency problems.

In the first part of the paper, I leverage the exogenous variation in judicial discretion to test whether judicial discretion in assigning attorneys generated reciprocity in the form of campaign donations by the affected attorneys to those judges. I do so using a difference-in-differences design that compares campaign donations by attorneys who disproportionately benefited from non-random appointments to those who did not, before and after the 2015 elimination of judicial discretion. Results indicate that prohibiting judges from making

of a Public Defense Delivery System, the first of which holds that "The public defense function, including the selection, funding, and payment of defense counsel, is independent."

[4]See Duhart Clarke et al. (2024) for a meta-analysis of criminal legal outcomes by counsel type. For example, the research finds that indigent defendants were 75 percent less likely to be released pretrial and 40 percent more likely to be convicted than defendants with retained counsel.

[5]Previous studies consistently show that defendants represented by court-appointed attorneys often face worse case outcomes compared to those represented by public defenders or privately retained counsel (Iyengar, 2007; Anderson and Heaton, 2012; Cohen, 2014; Roach, 2014; Agan et al., 2021; Shem-Tov, 2022). These facts imply that there is significant upside and also significant risk from allowing judges to nonrandomly assign attorneys to cases. In particular, to the extent nonrandom assignment impacts case outcomes in a meaningful way, it is important to consider that as a cost-effective way of improving outcomes for indigent defendants.

discretionary assignments led to a decline in campaign donations from court-appointed attorneys, consistent with the presence of *quid pro quo* before the reform. Specifically, results indicate that for the average attorney, the elimination of judicial discretion reduced the likelihood of donating by 21 percent. Results are statistically significant at the one percent level and robust to the inclusion of various fixed effects.

In the second part of the paper, I assess whether judicial discretion improved or worsened institutional performance, defendant case outcomes. I do so in two ways. I employ a regression discontinuity in time design, where a running variable is time in days between the reform date and case filing date, as well as a difference-in-differences design to show that the elimination of judicial discretion significantly improved outcomes for low-income defendants. Estimates indicate that, after the reform, an average case is 9-13 percent less likely to result in a conviction and 9-17 percent less likely to result in an incarceration sentence. These findings indicate that despite the potential promise of judicial discretion for improving defendant outcomes, the realized effect of the discretion led to significantly worse outcomes for indigent defendants.

Furthermore, I examine potential mechanisms through which discretion may affect defendant outcomes. First, I test whether discretionary assignments incentivizes attorneys to be more effective advocates for their clients, using a difference-in-differences methodology based on randomly assigned cases. If the prospect of receiving additional cases incentivizes better legal representation, then the elimination of that incentive should cause a larger decline in performance among attorneys who previously benefited more from the discretionary system. The results, however, show no differential change in performance across attorneys with varying exposure to discretion, suggesting that judicial discretion does not operate as an effective incentive mechanism. In addition, attorney quality analysis provides suggestive evidence that attorneys who were more likely to receive discretionary assignments are less effective at securing favorable outcomes for their clients, which may help explain the observed

improvements in case outcomes following the reform.

This paper makes two important contributions. First, to my knowledge, this is the first study to exploit exogenous variation in discretion to provide causal evidence on both sides of the discretion trade-off: the risk of impropriety through reciprocity, and the potential gains in institutional effectiveness. As emphasized by the seminal study by Banfield (1975), while discretion can promote institutional effectiveness by allowing officials to exercise judgment, it also creates opportunities for corruption. Yet existing studies typically focus on one of these two dimensions in isolation—either focusing on the potential for corruption, or analyzing institutional performance. Such partial analyses may limit our ability to evaluate discretion as an institutional mechanism, since effective policy design requires weighing both dimensions simultaneously.

A few empirical studies document the tension between discretion and corrupt practices. For example, Colonnelli et al. (2020) find that politicians in Brazil use their discretion in public employment decisions for *quid pro quo* patronage benefiting their campaign donors. Decarolis et al. (2025) provide correlational evidence from the Italian procurement context that more discretionary auction procedures are chosen more often by officials previously suspected of corruption. Teso (2023) finds that corporate leaders in the U.S. are more likely to make political donations when a politician is assigned to a committee relevant to their industries. These studies provide valuable insights, but none are able to directly examine the causal impact of how discretion affects institutional performance.

Other studies do examine the institutional performance side, albeit without also documenting whether there is reciprocity between parties. Despite a heavy reliance on human discretion in institutions, whether discretion improves institutional outcomes is ambiguous. This is because the effectiveness depends on the degree to which the interests of decision-makers align with institutional goals and on the accuracy of their expertise (Kelman, 1990; Aghion and Tirole, 1997). Empirical findings reflect this ambiguity. In the context of public

procurement, a setting closely analogous to court-appointed counsel system studied here in that both involve publicly funded work contracted out to private actors, Coviello et al. (2018) finds no negative effects of discretion in Italy. Bandiera et al. (2021) provides experimental evidence in Pakistan showing that discretion can lower costs and administrative burdens. By contrast, Szucs (2024) documents that discretion in Hungary raised procurement prices and led to the selection of less productive contractors, while Bosio et al. (2022) show that limiting discretion can be particularly effective in countries with low public sector capacity.

In related judicial contexts, Yang (2015) finds that when federal sentencing guidelines were struck down and judges gained more discretion, racial disparities in sentencing grew larger. This study is also similar to Vannutelli (2022), which shows that replacing discretionary auditor appointments with random assignment improved municipal fiscal performance, consistent with the findings in this paper that random assignment mechanisms can enhance institutional outcomes.[6] Yet none of these studies directly document whether discretion gives rise to the appearance of corruption. This paper contributes to the literature by providing novel evidence that discretion can foster rent-seeking through *quid pro quo* incentives, create the appearance of impropriety between elected officials and private-sector agents, and does not improve the welfare of those it is supposed to help.

The second contribution of this study is that it is the first to examine the causal effect of discretionary case assignments to publicly-funded defense attorneys. This is an important policy question in and of itself, given serious concerns about whether these attorneys provide adequate legal assistance. In addressing this question, this study complements a larger literature on the effectiveness of indigent defense attorneys and related interventions to improve outcomes for defendants. Agan et al. (2021) shows that court-appointed attorneys are less effective than privately retained attorneys and provides suggestive evidence that the attorney

[6]There is also a large body of literature on teacher discretion in student assessment, showing that teachers exercise discretion in contexts involving consequential decisions, such as grading high-stakes exams, that can affect both short- and long-term student outcomes (Lavy, 2008; Dee et al., 2011; Hanna and Linden, 2012; Diamond and Persson, 2016; Cornelisz et al., 2019; Beg et al., 2024; Figlio and Özek, 2025).

compensation structure leads to disparities between indigent and non-indigent defendants. Counsel quality within the indigent defense system varies substantially by attorney type. Several studies find that court-appointed attorneys achieve less favorable outcomes for their clients (Iyengar, 2007; Anderson and Heaton, 2012; Roach, 2014; Cohen, 2014; Shem-Tov, 2022). This paper is most closely related to Yáñez and Sukhatme (2023), which shows that judges are more likely to assign cases to attorneys who donate to their election campaigns in a given month. Their findings underscore the link between case assignments and campaign donations in the indigent defense system. However, this study differs from that paper in several important ways. First, while that paper uses variation in donation timing, and thus implicitly assumes that reciprocity works in a particularly temporal way, my study exploits the elimination of discretionary assignments. Second, I am able to examine not just the impacts of discretion on reciprocity, but also on the outcomes of the defendants. Doing so is not possible when using the temporal variation used by Yáñez and Sukhatme (2023).

The findings in this paper have important policy implications given the widespread use of both judicial discretion and campaign contributions. Thirty-nine states in the U.S. elect trial court judges and allow judicial candidates to accept campaign contributions.[7] While it is difficult to ascertain the proportion of jurisdictions in which judges exercise at least some discretionary authority, my review of the ten most populous counties in the U.S. indicates that at least six of those elect their judges and permit some discretion in indigent case assignments to court-appointed attorneys. As a result, the findings here are relevant to many important contexts in the U.S., and suggest that discretion in this context generates reciprocation and at least the appearance of *quid pro quo*. These results are especially noteworthy given this is a setting in which the participants—attorneys and judges—are well-versed in conflicts of interest and what it takes to avoid them. In addition, the results here indicate that while there are *ex ante* reasons to have believed judicial discretion might

[7]The following states do not select trial court judges through elections: Delaware, Hawaii, Massachusetts, New Hampshire, New Jersey, Rhode Island, Virginia, Connecticut, Maine, South Carolina, Vermont.

improve the quality of legal defense, the results suggest otherwise. In fact, this study indicates that, at least in contexts with elected judges and judicial discretion in case assignment, random assignment may offer a low-cost way of improving legal counsel and outcomes for indigent defendants, while simultaneously eliminating even the appearance of *quid pro quo*.

2 Institutional Background

2.1 The indigent defense system

The Sixth Amendment to the U.S. Constitution guarantees defendants the right to counsel in criminal cases. Following the landmark 1963 Supreme Court case *Gideon v. Wainwright*, jurisdictions adopted various methods to provide legal representation for defendants who are unable to afford a private attorney. Indigent defense in the U.S. typically operates through three models: public defender, assigned counsel, and contract-based defender. Public defenders are salaried employees who exclusively represent indigent defendants. In the assigned counsel model, private defense attorneys, commonly referred to as court-appointed attorneys, enroll in a court-maintained pool ("wheel") and are compensated either at an hourly rate or a flat fee per case. This form of compensation creates incentives for court-appointed attorneys to seek more cases. Lastly, contract-based defenders combine elements of both systems because they are private attorneys who sign agreements to handle a set number of cases for a fixed fee. Jurisdictions vary in their delivery of public defense services. For instance, some jurisdictions use only public defenders or assigned counsel, while others rely on both types of counsel.

To ensure an effective public defense system, the American Bar Association (ABA) established its *Ten Principles of a Public Defense Delivery System*[8], the first of which holds

[8]The Principles were sponsored by the ABA Standing Committee on Legal and Indigent Defendants and approved by the ABA House of Delegates in 2002. They were revised in 2022 to provide more detail and clarity to policymakers. See the ABA website for more details.

that public defense services should operate free from political or judicial influence[9]. This principle of independence has been a long-standing focus, particularly in the selection of attorneys. As early as 1992, the ABA recommended that judges or elected officials should not select the lawyers for indigent cases[10]. The rationale for this recommendation is that if lawyers rely on judges for future case assignments, they may prioritize maintaining favor with the judiciary, perhaps by resolving cases too hastily to move dockets, making campaign contributions or, in extreme cases, offering bribes, over serving their clients' best interests. This can undermine public trust by allowing judges to use their discretion for personal gain and by creating the appearance, if not realization, of impropriety in the legal system. Conversely, from an efficiency standpoint, independence may not always be beneficial. Judges, with their direct knowledge of the courtroom, may be better positioned to match attorneys with defendants, potentially improving the quality of representation. Similarly, judges could use their discretion to reward effective attorneys with additional assignments, which would help overcome the principal-agent problem between the defendant and his attorney. This tension has led to wide variation across jurisdictions in the degree of discretion allowed in case assignments. However, there has been no empirical evidence on the impact of that discretion on the indigent defense system.

In Texas, indigent defendants are guaranteed an attorney appointment at any stage of a criminal proceeding. The assigned counsel model is the default process per state law and the most widely used across the state. Less than 20 percent of Texas counties rely on public defenders. In counties that offer both assigned counsel and public defenders, judges determine which type of counsel to assign. Travis County, for instance, relied on assigned counsel for both felony and misdemeanor cases until a public defender's office was established

[9]"The public defense function, including the selection, funding, and payment of defense counsel, is independent."

[10]The 1992 ABA Standards for Criminal Justice: Providing Defense Services stated that "The selection of lawyers for specific cases should not be made by the judiciary or elected officials but should be arranged by the administrators of the defender, assigned-counsel and contract-for-service programs"

in 2021. Yet, court-appointed attorneys continue to handle the majority of indigent cases. Defense attorneys who meet certain eligibility requirements can sign up for specific "wheels" for different offense categories (e.g., misdemeanor or various felony degrees).[11]

2.2 Judicial discretion in Travis County's indigent defense system

The indigent defense system in Texas has long been criticized for granting judges excessive and largely unaccountable discretion (Texas Appleseed, 2000). Criminal court judges can appoint court-appointed attorneys to indigent cases and, in some instances, approve their compensation upon case termination. Because Texas judges are elected officials who fundraise for their campaigns, this structure has raised concerns about potential *quid pro quo* between judges and attorneys, in which attorneys may contribute to judicial campaigns to secure favorable treatment in case assignments.

These concerns prompted Travis County, the fifth-largest county in Texas (population of 1.3 million) and home to Austin, to implement a reform. The county introduced a new system, called Managed Assigned Counsel, and transferred judicial discretion in case assignment to the Office of Court Administration.[12] Prior to the reform, a case assignment process was supposed to be governed by a "wheel" system that rotates alphabetically through a list of eligible court-appointed attorneys. In practice, however, judges frequently bypassed this process. According to a report by the Texas Indigent Defense Commission (TIDC), nearly

[11]To be listed on a wheel, an attorney should submit an application for review. Required criteria include a valid license to practice law, residency in Travis County or adjoining counties, and substantial experience in the field of criminal law, with experience requirements varying by panel type.

[12]The Managed Assigned Counsel system also shifted authority to approve attorney compensation from judges to another county agency. However, it is unlikely that the authority to approve payments served as the main vehicle for favoritism or confounds identification of discretionary assignment effects, since the attorney fee schedule is standardized and applies uniformly to all assigned attorneys based on services they provide (e.g., case resolution, trial representation, or jail release), regardless of their political donations. This standardized structure leaves judges with little discretion in payment decisions, thereby limiting the scope for favoritism through differential compensation approval. Thus, opportunities for favoritism were more plausibly expressed through differential case assignments, as access to more assignments directly translated to higher attorney earnings.

45 percent of appointments in Travis County were made "off the wheel", meaning judges assigned cases to attorneys of their choosing (TIDC, 2018). This raised concerns that discretionary appointments were not always based on attorney competence. Anecdotes also gave cause for some concern. Sukhatme and Jenkins (2020) quotes a Texas attorney recalling that his former employer once said that campaign donations to judges were "necessary to keep his [a court-appointed attorney] lights on" and that "the elections of new judges meant that he "lost" some courts and had to begin donating more to different judges in order to keep getting appointments". Similarly, a former felony court judge in Travis County, who served nearly two decades on the bench, remarked in one interview that "Some judges would appoint lawyers they thought were competent. But the cynical view is that they would appoint lawyers they liked or who made contributions to their campaign efforts."[13]

In response to concerns about potential conflicts of interest between judges and attorneys, several judges proposed a reform in 2013. The reform had two objectives: to improve the quality of legal representation and to restore independence in the indigent defense system by addressing widespread concerns about favoritism and *quid pro quo* arrangements tied to campaign contributions. Beginning in January 2015, the Office of Court Administration took over responsibility for assigning attorneys, selecting them at random from "wheel" lists based on the most serious charge a defendant faces. Importantly, judges are no longer permitted to override these random assignments.[14] As shown in Figure 1, the rate of nonrandom assignments dropped sharply following the reform and has remained near one percent.

2.3 Judicial elections in Texas

Texas is one of thirty-nine states that elect judges, with judicial candidates appearing on the ballot during even-numbered years. The election process consists of primary elections, runoff

[13]See the interview at the following link https://www.newsweek.com/judges-pay-play-texas-1508197.

[14]In rare cases, judges may still make discretionary assignments, but they are now required to provide written justification for doing so.

elections if necessary, and general elections. Judges must seek reelection in partisan contests to serve subsequent terms. Once elected, judges typically remain in office for extended periods, spanning multiple election cycles.

Judicial campaigns in Texas are subject to strict rules regarding campaign financing. Candidates may accept contributions within a specific time window, from 210 days before the filing deadline to 120 days after they last appear on the ballot. Contribution limits vary by both the type of judicial office and the size of the district. For statewide judicial offices, candidates can accept up to $5,000 per contributor. For appellate courts, district courts, statutory county courts, and statutory probate courts, the limits vary by population: $5,000 in districts with populations over 1,000,000, $2,500 for populations between 250,000 and 1,000,000, and $1,000 for districts with fewer than 250,000 residents. [15]

3 Data

This paper leverages several administrative datasets to examine the effects of judicial discretion. Data on court activities, including indigent defense case assignments and historical felony court records, were obtained from the Travis County Office of Court Administration and the District Clerk's office. Information on judicial elections and individual campaign contributions was obtained from the Texas Ethics Commission. In this section, I describe these datasets and explain the matching of the datasets.

3.1 Court Data

The court data used in the paper consist of two sets of court records. The first is the wheel data, which lists court-appointed attorneys identified by their state bar numbers and the cases they represented. For each case attorneys worked on, I observe the case number

[15] According to the U.S. Census, the 2022 population in Travis County was 1,326,436.

identifier, the dates when attorneys submitted vouchers for their services, the approved compensation amounts, and an indicator of whether the assignment was made nonrandomly by a judge. The second datasets is the historical felony case records, which provide detailed information on case characteristics (e.g., filing date, offense date, offense type, attorney name, attorney type, case number, court) and defendant characteristics (e.g., name, gender, date of birth, race, defendant identifier). The data also include case outcomes, including whether a case results in a dismissal or a guilty conviction, as well as sentencing information if found to be guilty.

3.2 Electoral Data

I obtained publicly available campaign finance data for the 2008-2022 judicial elections from the Texas Ethics Commission. In Texas, candidates running for judicial offices are required to report campaign contributions. The data provide rich information on individual contributions, including contributor name, type (individual or entity), occupation, employer, contribution amount, contribution date, and the judicial candidate who received the contribution. I identify contributions made to judicial candidates in Travis County using candidate identifiers provided by the Texas Ethics Commission. Based on self-reported information on occupations, employers, and entity names, over 60 percent of the contributions came from attorneys or individuals affiliated with law firms.

3.3 Matching and Final Dataset

To construct the sample for the main analysis, I match the wheel data containing attorneys' appointment history to campaign contribution data. The matching procedure differs by contributor type, depending on whether the contributor is an individual or an entity. For individual contributors, I identify court-appointed attorneys in the wheel data using last

names and the first three letters of first names. For entity contributors, most of which are law firms, I first identify potential matches when an attorney's last name appears in the entity name (e.g., "Law Office of John Doe"). In these cases, I manually verify the match by searching the entity name online to confirm whether it corresponds to the same attorney.

The donation analysis sample is restricted to seven felony court judges and 140 court-appointed attorneys, all of whom were active both before and after the reform.[16][17] Each of the 140 court-appointed attorneys enters the data in all election cycles, while each of the seven judges enters the data in all cycles during which she either participated in an election or served on the bench. By merging the wheel and campaign finance datasets, I construct a comprehensive panel at the attorney-judge pair level that tracks each attorney's contributions to every candidate over time. The resulting dataset contains 3,360 unique attorney-judge-cycle tuples.[18] If an attorney makes no contribution to a candidate in a given election cycle, the contribution amount is coded as zero. This data structure allows me to examine how attorneys adjusted their donation behavior after the reform by observing donor status and donation amounts for each pair across election cycles. In the second part of the paper, I implement a regression discontinuity in time design and a difference-in-differences design to examine the impact of judicial discretion in case assignments on case outcomes, using all indigent felony cases from 2014–2015 assigned to judges in the donation sample.[19] To do so, I identify indigent cases in the historical court records using case number identifiers from the wheel data as unique identifiers.

[16]To avoid small-sample bias and focus on attorneys with sustained participation, I restrict the sample to those whose pre- and post-reform caseloads are each above the 25th percentile in the court data (2013–2022).

[17]Some attorneys opt out of the court-appointed system during the post-reform period. The main sample includes these attorneys, as attrition analysis shows no significant correlation between treatment intensity (i.e., discretionary assignment rates) and the likelihood of opting out in the post-period (p-value of 0.55).

[18]In Texas, judicial elections occur in every even-numbered year. Because judges serve four-year terms, the set of courts participating in each cycle varies, making the data an unbalanced panel.

[19]I note that the first part of the paper does not use a regression discontinuity in time approach, as this method requires high-frequency data with sufficient observations across time, which is not feasible given that campaign contributions are largely clustered around election periods.

3.4 Summary Statistics

Summary statistics are shown in Table 1, with pre-reform nonrandom assignment rates and donation outcomes calculated at the attorney-judge pair level. Column (1) shows results for the full sample, which includes both pre- and post-reform periods. Prior to the reform, judges made discretionary appointments with a probability of 18.6 percent.[20] In Panel A, the overall likelihood of donating is 14 percent and the unconditional mean donation amount is approximately $66, while the conditional mean is $480.[21] Columns (2) and (3) show summary statistics for the pre- and post-reform, respectively. There are notable differences in donation outcomes across periods. The likelihood of attorneys donating to judges' campaigns fell by 36 percent in the post-reform period (0.17 versus 0.11). Moreover, unconditional donation amounts declined by 48 percent ($85 versus $44), and conditional amounts declined by 19 percent ($514 versus $417).

Panels B, C, and D show summary statistics from the felony case data used in the defendant outcome analysis. Panel B reports defendant characteristics. In Column (1), 20 percent of defendants are female, with an average age of 34. Additionally, 33 percent of defendants are Black, and 63 percent have a prior offense. Panel C summarizes the offense types of the highest charge in each case. The most common charge types are property crimes (32 percent), followed by drug offenses (29 percent) and violent crimes (23 percent). More importantly, defendant and charge characteristics appear largely similar before and after the reform. Panel D summarizes case outcomes. Because cases often involve multiple charges, outcomes are defined at the case level: whether a case resulted in at least one guilty conviction, incarceration sentence, or probation sentence.[22] While probation sentence rates remain similar across periods, both conviction rates and incarceration sentence rates declined

[20] Figure A1 reports the mean discretionary assignment rates categorized by tercile groups to illustrate the distribution.

[21] Figure A2 shows the distributions of donation amounts conditional on donating, before and after the reform.

[22] Probation includes deferred adjudication and community supervision.

by 5 percent (0.737 versus 0.697) and 8 percent (0.67 versus 0.616), respectively.

4 Empirical Strategy

4.1 The effects of judicial discretion on donation outcomes

In the first part of the paper, I leverage the exogenous variation in judicial discretion to test whether discretion in assigning attorneys generated reciprocity in the form of campaign donations by the affected attorneys to those judges. To do so, I employ a difference-in-differences approach that estimates how donating behavior varied with attorneys' exposure to discretionary appointments before and after the reform that eliminated judicial discretion in assignments. A key challenge in estimating the effects of judicial discretion on donation outcomes is that donation decisions are endogenously determined. This raises concerns about simultaneity and omitted variable bias if other factors influencing donations are correlated with discretionary case assignments. For instance, competent judges may assign extra cases to more effective attorneys, and those attorneys may choose to donate to judges they believe to be especially capable. It is also possible that donations arise from personal relationships between judges and attorneys, which are not observed in the data. I overcome this challenge by leveraging two sources of variation. The first is the reform, which eliminated judges' discretion to make discretionary case assignments. The other variation comes from the fact that before the reform, some attorneys were receiving more discretionary assignments than others, which allows me to compare attorneys with varying levels of exposure to discretionary assignments. The model is specified as follows:

$$Y_{ijt} = \beta_0 + \beta_1 Benchrate_{ij} + \beta_2 Post_t + \beta_3 Benchrate_{ij} \times Post_t + X_{ijt} + \epsilon_{ijt} \qquad (1)$$

where Y_{ijt} is attorney donation outcomes, such as whether attorney i contributes to judge j in election cycle t. $Benchrate_{ij}$ represents a measure of exposure to judicial discretion, the rate at which judge j made discretionary assignments to attorney i in the pre-reform period.[23] $Post_t$ is an indicator equal to one if the election cycle occurs after 2015, the year of the reform. The coefficient of interest, β_3, measures the differential change in the donation behavior after the reform attributable to variation in pre-reform discretionary assignment rate ($Benchrate$). This continuous measure of exposure to judicial discretion reflects the hypothesis that attorneys who received discretionary case assignments at higher rates prior to the reform would be more likely to adjust their donation behavior once judges could no longer override the random assignment process.[24] X_{ijt} is a vector of fixed effects, including attorney fixed effects, election cycle fixed effects, or cycle-by-judge fixed effects. Attorney fixed effects control for time-invariant unobservable factors that vary across attorneys. Election cycle-by-judge fixed effects account for circumstances specific to a judge and election cycle, such as announcing retirement or losing a primary election, that could affect fundraising. ϵ_{ijt} is the error term, and robust standard errors are clustered at the attorney-by-judge level, which corresponds to the level of treatment intensity assignment. This accounts for the mechanical correlation in outcomes that arises from observing the same attorney-judge pairs across multiple periods in the data.[25]

To illustrate the intuition of the design, I present a simplified comparison using a 2×2 framework in Table 2. The identification strategy relies on comparing changes in donation behavior between attorneys who differentially benefited from discretionary assignments before and after the elimination of judicial discretion. I begin by examining the temporal variation in mean donation likelihood for attorneys in the top tercile of pre-reform discre-

[23] In the main specification, I use two years of discretionary assignment records to calculate the *Benchrate* due to a data limitation that records prior to 2013 are partially missing. However, results are robust to using rates based on a four-year window.

[24] I estimate a linear probability model rather than a logit or probit model because including a large number of fixed effects may cause the incidental parameters problem (Neyman and Scott, 1948)

[25] I show that the main results are robust to alternative clustering levels in Table A1.

tionary assignment rates (Column (1)). While this before-and-after difference could be due to the reform, it could also be due to other time-varying factors that affect donation. For instance, shifts in public sentiment regarding judicial elections or changing professional norms regarding attorney political engagement could systematically affect contribution behavior across all attorneys, regardless of their exposure to discretionary assignments. In the absence of the reform effect, the temporal change in donation likelihood for attorneys with higher bench rates should be equivalent to that for attorneys with lower bench rates. To confirm this, I compare the change in donation likelihood for attorneys in the top tercile (0.113 in Column (1)) with the corresponding change for attorneys with zero bench rates (0.012 in Column (2)). The disparity between these two differences is 0.101, representing a 2×2 difference-in-differences estimate of the reform effect. This intuition extends to my main difference-in-differences design with continuous treatment measure, allowing me to estimate the reform effect while capturing how outcomes vary with each marginal unit of treatment intensity. I also formally estimate standard 2×2 difference-in-differences models and provide results in section 5.

4.1.1 Identifying Assumptions

Identification of the causal evidence of potential *quid pro quo* using the difference-in-differences model relies on several assumptions. First, the parallel trends assumption requires that in the absence of the reform, attorneys with different treatment intensities would have experienced similar trends in donation outcomes. Since this assumption is about counterfactuals, I cannot test directly what would have happened in the absence of the treatment. Instead, I assess its plausibility using an event-study specification, which examines dynamic treatment effects and allows me to test for differential pre-trends before the reform. To do so, I estimate

the following regression equation:

$$Y_{ijt} = \beta_1 Benchrate_{ij} + \sum_{\substack{t=2008,2010,2012... \\ t \neq 2014}}^{2022} \beta_{2t} * \mathbb{1}(electioncycle = t) * Benchrate_{ij} + X_{ijt} + \epsilon_{ijt} \quad (2)$$

where the interaction between bench assignment rates and the post-reform indicator in Equation 1 is replaced by interactions between bench assignment rates and each election cycle dummy, leaving out the 2014 election cycle as the baseline. X_{ijt} is a vector of fixed effects, including attorney fixed effects and cycle-by-judge fixed effects. If there are no differential pre-trends in donation behavior between attorneys with high and low bench assignment rates, then the interaction estimates in the pre-reform period should not be statistically different from zero.

Figure 3 shows the event study coefficient plot using an indicator for whether the attorney donated to that judge as the dependent variable. In Figure 3, the pre-reform interaction estimates are flat and statistically indistinguishable from zero, indicating no evidence of differential pre-trends in donation behavior. This lends credence to the parallel trends assumption. In addition, Figure 3 shows no evidence that there were anticipation effects, consistent with the identifying assumption of difference-in-differences.

There has been growing interest in difference-in-differences models using continuous treatment measures, which require an additional identifying assumption that the "average treatment effect function" does not vary with the dose of treatment (Callaway et al., 2024). In this specific design, low-dose units (i.e., attorneys with low bench rates) serve as the counterfactual outcomes for high-dose units (i.e., attorneys with high bench rates). As such, it is crucial to assume that high-dose units would have experienced the same treatment effects, in addition to untreated potential outcomes, as the low-dose groups. This is unlikely to be true if the dose itself is correlated with observables. To address this, I follow the approach used in Cook et al. (2023) and Chiplunkar and Weaver (2023) by testing balance.

Table A2 shows results from a balance check that assesses the degree to which the dose is correlated with attorney characteristics, including years of experience, law school ranking, race, sex, and public disciplinary history. Each column shows estimates from a separate regression of the characteristics on the treatment dose. None of these estimates is statistically significant at conventional levels, indicating that there is little to distinguish between the high- and low-dose attorneys in terms of these characteristics.

4.2 The effects of judicial discretion on indigent defense case outcomes

In the second part of the paper, I assess whether removing judicial discretion improved or worsened defendant case outcomes. To do so, I employ a regression discontinuity in time (RDiT) approach, where the running variable is days between the case filing date and the reform date, January 1, 2015. Using observations with filing dates around the cutoff date, I estimate the following reduced-form equation:

$$Y_i = \gamma_0 + \gamma_1 Post_i + \gamma_2 RunningDate_i + \gamma_3 Post_i \times RunningDate_i + X_i + \epsilon_i \qquad (3)$$

where Y_i is a dummy for case outcome (e.g., guilty conviction or incarceration sentence). $Post_i$ equals one if the case was filed after the cutoff date and zero otherwise, and $RunningDate_i$ is the time in days between the case filing date and the cutoff date. I exclude cases filed in December 2014, the month immediately preceding the reform, to account for potential lags in time between case filing and attorney assignment. Such lags may arise from procedural delays, court holidays, or defendants requesting counsel several days after booking, which could cause some of the cases in December 2014 to be assigned after the reform date. X_i is a vector of control variables of defendant and case characteristics, including age, gender, race, whether the individual has a prior offense, and offense types. The coefficient of interest, γ_1,

documents the impact of restricting judicial discretion in case assignments on case outcomes.

In my main specification, I estimate these regressions using a 365-day bandwidth on each side of the cutoff date with a uniform kernel and report bias-corrected estimates with robust standard errors from the Stata program "rdrobust" (Calonico et al. (2014); Calonico et al. (2017)).[26] For robustness, I re-estimate the models using alternative specifications, including the mean-squared-error-optimal bandwidth, and a triangular kernel in section 6. I also perform falsification tests using placebo cutoff dates and present the distribution of placebo t-statistics.

Hausman and Rapson (2018) discuss several pitfalls of the regression discontinuity in time approach that are less relevant in the standard regression discontinuity design. However, the data-generating process in this study helps mitigate some of these concerns. First, because observations are individual cases rather than dates, the analysis maintains a cross-sectional nature, reducing risks associated with the reliance on time-series variations (e.g., autoregression in the outcome variable), which often challenge RDiT designs. Moreover, I can test for manipulation at the cutoff date in a similar fashion to the McCrary density test (McCrary, 2008), since the density of the running variable is not uniform. In addition, I test for balance around the cutoff by estimating discontinuities in the predicted likelihood of outcomes, which allows me to assess whether the types of cases differ before and after the reform date. These features are not typically feasible in standard RDiT settings discussed by Hausman and Rapson (2018), which helps strengthen the credibility of the design in this paper.

In addition to the regression discontinuity design, I also employ the same continuous difference-in-differences methodology used in the donation analysis to estimate the impacts of the removal of discretion on defendant case outcomes. In this exercise, I use a different

[26]Calonico et al. (2014) show that conventional nonparametric local polynomial estimators may substantially over-reject the null hypotheses and propose a robust bias-corrected estimator to address this issue. Their inference procedure, implemented in the "rdrobust" program, is robust to relatively large bandwidth choices and provides robust bias-corrected confidence intervals for average treatment effects at the cutoff.

continuous treatment measure than that used in Equation 1, which is pre-reform variation in the predicted likelihood of discretionary assignment across case types. Intuitively, the test asks whether case types that were more likely to be nonrandomly assigned experience different changes in court outcomes after the reform. The identifying assumption of this approach is that while case types with higher exposure to judicial discretion may have different underlying likelihood of conviction than those with lower exposure to discretion, in the absence of a treatment effect the difference in their conviction rates between them should remain stable over time. Any post-reform divergence can then be attributed to the removal of discretion. In section 5, I provide more details of this exercise and its results.

4.2.1 Balance checks

One common concern in any regression discontinuity design is potential manipulation of the running variable near the cutoff in a way that is correlated with potential outcomes. In this setting, one might be worried that cases could be selectively delayed or expedited to move defendants across the cutoff in a way that is correlated with the underlying propensity to be found guilty. While this is unlikely—since case filing dates largely depend on when crimes occur, over which filing agencies have no control—, I formally test for this possibility using a density test (McCrary, 2008). Figure B1 shows no noticeable discontinuity in the density of cases filed around the cutoff date. Additionally, results from the density test (Cattaneo et al., 2020) confirm that I fail to reject the null hypothesis of balanced case density around the cutoff (p-value of 0.8394).

Another concern would be that there may be compositional changes in cases filed with courts across the cutoff, which would violate the identifying assumption of the regression discontinuity design that treatment is the only factor causing a discontinuity, while other factors remain continuous across the cutoff. To test for this, I examine whether there is a discontinuity in the predicted conviction likelihood, as based on predetermined characteristics.

I predict conviction likelihood in the following steps. First, using an out-of-sample dataset of indigent felony cases filed in Travis County between 2002 and 2012, I regress an indicator for guilty conviction on various defendant and case characteristics, including defendant race, age at case filing, sex, offense types, as well as whether they have prior offenses. Next, I use the estimated out-of-sample coefficients to calculate predicted conviction likelihood for the in-sample cases. Figure 5 visualizes the discontinuity in the predicted conviction likelihood around the cutoff. Results show no statistically significant discontinuities in predicted probabilities of guilty conviction[27], suggesting no compositional changes in cases across the cutoff.

5 Results

5.1 Judicial discretion and campaign contributions

In this section, I examine how restricting judicial discretion over case assignments impacts the donation behavior of court-appointed attorneys. To test whether judicial discretion in assigning attorneys generated reciprocity in the form of campaign donations, I employ a difference-in-differences approach that uses two sources of variation: the 2015 reform that eliminated judicial discretion and the differences in the degree to which attorneys received discretionary assignments from judges before the reform. In the presence of reciprocity—including *quid pro quo*—one would expect contributions by attorneys who had previously received more cases to decline more post-reform than those by attorneys who had previously received fewer discretionary cases.

[27]RD estimate is 0.006 with a p-value of 0.126

5.1.1 Standard 2×2 Difference-in-differences approach

I begin by estimating a standard 2×2 difference-in-differences model that compares donation outcomes between attorneys in the top tercile of exposure to discretionary assignments (i.e., bench rates) and those with zero bench rates. Table 3 shows that attorneys in the top tercile were 10 percentage points less likely to donate once judges are no longer able to give extra cases. This effect is statistically significant at the one percent level, representing a 60 percent reduction relative to the baseline rate of 0.165. Additional results indicate that top-tercile attorneys also reduced their donation amounts by 65 percent relative to zero-rate attorneys (Table A4). Importantly, for both outcomes, the parallel trends check is consistent with the identifying assumptions of the difference-in-differences approach, and is reported in Figure 2 and Figure A3.

5.1.2 Difference-in-differences with a continuous treatment approach

To examine how the treatment effect varies with the marginal unit of exposure and avoid making admittedly arbitrary decisions about how to define comparison groups, I estimate a difference-in-differences model with a continuous treatment intensity (Equation 1), in which the outcome is an indicator equal to one if an attorney donated to a judge's campaign in a given election cycle and zero otherwise. Results are shown in Table 4. In Column (1), I report the coefficient of interest, β_3 ($benchrate \times post$) from the most parsimonious specification, which includes only the post-reform indicator, the pre-reform nonrandom assignment rate, and their interaction term. The coefficient captures the extent to which attorneys with more exposure to discretion adjusted their donation behavior following the removal of judicial discretion, relative to attorneys with less exposure. The estimated effect is -0.178, which is statistically significant at the one percent level. Taken literally, the magnitude of the coefficient means that (hypothetical) attorneys whose pre-reform assignments from a given judge were always discretionary are 17.8 percentage points less likely to donate to that judge

after the reform, relative to attorneys who never received such assignments. Prior to the reform, the average attorney had a bench rate of 18 percent. As a result, the restriction on judicial discretion is associated with a 3.2 percentage point decline in the donation likelihood for the average attorney[28], which represents a 19 percent reduction relative to the baseline donation likelihood of 16.5 percent. Column (2) shows that the results are robust to the inclusion of attorney fixed effects, which capture within-attorney variation in their propensity to donate. In Columns (3) and (4), I include judge fixed effects and election cycle fixed effects, respectively, and obtain estimates of -0.184 and -0.190. Results are stable across specifications and remain statistically significant at the 1 percent level.

Column (5) further includes election cycle-by-judge fixed effects to account for judge-cycle specific heterogeneity in donation outcomes. For example, a candidate facing no opposition may have little incentive to solicit donations, while a candidate who loses a primary may end fundraising early. Even after controlling for such heterogeneity, results consistently indicate that attorneys with higher pre-reform bench rates are less likely to give money to judges' election campaigns after the reform, compared to before. The coefficient of interest, -0.194 (standard error = 0.0513), implies that a one standard deviation increase in the bench rates is associated with a 4.3 percentage point decline in the donation likelihood, equivalent to a 26 percent decrease relative to the baseline of 0.165. Across all specifications, the estimates in Table 4 are robust and statistically significant at the 1 percent level. The reduction in the likelihood of donating, particularly among attorneys who benefited more from discretion, reveals the presence of *quid pro quo* prior to the reform.

To determine whether the reduction in donations is driven by small, symbolic contributions or by large donations from so-called mega donors, I estimate Equation 1 using as the outcome variable a series of indicator variables for whether an attorney donates more than thresholds ranging from 50 to 500 dollars. This exercise decomposes the overall effect

[28] $0.032 \approx 0.18 \times 0.178$

by contribution size, allowing me to identify which segment of the donation distribution was most affected by the reform. Results are shown in Figure 4, which plots the difference-in-differences estimates at each threshold.[29] The vertical axis represents the percentage change in the likelihood of donating more than X dollars associated with a one standard deviation increase in the pre-reform bench rate. The figure shows that the reductions are more pronounced in relatively small donations below 250 dollars. For thresholds above this level, the estimated effects are smaller in magnitude and largely statistically indistinguishable from zero. For example, a one standard deviation increase in the bench rate is associated with a statistically significant 25 percent decrease in the likelihood of giving more than 100 dollars, whereas the corresponding effect at the 400-dollar threshold is roughly half as large and not statistically different from zero. These patterns suggest that the reform primarily discouraged donations from attorneys making relatively modest contributions, rather than reducing larger amounts.

I also estimate the impacts on donation amounts to explore the intensive margin. For this outcome, I use the inverse hyperbolic sine (IHS) transformation, which provides a good fit for variables with many zero-valued observations that arise when an attorney does not contribute to a judge during a given election cycle.[30] This transformation allows the coefficients to be interpreted as approximate percentage changes in the donation amount when moving from never-receiver attorneys to always-receiver attorneys. Results in Table 5 are robust across specifications and remain statistically significant at the one percent level. For instance, in Column (5), which includes attorney fixed effects and judge-by-cycle fixed effects, the point estimate of -1.247 implies that a one standard deviation increase in the bench rate is associated with a $24 reduction in donation amounts after the reform, representing a 28 percent decrease relative to the unconditional baseline mean of $85. Taken together, the evidence indicates that the reform significantly reduced donations along both the extensive

[29]Event study plots for this exercise are reported in Figure A4, supporting the parallel trends assumption.
[30]Event study plots for this exercise are reported in Figure A5, supporting the parallel trends assumption.

and intensive margins.[31]

5.2 Judicial discretion and defendant case outcomes

The results in the previous section show that court-appointed attorneys reduced their donations to judicial election campaigns once judges no longer had discretion to reward donors. That finding indicates that discretion in case assignments led to reciprocity and the appearance, if not the realization, of *quid pro quo* among judges and attorneys. However, that result does not address whether discretionary assignments benefited or harmed defendants, whose welfare is the emphasis of the system of publicly funded defense attorneys. In the second part of the paper, I assess whether judicial discretion improved or worsened defendant case outcomes in two ways.[32]

5.2.1 Regression discontinuity approach

First, in order to directly estimate the effects of the reform that eliminated judicial discretion, I use a regression discontinuity design to test whether the elimination of judicial discretion improved outcomes for low-income defendants.

Specifically, I implement the regression discontinuity design, using time in days relative to the reform date as the running variable, as described in section 4. I begin by estimating a first-stage relation. Figure 7 visualizes the effects of the filing date cutoff on discretionary assignments. The figure plots the average share of nonrandom assignments within equally spaced intervals of the running variable, along with fitted lines. The figure shows a clear

[31] While one might say the estimated decline in dollar amounts appears modest, its implications for public trust are potentially large, as even the mere existence of financial conflicts can foster perceptions of corruption, regardless of their size.

[32] In the adversarial legal system, the definition of a "good" outcome is multifaceted. From a societal perspective, criminal justice aims to reduce recidivism and promote public safety. However, within the indigent defense system, the central objective is to ensure that low-income defendants receive the effective legal representation guaranteed by the Constitution. For this reason, I focus on whether their case outcomes improve following the reform.

discontinuity in the discretionary assignment rate at the cutoff date. To quantify this more precisely, I apply the robust bias-corrected estimator of Calonico et al. (2014) for statistical inference and estimate the discontinuity of -0.130 (standard error = 0.0249), statistically significant at the one percent level.[33]

Next, I estimate the effects of the reform on case outcomes, focusing on guilty conviction rates and incarceration rates. Results are shown in Table 6. Panel A shows the reduced-form regression discontinuity estimates for guilty convictions. Estimates indicate that cases filed after the reform were less likely to result in a guilty conviction relative to pre-reform cases. Column (1) reports the most parsimonious specification without controls and shows a 6.4 percentage point reduction in the conviction likelihood, statistically significant at the five percent level. In Column (2), I control for defendant and case characteristics, including defendant race, age, sex, offense type, and whether the defendant has prior offenses, and find a slightly larger reduction of 7.3 percentage points, significant at the five percent level. This effect translates into a ten percent reduction in the conviction rates, relative to the pre-reform baseline rate of 0.74. Columns (3) and (4) further include court fixed effects and attorney fixed effects, respectively, to account for time-invariant unobservables, such as judge leniency and attorney performance. The estimated effect is similar across both specifications and remains statistically significant at the five percent level.

Panel B in Table 6 examines the incarceration outcome. Results consistently indicate a reduction in incarceration rates following the reform, with estimates ranging from 6.2 to 6.7 percentage points. For example, Column (2) shows that cases filed after the restriction of discretionary assignments are 6.5 percentage points less likely to result in incarceration,

[33]Note that Figure 7 displays local average means in each bin and their fitted line, rather than the bias-corrected estimates of Calonico et al. (2014), which explains the slight discrepancy between the figure and the estimated results. In order to visually approximate the bias-corrected estimates, one needs to plot the second-order local polynomial fit, as suggested by one of the authors in Calonico et al. (2014). Visualizations reflecting the bias-corrected estimator are reported in Figure B5 See https://www.statalist.org/forums/forum/general-stata-discussion/general/1764569-rdplot-does-not-match-rdrobust-output.

statistically significant at the five percent level. When additionally controlling for court fixed effects in Column (3), the estimate becomes slightly larger in magnitude (-0.067) and remains statistically significant at the five percent level. On average, the estimated effects in Table 6 correspond to a ten percent decline relative to the mean incarceration rate prior to the reform. I also examine a probation outcome in Table B2 and find no significant impact, suggesting that the observed drop in guilty conviction rates is largely driven by reductions in incarceration sentences rather than probation.

Overall, the reduced-form regression discontinuity results reveal that the elimination of judicial discretion over case assignment improved outcomes for low-income defendants. Specifically, estimates in Column (4) indicate that cases filed after the reform were ten percent less likely to result in conviction and incarceration.

5.2.2 Difference-in-differences approach

Next, I employ a difference-in-differences approach that exploits pre-reform variation in non-random assignment propensities across case types. Intuitively, this test answers the following thought experiment: Did case types that were more likely to be nonrandomly assigned experience different changes in court outcomes after the reform? This approach allows case types with higher versus lower exposure to judicial discretion to result in conviction or incarceration at different rates, as long as the difference is similar over time in the absence of the reform. Any post-reform divergence can then be attributed to the removal of discretion. It provides an alternative method for estimating the impacts of the reform on defendants as a function of treatment intensity and helps mitigate a potential concern with the regression discontinuity design that unobserved factors coinciding with the treatment may also affect outcomes. To do so, I first predict the likelihood of nonrandom assignment for each pre-reform case using observables, such as defendant and case characteristics. I then average

these predicted probabilities by court and case type.[34] The resulting court-by-type specific averages serve as proxies for how often judges assigned certain types of cases nonrandomly before the reform. Finally, I estimate a difference in differences model that interacts these averages with the post-reform indicator.

Figure 9 shows little evidence of significant pre-reform divergences in outcomes between case types with higher versus lower bench assignment propensities, supporting the parallel trends assumption. I then formally estimate the difference-in-differences model and report results for different legal outcomes in Table 7. All specifications control for court-by-type specific predicted bench assignment rates, a post-reform indicator, case characteristics, as well as time, court, and attorney fixed effects.

Column (1) shows that the effect on the likelihood of bench assignment is -0.818, significant at the one percent level. This first-stage estimate implies that a ten percentage point increase in predicted bench rates is associated with an eight percentage point reduction in actual bench rates post-reform. Similarly, Columns (2) and (3) show the treatment effect on guilty conviction and incarceration sentence, respectively. Column (2) indicates that a ten percentage point increase in predicted bench rates is associated with reductions of 3.1 and 3.3 percentage points in conviction and incarceration likelihoods, respectively. They are both significant at the five percent level. Rescaling the case outcome effects by the first-stage estimate in Column (1) translates these effects into reductions of 3.8 percentage points in conviction rates and 4.1 percentage points in incarceration sentence rates.

Taken together, the DiD results also indicate that the removal of discretionary case assignments improves legal outcomes for low-income defendants, consistent with the regression discontinuity findings. Moreover, once rescaled by first-stage estimates for easy comparison, the DiD and RD estimates are not statistically different from each other after scaling each by

[34]Cases are classified into one of six categories, violence, property, DWI, drug, sex crimes, and other felonies, based on the highest-level charge.

the magnitude of the first-stage used in that approach.[35] Overall, both approaches indicate that discretionary case assignments negatively affect defendants, as their outcomes improve when judicial discretion is removed.

5.2.3 Potential mechanisms

The findings in the previous section establish that, on average, judges did not effectively use their discretion to improve defendant outcomes. In fact, the evidence indicates that judicial discretion led to worse outcomes for defendants. In this section, I explore why discretion does not help defendants by testing for potential channels through which discretionary assignments could affect defendants. I first test whether discretionary assignments serve as incentives for attorney performance and whether discretion influences the distribution of cases across attorneys in ways that could contribute to the deterioration in defendant outcomes.

First, I test directly whether judges used the nonrandom assignment to incentivize attorneys to be more effective advocates for their clients. Granting decision-makers discretion can improve the performance of those under their authority, especially when discretionary rewards are involved (Bénabou and Tirole, 2003; Suvorov and Van de Ven, 2009). To examine this possibility, I use the same difference-in-differences framework to compare attorney performance measured by the likelihood of guilty convictions of cases handled by attorneys with higher versus lower bench rates, using cases filed between 2013 and 2017. If the prospect of receiving additional cases provided incentives for better legal representation, then the elimination of that incentive should cause a larger decline in performance for attorneys who benefited more from the discretionary system, compared to other attorneys. Figure B2

[35]To test whether the DiD and RD estimates are statistically different from each other, I rescale reduced-form estimates by first-stage effects in each model. For example, the rescaled RD estimate for guilty convictions in Table 6 Column (4) is -0.570 (= -0.073/0.128, standard error = 0.24), compared to the rescaled DiD estimate -0.376 (=-0.308/0.818, standard error = 0.170) in Table 7 Column (2). Similarly, incarceration sentence estimates are not statistically different across the two approaches.

shows that attorneys with different levels of pre-reform exposure to discretion do not exhibit divergent trends in guilty conviction rates prior to the reform, supporting the plausibility of the parallel trend assumption.[36]

Results are shown in Table B1, where the outcome variable is an indicator variable equal to one if a case results in at least one guilty conviction and zero otherwise. For the purpose of this exercise, I exclude nonrandomly assigned cases from the analysis to address potential compositional changes in the types of cases handled by attorneys with varying bench rates. If judicial discretion improves attorney performance, one would expect attorneys with higher bench rates to show a decline in performance once that discretion is removed.

Table B1 shows that discretionary assignment had no effect on attorney performance, as measured by whether cases ended in a guilty outcome. In particular, results indicate there was no differential change in attorney performance after the reform between attorneys with high versus low discretionary assignment rates. The main difference-in-differences estimates are negative, but small in magnitude, ranging from -0.005 to -0.01 across specifications, and statistically indistinguishable from zero. For instance, Column (3) shows that always-receiver attorneys were 0.3 percentage points less likely to secure a guilty conviction after the reform, relative to never-receiver attorneys. This represents only a 0.5 percent decline compared to the baseline of 0.746 and is not statistically significant at the conventional level. In addition, Table A3 reports difference-in-differences estimates on the likelihood of having at least one incarceration sentence within a case. Again, results indicate no differential change in attorney performance after the reform between attorneys with high versus low bench rates. These results indicate that discretion does not operate as an effective incentive mechanism for attorney performance.

[36]This exercise is distinct from the regression discontinuity and difference-in-differences tests in the previous section in that they document the system-wide impact of removing discretion on defendant outcomes. Instead, this exercise compares attorneys with higher and lower discretionary assignment rates to examine differential changes in performance between those with varying levels of exposure to discretion before and after the reform.

I also test whether judges used their discretion to assign more cases to below-average-quality attorneys. To do so, I first estimate individual attorney quality using a random effects model using data from only the post-reform cases, when assignments became randomized. I define attorney quality as the propensity of an attorney's cases to result in at least one guilty conviction charge. Because the data are at the case level and a case often includes multiple charges, conviction incidence within a case would provide a more relevant assessment of attorney effectiveness than dismissals. I begin by regressing the guilty conviction dummy on time fixed effects and controls as in Table 7, and keep the residuals. I then use those residuals and the Stata command *mixed* to estimate a random effects model, and then compute the random effect for each attorney. I perform the analysis with the same pool of attorneys used in the donation analysis, which are those attorneys, for whom bench rates can be calculated and who actively represented indigent defendants both before and after the reform.

Results are shown in Figure 6, which is a binscatter of estimated attorney quality against bench assignment rates. Because attorney quality measures the propensity for conviction, a higher value of the measure indicates lower effectiveness. The upward slope of the fitted line indicates that attorneys with greater exposure to pre-reform discretionary assignments are less effective, as cases they handle are more likely to result in guilty convictions. When regressing attorney quality on bench rates, the correlation is estimated to be 0.003, although it is not statistically significant at conventional levels. This result provides evidence that judges used their discretion to assign cases to attorneys that were no better, and perhaps slightly worse, than average. I note, however, that this exercise does not address whether judges assigned cases to attorneys who were worse matches for that case type, or the possibility that discretionary assignment led to congestion effects that reduced the performance of those attorneys.

6 Robustness Checks

6.1 Attorney donation analysis

In the main analysis, I compute the rate using cases assigned to attorneys within two years prior to the reform in 2015. This is due to a data limitation that case assignment records before 2013 are partially missing in the county court system. To address the concern that these missing records may disproportionately consist of discretionary appointments, I restrict the baseline measure to cases from 2013 onward. For robustness, I calculate the bench rate using cases assigned within four years prior to the reform and re-estimate the difference-in-differences model. Supporting evidence for the parallel trends assumption and the estimation results are reported in Figure A6 and Table A5. As shown in Column (5) of Table A5, the estimated effect is similar to the main results (-0.194 versus -0.219) and statistically significant at the one percent level.

I also assess the robustness of the results to alternative levels of clustering standard errors. The main specification clusters standard errors at the attorney-by-judge level, the level at which treatment intensity is defined, in order to account for the mechanical correlation in outcomes that would arise when the same attorney-judge pair appears repeatedly over time in the data. In Table A1, I evaluate the sensitivity to alternative clustering levels: attorney, judge, and two-way clustering at the attorney and judge level. For the two-way clustering (Column (4)), I report a p-value and 95% confidence interval from a wild bootstrap procedure. Results are robust to alternative clustering levels. The estimates remain statistically significant at the 1% level when clustering at either attorney- or judge-level and at the 5% level when two-way clustering at the attorney and judge level.

6.2 Case outcome analysis

For the main estimates in the defendant outcome analysis, I use a 365-day bandwidth surrounding the cutoff date and a uniform kernel weighting function. To assess robustness, I re-estimate the baseline regression discontinuity model (Equation 3) under six alternative specifications: extending the bandwidth to 500 days, applying a triangular kernel weighting function, and using the mean-squared-error-optimal bandwidth, which ranges from 150 to 180 days depending on the outcome variable. As shown in Table B4, the estimated effects are robust across these alternative specifications and not statistically different from those in the main analysis. Each of the results from the alternative specification ranges between -0.087 and -0.046 (compared to -0.073 in the main result in Table 6).

In addition, I perform falsification tests using placebo reform dates to check for significant discontinuities in outcomes of interest at points further away from the true reform date. Frequent detection of such discontinuities would cast doubt on the validity of the design, method of statistical inference, or both. To implement this, I re-estimate the baseline regression discontinuity model (Equation 3) using placebo cutoff dates obtained by shifting the true cutoff date backward and forward in one-day or one-week increments.

Figure B3 plots the distributions of t-statistics from these placebo estimates for the conviction outcome. Panel A shifts the cutoff date in one-day increments from 180 days before to 180 days after the reform. Out of 360 placebo estimates, only two have t-statistics larger than the main estimate in absolute value, suggesting a significance level below one percent. When applying one-week increments in Panel B, from 26 weeks before the reform to 26 weeks after the reform, none of the 52 placebo t-statistics exceeds the main estimate in absolute value. Similarly, Figure B4 shows the placebo distributions for the incarceration outcome. The distributions imply a significance level below one percent in both increment schemes. Overall, I find little evidence of significant effects at placebo reform dates, reinforcing the validity of the main findings.

7 Conclusion

Discretion in resource allocation can enable informed decision-making, but can also create opportunities for bias and corruption. In this paper, I use exogenous variation in judicial discretion in indigent defense case assignment to test whether receiving extra cases through discretion *caused* attorneys to donate to judges, and whether the removal of discretion benefited or harmed defendants. Leveraging a 2015 reform in Travis County, Texas that eliminated judicial discretion over case assignments, I find that restricting judicial discretion led to a decline in campaign donations from court-appointed attorneys. Specifically, the difference-in-differences estimates indicate that for the average attorney, the elimination of judicial discretion reduced the likelihood of donating by 19 percent. Results are statistically significant at the one percent level and robust to the inclusion of various fixed effects. This result indicates that there was reciprocity between judges and the attorneys to whom they assigned cases, and at least the appearance of *quid pro quo*.

I also examine whether removing judicial discretion improved or worsened indigent defendant outcomes. I use a regression discontinuity in time design and a difference-in-differences design to assess the direct impact of the elimination of discretion on outcomes for low-income defendants. Estimates indicate that post-reform cases were 9-13 percent less likely to result in conviction and 9-17 percent less likely to result in incarceration.

The findings of this paper have important implications for both our understanding of discretion and its consequences, and for how to best implement a system that provides legal assistance to indigent defendants. The results demonstrate that even in a context where the parties are aware of conflicts of interest, and where there is much concern about whether the system is doing a satisfactory job of assisting a vulnerable population that it was designed to help, discretion can cause the appearance, if not realization, of *quid pro quo*. Moreover, the results demonstrate that removing that discretion benefits those vulnerable populations.

These results demonstrate that discretion can have important drawbacks with respect to (at minimum) the appearance of impropriety, as well as the effectiveness of the institution at benefitting those it was designed to benefit. In addition, results indicate that removing discretion in case assignment by elected judges can be a cost-effective way of improving the quality of legal defense provided to indigent defendants.

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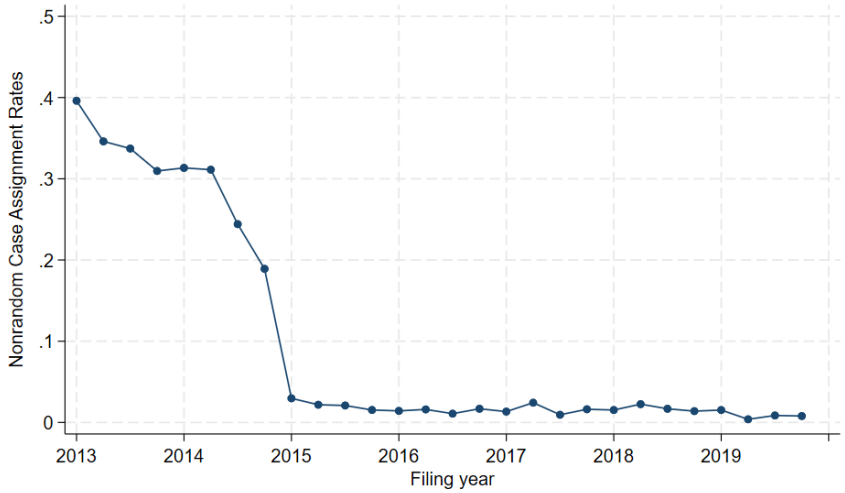
8 Tables and figures

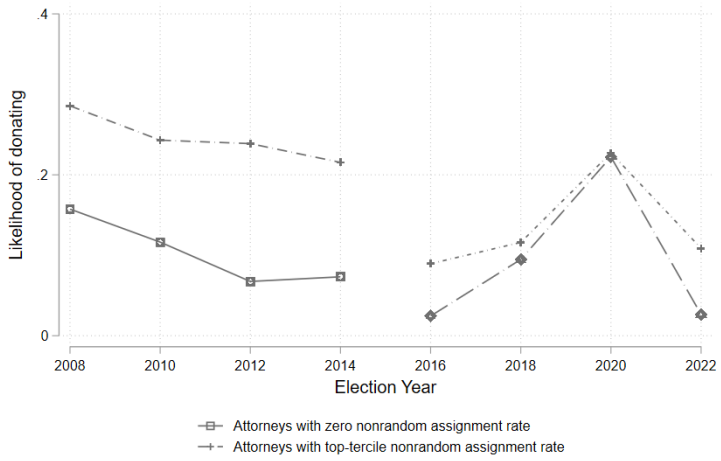
Figures

Figure 1: Discretionary assignment rate over time

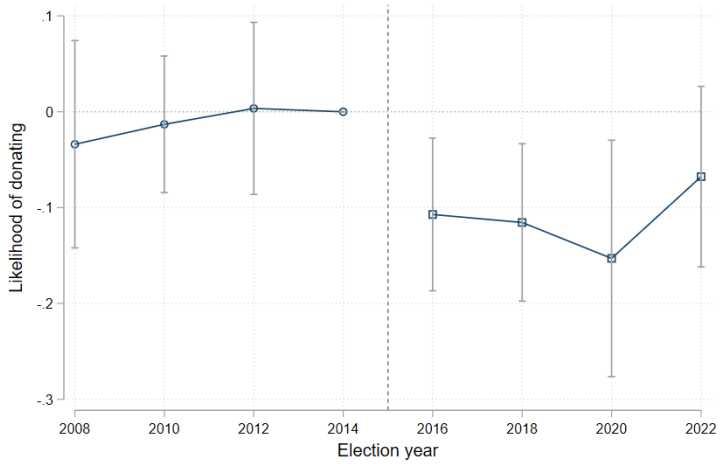


Notes: This figure shows the trend in the rate at which judges override random attorney assignments. The nonrandom assignment rate by judges ("bench rate") began to decline after the reform was implemented in January 2015.

Figure 2: Parallel trends assumption (2×2) - likelihood of making donations



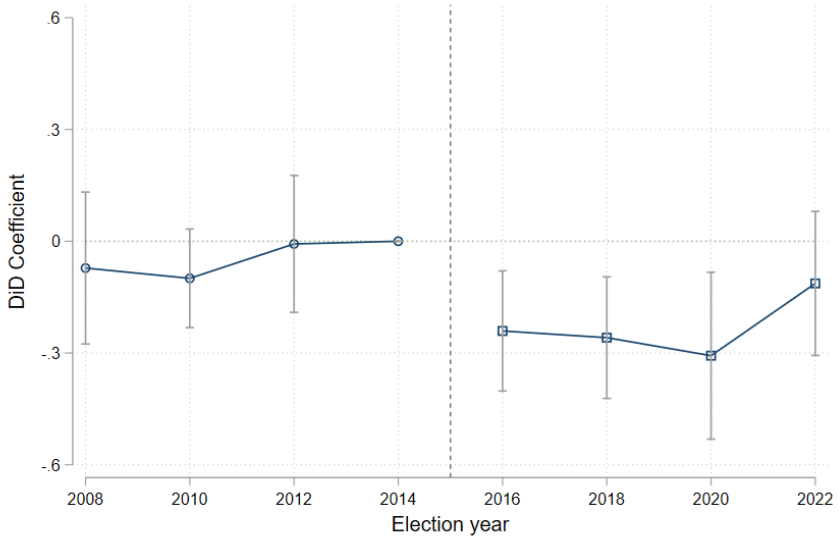
Panel A: Trends in the likelihood of donating



Panel B: Event study estimation

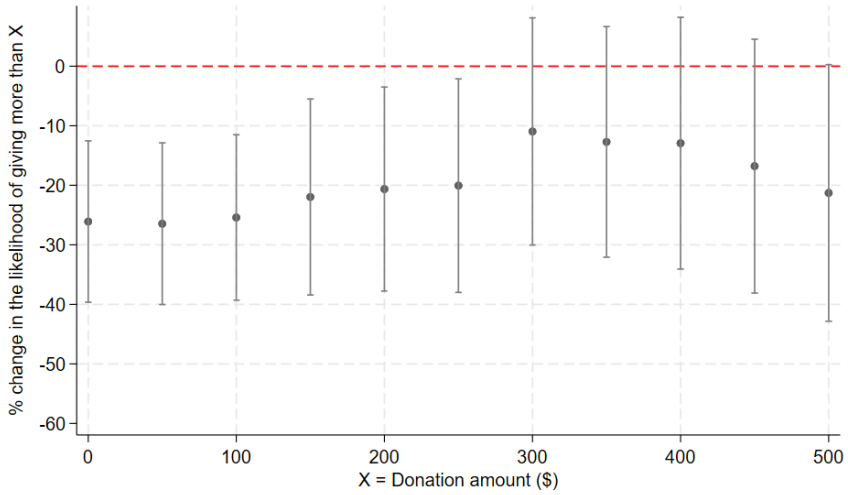
Notes: These figures show evidence in support of the parallel trends assumption for the likelihood of donating. Panel A descriptively plots the average donation likelihood for each group over time, while Panel B plots event-study estimates comparing the two attorney groups.

Figure 3: Parallel trends assumption (Continuous treatment) - likelihood of making donations



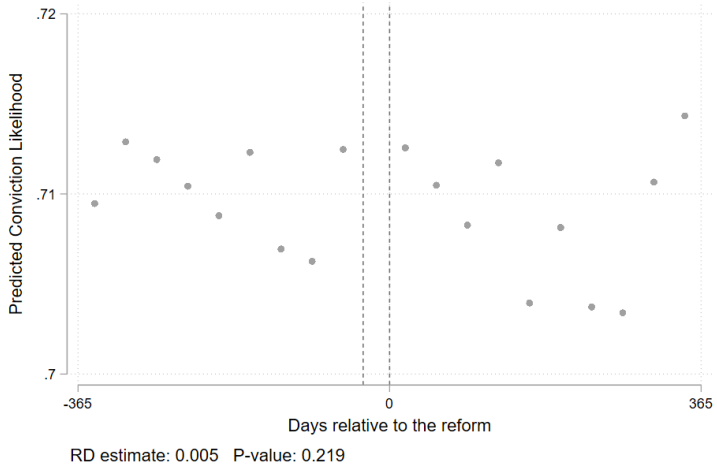
Notes: This figure shows the main DiD coefficients in the event study model (Equation 2) for each year, providing evidence in support of the parallel trends assumption. Pre-reform estimates show no divergence in the likelihood of donating between attorneys with varying levels of bench rate. The dashed line indicates the year when the reform took place.

Figure 4: Differences-in-Differences estimates - likelihood of giving more than X dollars



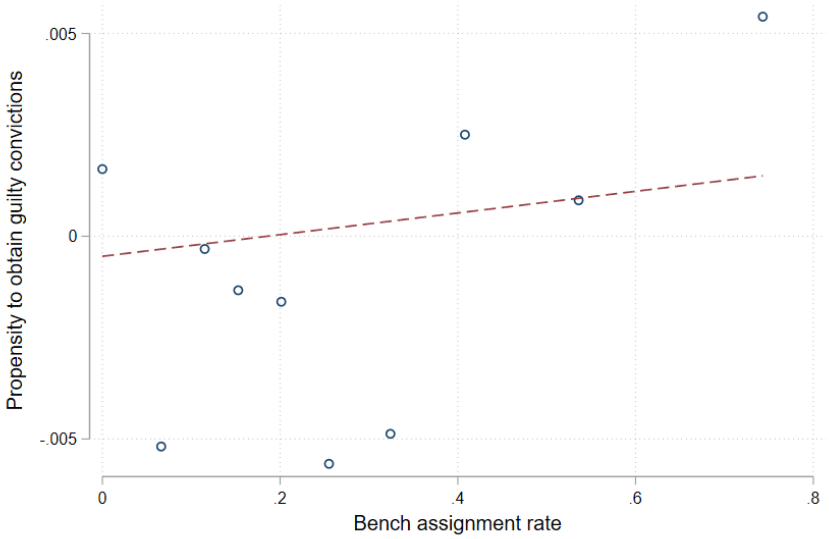
Notes: This figure shows difference-in-differences estimates of the reform's impact on the likelihood of donating more than X dollars. The Y-axis represents the percentage change in the likelihood associated with a one standard deviation increase in treatment intensity, measured relative to the outcome mean. For example, the third estimate indicates that a one standard deviation increase in the bench rate is associated with a 25 percent increase in the likelihood of donating more than $100 after the reform, compared to the pre-reform period.

Figure 5: Testing for manipulation - Predicted conviction outcome



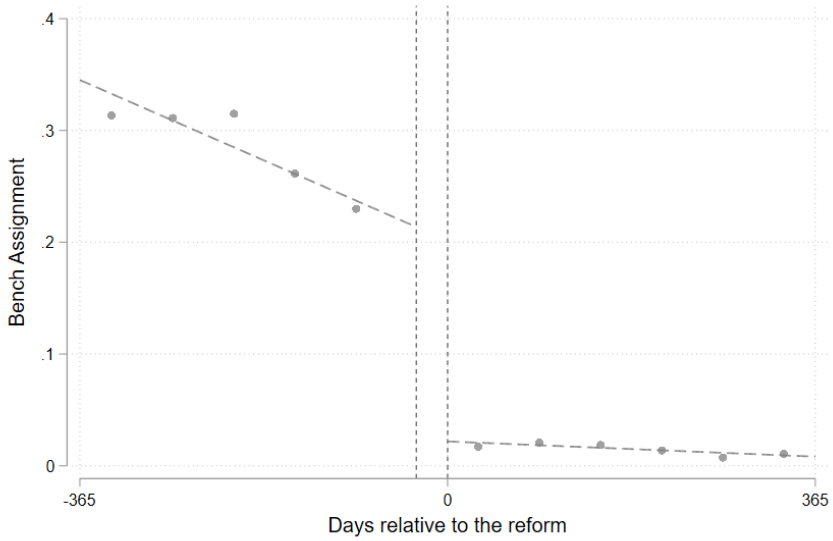
Notes: This figure tests for discontinuities in the predicted likelihood of conviction for cases filed around the cutoff date. To predict conviction likelihoods for cases in the main sample, I first estimate a model using felony cases filed in Travis County between 2002 and 2012, regressing a guilty conviction dummy on observable characteristics. I then apply these out-of-sample estimates to cases in the main sample to compute their predicted conviction probabilities. Robust bias-corrected RD estimates (Calonico et al., 2014) show no statistically significant discontinuity at the cutoff.

Figure 6: Individual attorney effects



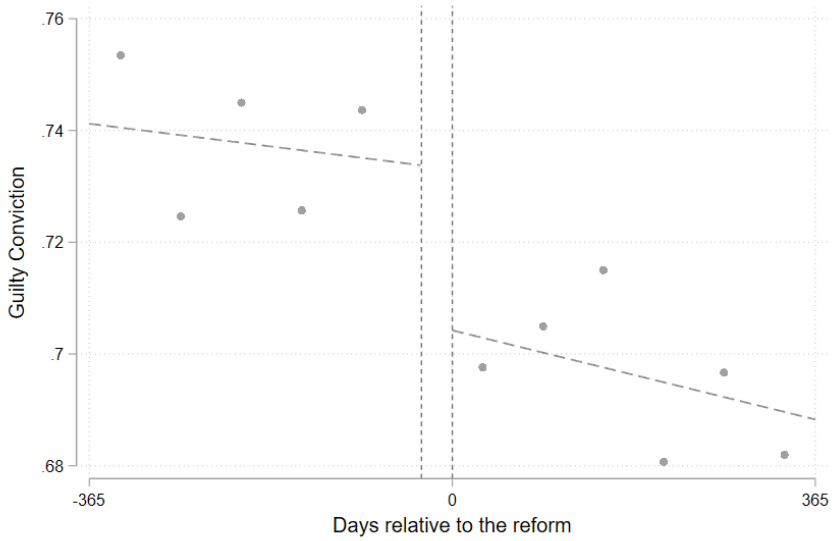
Notes: This figure shows a relationship between individual attorney (shrunken) effects in terms of propensity for guilty conviction and pre-reform discretionary case assignment rate. Note that the greater the attorney quality measure on a vertical axis, the less effective an attorney is. The binscatter indicates that attorneys with higher bench rates are likely less effective than those with lower rates, though the correlation is not estimated to be significant at conventional levels (coefficient = 0.003, standard error = 0.005).

Figure 7: Regression discontinuity plot - First-stage effects

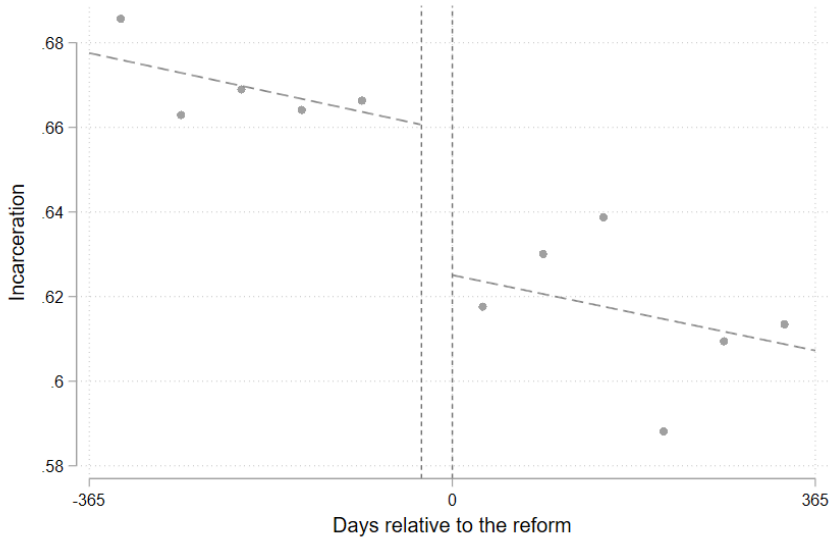


Notes: This figure shows discontinuity in the likelihood of discretionary assignment for cases filed around the cutoff date. Using the robust bias-corrected estimator for statistical inference (Calonico et al., 2014), I estimate a discontinuity of -0.130 (standard error = 0.0249) around the cutoff date, which is statistically significant at the 1 percent level. The gray circles show binned averages for the y-axis variable, and the dashed lines threading the binned averages represent local linear fits. The analysis uses a 365-day bandwidth and uniform kernel weighting function.

Figure 8: Regression discontinuity plot - Indigent case outcomes



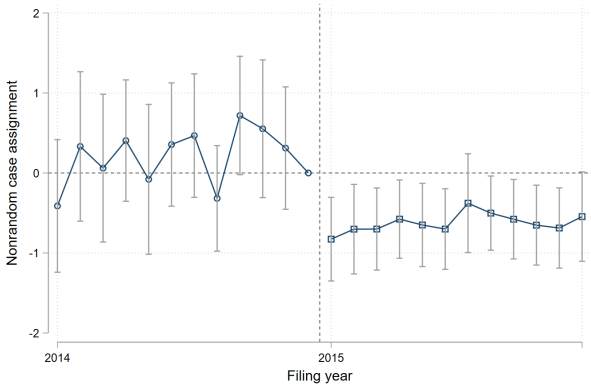
Panel A: Guilty conviction



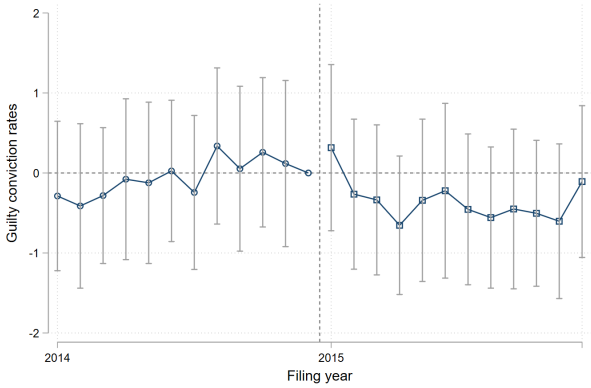
Panel B: Incarceration sentence

Notes: These figures show discontinuity in the case outcomes for cases filed around the cutoff date. The gray circles show binned averages for the y-axis variable, and the dashed lines threading the binned averages represent local linear fits. The analysis uses a 365-day bandwidth and uniform kernel weighting function. Robust bias-corrected estimates (Calonico et al., 2014) are reported in Table 6.

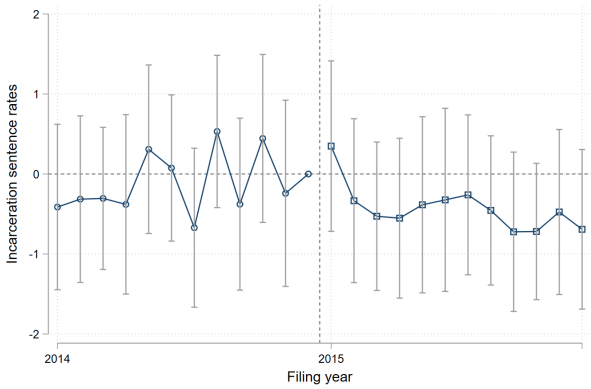
Figure 9: Parallel trends assumption - using predicted bench rates as treatment intensity



Panel A: Bench assignment (First-stage)



Panel B: Guilty conviction



Panel C: Incarceration sentence

Notes: These figures show the DiD coefficients from the event-study framework by month, providing evidence in support of the parallel trends assumption. The pre-reform estimates show no divergence in the legal outcomes of case types with varying levels of predicted bench rates. The dashed line indicates the year when the reform took place.

Tables

Table 1: Summary Statistics

	Overall	Pre-reform	Post-reform
Panel A: Donation Outcomes			
Pre-reform nonrandom assignment rate	0.186		
	(0.223)		
Likelihood of donating	0.138	0.165	0.105
	(0.344)	(0.372)	(0.306)
Donation amount ($)	66.005	84.964	43.600
	(271.963)	(310.958)	(214.984)
Donation amount conditional on donating ($)	480.037	513.733	417.039
	(582.876)	(604.389)	(536.514)
Panel B: Defendant Characteristics			
Female	0.197	0.200	0.195
Age	34.081	34.080	34.081
Prior Offense	0.627	0.627	0.627
Black	0.328	0.318	0.337
Panel C: Case Characteristics			
Violent Crime	0.225	0.217	0.234
Property Crime	0.321	0.332	0.311
Sex Crime	0.035	0.032	0.039
DWI	0.043	0.044	0.042
Drug	0.290	0.294	0.287
Other Felonies	0.108	0.105	0.110
Panel D: Case Outcomes			
Any Convictions	0.717	0.737	0.697
Any Incarceration Sentences	0.643	0.670	0.616
Any Probation Sentences	0.169	0.168	0.169

Notes: This table summarizes the sample used in the main analysis. Panel A describes the main outcomes used in the attorney donation analysis. Panels B, C, and D summarize the sample used in the defendant outcome analysis. Standard deviations are reported in parentheses.

Table 2: Difference-in-Differences - 2×2 comparison

	(1)	(2)	(3)
	Top tercile	Zero bench	Difference-in-differences
Pre	0.240	0.098	
Post	0.127	0.086	
Difference	0.113	0.012	0.101

Notes: This table shows a simple representation of the 2×2 difference-in-differences approach. Each cell represents the average donation likelihood for a specific group in either pre- or post-reform. For example, the first row in Column (1) shows the donation likelihood for attorneys in the top tercile bench rate group in the pre-reform period.

Table 3: Difference-in-differences estimates (2×2) - Donor dummy

	(1)	(2)	(3)	(4)	(5)
Outcome: Donor Dummy					
Post*Top-tercile	-0.101***	-0.104***	-0.104***	-0.103***	-0.103***
	(0.0262)	(0.0272)	(0.0272)	(0.0271)	(0.0262)
Observations	2433	2433	2433	2433	2433
Outcome Mean in Pre-period	0.165	0.165	0.165	0.165	0.165
Unconditional	Y	N	N	N	N
Attorney FE	N	Y	Y	Y	Y
Judge FE	N	N	Y	Y	N
Cycle FE	N	N	N	Y	N
Cycle*Judge FE	N	N	N	N	Y

Standard errors in parentheses

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

Notes: This table reports the 2×2 difference-in-differences estimates of the effect of removing judicial discretion on the likelihood that attorneys make campaign donations. The coefficients compare attorneys in the top tercile bench rate group with attorneys in the zero bench rate group before and after the reform. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(5) sequentially add fixed effects as indicated at the bottom of the table. Robust standard errors, clustered at the attorney–judge level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Difference-in-Differences estimates (Continuous treatment) - likelihood of making donations

	(1)	(2)	(3)	(4)	(5)
Outcome: Donor Dummy					
Post*Bench Assignment Rate	-0.178***	-0.180***	-0.184***	-0.190***	-0.194***
	(0.0527)	(0.0540)	(0.0539)	(0.0533)	(0.0513)
Observations	3360	3360	3360	3360	3360
Estimates Associated w/ 1 SD	-0.040	-0.040	-0.041	-0.042	-0.043
Outcome Mean in Pre-period	0.165	0.165	0.165	0.165	0.165
Unconditional	Y	N	N	N	N
Attorney FE	N	Y	Y	Y	Y
Judge FE	N	N	Y	Y	N
Cycle FE	N	N	N	Y	N
Cycle*Judge FE	N	N	N	N	Y

Standard errors in parentheses

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

Notes: This table reports the difference-in-differences estimates of the effect of removing judicial discretion on the likelihood that attorneys make campaign donations. The coefficients compare "always-receiver" attorneys (treatment intensity = 1) with "never-receiver" attorneys (treatment intensity = 0) before and after the reform. I also report the effect associated with a one standard deviation increase in treatment intensity, along with the pre-reform mean of the outcome variable. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(5) sequentially add fixed effects as indicated at the bottom of the table. Robust standard errors, clustered at the attorney–judge level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Difference-in-Differences estimates - donation amount

	(1)	(2)	(3)	(4)	(5)
Outcome: Donation Amount (IHS)					
Post*Bench Assignment Rate	-1.133***	-1.144***	-1.174***	-1.212***	-1.247***
	(0.348)	(0.356)	(0.354)	(0.351)	(0.337)
Observations	3360	3360	3360	3360	3360
Estimates Associated w/ 1 SD	-0.252	-0.255	-0.261	-0.270	-0.278
Outcome Mean in Pre-period	84.964	84.964	84.964	84.964	84.964
Unconditional	Y	N	N	N	N
Attorney FE	N	Y	Y	Y	Y
Judge FE	N	N	Y	Y	N
Cycle FE	N	N	N	Y	N
Cycle*Judge FE	N	N	N	N	Y

Standard errors in parentheses

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

Notes: This table reports the difference-in-differences estimates of the effect of removing judicial discretion on the donation amount. To address the large number of zeros in the outcome variable, I apply the inverse hyperbolic sine transformation to donation amounts. The coefficients compare "always-receiver" attorneys (treatment intensity = 1) with "never-receiver" attorneys (treatment intensity = 0) before and after the reform. I also report the effect associated with a one standard deviation increase in treatment intensity, along with the pre-reform mean of the outcome variable. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(5) sequentially add fixed effects as indicated at the bottom of the table. Robust standard errors, clustered at the attorney–judge level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Regression discontinuity estimates - Case outcomes

	(1)	(2)	(3)	(4)
Panel A: Guilty Conviction				
Post-reform	-0.064**	-0.073**	-0.074**	-0.073**
	(0.031)	(0.031)	(0.030)	(0.030)
Observations	12137	12106	12106	12106
Outcome mean	0.737	0.737	0.737	0.737
Panel B: Incarceration Sentence				
Post-reform	-0.062*	-0.065**	-0.067**	-0.065**
	(0.033)	(0.033)	(0.032)	(0.032)
Observations	12137	12106	12106	12106
Outcome mean	0.669	0.669	0.669	0.669
Defendant characteristics	N	Y	Y	Y
Case characteristics	N	Y	Y	Y
Court fixed effects	N	N	Y	Y
Attorney fixed effects	N	N	N	Y

Notes: This table shows reduced-form RD estimates of the reform's impact on the indigent defense case outcomes using a robust bias-corrected estimator proposed in Calonico et al. (2014). Each panel reports results from estimating Equation 3 with guilty conviction and incarceration sentence dummies as outcome variables, respectively. Column (1) shows results from the most parsimonious specification. Column (2) controls for defendant and case characteristics, including defendant race, age, gender, prior offense, and offense type. Column (3) additionally includes court fixed effects, and Column (4) includes attorney fixed effects. The analysis uses a 365-day bandwidth and uniform kernel weighting function. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Difference-in-difference estimates - Case outcomes

	(1) Bench assignment	(2) Conviction	(3) Incarceration
Predicted bench rate× Post	-0.818***	-0.308**	-0.333**
	(0.136)	(0.139)	(0.152)
Observations	13191	13035	13035
Effects for the average case	-0.214	-0.080	-0.087
Outcome mean	0.261	0.737	0.670
Filing Month-Year FE	Y	Y	Y
Case Characteristics	Y	Y	Y
Court FE	Y	Y	Y
Attorney FE	Y	Y	Y

Standard errors in parentheses

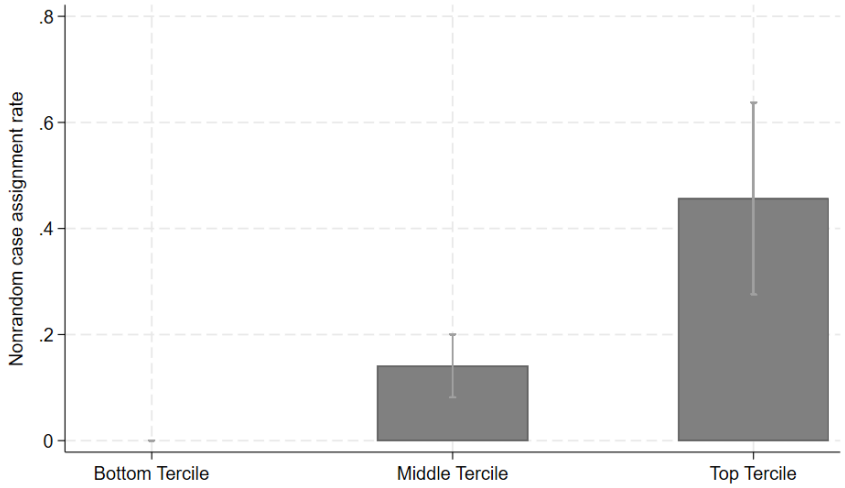
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports difference-in-differences estimates of the reform's impact on indigent defense outcomes, using variation in case-specific nonrandom assignment propensities. The estimates capture how outcomes for case types that were always nonrandomly assigned changed relative to those that were never nonrandomly assigned, before and after the reform. Each column corresponds to a different outcome. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

9 Online Appendix

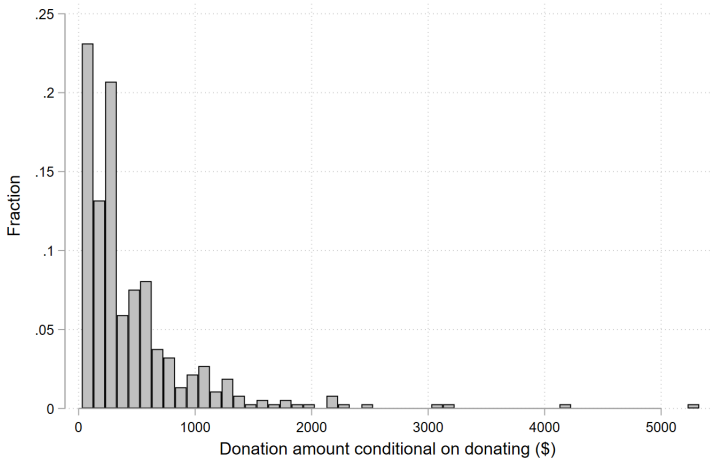
Appendix A: Additional figures and tables for the donation analysis

Figure A1: Pre-period nonrandom assignment rates by tercile group

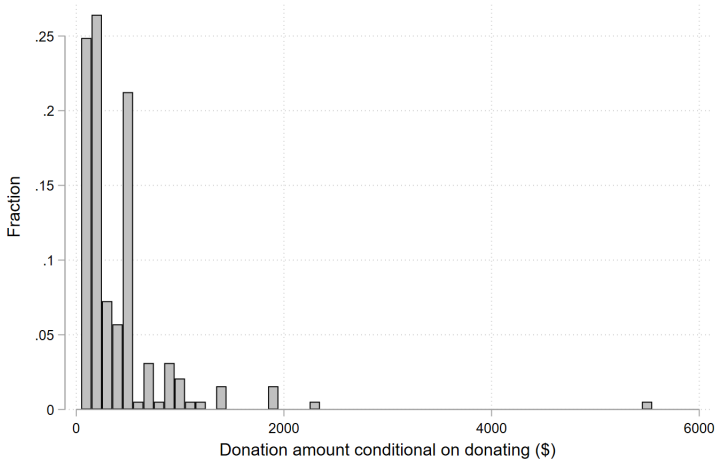


Notes: This figure shows mean nonrandom case assignment rates by tercile. While attorneys in the bottom tercile never received the discretionary assignments from the judge, those in the top tercile received over 40 percent of their cases at judicial discretion.

Figure A2: Distribution of donation amount



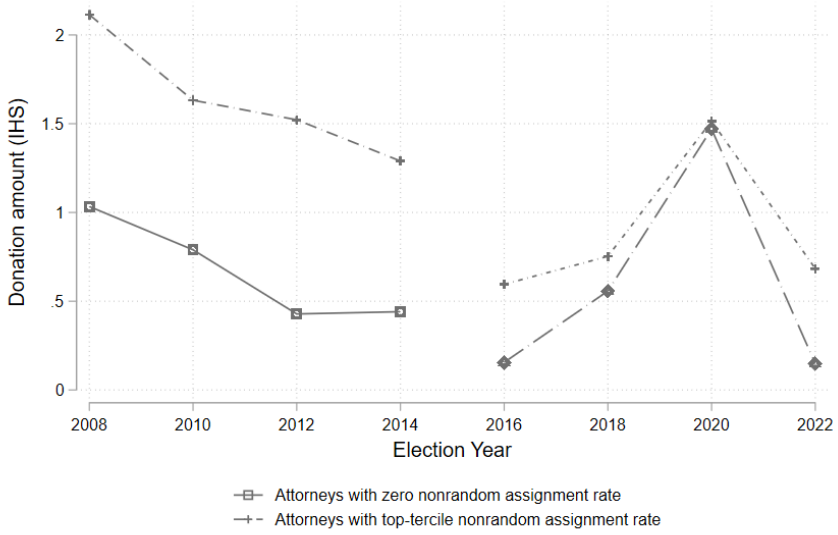
Panel A: Pre-reform distribution



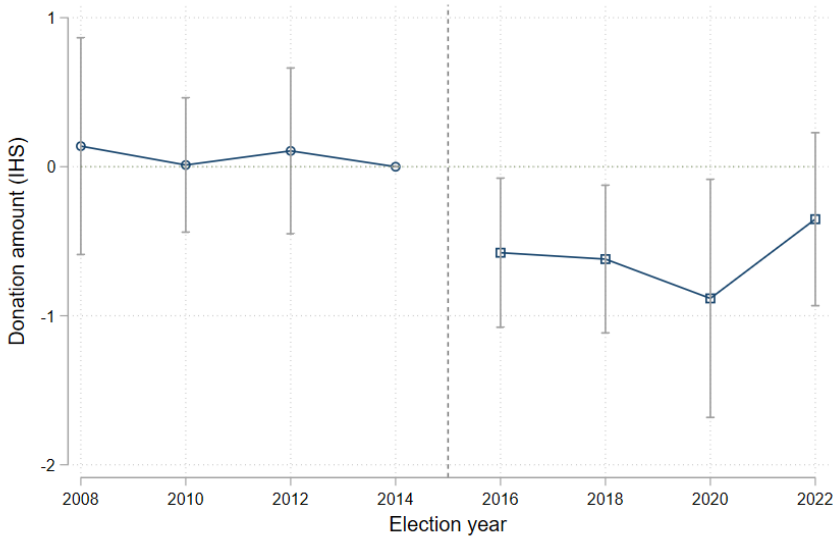
Panel B: Post-reform distribution

Notes: These figures show the distribution of donation amount conditional on donating. In the pre-reform period (Panel A), the conditional mean donation is $476 with a standard deviation of $563. In the post-reform period (Panel B), the conditional mean donation is $409 with a standard deviation of $517.

Figure A3: Parallel trends assumption (2×2) - Donation amount



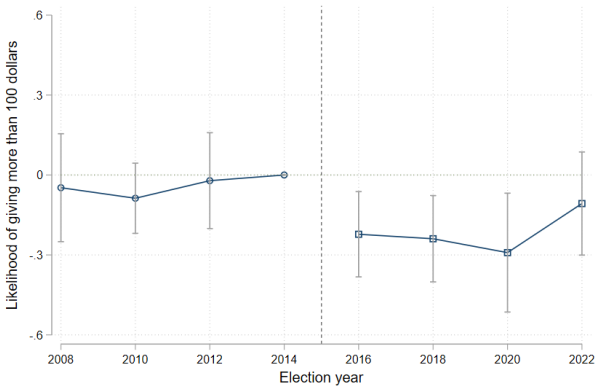
Panel A: Trends in the donation amount



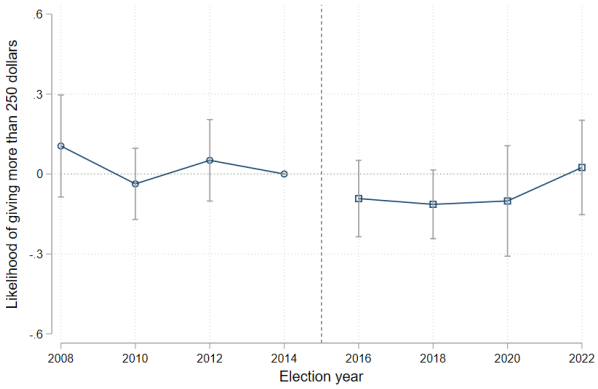
Panel B: Event study estimation

Notes: These figures show evidence in support of the parallel trends assumption for the donation amount. Panel A descriptively plots the average donation amount for each group over time, while Panel B plots event-study estimates comparing the two attorney groups.

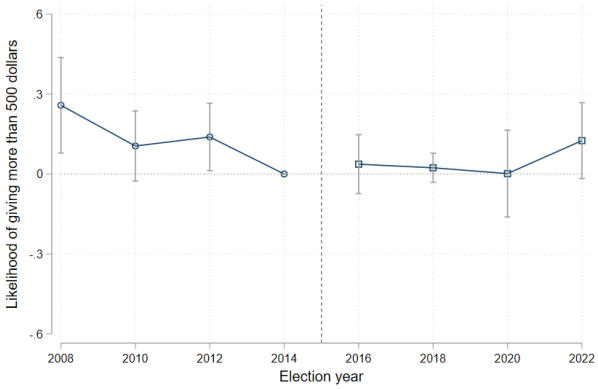
Figure A4: Parallel trends assumption - Likelihood of donating more than X dollars



Panel A: More than 100 dollars



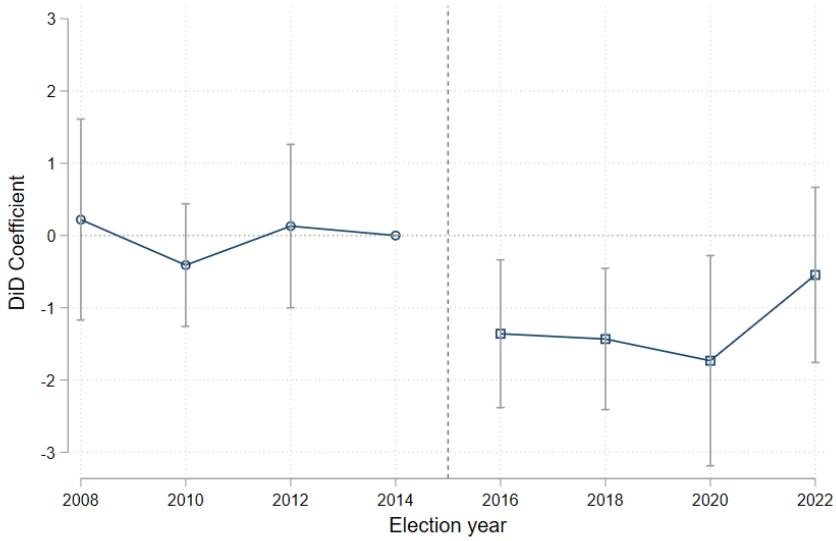
Panel B: More than 250 dollars



Panel B: More than 500 dollars

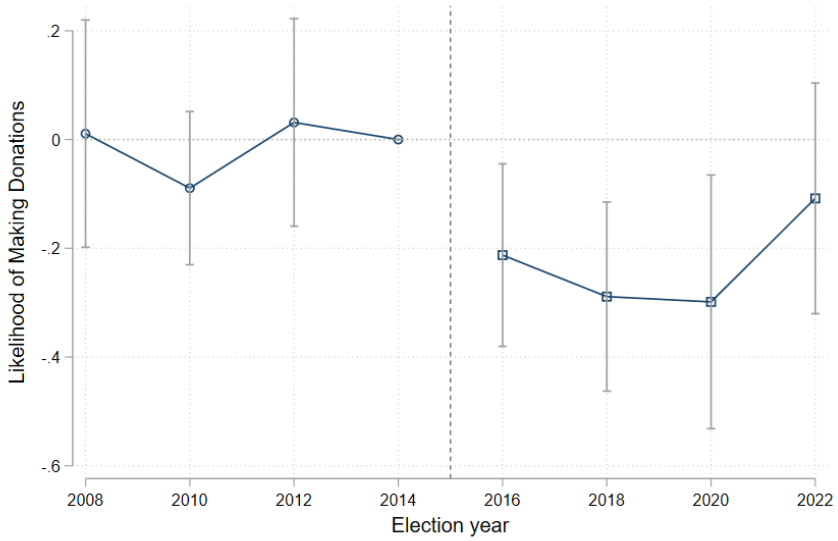
Notes: This figure shows the main DiD coefficients in the event study model (Equation 2) for each quarter, providing evidence in support of the parallel trends assumption. Pre-reform estimates show no divergence in guilty conviction rates (Panel A) and in incarceration sentence rates (Panel B) between attorneys with varying levels of bench rate. The dashed line indicates the year when the reform took place.

Figure A5: Parallel trends assumption - Donation amount



Notes: This figure shows the main DiD coefficients in Equation 2 for each year, providing evidence in support of the parallel trends assumption. Pre-reform estimates show no divergence in donation amount between attorneys with varying levels of bench rate. The dashed line indicates when the reform took place.

Figure A6: Parallel trend assumption - Alternative treatment intensity measure



Notes: This figure shows the main DiD coefficients in the event study model (Equation 2) for each year, providing evidence in support of the parallel trends assumption. In estimating the model, I calculate bench rates using cases assigned within four years before the reform (2011-2014). Pre-reform estimates show no divergence in the likelihood of donating between attorneys with varying levels of bench rate. The dashed line indicates when the reform took place.

Table A1: Difference-in-Differences - Alternative levels of clustering

	(1) Attorney*Judge level	(2) Attorney level	(3) Judge level	(4) Two-way clustering
Panel A: Donor dummy				
Post*Bench Assignment	-0.194***	-0.194***	-0.194**	-0.194**
P-value	[0.000]	[0.002]	[0.029]	[0.035]
95% CI	[-0.295, -0.093]	[-0.334, -0.054]	[-0.314, -0.073]	[-.3792, -.02208]
Outcome Mean in Pre-period	0.165	0.165	0.165	0.165
Panel B: Donation amount (IHS)				
Post*Bench Assignment	-1.247***	-1.247***	-1.247***	-1.247**
P-value	[0.000]	[0.002]	[0.002]	[0.045]
95% CI	[-1.907, -0.587]	[-2.206, -0.288]	[-2.206, -0.288]	[-2.53, -.05897]
Outcome Mean in Pre-period	84.964	84.964	84.964	84.964
Observations	3,360	3,360	3,360	3,360
Attorney FE	Y	Y	Y	Y
Cycle*Judge FE	Y	Y	Y	Y

Notes: This table reports the difference-in-differences estimates of the effect of removing judicial discretion on donation outcomes using alternative levels of clustering. Each column shows results from separate regressions with different clustering levels as indicated in the column headers. Column (1) presents results from the main specification. Columns (2) and (3) cluster standard error at the attorney and judge level, respectively. Column (4) reports p-values and 95% confidence intervals using wild bootstrap procedures. The coefficients compare "always-receiver" attorneys (treatment intensity = 1) with "never-receiver" attorneys (treatment intensity = 0) before and after the reform. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Difference-in-Differences estimates - Balance check

	(1) Experience (years)	(2) Law school ranking	(3) White	(4) Hispanic	(5) Black	(6) Male	(7) Disciplinary history
Bench assignment rate	2.948	-11.00	-0.159	0.123	0.0358	0.150	-0.0322
	(3.136)	(16.19)	(0.121)	(0.112)	(0.0566)	(0.121)	(0.0622)
Outcome mean	21.76	69.11	0.793	0.179	0.0286	0.721	0.0643

Notes: This table reports results from a balance check evaluating the correlation between bench rates and attorney characteristics. The results provide evidence supporting a key identifying assumption in the difference-in-differences with continuous treatment model (Callaway et al., 2024) that the "average treatment effect function" does not vary with the dose of treatment. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Difference-in-Differences estimates - Incarceration sentence

	(1)	(2)	(3)	(4)
Outcome: Incarceration				
Bench Rate*Post	0.02229	0.01252	0.02650	0.02697
	[0.383]	[0.623]	[0.240]	[0.238]
95% CI	[-0.0447, 0.0874]	[-0.0590, 0.0795]	[-0.0328, 0.0810]	[-0.0295, 0.0811]
Observations	25,733	25,733	25,655	25,655
Outcome Mean	0.671	0.671	0.671	0.671
Filing month*year FE	N	Y	Y	Y
Case Characteristics	N	N	Y	Y
Attorney FE	N	N	N	Y

Notes: This table reports the difference-in-differences estimates of the effect of removing judicial discretion on incarceration sentence rates. The goal of this exercise is to assess the degree to which judicial discretion induces better performance from the attorneys. For the purpose of this exercise, I exclude cases that were nonrandomly assigned by judges. The coefficients compare "always-receiver" attorneys (treatment intensity = 1) with "never-receiver" attorneys (treatment intensity = 0) before and after the reform. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(4) sequentially add fixed effects and control variables as indicated at the bottom of the table. Robust standard errors, clustered at the attorney–judge level, are reported in parentheses. Wild bootstrap with Webb weights (1000 replications) p-values are reported in brackets, with 95% confidence intervals below. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Difference-in-differences estimates (2×2) - Donation amount

	(1)	(2)	(3)	(4)	(5)
Outcome: Donation amount (IHS)					
Post*Top-tercile	-0.644***	-0.660***	-0.658***	-0.655***	-0.653***
	(0.170)	(0.177)	(0.177)	(0.176)	(0.170)
Observations	2433	2433	2433	2433	2433
Outcome Mean in Pre-period	84.964	84.964	84.964	84.964	84.964
Unconditional	Y	N	N	N	N
Attorney FE	N	Y	Y	Y	Y
Judge FE	N	N	Y	Y	N
Cycle FE	N	N	N	Y	N
Cycle*Judge FE	N	N	N	N	Y

Standard errors in parentheses

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

Notes: This table reports the 2×2 difference-in-differences estimates of the effect of removing judicial discretion on the donation amount. The coefficients compare attorneys in the top tercile bench rate group with attorneys in the zero bench rate group before and after the reform. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(5) sequentially add fixed effects as indicated at the bottom of the table. Robust standard errors, clustered at the attorney–judge level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

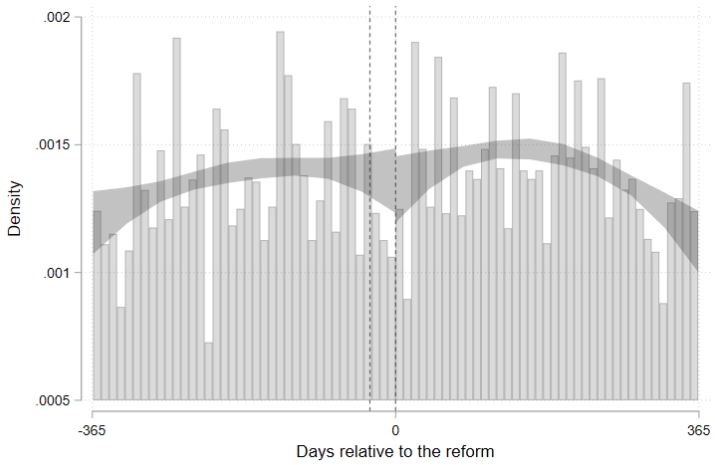
Table A5: Difference-in-differences estimates - alternative treatment intensity measure

	(1)	(2)	(3)	(4)	(5)
Outcome: Donor Dummy					
Post*Bench Assignment Rate	-0.204***	-0.201***	-0.205***	-0.211***	-0.219***
	(0.0541)	(0.0559)	(0.0557)	(0.0551)	(0.0533)
Observations	3360	3360	3360	3360	3360
Estimates Associated w/ 1 SD	-0.044	-0.043	-0.044	-0.046	-0.047
Outcome Mean in Pre-period	0.165	0.165	0.165	0.165	0.165
Unconditional	Y	N	N	N	N
Attorney FE	N	Y	Y	Y	Y
Judge FE	N	N	Y	Y	N
Cycle FE	N	N	Y	Y	N
Cycle*Judge FE	N	N	N	N	Y

Notes: This table reports the difference-in-differences estimates of the effect of removing judicial discretion on the likelihood that attorneys make campaign donations. In estimating the model, I calculate bench rates using cases assigned within four years before the reform. The coefficients compare "always-receiver" attorneys (treatment intensity = 1) with "never-receiver" attorneys (treatment intensity = 0) before and after the reform. I also report the effect associated with a one standard deviation increase in treatment intensity, along with the pre-reform mean of the outcome variable. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(5) sequentially add fixed effects as indicated at the bottom of the table. Robust standard errors, clustered at the attorney–judge level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

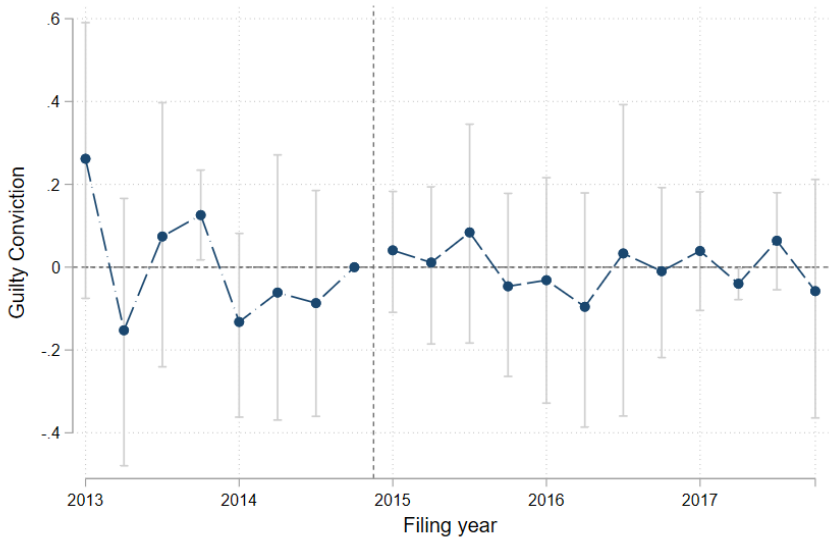
Appendix B: Additional figures and tables for the defendant outcome analysis

Figure B1: Testing for manipulation - Case density by filing date

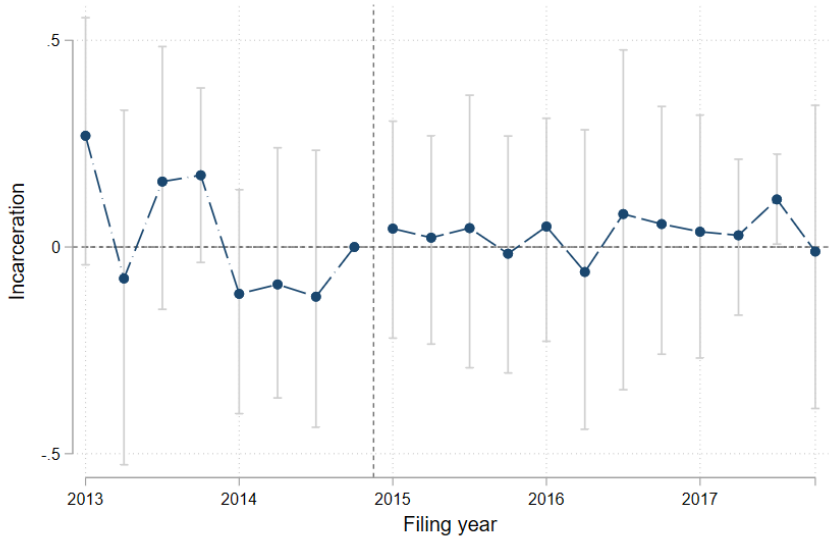


Notes: This figure shows the density of cases filed for 365 days on each side of the cutoff date. The goal of this exercise is to assess whether there is sorting around the cutoff. The p-value from the regression discontinuity test outlined in Cattaneo et al. (2020) is 0.8394, which indicates that I fail to reject the null hypothesis of balanced case density.

Figure B2: Differences-in-Differences estimates - Attorney performance



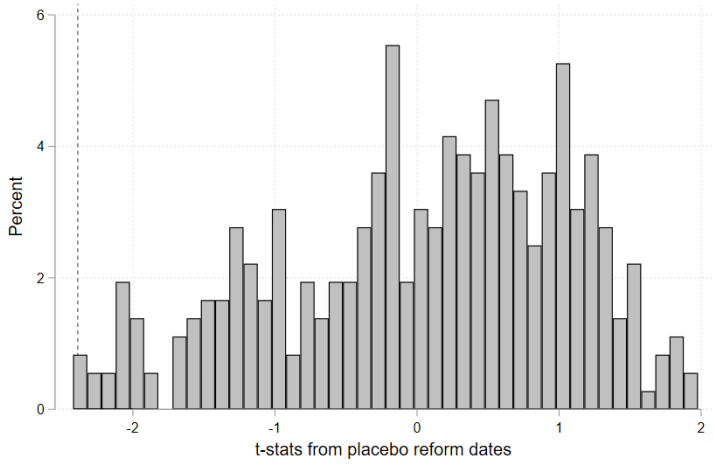
Panel A: Guilty conviction



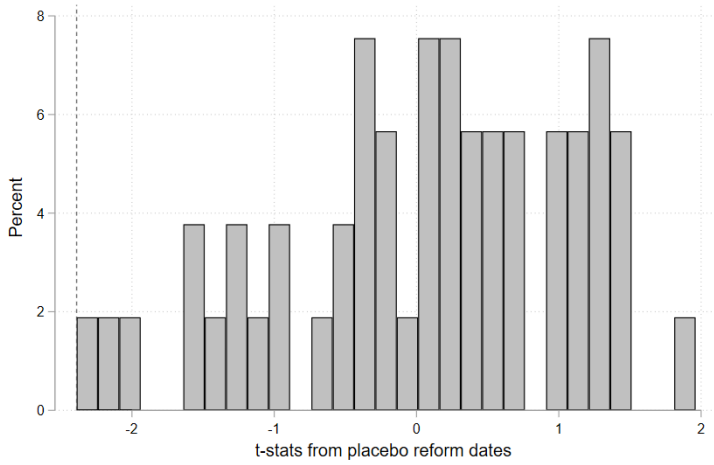
Panel B: Incarceration

Notes: This figure shows the main DiD coefficients in the event study model (Equation 2) for each quarter, providing evidence in support of the parallel trends assumption. Pre-reform estimates show no divergence in guilty conviction rates (Panel A) and in incarceration sentence rates (Panel B) between attorneys with varying levels of bench rate. The dashed line indicates the year when the reform took place.

Figure B3: Robustness - Placebo RD estimates of guilty conviction outcome



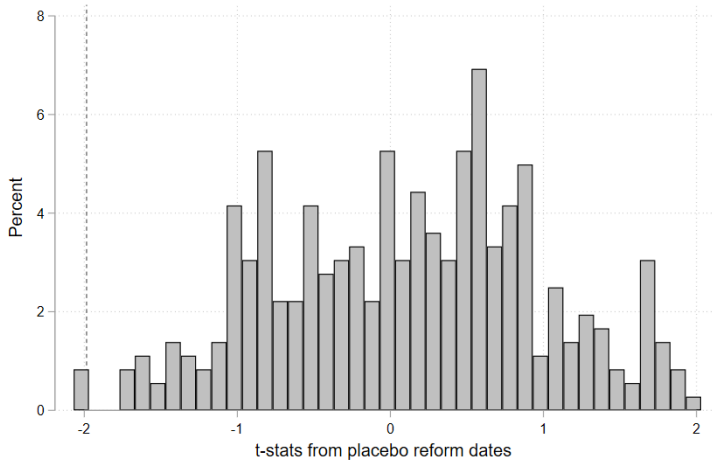
Panel A: 1-day increments



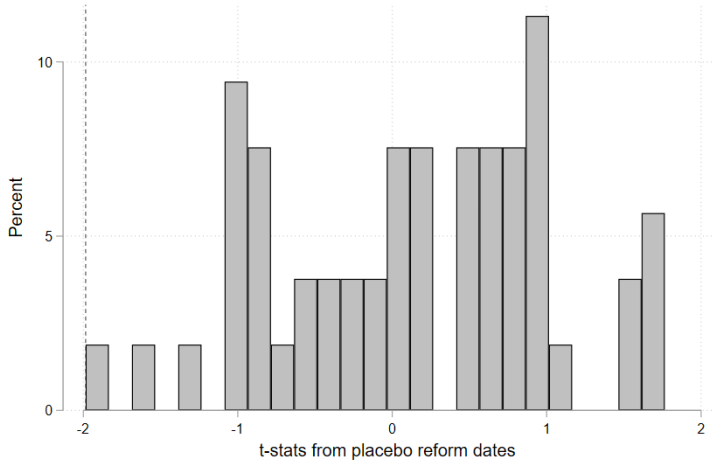
Panel B: 1-week increments

Notes: These figures show the distribution of t-statistics using placebo cutoff dates obtained by shifting the cutoff date backward and forward in one-day or one-week increments. The vertical dotted line represents the t-statistics from using the true cutoff date.

Figure B4: Robustness - Placebo RD estimates of incarceration outcome



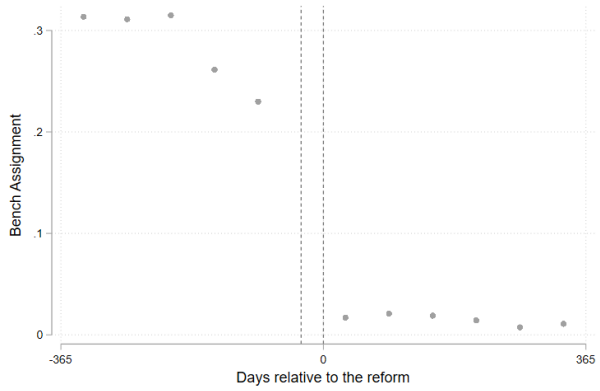
Panel A: 1-day increments



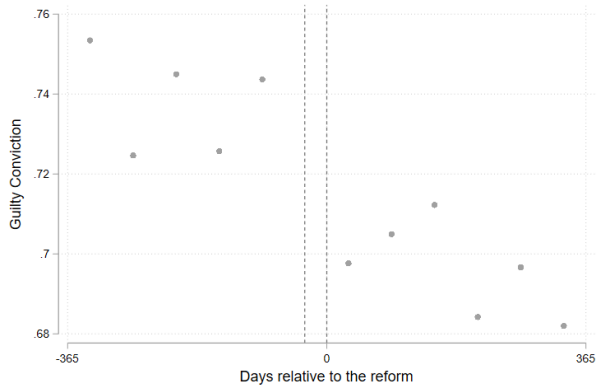
Panel B: 1-week increments

Notes: These figures show the distribution of t-statistics using placebo cutoff dates obtained by shifting the cutoff date backward and forward in one-day or one-week increments. The vertical dotted line represents the t-statistics from using the true cutoff date.

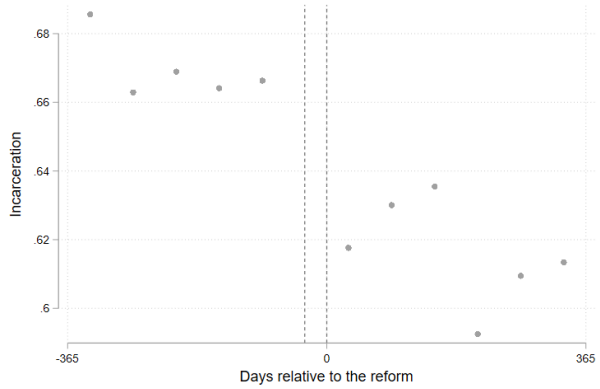
Figure B5: Regression discontinuity plot - Using a bias-corrected estimator



Panel A: Discretionary case assignment



Panel B: Guilty conviction



Panel C: Incarceration sentence

Notes: These figures show discontinuity in outcome variables of interest for cases filed around the cutoff date. To closely approximate the bias-corrected discontinuity estimates in Table 6, the analysis applies a local quadratic polynomial with a 365-day bandwidth and a uniform kernel weighting function.

Table B1: Difference-in-Differences estimates - Guilty conviction

	(1)	(2)	(3)	(4)
Outcome: Guilty Conviction				
Bench Rate*Post	0.00626	-0.00070	0.01128	0.00583
	[0.765]	[0.971]	[0.602]	[0.816]
95% CI	[-0.0473, 0.0591]	[-0.0549, 0.0505]	[-0.0431, 0.0641]	[-0.0549, 0.0651]
Observations	25,733	25,733	25,655	25,655
Outcome Mean	0.738	0.738	0.738	0.738
Filing month*year FE	N	Y	Y	Y
Case Characteristics	N	N	Y	Y
Attorney FE	N	N	N	Y

Standard errors in parentheses

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

Notes: This table reports the difference-in-differences estimates of the effect of removing judicial discretion on guilty conviction rates. The goal of this exercise is to assess whether judicial discretion induces better performance from the attorneys. For the purpose of this exercise, I exclude cases that were nonrandomly assigned by judges. The coefficients compare "always-receiver" attorneys (treatment intensity = 1) with "never-receiver" attorneys (treatment intensity = 0) before and after the reform. Column (1) presents the most parsimonious specification, including only the post-reform indicator, bench rate, and their interaction. Columns (2)–(4) sequentially add fixed effects and control variables as indicated at the bottom of the table. Robust standard errors, two-way clustered at the attorney and judge level, are reported in parentheses. Wild bootstrap with Webb weights (1000 replications) p-values are reported in brackets, with 95% confidence intervals below. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B2: RDD estimates - Probation sentence

	(1)	(2)	(3)	(4)
Outcome: Probation				
Post-reform	0.006	-0.004	-0.004	-0.015
	(0.026)	(0.025)	(0.025)	(0.025)
Observations	12137	12106	12106	12106
Outcome mean	0.175	0.175	0.175	0.175
Defendant characteristics	N	Y	Y	Y
Case characteristics	N	Y	Y	Y
Court fixed effects	N	N	Y	Y
Attorney fixed effects	N	N	N	Y

Standard errors in parentheses

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

Notes: This table shows reduced-form RD estimates of the reform's impact on the probation sentence rates using a robust bias-corrected estimator proposed in Calonico et al. (2014). Column (1) shows results from the most parsimonious specification. Column (2) controls for defendant and case characteristics, including defendant race, age, gender, prior offense, and offense type. Column (3) additionally includes court fixed effects, and Column (4) includes attorney fixed effects. The analysis uses a 365-day bandwidth and uniform kernel weighting function. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B3: RDD estimates - Heterogeneity analysis

	(1) Violent	(2) Property	(3) Sex crimes	(4) DWI	(5) Drug	(6) Others	(7) Severe
Panel A: Guilty Conviction							
Post-reform	-0.062	-0.094*	0.084	0.031	-0.042	-0.013	0.026
	(0.068)	(0.050)	(0.173)	(0.066)	(0.056)	(0.099)	(0.148)
Observations	2801	4041	444	535	3665	1346	661
Outcome mean	0.704	0.767	0.715	0.945	0.718	0.643	0.591
Panel B: Incarceration Sentence							
Post-reform	-0.096	-0.075	-0.040	0.010	-0.016	0.016	-0.061
	(0.071)	(0.053)	(0.176)	(0.146)	(0.058)	(0.102)	(0.150)
Observations	2801	4041	444	535	3665	1346	661
Outcome mean	0.622	0.708	0.663	0.756	0.665	0.584	0.505
Defendant characteristics	Y	Y	Y	Y	Y	Y	Y

Notes: This table shows reduced-form RD estimates of the reform's impact on indigent defense case outcomes by case types using the robust bias-corrected estimator proposed in Calonico et al. (2014). Each column represents separate regressions using different subgroups based on case types. Each panel reports results from estimating Equation 3 with guilty conviction and incarceration sentence dummies as outcome variables, respectively. All specifications control for defendant characteristics, including defendant race, age, gender, and prior offense. In Column (7) using severe felony cases (capital felony or first-degree felony), I additionally control for case types. The analysis uses a 365-day bandwidth and a uniform kernel weighting function. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4: Regression discontinuity estimates - Alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Guilty Conviction						
Post-reform	-0.073**	-0.071**	-0.046*	-0.061**	-0.087**	-0.078*
	(0.031)	(0.034)	(0.024)	(0.026)	(0.035)	(0.044)
Observations	12105	12105	16737	16708	5327	5074
Outcome mean	0.737	0.737	0.739	0.739	0.733	0.736
Panel B: Incarceration Sentence						
Post-reform	-0.065**	-0.068*	-0.043*	-0.056**	-0.056	-0.081
	(0.033)	(0.036)	(0.025)	(0.028)	(0.042)	(0.049)
Observations	12105	12105	16737	16708	4887	4903
Outcome mean	0.669	0.669	0.669	0.669	0.669	0.669
Defendant characteristics	Y	Y	Y	Y	Y	Y
Case characteristics	Y	Y	Y	Y	Y	Y
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Bandwidth	365	365	500	500	Optimal	Optimal

Notes: This table shows robustness of the main results in Table 6 to alternative RD specifications as indicated at the bottom of the table. Each panel reports results from estimating Equation 3 with guilty conviction and incarceration sentence dummies as outcome variables, respectively. Each column shows results from separate regression models using a robust bias-corrected estimator proposed in Calonico et al. (2014). Column (1) shows results using the main specification in Table 6. All specifications control for defendant and case characteristics, including defendant race, age, gender, prior offense, and offense type. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$