

Does Public Financing Affect Judicial Behavior?

Evidence from the North Carolina Supreme Court

Morgan L. W. Hazelton
Department of Political Science
Saint Louis University
hazeltonml@slu.edu

Jacob M. Montgomery
Department of Political Science
Washington University in St. Louis
jacob.montgomery@wustl.edu

Brendan Nyhan
Department of Government
Dartmouth College
nyhan@dartmouth.edu

ABSTRACT

Many observers are concerned that campaign contributions could affect the decisions of elected judges. However, the empirical correlation between contributions and judicial decisions is consistent with two different explanations of judicial behavior: (a) money influences judges; or (b) contributors choose to support candidates with a similar philosophical or legal perspective. In this paper, we take advantage of North Carolina's shift to a voluntary public finance system for state Supreme Court candidates to obtain more credible estimates of the contributions-behavior relationship. Applying a difference-in-differences research design, we provide evidence that justices who opted into public financing became relatively less favorable toward attorney donors. We also find partial support for our hypothesis that participating justices became more moderate in their voting patterns. Taken together, these findings suggest that public financing reduced responsiveness to donors among participating justices.

We are grateful to James Gibson, Chris Bonneau, Andrew Martin, Mark McKenzie, Anne O'Connell, and audiences at the Midwest Political Science Association annual meeting and Political Economy and Public Law Conference for helpful comments. We also thank Tessa Bazier, Brigid Kilcoin, Suzanne O'Gawa, Benjamin Prager, Derek Sutton, and Nelson Wong for excellent research assistance.

1 INTRODUCTION

Do campaign contributions affect the decisions of elected officials? If donors receive favored treatment as some have alleged, it represents a distortion of the democratic system and undermines the legitimacy of existing campaign finance regulations. The possibility of contributor influence is especially troubling for elected judges, who have a professional responsibility to serve as dispassionate arbiters of the law but face conflicts of interest when attorney-contributors represent clients in cases before them. Members of the bar frequently have business before state courts and contribute a significant proportion of the money raised in judicial elections. As a result, observers worry that justices may be biased, either consciously or unconsciously, in favor of attorneys who supported their election bids in the past. This concern is so pronounced that the Supreme Court cited it in a 2015 decision (*Williams-Yulee v. Florida Bar*) as among the chief reasons for finding the states have a compelling interest in restricting the fundraising of candidates for judicial office: “Potential litigants then fear that ‘the integrity of the judicial system has been compromised, forcing them to search for an attorney in part based upon the criteria of which attorneys have made the obligatory contributions’” (*Simes v. Arkansas Judicial Discipline & Disability Comm’n*, 247 S. W. 3d 876, 882 [2007]).

A related concern is that donors exert undue influence on the judicial philosophies of elected judges. Numerous scholars have argued that the interests of contributors are better represented than the interests of the average citizen in various political institutions (e.g., Bartels, 2008). In particular, activist influence has been linked to growing levels of ideological extremism and polarization (Layman et al., 2010), suggesting that donors in judicial elections may contribute to increased levels of extremism on the bench.¹

These suspicions about the influence of money on judicial behavior have have been exacerbated by the marked increase in spending on judicial elections in recent years (Sample et al., 2010). Fears about the corrupting influence of money have escalated to alarming heights with prominent critics

¹We use the term extremism throughout this manuscript to indicate distance from the ideological center. Our use of the term is intended to be descriptive and not pejorative.

claiming that this trend “threatens to destabilize fair and impartial courts” (Brandenburg, 2012, 1). For instance, former Supreme Court Justice Sandra Day O’Connor has written that, if “[l]eft unaddressed, the perception that justice is for sale will undermine the rule of law that the courts are supposed to uphold” (quoted in Sample et al., 2010).

Despite growing concerns among policymakers, reformers, legal observers, and even the public, the political science literature has struggled to provide a clear answer about the effect of judicial contributions. Is the recent flood of money into the judicial system corrupting justice? Are judges beholden to their donors? If so, what reforms would ameliorate these problems? With only a few exceptions discussed below, the quantitative political science literature has failed to address these issues effectively.

The problem, of course, is the inherent difficulty of establishing the direction of the contributions-behavior relationship, which hinders research on the effect of campaign contributions on judicial decision-making. While it is possible to demonstrate a correlation between contributions and behavior in nearly any electoral institution, providing credible evidence that such a correlation represents a *causal* effect is much more difficult. Any such correspondence could be the result of elected officials taking actions that are biased toward contributors or the more prosaic tendency for contributors to support candidates with similar philosophical or ideological perspectives.

These possibilities have dramatically different normative implications for the proper functioning of the judicial system. Observing a correlation between the interests of contributors and the decisions of justices *may* demonstrate that justices are influenced by contributions. Special treatment of this kind—even if unconscious or subtle—threatens the legitimacy of judicial institutions and undermines the notion that all citizens are equal before the law. Alternatively, contributors may give to candidates whom they anticipate will be favorable to their interests. Candidates for the high court are often well-known in the legal community. Contributors may simply forecast candidate’s future behavior in office and choose to support candidates with similar ideological perspectives. In this scenario, a positive correlation between contributions and voting simply reflects the ability of donors to anticipate judicial decision-making.

As a result of these challenges, most scholars of judicial behavior have neglected either the topic or the inferential challenges it presents. Failing to study the effects of judicial contributions is unsatisfying and contributes to the marginalization of the discipline in contemporary debates (Lupia & Aldrich, 2014). However, research that does not address the causal inference issues described above can sometimes be counterproductive. Palmer & Levendis (2008, 1291), for instance, used correlational data to “question the voting behavior of Louisiana’s highest court,” an approach that was sharply criticized by fellow scholars (Newman et al., 2009; Tully & Gay, 2009) and resulted in the Dean of Tulane Law School writing a letter of apology to the court (Finch, 2008).

We seek to resolve this dilemma by eschewing the typical correlational approach in favor of a research design focused on North Carolina’s shift to a voluntary public financing system for judicial candidates, an institutional reform that provides us with the leverage needed to make plausible causal claims about the relationship between campaign contributions and judicial behavior. In so doing, we join a more limited body of scholarship aimed at carefully considering the difficult endogeneity issues surrounding the contributions-behavior relationship. This design requires a careful tradeoff. North Carolina is, of course, only one state and the effects of the reforms can only be estimated among a limited number of judges whose tenure overlapped the reform. These facts limit the generalizability of our study. However, our design allows us to more carefully estimate the *effects* of clean money reforms and thereby help to disentangle the contributions-

behavior relationship.²

The rest of this paper proceeds as follows. We begin by reviewing past research on the relationship between contributions and the behavior of judges and specifying our research hypotheses. Using a unique dataset of judicial decisions and attorney contributions, we then apply a difference-in-differences strategy to examine two specific mechanisms by which reform may change the relationship between contributions and judicial decision-making. First, we provide evidence that justices who opted into public financing became relatively less favorable toward attorney donors than those who did not. Second, we find partial support for our expectation that judges who opted in would become more ideologically moderate than we would have otherwise expected. In combination, these results suggest that public financing makes participating justices less responsive to the interests of donors, lending support to reformers advocating for public financing in state judicial elections. We conclude by considering the implications and the limitations of our findings.

2 THE EFFECTS OF CAMPAIGN FINANCE ON STATE COURTS

As the cost of running for state Supreme Court seats have escalated in the past two decades (Bonneau, 2005), concerns have grown about the potential influence of donors on judges who rely on their support to win elections. As misgivings about the integrity of the courts have grown,

²Like scholars conducting experiments on a college campus (Kahneman, 2011), field experiments in an isolated village (Ostrom, 1990), or comparing regions within a single country (Putnam et al., 1994), we have chosen to sacrifice some degree of generalizability to obtain a clearer picture of the underlying causal relationship of interest. Our strategy is to focus on an unusually revealing context in order to obtain a valid estimate of a broader phenomenon of interest. Just as the Confederate Congress (Jenkins, 2000) and the Nebraska state legislature (Wright & Schaffner, 2002) offer institutional variation that helps us understand the role of parties in legislatures, North Carolina's shift to public financing provides insight into the contributions-votes relationship that cannot be obtained using traditional research designs. While imperfect, we believe this approach will contribute to our understanding of the contributions-behavior relationship in state courts.

some states have enacted policies to shield justices from potentially corrupting influences. While some reforms aim to encourage more recusals (Meizlish, 2010), others seek to alter the campaign finance system itself. In particular, reformers have proposed providing public financing for judicial elections, which advocates claim “removes the influence of special interests” (*Raleigh News & Observer*, 2013). Following the passage of public financing reforms in North Carolina in 2002, New Mexico, Wisconsin, and West Virginia used the system as a template for their own public funding systems (National Center for State Courts, 2011). The policy has also been considered in several other states including Kentucky, Ohio, Maryland, and Washington (National Center for State Courts, 2011).

The effects of judicial campaign contributions are also a central issue in the larger debate over the merits of judicial elections and have received extensive attention from judges (see, e.g., O’Connor, 2010; Cobb, 2013), commentators (see, e.g., *Philadelphia Inquirer*, 2013), and legal scholars (see, e.g., Chemerinsky, 1998; Kang & Shepherd, 2011). Perhaps most notably, the U.S. Supreme Court ruled in *Caperton v. A.T. Massey Coal Co.* that “a serious, objective risk of actual bias” required the recusal of a West Virginia state Supreme Court justice from a case involving a company whose CEO had spent more than \$3 million through a non-profit organization in support of the justice’s candidacy. While *Caperton* is obviously unusual, fundraising in judicial elections raises concerns for many observers about the rule of law and the legitimacy of the courts. For instance, the American Constitution Society report *Justice at Risk* concludes that “Until reforms are enacted, powerful interest groups’ influence on judicial outcomes will only intensify” (Shepherd, 2013).

Despite this large and growing debate, court scholars have struggled to reach a consensus on the effect (if any) that campaign finance regulations have on judicial decision-making. Political scientists and legal scholars have of course spent considerable time and effort investigating the nature of judicial decision-making more generally, including the influence of various political institutions. Unlike appointed federal judges, who enjoy life tenure, many state judges are elected and must be re-elected to continue to serve. Selection and retention systems have been shown

to shape the decisions that judges make (e.g., Choi et al., 2010; Brace et al., 2000). However, while previous research has examined the effects of various institutional arrangements for selecting and retaining judges (e.g., Hall, 2001), these studies have focused mostly on electoral rules (e.g., party labels) rather than campaign finance regulations. The specific institutional regime we analyze here—voluntary public financing for high court candidates—has received considerably less attention in the empirical literature.³

2.1. *Approaches to understanding the effect of judicial contributions*

The most closely related empirical literature examines whether judges are more likely to rule in favor of parties or attorneys who are contributors (e.g., Bonneau & Cann, 2009; Cann, 2007; McCall & McCall, 2007). A related stream of research has focused on the influence of campaign donors on broader voting patterns. Waltenburg & Lopeman (2000), for instance, argue that judges backed by plaintiffs' attorneys vote more favorably toward plaintiffs in tort cases, while Ware (1999) finds that Alabama Supreme Court justices are more likely to rule in favor of donors' interests in the area of arbitration law. Similarly, Kang & Shepherd (2011) and Shepherd (2013) found evidence that judges are more likely to vote in favor of business litigants when they have received more donations from business groups.

While such studies are important for establishing empirical regularities, these results can be difficult to interpret. In particular, it remains unclear what *effects* contributions have on judicial decision-making and how they might be altered by public financing. The endogenous nature of campaign contributions makes untangling the contributions-votes relationship very difficult (as many of the empirical works cited above explicitly acknowledge). Observing a correlation between the interests of donors and the decisions of judges is consistent with multiple causal relationships, not all of which impugn the integrity of the judicial system. There are many reasons why we might

³See Panagopoulos (2011) for general analyses of the effects of public financing. A particular focus has been the effects of clean money reforms in state legislative elections (e.g., Malhotra, 2008). However, these studies have primarily focused on the effect of reforms on electoral outcomes rather than on the behavior of elected officials *per se*.

expect judges to vote more often in favor of lawyers who have made campaign contributions. Most simply, attorneys may support judges whose philosophy is most consistent with their own or the interests of their clients.

In this paper, we contribute to a smaller branch of research that seeks to more carefully identify the causal effects of contributions on judges. McCall (2003), for instance, examines whether judges are more likely to vote in favor of parties represented by attorney contributors despite indications that they are not ideologically aligned. Similarly, Cann et al. (2012) uses a matched case-control design to compare voting by justices who have similar ideologies but differ in whether they received a contribution from an attorney in a case. Finally, one published study has analyzed the question using an instrumental variables (IV) framework (Cann, 2007).⁴ Each of these studies finds evidence that contributions from attorneys increase judicial support for the position they advocate.

While these studies make important contributions, they also rely on identifying assumptions that may be open to question. For instance, the validity of the Cann (2007) model, like all IV models, depends on two untestable assumptions that may be invalid in practice (Sovey & Green, 2010). As an example, consider one of the instruments used in the study: whether cases are argued by a public defender. For that instrument to be valid, we must accept two assumptions. First, the presence of a public defender as an attorney in the case must have no effect on the justice's voting except through contributions—an implausible assumption given the evidence that indigent defendants often lack effective counsel (e.g., American Bar Association, 2012). Second, there must be no unmeasured variables that affect both the probability of having a public defender and the likelihood of receiving support from a justice. However, many factors that lead parties to be represented by public defenders could also affect later decision-making. For example, racial minorities are disproportionately likely to be represented by public defenders (e.g., Marcus, 1994) and may be treated in a biased fashion by judges (e.g., Alesina & Ferrara N.d.). Similar concerns apply to the matched case-control design in Cann et al. (2012), which assumes that judges are matched on *all* characteristics related to receiving contributions from an attorney in a case.

⁴Bonneau & Cann (2009) also produced a working paper.

Given these concerns, it seems reasonable to use a different approach that relies on different assumptions that could increase our confidence in the validity of prior estimates. In this paper, we therefore apply difference-in-differences models to identify the effect of the North Carolina public financing reform on the relationship between contributions and judicial behavior — a context that we argue is especially useful for obtaining a valid causal inference (SI-A provides details on the law.)

2.2. *Theory and hypotheses*

We leverage the North Carolina public financing reform to examine two common claims about the effects of campaign contributions on judicial decisions. First, we ask whether public financing reduces the bias that critics allege judges exhibit in favor of parties whose attorneys have contributed to their campaigns. The theory of why campaign contributions could be valuable enough to judges to influence their behavior is relatively straightforward. Elected judges must prioritize re-election above other goals; they cannot satisfy any of their preferences if they are not re-elected (Cann, 2007). The campaign activities funded by such contributions are important to re-election and thus significant to elected judges (see, e.g., Bonneau & Cann, 2011).⁵ In addition, contributors may provide not only direct financial support but other forms of indirect assistance to judicial campaigns, including advocacy on behalf of the candidate within and outside of the legal community. A contribution may be seen as an indicator of support that is far more valuable than the dollar amount alone suggests. Further, judges may vote in favor of donors to help ensure future contributions, which are most likely to come from past supporters (Min, Miller & Curry N.d.). Thus, while judges must *also* work to safeguard their reputations as unbiased arbiters in order to please voters, this may on occasion be outweighed (perhaps unconsciously) by the desire to please contributors whose support is necessary in modern judicial elections. As one judge summarized these concerns, “A saint would be hard-pressed to disregard the fact that one litigant gave them a

⁵The literature proposes several motivations for judicial behavior. Our argument is that judges’ ability to accomplish their objectives are dependent on retaining office, which implies that the need to raise money could affect judicial decision-making even if other motivations are more important.

huge contribution while the other gave them nothing. Most of our judges are not saints” (quoted in Meizlish, 2010).

While public funding does not eliminate the role of donors entirely, it greatly reduces the potential influence of an individual donor. Under the public finance system, donors are no longer the financiers of the campaigns, but instead serve as a safeguard against truly unpopular candidates taking advantage of public funding (Kotey, 2005). All donors in the public financing system are small donors — no contributor can give more than \$500. More importantly, candidates only require and are limited to a small number of donors to qualify for public financing, which is allocated to qualifying candidates during each election.⁶ Thus, a candidate who receives public funding need only replace a small number of disaffected donors to achieve the threshold for receiving public funds, which should in turn reduce any pro-contributor bias.

What effects might this shift have? First, public financing might decrease any tendency for officials to offer particularistic benefits to donors (e.g., Snyder, 1992). These benefits are hypothesized not as bribes, but rather as a tendency for officials to consciously or unconsciously favor contributors or be more attentive to them at the margin. For instance, donors may receive increased access to elected officials or exert more effort on their behalf (Austen-Smith, 1995). Similarly, judges who have cultivated contributors may be more aware of or sensitive to their concerns as a result (perhaps inadvertently).

Our study focuses specifically on the effects of public financing on judicial voting in cases involving attorney donors, who are a key source of campaign funds for judicial candidates. During the 2000–2009 period, for instance, 29% of total funds contributed in high court elections came from attorneys (Sample et al., 2010). Further, attorneys appear to be a far more important source of funds than litigants themselves. One study of civil, non-family law cases in four states found that 94% of contributions to judges from attorneys or litigants came from attorneys (Min, Miller & Curry N.d.). Our expectation is that the shift to public financing should make justices who opted

⁶Assuming all contributors give the statutory maximum, a candidate could qualify for public financing with contributions from a minimum of 62 and a maximum of 124 donors.

into the system less likely to favor attorney donors in their votes (relative to non-donors) than those who did not receive public financing.

***Hypothesis 1:** Judges who opt into public financing will be relatively less likely to favor attorney donors after the reforms take effect in comparison to judges who did not opt into the system.*

Second, reform may reduce the polarizing effects of judicial candidates' reliance on private funding. Past research has shown that campaign contributors are more ideologically extreme than the general public (e.g., Francia et al., 2003) and that the views of donors and other political activists are disproportionately represented by elected officials' behavior (Bartels, 2008), contributing to polarization (e.g., Layman et al., 2010). If, as some have argued, public financing frees candidates from needing to cater to the concerns of their donors, justices who opt into the public financing system should become more moderate in their voting relative to those who do not participate.

***Hypothesis 2:** Judges who opt into public financing will vote in a less extreme manner after the reforms take effect in comparison to judges who did not opt into the system.*

3 CHANGES IN VOTING ON ATTORNEY DONOR CASES

We first consider whether North Carolina Supreme Court justices who opted into the public financing system changed the way they voted on cases argued by donor attorneys. Before presenting our results, we describe our data and the difference-in-differences-in-differences methods we use to identify the effects of participation in the public financing system.

3.1. *Data*

Our unit of analysis here is a vote by a North Carolina Supreme Court justice in a non-unanimous case⁷ with a clear outcome⁸ that was decided between January 1, 1997 and December 31, 2009 and accompanied by a published opinion.⁹ The cases in our data were identified using two different searches via Lexis-Nexis (“dissenting” and “dissent”) and then compared and verified by a research assistant. For each case, we recorded the identities of the parties in the case, the case history, the outcome, the nature of the suit (civil or criminal), and whether a governmental entity was a party to the case. Additionally, the reported opinions identified the author of the majority opinion, the author and signers of any separate opinions, and which justices did not participate in the decision (if any). This information was compared against the composition of the Court at the time of the decision to determine the votes of all the relevant justices.¹⁰

For each case, we used state campaign finance records to identify prior contributions made by

⁷The N.C. Supreme Court issues hundreds of unanimous orders and decisions per year, most of which are routine. In SI-B, we show that non-unanimous cases are significantly broader in terms of the number of legal concepts discussed and are more likely to be cited in other decisions than unanimous cases (see the conclusion for further discussion of this issue).

⁸Five cases with mixed outcomes were dropped because there was no clear prevailing party.

⁹A breakdown of the case type before and after the reform between participating and non-participating is available in Table SI-3 in SI-C.

¹⁰Information about the personnel on the Court was derived from the official court information found at the beginning of print editions of *North Carolina Reporter* (2009).

any attorney representing a party in the case to a voting justice.¹¹ We looked for contributions from these attorneys¹² over the previous eight years, which matches the duration of a North Carolina Supreme Court electoral cycle.¹³ The contribution data included the donor's name and profession as well as the date and value of the contribution. For those who listed job titles or professions that suggested they were attorneys, we verified that the person was a practicing attorney in the state using the relevant *North Carolina Legal Directory* for that year (2009), which is the official directory of the North Carolina State Bar. Extensive work was undertaken to properly match attorney names across cases and contribution records. Finally, we used news reports and campaign finance records to determine which justices opted into the public financing system.

It is worth emphasizing that a significant proportion of non-unanimous cases heard before the court are (potentially) affected by attorney donations. Of the 125 qualifying cases in our sample,

¹¹While some studies have measured attorney giving at the law firm level (Palmer & Levendis, 2008; Palmer, 2010), this approach has been strongly criticized due to the attenuated link between individual attorney contributions and a judge's treatment of other attorneys from their firm (Tully & Gay, 2012). Furthermore, concerns regarding the influence of donations are generally framed in terms of individual lawyers rather than law firms (see *Williams-Yulee v. Florida Bar*, 2015). We believe that the relationship between contributions and judicial behavior should be strongest for individual contributors and thus focus on attorney-level giving in this article. However, we show in the SI that there is no evidence of post-reform changes in the treatment of attorneys from donor firms by participating justices (see Table SI-7 in SI-E).

¹²We focus on *attorneys* in this study because they are major contributors in judicial elections and key players in the legal process (McCall, 2003; Goldberg & Sanchez, 2004; Sample et al., 2010). We are not considering contributions from *parties* because a much smaller proportion of non-attorney donors are likely to be involved in a Supreme Court case during this time period.

¹³Online Appendix D documents that the findings below are unchanged if we instead use a four-year window. We believe the eight-year window to be most appropriate, however, because it captures funds raised by judges during their last campaign.

more than one-third involved at least one justice who had received a donation from at least one of the attorneys (43 cases; 34.4%) and more than one in ten (14 cases; 11.2%) involved two or more judges, including one case in which *seven* of the nine justices had received contributions from attorneys of one or more of the parties.

In the pre-reform period (1990–2002), we identified \$955,872 in contributions from attorneys to justices (median: \$250). Corresponding with national trends, the amount of money contributed by attorneys increased dramatically over this period, increasing from approximately \$20,000 in the 1992–1993 period to nearly \$440,000 in the 2000–2001 period. In the post-reform period (2003–2009), we identified contributions from attorneys totaling \$719,630 (median: \$200). These data suggest that the reforms did not dramatically reduce the flow of attorney contributions in general.

Despite this trend, however, the reforms did have a remarkable effect on the amount of money raised by the specific justices who opted into the system when compared to those who did not. Of those who served both before and after the reforms, the two participating justices received roughly \$180,000 (median: \$100) during the post-reform period while the four non-participating justices received over \$480,000 (median: \$250).

3.2. *Difference-in-difference-in-differences analysis of donor treatment*

Though we can establish that judges who opted into the system received considerably less money in the post-reform period than their counterparts who chose not to participate, the question we wish to answer is whether the reforms also affected their voting behavior in non-unanimous cases. To do this, we conduct a variant of a difference-in-differences analysis. These models are a common approach to estimating the effects of public policy changes (for recent reviews, see Imbens & Wooldridge 2009 and Lechner 2011). Under certain assumptions discussed further below, difference-in-difference models can recover the effect of a policy change by comparing the *changes* in the behavior of those affected by the reform (i.e., those justices who opted into the public financing system) with the *changes* in the behavior of those not affected by the reforms (i.e., those justices who did not opt into the public financing scheme) over the same time period. If the behavioral changes observed in the time period differ significantly between the two groups, we

can attribute this difference-in-differences to the reform itself. Unfortunately, this design requires that we restrict our analysis to the votes cast by the six justices who served both before and after reform, which also ensures that our results are not affected by changes in the composition of the court over time.

In our first analysis, however, the unit of analysis is not the justice, but the justice-vote. We do not anticipate that the reforms will affect each and every vote cast by the justices who participated in the public financing scheme. Rather, we expect the reform to affect *only* justice-votes in which an attorney in the case previously contributed to the justice in question. We do not expect there to be an effect on justice-votes when no attorney on either side has previously contributed to the justice. When only *some* observations are affected by the reform (e.g., votes on cases involving attorney donors), we can use a difference-in-difference-in-differences (DDD) model to estimate the treatment effect (Gruber & Poterba, 1994).

To make this DDD approach clear, it is helpful to think through a hypothetical example. Assume that donations come only from plaintiff attorneys and that there is no need for any additional covariates. Further assume that justice behavior is summarized in Table 1, which shows the proportion of the time justices vote in favor of plaintiffs in our hypothetical.

[Table 1 about here.]

Under these assumptions, the effect of the reform can be calculated as

$$\left(\underbrace{(0.48 - 0.44)}_{\text{Pre-reform difference}} - \underbrace{(0.62 - 0.61)}_{\text{Post-reform difference}} \right) - \left(\underbrace{(0.58 - 0.51)}_{\text{Pre-reform difference}} - \underbrace{(0.66 - 0.60)}_{\text{Post-reform difference}} \right) = \underbrace{0.02}_{\text{DDD estimate}} . \quad (1)$$

We compare the donor/non-donor difference in voting patterns pre- and post-reform between justices who served under private financing and later opted into the public financing system and those who served pre- and post-reform but did not receive public financing.¹⁴ If the North Carolina

¹⁴See SI-D for additional information about the justices in this analysis as well as results for alternative specifications of the “treatment” and “control” justices.

reforms were successful in reducing pro-donor favoritism, justices who opted into the public financing system should have voted less frequently in favor of donor attorneys after reform (relative to non-donor cases) than before the reform compared with those justices who did not receive public financing.

We define a donor as an attorney on a case who gave \$100 or more to a justice voting on the case in prior eight years before the case was heard — the duration of a North Carolina Supreme Court term. Restricting our definition to larger contributions or shorter time periods would mean identifying the effect of contributions based on fewer affected justice votes, which would further limit the generalizability of our results. While \$100 might seem small, very few non-unanimous cases are decided by the North Carolina Supreme Court each year (an average of ten per year during the 1997–2009 period), reducing incentives for every attorney to contribute. Moreover, even modest financial contributions are an indicator that the attorney in question is a political supporter of the justice more generally. As such, the contributor might also have provided other forms of non-financial assistance to the justice’s campaign in the past or could be expected to do so in the future—a relationship that could induce favoritism or disparate treatment. Most importantly, this definition does not affect our results, which we show in SI-D are robust to defining a donor using a higher contribution threshold (\$250) or shorter time period (four years).

In addition, we define the treatment as taking place after passage of the law in 2002 for those justices who eventually opted into the system. In other words, while justices could not formally opt in to the public financing system until January 1 of the year prior to the election in which their seat would be contested, we assume that justices anticipated this process and changed their behavior beginning in the year following the passage of the law (i.e., cases decided in 2003). Thus, the relevant treatment period is defined in our models below as post-2002 rather than the period in which the justice formally entered the public financing system. (See SI-A for additional details about the reforms.)

Differences between justices: One of the core advantages of the DDD model is that it accounts for time-invariant individual- and group-level confounding factors. To make this point clear, *it*

does not matter if the judges who opted into the system differ in their general voting patterns from the justices who did not opt into the system (e.g., Angrist & Pischke, 2009). Any time-invariant differences between participating and non-participating justices are accounted for by the model. Indeed, it is precisely to handle individual-level heterogeneity that DDD models were developed.

In Table 1, for instance, the hypothetical non-participating justices are more likely to vote in favor of donor attorneys than participating justices in both the pre- and post-reform periods.¹⁵ However, any such time-invariant difference in behavior is irrelevant to the calculation of the treatment effect; each group’s average tendency to favor donor attorneys is “differenced out” in Equation (1) when the post-reform difference is subtracted from the pre-reform difference within each group of justices.

Turning to our real data, one of the justices who opted into the system is a Democrat (Parker) and one is a Republican (Edmunds). On the other hand, all of the justices who chose not to participate in the public financing system are Republicans (Lake, Martin, Orr, and Wainwright). Nonetheless, any time-invariant effects of party affiliation *do not confound our analysis*, which still provides valid estimates the effect of reform so long as the parallel paths assumption described below holds. A similar response can be made regarding *any* potential confound associated with time-invariant differences between participating and non-participating justices including their differences in their ties to specific law firms or professional organizations, ideological leanings, level of perceived electoral security, or attitudes towards fundraising and donors.

Differences between periods: A further advantage of the DDD model is that it accounts for time-varying confounders that affect all observations. In our toy example, for instance, Table 1 shows that the hypothetical justices in both groups support plaintiffs more often in the post-reform period. However, this difference again does not confound the estimated treatment effect because the overall time trend is again “differenced out” in Equation (1) by subtracting the change among

¹⁵This difference among treated justices is 0.04 in the pre-reform period and 0.01 in the post-reform period. The respective numbers for the control justices are 0.07 and 0.06.

non-participants from the change among participants.

Thus, the DDD model accounts for any time-specific shocks to justice voting resulting from changes in the overall case mix, composition of the court, or external political factors. For instance, we need not assume that overall composition of the court is comparable before and after the reforms (it is of course possible that the differing composition of the court after reform could change how its members behave relative to the pre-reform period). Similarly, we do not assume that the types of cases that were appealed to the North Carolina Supreme Court are the same between the pre- and post-reform periods. These assumptions are not necessary to support our conclusions.

Time-varying shocks to judges: A third advantage of the DDD approach is that the model controls for time-specific shocks to treated justices that may have affected their overall voting patterns but did not change the *difference* in how they treat cases argued by donor and non-donor attorneys. In our toy example above, for instance, it is clear that the hypothetical participating justices became much more likely to vote in favor of plaintiffs in the post-reform period regardless of their donor status. Again, however, this shock does not confound our estimates because our model focuses on the difference in how often justices voted in favor of plaintiffs with and without donor attorneys; the general post-reform shift in voting behavior among participating justices is “differenced out” in Equation (1) by subtracting their probability of voting for non-donor attorneys from their probability of voting for donor attorneys in that period.

In this sense, the DDD model accounts, for instance, for a justice becoming more likely to vote in favor of criminal defendants after the reform. Likewise, the model will account for changes in the role played by a justice in the court after reform (e.g., Justice Parker becoming chief justice) if those changes only affect their overall voting patterns.

Limitations of the DDD model: While the model is robust to the potential confounds discussed above, it relies on three key assumptions. First, we assume that the treatment and control groups would follow parallel paths in the absence of treatment—an unobserved counterfactual. In this case, we assume that justices who opted in to public financing would have changed their post-

reform treatment of donors relative to non-donors identically to those justices who did not.

Second, in order to make a causal claim we must assume that selection into treatment is not influenced by temporary, individual-specific shocks that simultaneously affect how justices differentially voted on cases with donor and non-donor attorneys and also whether they opt into the public financing system. Substantively, then, we are making the very plausible assumption that those justices who opted into public financing did not do so as a result of or for reasons related to their pre-reform behavior in cases involving attorney donors. For instance, we are aware of no evidence suggesting that the participating justices had been differentially criticized for their voting on cases involving donors in the pre-treatment period or that they had felt disproportionately pressured to vote in favor of donor attorneys.

Finally, as noted above, the DDD strategy is designed only to estimate the effect of the reforms on justices who opted in to the system relative to a counterfactual scenario in which they did not participate — a treatment effect that is often referred to as the average treatment effect on the treated. We cannot estimate the potential effect of the reforms on the justices who did not opt in to public financing had they somehow been forced to participate. We return to these points again in the concluding section below and in SI-D.

3.3. *Results*

We now compare the voting records of those justices who opted into the public financing system with those untreated justices who did not participate. The data are presented in Table 2 for both groups.

[Table 2 about here.]

Before reform, justices who later opted into public financing were extremely unlikely to vote for the plaintiff¹⁶ in a case in which a defendant attorney was a donor (0%), somewhat likely to vote for the plaintiff in cases where neither party's attorneys were donors (48%), and very likely to vote for the plaintiff when a plaintiff's attorney was a donor (80%). This pattern is consistent with justices favoring donor attorneys. After reform, however, the relationship between contributions and votes diminished.

The change in voting patterns pre- and post-reform is very different for justices who did not receive public financing. In particular, the expected relationship between contribution status and contributions is reversed in the pre-reform period among justices who did not later receive public financing. Before 2003, these justices were *more* likely to vote for plaintiffs in cases where the defendant attorneys were donors (100%) compared with control cases (56%) and *less* likely to do so when plaintiff's attorneys previously contributed (43%). However, this relationship again dissipated in the 2003–2009 period.

To maximize the statistical power of our analysis, our attorney contributions variable is coded symmetrically, taking a value of -1 for cases in which one or more defendant attorneys gave \$100 or more to the justice in question, 0 when no attorneys did so (our control cases), and 1 when one or more plaintiff attorneys gave \$100 or more to the justice in question in the previous eight years.¹⁷

We then test our hypotheses using a least squares difference-in-difference-in-differences model¹⁸ that predicts each justice vote as a function of attorney contribution status, whether the case was

¹⁶In this paper, we use the term “plaintiff” to encompass the original plaintiff in a civil case or the prosecuting body in criminal matters. Likewise, “defendant” refers to the defending party in both civil and criminal matters. We do so to avoid any confusion with which parties were “petitioners” and “respondents” at the appellate level (appellants and appellees).

¹⁷No cases were observed in which qualifying contributions were made by attorneys on both sides of the case.

¹⁸A generalized linear model with a functional form that matches our binary dependent variable does not identify the actual effect of interest (Blundell & Dias, 2009; Lechner, 2011).

decided after reform (i.e., in 2003 or later), whether the justice in question was one of those who opted in to the public financing system, and the two- and three-way interactions among those variables. We also include standard control variables that might affect either the tendency of judges to vote in a specific direction or the overall salience of the case — indicators for criminal cases, cases in which the plaintiff or defendant was a government entity, and cases in which one or more amicus briefs were filed.¹⁹ These controls should account for any changes in the pre- and post-reform period case mix between donor and non-donor cases.

[Table 3 about here.]

Substantively, the indicator terms for attorney donors, the post-reform period, and participating justices are the needed controls to “difference out” time-invariant differences in pro-plaintiff voting between cases with and without a donor attorney, time-varying shocks that affect all justices equally, and time-invariant differences between justices in our treatment and control groups. The two-way interactions account for court-wide changes in voting on cases including donors in the post-reform period (donor \times post-reform), time-invariant differences in tendencies to vote for donors among justices who opted into public financing (donor \times participating), and overall changes in post-reform voting patterns among justices not specifically related to the presence of attorney donors (post-reform \times participating). Our primary coefficient of interest, however, is the three-way interaction between donor status, the post-reform period, and participating justices. This term, which is in essence the estimate shown in Equation 1, is our DDD treatment effect. If our hypothesis is correct, this variable should be negative, indicating that the reforms caused participating justices to be less likely to vote in favor of donor attorneys in the post-reform period.

The results of our least squares models are presented in the first column of Table 3. Consistent with our expectations, we find that attorney donors are treated relatively worse by judges who opted into public financing after the passage of reform. The coefficient for the three-way interaction

¹⁹The presence of amicus parties was coded based on information from LexisNexis, Westlaw, and the North Carolina Supreme Court website.

between donor status, the post-reform period, and participation in the public financing system is $\beta = -.605$, which is reliably distinguishable from zero. This finding indicates that, compared with otherwise identical cases not involving attorney donors, the predicted probability of a vote in favor of an attorney-donor decreased by more than 60% relative to justices who did not receive public financing. In other words, the estimated effect of the reforms on participating judges was a 60% reduction in the likelihood they would vote in favor of attorney-donors appearing before the court.

However, fully understanding the substantive and statistical importance of a coefficient can be difficult in a model with multiple interactions. To aid interpretation, we therefore summarize the predicted probabilities generated from this model in Figure 1. Specifically, we show the predicted probabilities for a civil case in which government entities are not parties to the case and one or more amicus briefs are filed.

[Figure 1 about here.]

Again, the results show that voting patterns changed dramatically among justices who opted into public financing after 2002. In particular, their voting records became much less favorable toward attorney donors—a change that was the opposite of the shift observed among justices who did not receive public funding. That is, while judges participating in the public financing system became less likely to vote in favor of donor attorneys in the post-reform period, non-participating judges became more likely to vote in favor of donor attorneys relative to their behavior in the pre-reform period.

We estimate several models to ensure that our results are robust to various forms of potential non-independence among observations. The least squares model in the first column, for instance, clusters the standard errors by case, while the second model includes two-way clustered standard errors by justice and year (Thompson, 2011; Cameron et al., 2011) to account for non-independence in voting over time by each justice as well as time-level shocks to voting that might have correlated effects across justices. Since we have a small number of justices in our sample, the third column includes a model with standard errors clustered by justice estimated using the Cameron et al. (2008) wild bootstrap procedure, which has been shown to perform adequately

with as few as five clusters.²⁰ Our difference-in-difference-in-differences result is statistically significant across models.

Finally, these results are robust to numerous plausible perturbations in our assumptions. As we show in SI-D, our results are unchanged if we use four-year window for previous contributions instead of eight years or a \$250 contribution threshold for donor status instead of \$100. The results are also consistent if we exclude any or all justices from the control group who retired rather than running for re-election under the new system or if we use robust regression to reduce the influence of potential outliers (see SI-D for further details). Thus, our results are not being driven by the behavior of any one judge in the control group.²¹

4 CHANGES IN JUDICIAL VOTING PATTERNS

A second concern raised in the literature on campaign donors is the polarizing effect of contributions on elected officials. Donors tend to be ideologically extreme relative to the general public. If judges are forced to cater to the interests of donors, their voting behavior may be more extreme than it would be under an alternative financing system. In this section, we therefore estimate the effect of the North Carolina reforms on the ideological extremism of justices who opted into the public financing system relative to those justices who did not.

4.1. *Difference-in-differences analysis of judicial ideological extremism*

To test Hypothesis 2, we fit a one-dimensional Bayesian item response model²² based on votes by justices in our sample in non-unanimous cases before the North Carolina Supreme Court. We generate separate estimates for each justice in the pre-treatment (1997–2002) and post-treatment

²⁰We conduct 1,000 bootstrap replications imposing the null hypothesis.

²¹While it is tempting to further disaggregate the data for these analyses, doing so is of limited value given the already attenuated sample size. In such cases, a lack of statistical significance has no clear interpretation since it may simply be a result of small sample sizes. Thus, we do not analyze subsets of data based on their status as criminal cases or by issue area.

²²Additional details about the model in this section are provided in SI-F.

periods (2003–2009) periods. To avoid strong assumptions, we constrain the estimates for the untreated justices (i.e., justices who did not participate in the public financing system) to be constant across periods, which place the estimates for the two time periods on the same scale.²³

Our focus is the *change* in the estimated ideal point of the treated (unconstrained) judges who served on the court before and after the reforms. In essence, we place our estimates of the pre- and post-reform periods on the same scale by constraining the position of the untreated justices (i.e., justices who did not receive public financing) to be constant across periods. In reality, these justices might also have changed their ideological voting patterns across time periods. Thus, our analysis of the ideological position of the justices is also a difference-in-differences analysis because the effect of the North Carolina reforms on each of the treated justices is estimated *relative* to ideological positions of the untreated justices, which may also be changing.

While this approach limits the conclusions we can draw about the *absolute* positions of the justices, it allows us to estimate the change in the positions of the treated justices *relative* to the untreated justices. Substantively, we estimate whether the treated justices became more liberal or conservative relative to untreated justices while making minimal assumptions about the positions of justices in the control group. To our knowledge, this analysis represent the first use of an item-response framework as part of a difference-in-differences analysis.

As before, this approach accounts for the any time-invariant factors affecting justices (e.g., party) as well as any time trends that affect all observations equally (e.g., political trends). We specifically note that it is *not* necessary that the justices who opted in to public financing be identical ideologically with those who did not. However, an important assumption here is that justices not choose to participate due to a temporary, individual-specific shock — for instance, some shift in ideology that temporarily alienated donors and thereby increased the likelihood that a justice opted into public financing. In addition, we make the same parallel paths assumption described above, meaning that the average post-reform change in ideology would have been equivalent between

²³This strategy is a bridging procedure that is used to make votes comparable over time. Similar approaches are frequently used in the literature on Congressional ideal points.

groups in a counterfactual scenario with no public financing.

4.2. Results

The coefficient estimates and posterior quantiles for our analysis are summarized in Table 4, where positive values indicate a more conservative justice, and negative values indicate a more liberal justice.

[Table 4 about here.]

Two aspects of Table 4 are notable. First, the ordering of the justices on the ideological scale is consistent with expectations. For instance, the model estimates Republican justices to be, on average, more conservative than their Democratic counterparts.

Second, Table 4 shows that Justice Parker, a Democrat, became relatively more moderate in the period after reform, moving away from her party's ideological base relative to other justices in this time period. To confirm this, we calculate a posterior estimate for the effect of the treatment on Parker, which is defined as $\Delta\text{Parker} = (\theta_{\text{Parker}}^{\text{Pre}} - \theta_{\text{Parker}}^{\text{Post}})$. By calculating this value for each draw from the posterior, we can get a full posterior estimate for the treatment effect on Parker, which has a median of 1.62 and a 95% credible interval of (0.58, 3.11). In substantive terms, this finding indicates that Parker became significantly more conservative (less liberal) relative to untreated justices.

We also calculate a posterior estimate for the effect of the shift to public financing on Justice Edmunds, a Republican, which is defined as $\Delta\text{Edmunds} = (\theta_{\text{Edmunds}}^{\text{Pre}} - \theta_{\text{Edmunds}}^{\text{Post}})$. In this case, our hypothesis suggests that we should observe change in the liberal (negative) direction relative to the four control justices. Note, however, the 95% credible interval for his pre-reform ideology (-2.22, 0.56) is quite wide. As a result, though the median posterior estimate for his change in position after reform is 1.05, suggesting that he actually became somewhat more conservative, the 95% credible interval overlaps zero (2.70, -0.21). Public financing thus had no statistically significant effect on Justice Edmunds, which seems to reflect the imprecision with which his ideological position was estimated in the pre-treatment period.

5 LIMITATIONS AND CONCLUSIONS

We contribute to the growing literature seeking to carefully estimate the causal relationship between donations and judicial behavior by providing the estimates for the effect of public financing on North Carolina's state supreme court. After reviewing the inherent difficulties of making inferences about the contributions-behavior relationship, we apply a difference-in-differences strategy to a unique dataset consisting of judicial votes in non-unanimous cases and campaign contributions from attorneys to justices. Our results provide evidence that justices who opted into the public financing scheme became relatively less favorable toward attorney donors once the reforms came into effect relative to justices who did not participate. We also find partial support for our hypothesis that justices who opted into the system become more ideologically moderate relative to non-participating justices in the post-reform period. These results, which suggest that donors do in fact have distorting influence on judicial decision-making, make a substantial contribution to the literature on the relationship between contributions and judicial behavior.

Before concluding, it is important to discuss the limitations of this study. A chief concern is the relatively small sample size. In particular, our results are based on the six judges whose tenure spanned the implementation of the reforms in North Carolina. As noted in the introduction, the number of justices we consider is a fundamental limitation of the present study. However, it is also important to note some of the strengths of our research design. First, the size of our sample actually biases us *against* finding significant results by reducing the statistical power of our models. Also, our analyses consider a large number of votes by those justices (492 justice-votes on 125 non-unanimous cases with clear outcomes), which increases our statistical power. While having more non-unanimous votes would obviously be desirable, more than one-third of cases in our sample (34.4%) involved at least one justice who had received a donation from an attorney, suggesting that our study examines a widespread and substantively important phenomenon. Finally, while we agree that scholars should be cautious in generalizing from small samples, we note that many previous studies have analyzed courts in one state to test general theories of judicial behavior (e.g., Cann 2007; Traut & Emmert 1998; McCall 2003; see also Nicholson-Crotty & Meier 2002)

and that our sample size is consistent with other studies of the courts (e.g., Hall, 1987; George & Epstein, 1992; Segal et al., 2000) and of specific influences (e.g., contributions) on Congressional behavior (e.g., Bartels, 1991; Hall & Wayman, 1990; Wright, 1990). As with these other studies, the results in this article provide specific evidence about a broader problem of general interest. Our results are not definitive, but instead seek to contribute to the broader scholarly dialogue surrounding the effects of campaign donations in the judicial system. Our hope is that future studies will subject our findings to additional scrutiny as more observations become available by broadening the set of donors, expanding the data to include the North Carolina Court of Appeals, and evaluating the effects of reforms in other states.

Two other concerns should be considered. First, only cases that were appealed from lower courts are considered by the Supreme Court. If attorneys strategically altered the cases they appeal based on the anticipated effects of contribution status on justice behavior, this anticipatory effect could bias our estimates, though we know of no evidence to support this speculation. Second, some scholars have argued that focusing on non-unanimous cases can be misleading (Tully & Gay, 2009; Brace & Hall, 1990). While we would not claim that unanimous cases are inconsequential, these cases, which make up the vast majority of those heard by the court, do not provide information about the ideological disposition of judges in the IRT framework. Without any variation in behavior between justices, these cases are not informative for our second analysis. Additionally, as detailed in SI-B, an analysis of three years of decisions data reveals that non-unanimous cases decided by the North Carolina Supreme Court are significantly broader and have a greater impact than unanimous decisions.

Finally, it is important to emphasize that the inferences we draw depend on the assumptions employed in our analysis. We make two key assumptions. We assume that the “treatment” — public financing — affected the justices who eventually participated in the system beginning in 2003. Additionally, we assume that no other factor differentially affected the treatment of donors versus non-donors between participating and non-participating justices. Under the parallel paths assumption, the change in post-reform treatment of attorney donors versus non-donors among

participating justices (relative to changes among non-participating judges) is attributed to public financing. Similar assumptions are necessary for any change in ideological voting behavior among justices who opted in to public financing to be attributed to their participation in the system. (See SI-D for additional discussion of potential confounds.) While we are aware of no specific reason to believe that these assumptions are not met, it is worth emphasizing that the validity of our results rests on the validity of these untestable assumptions.

Though these limitations are important to note, we believe that our findings make a significant contribution to the ongoing debate over judicial campaign contributions and public financing reforms. Just last year, West Virginia made their judicial public financing program permanent (Smith, 2013) while North Carolina ended theirs (Parker, 2013). Our findings suggest that public financing systems like these may reduce the likelihood that participating justices vote in favor of donor attorneys. These reforms might therefore have a beneficial effect on the proper functioning of state judicial systems as well as public perceptions of their fairness. However, given the importance of ensuring the independence, fairness, and legitimacy of the courts, additional research is clearly warranted to better understand the effect of campaign contributions and public financing on judicial behavior.

5 References

- Alesina, A. F. & Ferrara, E. L. A test of racial bias in capital sentencing. National Bureau of Economic Research Working Paper 16981.
- American Bar Association (2012). National indigent defense reform: The solution is multifaceted.
- Angrist, J. D. & Pischke, J. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.
- Austen-Smith, D. (1995). Campaign contributions and access. *American Political Science Review*, 89, 566–581.
- Bartels, L. M. (1991). Constituency opinion and congressional policy making: The Reagan defense build up. *The American Political Science Review*, 85, 457–474.
- Bartels, L. M. (2008). *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton, NJ: Princeton University Press.
- Blundell, R. & Dias, M. C. (2009). Alternative approaches to evaluation in empirical microeconomics. *Journal of Human Resources*, 44, 565–640.
- Bonneau, C. W. (2005). What price justice(s)? Understanding campaign spending in state supreme court elections. *State Politics and Policy Quarterly*, 5, 107–125.
- Bonneau, C. W. & Cann, D. M. (2009). The effect of campaign contributions on judicial decisionmaking. Available at SSRN: <http://ssrn.com/abstract=1337668> or <http://dx.doi.org/10.2139/ssrn.1337668>.
- Bonneau, C. W. & Cann, D. M. (2011). Campaign spending, diminishing marginal returns, and campaign finance restrictions in judicial elections. *Journal of Politics*, 73, 1267–1280.
- Brace, P., Langer, L., & Hall, M. (2000). Measuring the preferences of state supreme court judges. *The Journal of Politics*, 62, 387–413.
- Brace, P. R. & Hall, M. G. (1990). Neo-institutionalism and dissent in state supreme courts. *Journal of Politics*, 52(1), 54–70.
- Brandenburg, B. (2012). Judicial elections justice for sale? *American Bar Association: Human Rights Magazine*, 39.
- Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *Review of Economics and Statistics*, 90, 414–427.
- Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2011). Robust inference with multiway clustering. *Journal of Business & Economic Statistics*, 29, 238–249.
- Cann, D. M. (2007). Justice for sale? Campaign contributions and judicial decisionmaking. *State Politics and Policy Quarterly*, 7, 281–297.

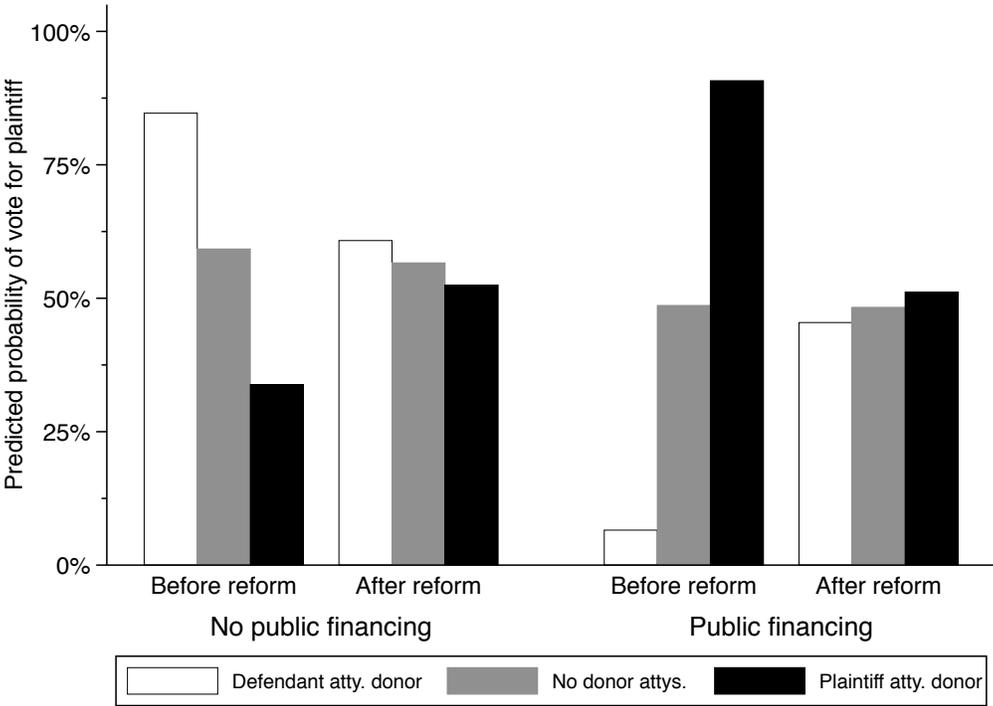
- Cann, D. M., Bonneau, C. W., & Boyea, B. D. (2012). Campaign contributions and judicial decisions in partisan and nonpartisan elections. In K. T. McGuire (Ed.), *New Directions in Judicial Politics*. New York: Routledge.
- Chemerinsky, E. (1998). Perserving an independent judiciary: The need for contribution and expenditure limits in judicial elections. *Kent Law Review*, 74, 133–149.
- Choi, S., Gulati, G., & Posner, E. (2010). Professionals or politicians: The uncertain empirical case for an elected rather than appointed judiciary. *Journal of Law, Economics, and Organization*, 26, 290–336.
- Cobb, S. B. (2013). Introduction. In *Justice at risk: An empirical analysis of campaign contributions and judicial decisions*. Washington, DC: American Constitution Society for Law and Policy.
- Finch, S. (2008). Tulane law school issues apology to Louisiana Supreme Court. *Times-Picayune*, September 16, 2008.
- Francia, P. L., Green, J. C., Herrnson, P. S., Powell, L. W., & Wilcox, C. (2003). *The Financiers of Congressional Elections*. New York: Columbia University Press.
- George, T. E. & Epstein, L. (1992). On the nature of Supreme Court decision making. *American Political Science Review*, 86, 323–337.
- Goldberg, D. & Sanchez, S. (2004). *The New Politics of Judicial Elections 2002*. Washington, DC: Justice at Stake Campaign.
- Gruber, J. & Poterba, J. (1994). Tax incentives and the decision to purchase health insurance: Evidence from the self-employed. *Quarterly Journal of Economics*, 109, 701–733.
- Hall, M. G. (1987). Constituent influence in state supreme courts: Conceptual notes and a case study. *Journal of Politics*, 49, 1117–1124.
- Hall, M. G. (2001). State supreme courts in American democracy: Probing the myths of judicial reform. *American Political Science Review*, 95, 315–330.
- Hall, R. L. & Wayman, F. W. (1990). Buying time: Moneyed interests and the mobilization of bias in congressional committees. *American Political Science Review*, 84, 797–820.
- Imbens, G. W. & Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, 47, 5–86.
- Jenkins, J. A. (2000). Examining the robustness of ideological voting: Evidence from the Confederate House of Representatives. *American Journal of Political Science*, 44, 811–822.
- Kahneman, D. (2011). *Thinking, Fast and Slow*. New York: Farrar, Straus and Giroux.
- Kang, M. S. & Shepherd, J. M. (2011). The partisan price of justice: An empirical analysis of campaign contributions and judicial decisions. *New York University Law Review*, 86, 69–130.

- Kotey, P. W. (2005). Public financing for non-partisan judicial campaigns: Protecting judicial independence while ensuring judicial impartiality. *Akron Law Review*, 38, 597–624.
- Layman, G. C., Carsey, T. M., Green, J. C., Herrera, R., & Cooperman, R. (2010). Activists and conflict extension in American party politics. *American Political Science Review*, 104, 324–346.
- Lechner, M. (2011). The estimation of causal effects by difference-in-difference methods. *Foundations and Trends in Econometrics*, 4, 165–224.
- Legal Directories Publishing Company, Inc. (1997–2009). *The North Carolina Legal Directory*.
- Lupia, A. & Aldrich, J. H. (2014). *Improving Public Perceptions of Political Science's Value: Report of the Task Force on Improving Public Perceptions of Political Science's Value*. Washington, DC: American Political Science Association.
- Malhotra, N. (2008). The impact of public financing on electoral competition: Evidence from Arizona and Maine. *State Politics and Policy Quarterly*, 8, 263–281.
- Marcus, R. (1994). Racism in our courts: The underfunding of public defenders and its disproportionate impact upon racial minorities. *Hastings Constitutional Law Quarterly*, 22, 219–268.
- Martin, A. D., Quinn, K. M., & Park, J. H. (2011). MCMCpack: Markov chain Monte Carlo in R. *Journal of Statistical Software*, 42(9).
- McCall, M. M. (2003). The politics of judicial elections: The influence of campaign contributions on the voting patterns of Texas supreme court justices, 1994-1997. *Politics and Policy*, 31, 314–343.
- McCall, M. M. & McCall, M. A. (2007). Campaign contributions, judicial decisions, and the Texas supreme court. *Judicature*, 90, 214–225.
- Meizlish, L. F. (2010). Recuse me - *Caperton*, campaign spending and disqualification of judges in Michigan. *Wayne Law Review*, 56, 1851–1888.
- Min, B., Miller, B., & Curry, B. Repeat campaign donors and state supreme court decision making. Unpublished manuscript.
- National Center for State Courts (2011). Special edition: Public financing of judicial campaigns. *Gavel to Gavel*, June 27, 2011.
- Newman, R., Speyrer, J., & Terrell, D. (2009). A methodological critique of ‘The Louisiana supreme court in question: An empirical and statistical study of the effects of campaign money on the judicial function’. *Louisiana Law Review*, 69(2), 307–316.
- Nicholson-Crotty, S. & Meier, K. J. (2002). Size doesn't matter: In defense of single-state studies. *State Politics & Policy Quarterly*, 2, 411–422.
- O'Connor, S. D. (2010). Choosing (and recusing) our state court justices wisely: Keynote remarks by Justice O'Connor. *Georgetown Law Review*, 99, 151.

- Ostrom, E. (1990). *Governing the Commons: The Evolution of Institutions for Collective Action*. New York: Cambridge University Press.
- Palmer, V. V. (2010). The recusal of American judges in the post-*Caperton* era: An empirical assessment of the risk of actual bias in decisions involving campaign contributors. *Global Jurist*, 10(3), Article 4.
- Palmer, V. V. & Levendis, J. (2008). The Louisiana supreme court in question: An empirical and statistical study of the effects of campaign money on the judicial function. *Tulane Law Review*, 82, 1291–1314.
- Panagopoulos, C. (Ed.). (2011). *Public Financing in American Elections*. Philadelphia: Temple University Press.
- Parker, M. (2013). Judicial races could soon be off the public tab. *Wilmington Star News*, June 22, 2013.
- Peterson, M. A. cluster2.ado. Available at http://www.kellogg.northwestern.edu/faculty/petersen/html/papers/se/se_programming.htm.
- Philadelphia Inquirer* (2013). Judicial elections lead to distrust of the courts. Editorial, January 26, 2013.
- Putnam, R. D., Leonardi, R., & Nanetti, R. Y. (1994). *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton, NJ: Princeton University Press.
- Raleigh News & Observer* (2013). Democracy undone by ending funding for NC court races. Editorial, June 14, 2013.
- Sample, J. J., Hall, C., & Casey, L. (2010). The new politics of judicial elections. *Judicature*, 94, 50–57.
- Segal, J. A., Timpone, R. J., & Howard, R. M. (2000). Buyer beware? Presidential success through Supreme Court appointments. *Political Research Quarterly*, 53, 557–595.
- Shepherd, J. (2013). *Justice at Risk: An Empirical Analysis of Campaign Contributions and Judicial Decisions*. Washington, DC: American Constitution Society.
- Smith, A. (2013). WV House passes legislation to make permanent judicial clean elections system. *Public Campaign*, Downloaded April 3, 2013, publiccampaign.org/blog/2013/04/03/wv-house-passes-legislation-make-permanent-judicial-clean/-elections-system.
- Snyder, J. M. (1992). Long-term investing in politicians: Or, give early, give often. *Journal of Law and Economics*, 35, 15–43.
- Sovey, A. J. & Green, D. P. (2010). Instrumental variables estimation in political science: A readers' guide. *American Journal of Political Science*, 55, 188–200.

- Thompson, S. (2011). Simple formulas for standard errors that cluster by both firm and time. *Journal of Financial Economics*, 99, 1–10.
- Thomson West (1997–2009). *North Carolina Reporter*.
- Traut, C. A. & Emmert, C. F. (1998). Expanding the integrated model of judicial decision making: The California justices and capital punishment. *Journal of Politics*, 60, 1166–1180.
- Tully, K. R. & Gay, E. P. (2009). The Louisiana Supreme Court defended: A rebuttal of the Louisiana supreme court in question: An empirical and statistical study of the effects of campaign money on the judicial function. *Louisiana Law Review*, 69, 281–306.
- Tully, K. R. & Gay, E. P. (2012). Rebuttal of Vernon Palmer’s thesis, take two. Unpublished manuscript. Downloaded May 28, 2013 from http://www.lasc.org/press_room/press_releases/2012/Rebuttal.pdf.
- Waltenburg, E. N. & Lopeman, C. S. (2000). Tort decisions and campaign dollars. *Southeastern Political Review*, 28, 241–263.
- Ware, S. J. (1999). Money, politics and judicial decisions: A case study of arbitration law in Alabama. *Journal of Law and Politics*, 15, 645–685.
- Williams-Yulee v. Florida Bar, No. 13-1499 (U.S. Apr. 29, 2015).
- Wright, G. C. & Schaffner, B. F. (2002). The influence of party: Evidence from the state legislatures. *American Political Science Review*, 96, 367–379.
- Wright, J. R. (1990). Contributions, lobbying, and committee voting in the U.S. House of Representatives. *American Political Science Review*, 84, 417–438.

Figure 1: Predicted probabilities of N.C. Supreme Court justice votes: Civil case



Predicted probabilities for a civil case in which neither side includes a government entity as a party to the case and one or more amicus briefs are filed. Drawn from the model in the first column of Table 3.

Table 1: Understanding DDD: Hypothetical justice voting before and after reform

(a) Treated: Judges who opted into public financing

	<i>Pr(vote plaintiff)</i>	
	Before reform	After reform
No donor attorneys	0.44	0.61
Plaintiff attorney donor	0.48	0.62

(b) Controls: Judges who did not receive public financing

	<i>Pr(vote plaintiff)</i>	
	Before reform	After reform
No donor attorneys	0.51	0.60
Plaintiff attorney donor	0.58	0.66

Each cell shows the proportion of the time the justices vote in favor of the plaintiff in a *hypothetical example*.

Table 2: N.C. Supreme Court voting before and after reform

(a) Treated: Judges who opted into public financing

	<i>Pr(vote plaintiff)</i>	
	Before reform	After reform
Defendant attorney donor	0.00	0.17
No donor attorneys	0.48	0.51
Plaintiff attorney donor	0.80	0.20
Number of justice-votes	64	140

(b) Controls: Judges who did not receive public financing

	<i>Pr(vote plaintiff)</i>	
	Before reform	After reform
Defendant attorney donor	1.00	0.40
No donor attorneys	0.56	0.59
Plaintiff attorney donor	0.43	0.33
Number of justice-votes	159	127

Sample includes all votes cast from 1997–2009 on non-unanimous cases by North Carolina Supreme Court justices who served prior to and after the passage of the Judicial Campaign Reform Act.

Table 3: OLS models of N.C. Supreme Court voting by donor status

	Standard error clustering		
	Court case	Justice, year	Justice
Attorney donor	-0.254** (0.115)	-0.254** (0.057)	-0.254* (<i>p</i> =.098)
Post-reform	-0.026 (0.073)	-0.026 (0.050)	-0.026 (<i>p</i> =.564)
Participating justice (public financing)	-0.106* (0.062)	-0.106** (0.050)	-0.106 (<i>p</i> =.546)
Attorney donor × post-reform	0.213 (0.190)	0.213* (0.109)	0.213 (<i>p</i> =.218)
Attorney donor × participating justice	0.675** (0.156)	0.675** (0.074)	0.675** (<i>p</i> =.042)
Post-reform × participating justice	0.023 (0.087)	0.023 (0.050)	0.023 (<i>p</i> =.720)
Donor × post-reform × participating	-0.605** (0.237)	-0.605** (0.104)	-0.605* (<i>p</i> =.070)
Government plaintiff	0.023 (0.104)	0.023 (0.099)	0.023 (<i>p</i> =.736)
Government defendant	-0.016 (0.082)	-0.016 (0.042)	-0.016 (<i>p</i> =.638)
Criminal case	0.146 (0.114)	0.146 (0.093)	0.146* (<i>p</i> =.068)
Amicus brief(s)	0.106 (0.065)	0.106** (0.047)	.106* (<i>p</i> =.08)
Constant	0.486** (0.071)	0.486** (0.041)	.486 (<i>p</i> =NA)
R ²	0.05	0.05	0.05
N	492	492	492

* $p < .10$; ** $p < .05$. Models estimated for all votes cast from 1997–2009 on non-unanimous cases by those North Carolina Supreme Court justices who served before and after the passage of the Judicial Campaign Reform Act. Sample includes all votes cast from 1997–2009 on non-unanimous cases by North Carolina Supreme Court justices who served prior to and after the passage of the Judicial Campaign Reform Act. Pro-plaintiff votes are coded as 1; pro-defendant votes are coded as 0. Participating justices are those who opted in to the state’s public financing system for judicial campaigns. Results in the second column include two-way clustered standard errors by justice and year calculated using the Peterson (N.d.) Stata implementation. Results in the third column include p -values clustered by justice from the Cameron et al. (2008) wild bootstrap (it does not calculate standard errors).

Table 4: Quantiles for posterior estimates of justice ideal points (1997–2009)

	2.5%	25%	50%	75%	97.5%
<i>Treatment group: Justices who received public financing</i>					
Parker (pre-reform)	-2.50	-1.57	-1.11	-0.74	-0.23
Parker (post-reform)	0.10	0.33	0.46	0.63	0.95
Δ Parker	3.11	2.83	1.62	0.72	0.58
Edmunds (pre-reform)	-2.22	-1.10	-0.61	-0.18	0.56
Edmunds (post-reform)	0.06	0.29	0.41	0.54	0.80
Δ Edmunds	2.70	2.38	1.05	0.01	-0.21
<i>Control group: Justices who did not receive public financing</i>					
Lake	-0.27	-0.01	0.13	0.27	0.56
Martin	-0.22	-0.01	0.11	0.23	0.46
Orr	-0.50	-0.22	-0.09	0.03	0.27
Wainwright	-0.38	-0.10	0.05	0.19	0.52

Results of one-dimensional item response models estimated using `MCMCpack` (Martin et al., 2011) on votes cast in non-unanimous cases from 1997–2009 by North Carolina Supreme Court justices. Pre-treatment estimates generated using all non-unanimous votes cast in the 1997–2002 period. Post-treatment estimates are generated using votes cast in the 2003–2009 period. The remaining estimates are calculated using all available votes for the relevant justice. Positive scores indicate a more “conservative” justice. “Untreated” justices who did not participate in the public financing system but served before and after reform are constrained to have a constant position to place the ideological estimates in both periods on the same scale. Estimates for additional justices in neither the control or treatment group are shown in SI-F.

SI-A BACKGROUND ON NORTH CAROLINA LAW

In 2001, the North Carolina legislature began to consider public financing of state legislative races (Carrington 2010, 1998). Around the same time, the North Carolina Committee on Judicial Elections began floating a proposal to distribute a publicly-funded voter guide in judicial races (Carrington 2010). The two proposals were ultimately combined, modified, and supplemented. In 2002, the North Carolina legislature enacted and the governor signed the Judicial Campaign Reform Act (JCRA), which included provisions for public finance of judicial elections, a voters' guide, and nonpartisan elections.¹ Non-partisan primaries for judicial positions are held in conjunction with the partisan primaries² if there are more than the twice the number of candidates for a position or group of positions (N.C. Gen. Stat. §163-322).³ Although the primaries are non-partisan, the parties, at least sometimes, identify ideologically aligned candidates (see, e.g., Carrington 2010, 1999).

Beginning in the 2004 elections, North Carolina provided public funding to eligible candidates running for judicial positions, including candidates for the North Carolina Supreme Court. The provisions in the JCRA set out clear restrictions on fundraising and spending. In the period after January 1 of the year before the election and before a candidate declares her intention to seek public financing, she may raise and spend up to \$10,000 without forfeiting her eligibility (N.C. Gen. Stat. §163-278.64(d)(1)). In order to qualify for such public funds, the candidate must file a declaration of intent “before or during” the qualifying period (§163-278.64), which runs from September 1st in the year before the election until the day of the primary (§163-278.62(16)). The candidate must raise “30 times the filing fee for candidacy for the office” (§163-278.62(9)) from at least 350 registered voters (§163-278). The filing fee is “one percent (1%) of the annual salary of the office sought [by the candidate]” (§163-324(a)). Thus, the minimum qualifying fundraising amount was approximately \$35,000 for Supreme Court candidates during the relevant time period (Bend 2004).

All qualifying contributions, which can range from \$10–500 (§163-278.62(15)), must be collected after the candidate's declaration of intent. Additionally, a candidate may not raise more than 60 times the filing fee during the qualifying period, limiting Supreme Court candidates to raising a maximum of roughly \$66,000 during our study period (Bend 2004).⁴ Spending by the candidate was limited to qualifying contributions collected during the qualifying period and prior contributions under the \$10,000 limit (§163-278).

Public funds are distributed primarily to candidates in contested general elections (§163-278.62(15)). The funding amount for candidates to the high court is equal to 175 times the filing fee to run for the Supreme Court (§163-278.62(15)), which is approximately \$200,000 (Bend 2004). All funds must be spent on legitimate campaign expenditures as defined by the State Board of Elections (§163-278.64(d)(5)) and all unspent funds must be returned after the election (§163-278.64(d)(7)).

¹In the 1996, 1998, 2000, and 2002 elections, partisan affiliations were listed on the ballot.

²Non-partisan races are listed after partisan ones (N.C. Gen. Stat. §163-165.6(b)(3)).

³For a single office, the top two candidates in terms of votes are considered nominated. Where there is a group of open positions, the number of top candidates equivalent to twice the number of open seats are nominated. As discussed below, however, our difference-in-differences approach accounts for time-specific shocks that affect all justices equally.

⁴Additional restrictions exist on contributions by the candidate and his or her family (§163-278.64(d)(4)).

Additionally, the legislature attempted to equalize participating and non-participating candidates in several key ways. The law provided matching funds intended to assist participating candidate in races against non-participating candidates. If an opponent spent more than the amount of funds available to the participating candidate in the general election, the participating candidate received matching funds up to twice the spending limit they would otherwise face (§163-278.67(c)). Additionally, non-participating candidates could not raise funds in the twenty-one days before an election if his or her opponent is participating in public financing and has not received matching funds (Bend 2004; Robertson 2005)⁵ starting in 2004 (Carrington 2010).

In June 2013, funding for the state's judicial public financing system was revoked by the North Carolina state legislature (Parker 2013).

⁵The public financing program has been the subject of legal challenges (Carrington 2010). Judge Barbara Jackson, a successful candidate for the North Carolina Court of Appeals in 2004, unsuccessfully pursued a suit in federal district court claiming that the system violated the First Amendment (Robertson 2005). However, the United States Supreme Court ruled in an unrelated case decided in 2011 that escalating matching funds violated the First Amendment (*Arizona Free Enterprise Club's Freedom Club PAC v. Bennett*). While this ruling falls outside of time period considered in our study, it will affect the future implementation of public financing in North Carolina and elsewhere.

SI-B COMPARISON OF DECISIONS WITH DISSENTS TO UNANIMOUS DECISIONS

In this appendix, we discuss features of decisions with dissents relative to unanimous decisions based on analyses of a subset of cases considering measures of legal breadth and impact. Our data was restricted to non-unanimous, published decisions.⁶ These decisions were substantive decisions on the merits of the case. In order to compare these non-unanimous cases with unanimous cases, we identified equivalent decisions without dissents decided by the North Carolina Supreme Court between 2007 and 2009. We identified these cases via Lexis-Nexis and verified them where applicable with the Hall and Windett (2007) data. These cases represented published opinions and decisions designated in the Lexis-Nexis database as “Cases Reported Without Published Opinion,” which were published dispositions that were not accompanied by a written opinion.⁷ We excluded decisions marked in Lexis-Nexis as “Decision Without Published Opinion” as these documents represented decisions other than substantive decisions on the merits. There were 3,485 such decisions. Likewise, we excluded published orders on motions (of which there were 211).

In order to assess legal breadth and impact, we took advantage of two features of decisions reported in Lexis-Nexis: headnotes and citing cases. The headnotes capture distinct legal concepts, as identified by Lexis-Nexis, to which the decision relates (see, e.g., Hall and Windett 2007). We used the number of headnotes to identify the breadth of the decision in terms of number of legal issues addressed. Citing decisions are other legal decisions by courts that cite the decision in question. We use the number of a citations as a proxy for the impact and reach of the decision (see, e.g., Hansford and James F. Spriggs 2006).

As Tables SI-1 and SI-2 illustrate, the mean numbers of headnotes and citing decisions in non-unanimous cases are significantly larger than those found in unanimous decisions. There were nearly twelve headnotes in the average decision with a dissent, which represents approximately three times more legal issues covered in the decision than the nearly four headnotes in the average unanimous case. Similarly, non-unanimous cases were on average cited around 27 times by courts in later decisions compared with 17 citations for the average unanimous case.

Table SI-1: Number of headnotes

	Observations	Mean	Standard error
Unanimous	193	3.87	0.56
With dissents	40	11.6	1.22
Difference		-7.73**	1.35

* $p < .10$; ** $p < .05$; two-sample t -test for difference of means with unequal variance

⁶We are unaware of any non-unanimous decisions not marked for publication.

⁷One such designated case was dropped from the data because it contained no reported disposition.

Table SI-2: Number of citing decisions

	Observations	Mean	Standard error
Unanimous	193	16.73	2.88
With dissents	40	26.58	3.91
Difference		-9.85**	4.85

* $p < .10$; ** $p < .05$; two-sample t -test for difference of means with unequal variance

SI-C JUSTICE-DONOR CASES

Table SI-3: Nature of cases by era and public financing participation

	Before reform		After reform	
	Case ID	Case type	Case ID	Case type
Not participating	SC-0034	Tort	SC-0061	Criminal
	SC-0035	Insurance Coverage	SC-0062	Voting Rights Act
	SC-0039	Administrative	SC-0063	Family Law
	SC-0041	Administrative	SC-0064	Land
	SC-0048	Damages	SC-0066	Tort
	SC-0050	Contracts	SC-0068	Expert
	SC-0052	Administrative	SC-0071	Administrative
	SC-0053	Family Law	SC-0075	Criminal
	SC-0055	Land	SC-0078	Tort
	SC-0056	Criminal	SC-0080	Administrative, Land
	SC-0057	Redistricting	SC-0090	Tort
	SC-0058	Administrative, Land	SC-0091	Workers' Comp
			SC-0092	Non-Profit Law
			SC-0097	Workers' Comp
			SC-0101	Criminal
			SC-0108	Criminal
			SC-0109	Land
			SC-0115	Expert
		SC-0125	Criminal	
		SC-0128	Tort	
		SC-0129	Damages	
Participating	SC-0008	Administrative	SC-0064	Land
	SC-0009	Family Law	SC-0066	Tort
	SC-0011	Contracts	SC-0068	Expert
	SC-0012	Workers' Comp	SC-0080	Administrative, Land
	SC-0035	Administrative, Land	SC-0097	Workers' Comp
	SC-0034	Tort	SC-0108	Criminal
	SC-0038	Workers' Comp	SC-0118	Administrative
	SC-0054	Family Law	SC-0125	Criminal
			SC-0129	Damages

SI-D ROBUSTNESS: ATTORNEY DONOR CASES

In this appendix, we discuss a number of potential objections to the statistical analysis presented in Section 3 and provide additional evidence in support of the findings in the main text. First, the justices who did not opt in serve as our control group. Of these, one (Martin) ran for re-election after reform without seeking public financing, two chose to retire rather than run for re-election under the new system when their terms expired (Orr and Wainwright), and one was forced to retire after reform passed due to a mandatory retirement age (Lake). A potential concern is whether justices who did not run for re-election under the new system using only private funds belong in the control group. We cannot control for subsequent retirement by justices because it is potentially a post-treatment variable that is affected by changes to the campaign financing system and could therefore bias our effect estimates (King and Zeng 2006). However, we show in Table SI-4 that the difference-in-difference-in-differences results presented below are robust to excluding any or all of the justices who retired from the data.

A second possible objection concerns our coding of individuals who had given as little as \$100 as much as eight years ago as donors. In the main text, we discuss why this small amount of money may serve as a valid indicator of much more valuable and widespread support. Further, we note that some judges may not engage in significant fundraising between elections, a fact that is particularly relevant for judges who have the option to run again but have not determined whether they will seek re-election. However, Table SI-5 shows that our results are robust to alternative coding schemes. Our main results hold whether we use only individuals who have given in the prior four years and if we instead focus on individuals who gave a total of \$250 or more.

Third, one could object that we code attorney donations symmetrically. The attorney donor variable takes a value of -1 for cases in which one or more defendant attorneys gave \$100 or more to the justice in question, 0 when no attorneys did so (our control cases), and 1 when one or more plaintiff attorneys gave \$100 or more to the justice in question in the previous eight years. However, our difference-in-difference-in-differences estimates are substantively similar when plaintiff and defendant attorney donors are analyzed separately relative to control votes in which no attorneys were donors, suggesting that treating the effects as symmetric is not driving our finding that donors are treated relatively less favorably by judges after opting in to the public financing system ($\beta = -0.684, p < .01$ for defendants, $\beta = -0.525, p < .18$ for plaintiffs; results available upon request).

Fourth, another possible concern is the effect of outliers, especially given that we use ordinary least squares even though our outcome variable is binary. Our use of OLS is necessary to estimate the correct effect size in the DDD framework; generalized linear models will not produce the correct quantity of interest (Blundell and Dias 2009; Lechner 2011). Table SI-6 shows, moreover, that our main results still hold if we instead estimate a robust regression model. Further, none of the predicted probabilities that the model generates fall outside the $[0, 1]$ interval.

Fifth, we find no evidence that our results are attributable to a decline in the quality of attorney donors in the post-reform period for justices who opted into public financing, which we proxy using attorneys being named as *Super Lawyers* in North Carolina (<http://www.superlawyers.com/north-carolina>; results available upon request).

Finally, one possible concern is the shift to a nonpartisan election system in 2004, which was a potentially relevant shock to judicial voting patterns during this period that coincides with the introduction of public financing. However, our design accounts for time-specific changes in voting patterns after reform resulting from other factors such as nonpartisan elections by comparing how the voting of participating judges changed *relative to* those who did not participate, which differ-

Table SI-4: OLS models of N.C. Supreme Court voting: Control group variants

	Lake	Excluded justices		L/O/W
		Orr	Wainwright	
Attorney donor	-0.251* (0.134)	-0.226** (0.113)	-0.233* (0.126)	-0.213 (0.178)
Post-reform	-0.033 (0.075)	-0.071 (0.080)	0.005 (0.077)	-0.141 (0.119)
Participating justice (public financing)	-0.094 (0.067)	-0.136** (0.063)	-0.113* (0.063)	-0.223** (0.105)
Attorney donor × post-reform	0.255 (0.205)	0.229 (0.197)	0.187 (0.201)	0.270 (0.245)
Attorney donor × participating justice	0.678** (0.169)	0.662** (0.154)	0.649** (0.164)	0.657** (0.207)
Post-reform × participating justice	0.034 (0.088)	0.058 (0.088)	-0.008 (0.091)	0.130 (0.123)
Donor × post-reform × participating	-0.655** (0.251)	-0.643** (0.234)	-0.575** (0.243)	-0.697** (0.276)
Government plaintiff	0.059 (0.103)	0.055 (0.107)	0.018 (0.103)	0.115 (0.109)
Government defendant	-0.001 (0.084)	-0.039 (0.087)	-0.013 (0.080)	-0.025 (0.097)
Criminal case	0.107 (0.114)	0.177 (0.117)	0.146 (0.115)	0.134 (0.122)
Amicus brief(s)	0.102 (0.066)	0.137* (0.069)	0.092 (0.064)	0.118 (0.075)
Constant	0.466** (0.073)	0.488** (0.076)	0.500** (0.072)	0.562** (0.115)
R ²	0.04	0.08	0.05	0.09
N	416	429	435	296

* $p < .10$; ** $p < .05$ (standard errors clustered by case). Models estimated for all votes cast from 1997–2009 on non-unanimous cases by those North Carolina Supreme Court justices who served before and after the passage of the Judicial Campaign Reform Act. Sample includes all votes cast from 1997–2009 on non-unanimous cases by North Carolina Supreme Court justices who served prior to and after the passage of the Judicial Campaign Reform Act excluding those listed in the columns above: Lake (mandatory retirement), Orr (voluntary retirement), and Wainwright (voluntary retirement). Donor attorneys are defined as those who gave \$100 or more to the justice in question in the four years prior to the year in which the case was heard. Participating justices are those who opted in to the state’s public financing system for judicial campaigns.

Table SI-5: OLS models of N.C. Supreme Court voting by donor status

	<u>Donor definition</u>	
	\$100 or more Previous four years	\$250 or more Previous eight years
Attorney donor	-0.255** (0.115)	-0.225* (0.127)
Post-reform	-0.026 (0.073)	-0.026 (0.073)
Participating justice	-0.099 (0.063)	-0.100 (0.066)
Attorney donor × post-reform	0.219 (0.197)	0.241 (0.198)
Attorney donor × participating justice	0.661** (0.165)	0.798** (0.131)
Post-reform × participating justice	0.016 (0.087)	0.017 (0.089)
Donor × post-reform × participating	-0.603** (0.243)	-0.808** (0.316)
Government plaintiff	0.020 (0.103)	0.015 (0.104)
Government defendant	-0.019 (0.082)	-0.021 (0.082)
Criminal case	0.147 (0.114)	0.151 (0.115)
Amicus brief(s)	0.108 (0.066)	0.106 (0.066)
Constant	0.488** (0.071)	0.488** (0.071)
R ²	0.05	0.04
N	492	492

* $p < .10$; ** $p < .05$ (standard errors clustered by case). Models estimated for all votes cast from 1997–2009 on non-unanimous cases by those North Carolina Supreme Court justices who served before and after the passage of the Judicial Campaign Reform Act. Sample includes all votes cast from 1997–2009 on non-unanimous cases by North Carolina Supreme Court justices who served prior to and after the passage of the Judicial Campaign Reform Act. Donor attorneys are defined as those who gave \$100 or more to the justice in question in the four years prior to the year in which the case was heard (model 1) or \$250 or more to the justice in question in the eight years prior to the year in which the case was heard (model 2). Participating justices are those who opted in to the state’s public financing system for judicial campaigns.

Table SI-6: OLS model of N.C. Supreme Court voting: Robust regression

Attorney donor	-0.272*
	(0.163)
Post-reform	-0.029
	(0.068)
Participating justice (public financing)	-0.118
	(0.082)
Attorney donor × post-reform	0.224
	(0.235)
Attorney donor × participating justice	0.714**
	(0.257)
Post-reform × participating justice	0.022
	(0.107)
Donor × post-reform × participating	-0.635*
	(0.351)
Government plaintiff	0.026
	(0.080)
Government defendant	-0.019
	(0.069)
Criminal case	0.165*
	(0.092)
Amicus brief(s)	0.118**
	(0.057)
Constant	0.486**
	(0.063)
R ²	0.05
N	492

* $p < .10$; ** $p < .05$. Models estimated for all votes cast from 1997–2009 on non-unanimous cases by those North Carolina Supreme Court justices who served before and after the passage of the Judicial Campaign Reform Act. Sample includes all votes cast from 1997–2009 on non-unanimous cases by North Carolina Supreme Court justices who served prior to and after the passage of the Judicial Campaign Reform Act. Pro-plaintiff votes are coded as 1; pro-defendant votes are coded as 0. Donor attorneys are defined as those who gave \$100 or more to the justice in question in the eight years prior to the year in which the case was heard. Participating justices are those who opted in to the state’s public financing system for judicial campaigns.

ences out any common shock to voting patterns during this period. Even more, the DDD strategy allows us to further difference out any post-reform changes in voting between participating and non-participating justices resulting from the change that do not affect the *difference* in voting patterns between cases argued by donor and non-donor attorneys. The switch to nonpartisan elections would only confound our DDD estimates if it affected the *difference* in voting on cases argued by donor and non-donor attorneys for participating justices relative to the pre-reform period when compared to non-participating justices. We know of no theoretical reason to expect such a change and thus believe that our estimates are valid, but we discuss this issue further in the conclusion. However, future research should validate and extend these analyses in other circumstances where the electoral rules used to elect judges were not altered.

SI-E FIRM DONOR CASES

The following table reports the results of difference-in-difference-in-differences models identical to those reported in Table 3 of the main text with firm donation status substituted for attorney donor status. To construct this measure, it was necessary to link firms over time, tracking those that changed names as they added and dropped partners.⁸ This measure is coded similarly to the measure in the main text but uses a higher \$1,000 threshold to qualify as a donor due to the generally higher levels of giving that are observed among firms.⁹ The firm donor variable used below takes a value of -1 when one or more defendant attorneys work at firms that have given \$1,000 or more to the justice in question over the previous eight years (the same pre-contribution period used in the main text), 0 if neither side includes an attorney whose firm has given \$1,000 or more, and 1 if one or more plaintiff attorneys work at firms who have given \$1,000 or more to the justice in question over the previous eight years.¹⁰

⁸We made extensive efforts to match attorneys to firms at the time cases were argued and to link firms over time.

⁹Specifically, the observations that qualify under this standard represent approximately the top two percent of justice-votes with the highest level of firm contributions on the plaintiff or defendant side — the same proportion of justice-votes that qualify on the plaintiff and defendant sides under the \$100 attorney threshold in the main text.

¹⁰We exclude justice-votes in which attorneys from both sides are donors ($n=8$) because our theoretical expectations for these observations are ambiguous and they are unlikely to pool with observations for which no attorneys on either side are donors.

Table SI-7: OLS models of N.C. Supreme Court voting by firm donation status

	Standard error clustering		
	Court case	Justice, year	Justice
Firm donor	-0.164 (0.164)	-0.164 (0.104)	-0.164 (<i>p</i> =.476)
Post-reform	-0.027 (0.074)	-0.027 (0.047)	-0.027 (<i>p</i> =.568)
Participating justice (public financing)	-0.118* (0.071)	-0.118* (0.062)	-0.118 (<i>p</i> =.548)
Firm donor × post-reform	0.344 (0.218)	0.344** (0.130)	0.344 (<i>p</i> =.216)
Firm donor × participating justice	0.233 (0.193)	0.233** (0.073)	0.233 (<i>p</i> =.428)
Post-reform × participating justice	0.034 (0.094)	0.034 (0.071)	0.034 (<i>p</i> =.792)
Firm × post-reform × participating	-0.023 (0.286)	-0.023 (0.189)	-0.023 (<i>p</i> =.974)
Government plaintiff	0.039 (0.109)	0.039 (0.107)	0.039 (<i>p</i> =.518)
Government defendant	-0.054 (0.084)	-0.054 (0.040)	-0.054 (<i>p</i> =.168)
Criminal case	0.107 (0.120)	0.107 (0.104)	0.107 (<i>p</i> =.120)
Amicus brief(s)	0.105 (0.066)	0.105 (0.065)	0.105 (<i>p</i> =.114)
Constant	0.508** (0.073)	0.508** (0.048)	0.508 (<i>p</i> =NA)
R ²	0.05	0.05	0.05
N	487	487	487

* $p < .10$; ** $p < .05$. Models estimated for all votes cast from 1997–2009 on non-unanimous cases by those North Carolina Supreme Court justices who served before and after the passage of the Judicial Campaign Reform Act. Sample includes all votes cast from 1997–2009 on non-unanimous cases by North Carolina Supreme Court justices who served prior to and after the passage of the Judicial Campaign Reform Act. Pro-plaintiff votes are coded as 1; pro-defendant votes are coded as 0. Participating justices are those who opted in to the state’s public financing system for judicial campaigns. Results in the second column include two-way clustered standard errors by justice and year calculated using the Peterson (N.d.) Stata implementation. Results in the third column include p -values clustered by justice from the Cameron, Gelbach, and Miller (2008) wild bootstrap (it does not calculate standard errors).

SI-F ADDITIONAL MODEL INFORMATION FOR CHANGES IN JUDICIAL VOTING PATTERNS ANALYSIS

It is necessary to set the directionality of the space to prevent reflective invariance (Martin, Quinn, and Park 2011) To ensure that positive numbers indicate “conservative” in both periods, we therefore constrained the estimates of four justices who are not part of our treatment or control groups. Justice Newby (who served post-2002) was constrained to be positive (conservative) and Justices Mitchell and Wichard (who served pre-2003) and Timmons-Goodson (who served post-2002) were constrained to be negative (liberal). We found that this set of constraints was the minimal possible necessary to ensure convergence and prevent “label-switching” in the posterior. The full set of results, including justices whose tenure did not overlap with the implementation of reform, are shown in Table SI-8.

Table SI-8: Quantiles for posterior estimates of justice ideal points (1997–2009)

	2.5%	25%	50%	75%	97.5%
<i>Treatment group: Justices who received public financing</i>					
Parker (pre-reform)	-2.47	-1.54	-1.11	-0.75	-0.18
Parker (post-reform)	0.08	0.33	0.47	0.61	0.96
Δ Parker	0.52	0.65	1.58	2.74	2.98
Edmunds (pre-reform)	-2.05	-1.03	-0.59	-0.18	0.57
Edmunds (post-reform)	0.05	0.28	0.40	0.53	0.82
Δ Edmunds	-0.24	-0.05	1.01	2.27	2.53
<i>Control group: Justices who did not receive public financing</i>					
Lake	-0.28	-0.01	0.13	0.25	0.54
Martin	-0.26	-0.02	0.10	0.21	0.44
Orr	-0.50	-0.22	-0.08	0.03	0.26
Wainwright	-0.36	-0.10	0.04	0.19	0.50
<i>Additional justices who didn't serve before and after reform</i>					
Brady	-0.04	0.18	0.30	0.42	0.70
Butterfield	-1.98	-0.56	0.14	0.78	1.88
Freeman	-1.47	-0.32	0.13	0.65	1.93
Frye	-0.61	-0.08	0.19	0.47	1.45
Hudson	-1.32	-0.85	-0.65	-0.48	-0.19
Mitchell	-1.74	-1.04	-0.79	-0.56	-0.21
Newby	0.95	1.51	1.90	2.31	3.29
Timmons-Goodson	-3.20	-2.30	-1.91	-1.54	-1.00
Webb	-0.59	-0.12	0.12	0.41	1.35
Wichard	-1.36	-0.84	-0.62	-0.40	-0.09

Results of one-dimensional item response models estimated using `MCMCpack` (Martin, Quinn, and Park 2011) on votes cast in non-unanimous cases from 1997–2009 by North Carolina Supreme Court justices. Pre-treatment estimates generated using all non-unanimous votes cast in the 1997–2002 period. Post-treatment estimates are generated using votes cast in the 2003–2009 period. The remaining estimates are calculated using all available votes for the relevant justice. Positive scores indicate a more “conservative” justice. “Untreated” justices who did not participate in the public financing system but served before and after reform are constrained to have a constant position to place the ideological estimates in both periods on the same scale. Note that estimates for some justices (e.g., Butterfield) were estimated from very few votes.

References

- Bend, Doug. 2004. "North Carolina's Public Financing of Judicial Campaigns: A Preliminary Analysis." *Georgetown Journal of Legal Ethics* 18: 597.
- Blundell, Richard, and Monica Costa Dias. 2009. "Alternative Approaches to Evaluation in Empirical Microeconomics." *Journal of Human Resources* 44: 565–640.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90: 414–427.
- Carrington, Paul D. 2010. "Public Funding of Judicial Campaigns: The North Carolina Experience and the Activism of the Supreme Court." *N. C. Law Rev.* 89: 1965.
- Hall, Matthew E.K., and Jason H. Windett. 2007. "New Data on State Supreme Court Cases." *State Politics and Policy Quarterly* 13 (4): 427-445.
- Hansford, Thomas G., and II James F. Spriggs. 2006. *The Politics of Precedent on the US Supreme Court*. Princeton, NJ: Princeton University Press.
- King, Gary, and Langche Zeng. 2006. "The dangers of extreme counterfactuals." *Political Analysis* 14 (2): 131–159.
- Lechner, Michael. 2011. "The Estimation of Causal Effects by Difference-in-Difference Methods." *Foundations and Trends in Econometrics* 4: 165–224.
- Martin, Andrew D., Kevin M. Quinn, and Jong Hee Park. 2011. "MCMCpack: Markov Chain Monte Carlo in R." *Journal of Statistical Software* 42 (9).
- Parker, Molly. 2013. "Judicial Races Could Soon Be Off the Public Tab." *Wilmington Star News*, June 22, 2013.
- Peterson, Mitchell A. N.d. "cluster2.ado." Available at http://www.kellogg.northwestern.edu/faculty/petersen/htm/papers/se/se_programming.htm.
- Robertson, Gary D. 2005. "N.C. Suit Filed Over Judge Vote Financing." *Los Angeles Times*, August 9, 2005.