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Original Article

Does Public Financing Chill Political Speech? Exploiting a Court Injunction as a Natural Experiment

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ABSTRACT

In 2011, the Supreme Court struck down the matching provisions in Arizona's campaign finance law on the grounds that they violate free speech by chilling private spending. In this article, we explicitly test the effects of Arizona's matching provisions in two ways. First, we find that privately funded state legislative candidates do not strategically cluster their spending below the threshold that would trigger money to their opponents. Second, we exploit a 2010 Court injunction as a natural experiment. When Arizona's matching provisions were removed, private spending did not increase relative to other states. Contrary to the view of the Court, we find no empirical evidence that campaign finance laws chill private political speech. More generally, our analysis demonstrates the value of exploiting court injunctions as natural experiments to assess the causal effects of laws.

According to supporters, campaign finance laws can help to reduce corruption, increase electoral competition, and thus improve political representation.[1] In recent years, reforms designed to improve the performance of campaign finance policies have been enacted federally and in many states. One class of reforms focusing on public financing of elections has been adopted in several jurisdictions, but these policies have been controversial (Panagopoulos 2011). Critics argue that the public financing of campaigns may crowd out the political spending of private groups and individuals, thus inhibiting certain forms of political speech (Gora 2011). In *McComish v. Bennett* 564 U.S. 664 (2011), the Supreme Court weighed these competing considerations. Arizona's campaign finance law allowed candidates to opt into or out of a public financing program. The state would provide public funds for any participating candidate to match, up to a certain point, the spending of any opposing nonparticipating candidates or independent groups. On one hand, the law protects equal access to resources and enables political speech of participating candidates. Conversely, the law may create a disincentive for nonparticipating candidates and independent groups to spend, thus chilling political speech. As a result of this latter concern, a 5–4 majority struck down the matching funds provision. Because similar cases are likely to arise in the near future, the arguments and empirical evidence deserve closer examination.

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[1]Whether such systems achieve these goals is still up for debate (see, e.g., Dowling 2011; GAO 2010; Kraus 2006; Malhotra 2008; Mayer, Werner, and Williams 2006; Mayer and Wood 1995; Miller 2011b; Milyo, Primo, and Jacobsmeier 2011; Primo and Milyo 2006). See Miller (2011a) for a recent review of the current state of knowledge concerning the effects of public election funding.

The majority's decision relies on the empirical claim that Arizona's matching provisions decreased campaign spending by nonparticipating candidates and independent groups. We do not address the Court's logic on whether this would constitute a violation of free speech. Instead, we focus on the claim noted in the majority opinion that the empirical question is untestable, but nevertheless "evident and inherent." Despite the assumption that the claim is untestable, we argue that such causal effects can and should be tested. We conduct several tests of this claim and find no evidence that matching funds inhibit political spending. Our methods and analysis could serve as a model for future studies interested in using natural experiments to measure judicial and policy effects.

The article proceeds as follows. First, we provide a summary of *McComish* and campaign finance law. Next, we discuss the Court's flawed scientific reasoning in this particular case. Then, we discuss the use of empirical data in campaign finance cases. Causal questions are always difficult to answer, but thresholds, injunctions, and other natural experiments can provide answers to questions previously deemed untestable. Finally, we provide two explicit tests of the claim that matching provisions inhibit speech. We find that the spending of privately funded candidates exhibits no signs of clustering below the triggering threshold (that is, there was not a disproportionate number of privately funded candidates just below the threshold that would trigger matching funds to a publicly funded opponent, as we would expect if such provisions inhibit speech), suggesting that candidates did not strategically restrict their spending in response to the law. Furthermore, the spending behavior of privately funded candidates and independent groups did not change relative to other states after the 2010 Supreme Court injunction that deactivated the matching provisions. If the matching funds provisions were inhibiting the spending (speech) of privately funded candidates, we would expect the spending behavior of these candidates to have changed after the Court's decision. Both sources of evidence suggest that matching provisions do not inhibit political speech.

CAMPAIGN FINANCE LAW AND *MCCOMISH V. BENNETT:* A BRIEF REVIEW

Campaign finance laws governing contributions to and spending by candidates, political parties, and interest groups are often reviewed by the courts, in many cases the Supreme Court, to ensure that they do not abridge individuals' or groups' First Amendment right to freedom of speech. As such, campaign finance laws are subject to strict scrutiny, requiring the state to prove that a law furthers a compelling government interest and is narrowly tailored to achieve that interest (*United States v. Carolene Products*, 304 U.S. 144, 153 n.4 (1938)). The Court has applied strict scrutiny standards to a number of campaign finance laws over the years (for a more detailed review than what is presented here, see, for example Esenberg 2011; Gora 2011; Lowenstein, Hasen, and Tokaji 2008).

In *Buckley v. Valeo*, 424 U.S. 1 (1976) *(per curiam)*, the Court upheld the contribution limits in the Federal Election Campaign Act (FECA) of 1971 (because the act of contributing, not the amount of a contribution, was deemed the expression of support that needed to be protected) but struck down FECA's expenditure limits (because it placed undue restrictions on the quantity of political speech). Other campaign finance laws have also been invalidated by the Supreme Court including those that restrict independent expenditures by express advocacy groups (*Federal Election Comm'n v. Massachusetts Citizens for Life, Inc.*, 479 U.S. 238 (1986)), those that limit uncoordinated political party expenditures (*Colorado Republican Federal Campaign Comm. v. Federal Election Comm'n*, 518 U.S. 604 (1996) (*Colorado I*)), and those that bar unions and corporations from making independent expenditures (*Citizens United v. Federal Election Comm'n*, 558 U.S. 50 (2010)). In addition, the Court has upheld limits on coordinated party expenditures (*Federal Election Comm'n v. Colorado Republican Federal Campaign Comm.*, 533 U.S. 431 (2001) (*Colorado II*)), limits on requirements that political donors disclose their identities (*Citizens United v. Federal Election Comm'n*, 558 U.S 50 (2010)), and limits on the presidential public financing program reviewed in *Buckley*.

Since *Buckley*, many states have instituted voluntary public financing systems. Nearly half (twenty-four) of the fifty states and sixteen local jurisdictions currently offer some form of public funding for campaigns (Stern 2011). In Arizona, a system of public financing of elections (passed by initiative in 1998) provides all candidates for state office—for example, candidates for governor, secretary of state, attorney general, and the state legislature (both the House and Senate)—funding for their primary and general election campaigns if they are (1) able to collect a certain number of $5 contributions from registered Arizona voters and (2) agree to certain campaign restrictions and obligations (limiting the expenditure of personal funds to $500, participating in at least one debate, and returning all unspent money to the State). Arizona's public financing system, similar in many ways to those of Connecticut and Maine, was put in place in an effort to reduce corruption and improve political representation. More specifically, according to a recent report by the Government Accountability Office (GAO), the goal of Arizona's full public funding systems was to increase electoral competition, voter choice, and voter participation, while reducing the influence of interest groups and curbing increases in the cost of campaigns (GAO 2010, 12–14).

One way Arizona attempted to achieve this goal was through the use of matching funds. In addition to an initial lump sum of public funding to those candidates that chose to participate, the state provides public funds to any participating candidate to match, roughly dollar for dollar up to a certain point, the funds of any opposing privately financed candidates or independent groups that spent money against the publicly funded candidate (see Ariz. Rev. Stat. Ann. § 16–952).[2] The state argued that such a matching provision was crucial, perhaps even necessary, to fulfill the goals of public financing systems outlined above. Certain (privately financed) candidates and independent expenditure groups, however, challenged the constitutionality of the matching provisions on the grounds that matching funds inhibit their freedom of speech by placing an undue burden on their ability to exercise their First Amendment rights (see, for example, the quotes from candidates in Miller 2008). In particular, the petitioners argued that the matching funds provision created a disincentive for nonparticipating candidates and independent groups to spend money during the campaign because they did not want to trigger matching funds that would be directed to their opponents. Based on this argument, "The District Court entered a permanent injunction against the enforcement of the matching funds provision. The Ninth Circuit reversed, concluding that the provision imposed only a minimal burden and that the burden was justified by Arizona's interest in reducing *quid pro quo* political corruption." *McComish v. Bennett*, 564 U.S. 664 (2011) at 667.

The Supreme Court heard oral arguments on March 28, 2011 and rendered its decision on June 27, 2011. Relying in large part on precedent set in *Davis v. Federal Election Comm'n*, 554 U.S. 724 (2008), the Court reversed the decision of the Ninth Circuit Court of Appeals, concluding that "Arizona's matching funds scheme substantially burdens political speech and is not sufficiently justified by a compelling interest to survive First Amendment scrutiny" at 667.[3]

FLAWED REASONING IN THE MAJORITY OPINION

After reviewing the evidence for the petitioners' claim that the presence of matching funds caused

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[2]More specifically, the Court summarized the Arizona matching funds provision as follows:

> Matching funds are available in both primary and general elections. In a primary, matching funds are triggered when a privately financed candidate's expenditures, combined with the expenditures of independent groups made in support of the privately financed candidate or in opposition to a publicly financed candidate, exceed the primary election allotment of state funds to the publicly financed candidate. §§16–952(A), (C). During the general election, matching funds are triggered when the amount of money a privately financed candidate receives in contributions, combined with the expenditures of independent groups made in support of the privately financed candidate or in opposition to a publicly financed candidate, exceed the general election allotment of state funds to the publicly financed candidate. §16–952(B)…Once matching funds are triggered, each additional dollar that a privately financed candidate spends during the primary results in one dollar in additional state funding to his publicly financed opponent (less a 6% reduction meant to account for fundraising expenses). §16–952(A). During a general election, every dollar that a candidate receives in contributions—which includes any money of his own that a candidate spends on his campaign—results in roughly one dollar in additional state funding to his publicly financed opponent…Once the public financing cap is exceeded, additional expenditures by independent groups can result in dollar-for-dollar matching funds as well. Spending by independent groups on behalf of a privately funded candidate, or in opposition to a publicly funded candidate, results in matching funds. §16–952(C).

564 U.S. 664, 672–673 (2011).

[3]*Davis* invalidated the "Millionaire's Amendment" of the Bipartisan Campaign Reform Act (BCRA) of 2002, 2 U.S.C. § 441a–1(a). The "Millionaire's Amendment" permitted a candidate for the U.S. House of Representatives to receive individual contributions of up to $6,900 per contributor (three times the regular contribution limit of $2,300) if the candidate's opponent spent more than $350,000 of his or her own funds, while the opponent remained subject to the regular contribution limit. The Court concluded that the Millionaire's Amendment was unconstitutional because it forced a candidate "to choose between the First Amendment right to engage in unfettered political speech and subjection to discriminatory fundraising limitations." *Davis* at 739.

a reduction in spending, Chief Justice John Roberts, writing for the majority, concedes that "it is never easy to prove a negative." Despite this admission of a lack of evidence, he declares it "evident and inherent" that the law imposes a burden, writing that it is "clear…that a candidate or independent group *might* not spend money" (italics added).

The majority argues that because there is the *potential* that the law might change behavior, it should be assumed that it does so. Moreover, the majority argues that the lack of evidence to support this claim is not consequential because the nature of the claim means that it is difficult to test. Here we demonstrate that Chief Justice Roberts' claim is not valid in this particular case or as a matter of general principle. Causal claims about the effects of a law can be tested, even when the claim implies a negative effect.

The claim that "it is never easy to prove a negative" appears to have originated in *Elkins v. United States*, 364 U.S. 206 (1960). From a scientific standpoint, this is a strange proposition. To accept the claim is to ignore the principles of modern science and to discount the countless scientific studies that have "proven" a negative effect.[4] Of course, in practice, testing a claim, positive or negative, is difficult in the complex social world. However, here we argue that, not only can we show very compelling evidence for the negative—but that, unlike Roberts, we also do not need to make assumptions about the effect of the law. Ample evidence is available for scientific inquiry. In fact, the Court, by its own actions, provides empiricists with tools that allow us to test for a negative or a positive. In June 2010, the Court issued an injunction that suspended the distribution of matching funds during the 2010 election cycle in Arizona. This intervention provides a unique opportunity to test for the effects of matching provisions on overall campaign spending. This is a generally applicable tool that the Court should harness whenever it is useful.

Consider the statement that a negative relationship is "never easy to prove." How could this be? Causal relationships are always difficult to test, but to the extent that we can ever answer causal questions, a negative effect is no more difficult to demonstrate than anything else. To see this, imagine where medical science would be if we thought that we could not test statements like "Aspirin is associated with a decrease in heart-attacks." Researchers demonstrate this sort of negative all the time—often through randomized, controlled experiments. Despite common sense and scientific thinking, the claim that it is "never easy to prove a negative" has appeared with explicit reference to *Elkins* in at least three opinions from the Supreme Court since the *Elkins* decision.[5] Since this notion has legal traction, we believe it is worth further discussion.[6]

Perhaps negatives are more difficult to demonstrate in the social world where a researcher deals with human behavior, rather than the medical world where Aspirin or smoking is under consideration. But the claim is also not valid here either: negatives can be demonstrated in the social world too. To see why negatives are also readily demonstrated in the social world, consider the opinion of Justice Stewart in *Elkins v. United States*, from which the claim originates. Stewart is considering whether laws excluding improperly obtained evidence deter law enforcement from conducting improper searches:

> Empirical statistics are not available to show that the inhabitants of states which follow the exclusionary rule suffer less from lawless searches and seizures than do those of states which admit evidence unlawfully obtained. Since, as a practical matter, it is never easy to prove a negative, it is hardly likely that conclusive factual data could ever be assembled. For much the same reason, it cannot positively be demonstrated that enforcement of the criminal law is either more or less effective under either rule.

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[4]To be more precise, in scientific inference, the researcher will usually set out to test a "Null Hypothesis" of no relationship between the variables of interest. If sufficient valid evidence exists that is not supportive of the Null Hypothesis, than the Null Hypothesis will be rejected in favor of evidence for a negative or positive effect.

[5]*Harrison v. United States*, 392 U.S. 219 (1968); *United States v. Janis*, 428 U.S. 433 (1976); *Reno, Attorney General v. Bossier Parish School Board et al.* (95-1455), 520 U.S. 471 (1997).

[6]To be clear, one sense of the phrase, "it is never easy to prove a negative," is correct. Philosophers and logicians will remind us that we can never prove claims of non-existence. For example, no scientific analysis could ever disprove the existence of unicorns, Santa Claus, or the Tooth Fairy. Certainly, the lack of empirical evidence should tell us something about the likelihood of these propositions, but any statement of non-existence (i.e., a negative statement) cannot be proven—although this *argumentum ad ignorantiam* is usually seen as a logical fallacy and previous courts have recognized it as an impossibly high burden of evidence (*Hamling v. United States*, 418 U.S. 87 (1974)). While negative propositions about existence cannot be proven, there is nothing special about causal claims that have a mathematically negative sign.

The rise of modern, statistically based criminology makes this statement outdated. Even in the 1960s, a researcher could have addressed the question, given the proper resources. Just as with Aspirin and heart attacks, a criminologist might approach this question by collecting data on searches and seizures in states with and without the exclusionary rule and then comparing them. The researcher could then decide, based on accepted standards of evidence, whether or not the negative relationship had been demonstrated. If a relationship between the exclusionary rule and searches and seizures existed, it should be revealed. However, at least two objections might be raised. First, that data separating "lawless searches and seizures" from other searches and seizures might not exist. This is a valid concern that can apply to any scientific domain: if there is no data, empirical investigation is not possible. Fortunately, in the case at hand, evidence is readily available: campaign spending and contributions are a matter of public record. Second, and more crucially, a sophisticated critic might point out that, even if the data did exist, a researcher could not establish a *causal* relationship between the exclusionary rule and the lawless searches and seizures. There might be a relationship between the exclusionary rule and lawless searches and seizures, but the relationship does not mean that the exclusionary rule *causes* fewer lawless searches and seizures. The number of lawless searches and seizures might be correlated with exclusionary rules for reasons that are not accounted for. This is the crucial and more difficult point. In Arizona, we can easily determine whether spending went up or down after the implementation of the law. However, it is more difficult to assess whether the law caused spending to change. This is where the power of the court to issue injunctions becomes particularly useful.

As we show in the following sections, the empirical claim surrounding *McComish* can and should be tested. Our tests provide the opportunity for a negative effect to manifest itself. However, the tests reveal that matching provisions have no detectable negative effect on political speech.

HOW CAN WE ASSESS THE CAUSAL EFFECTS OF LAWS?

While the Court has applied strict scrutiny to campaign laws on the basis that they *might* impede speech, the underlying logic is that the laws *cause* limited speech. In many cases, causal inference—that is, testing whether the action actually causes the outcome—is very difficult or impossible. Fortunately, ample opportunities exist to test for the effects of the law in question.

Consider the empirical question at hand: did Arizona's matching provisions inhibit speech? Even with easy access to data, we cannot easily demonstrate this causal effect. Suppose that a researcher decided to compare spending in states with matching provisions (Arizona, Connecticut, and Maine) to those without matching provisions to see whether states with matching provisions see less spending. This would not answer the question at hand, because the states that adopted these provisions were probably different to begin with. Those differences between states might explain any differences in campaign finance law and campaign spending. Moreover, campaign spending probably influences campaign laws just as much as laws influence spending, so a strong correlative relationship would not reveal the direction of causation. For example, maybe these states felt free to adopt the laws because candidate spending was already low. Now suppose a second researcher proposes another approach; she will test for changes in campaign spending immediately after the adoption of a new law. This strategy is an improvement, but we would still worry that any changes in behavior were explained by other changes around that time. For example, a state might only adopt a new law when the amount of political speech is expected to change in the future.

Randomized experiments are the ideal method for assessing causal effects. In an ideal world, we could test for the effect of matching provisions by randomly assigning the law to be in place in some states and not others. This study can serve as a model for future assessment of the causal effects of the law. For most legal questions, we cannot design the ideal, randomized experiment. In the case of *Elkins*, we cannot randomly assign some states to use the exclusionary rule and other states to not, just like we cannot randomly assign campaign finance laws. For this reason, social scientists must search for opportunities where the conditions of an experiment have been implemented without interference by the researcher or by those being studied, often referred to as "natural experiments" (see, for example, Robinson, McNulty, and Krasno 2009).

Occasionally, institutional features of a law or a natural event closely mimic the ideal experiment. In this case, we identify two such opportunities. First, matching funds are only triggered when a privately funded candidate spends above a specific threshold. Therefore, if matching funds truly inhibit spending, candidates should cluster their spending just below the threshold. A strategic candidate that would otherwise spend above the threshold might hold back their spending to avoid triggering the matching funds for their opponent.[7] This threshold is a unique feature of Arizona's law that we can exploit for the purpose at hand. Second, the June 2010 Supreme Court injunction serves as a natural experiment where Arizona's law was suddenly deactivated. When the court issues injunctions, they often unintentionally act as social experimenters, giving researchers the opportunity to examine the effects of the law in a way that makes clear causal inferences. In this sense, the court acts as both investigator and judge, providing the evidence for their own answer to the question. As such, the Court itself allows the opportunity to avoid reliance on proclamations like that of Chief Justice Roberts that the effect of the law is self-evident.

The legal world generally provides a promising place to look for natural experiments in socio-political processes, because the courts are isolated from immediate socio-political influences. In an experiment, the researcher intervenes into the relationship between the subject and the outcome of interest, perhaps by administering a drug or some other treatment. Importantly, the researcher intervenes for reasons that are unrelated to the characteristics of the subject. After the intervention, the researcher can measure its effect. In the social world, where we want to understand the effects of laws, the courts often intervene into the relationship between the law and the law's effects—for example by issuing an injunction. Although this injunction has not been randomly assigned, it is a sudden intervention that allows researchers to assess its effects. By examining behavior before and after the injunction and by comparing Arizona to other states, we can understand the effect of the law.

Here we examine the effect of matching provisions on election spending in Arizona, using the triggering threshold and the Court's injunction as evidence. We find no evidence that the law affected the spending of non-participating candidates. Although it is difficult to assess the causal effects of laws, we offer this analysis as a model for future investigation regarding the effects of campaign finance law. We now to turn to our tests as a demonstration of how empirical data can be harnessed to address causal claims—even negative ones—in questions of law.

NONPARTICIPATING CANDIDATE SPENDING DID NOT CLUSTER BELOW THE THRESHOLD

If nonparticipating candidates in Arizona consciously hold back their campaign spending as a result of the matching provisions, we would expect some clustering just below the triggering threshold. As noted in the majority opinion, in order to maximize the available funds, candidates can, of course, simply participate in the public financing option. However, not all candidates did participate, so among those that are privately financed and want to win their elections, if the law was a burden, we should see this reflected in their spending behavior. We can assume that every candidate should spend as much as they have to spend to win. However, if candidates react strategically to the expectation that their opponent will receive public funding, as petitioners have argued, then we would expect some candidates to decide that the advantage they gain from any amount spent above the threshold will be a net disadvantage because of the spending it would trigger for their opponents. These candidates should spend every dollar up to the threshold (because their opponent has this amount anyway) and then stop their spending. If candidates think this way, as the petitioners assert, then we should see a significant clustering of candidates near the threshold amount. If we see candidates clustered near the threshold, this would be key evidence of a burden.

To test for clustering, we collected general election spending data for every nonparticipating state legislative candidate in Arizona for 2006, 2008, and 2010 that ran against at least one participating

[7]Alternatively, a strategic candidate might hold back their spending until right before Election Day to avoid triggering matching funds with sufficient time for the publicly funded candidate to respond. If such behavior was common, we would observe a cluster of spending just above the triggering threshold. As we show below, we find no evidence for this sort of clustering either.

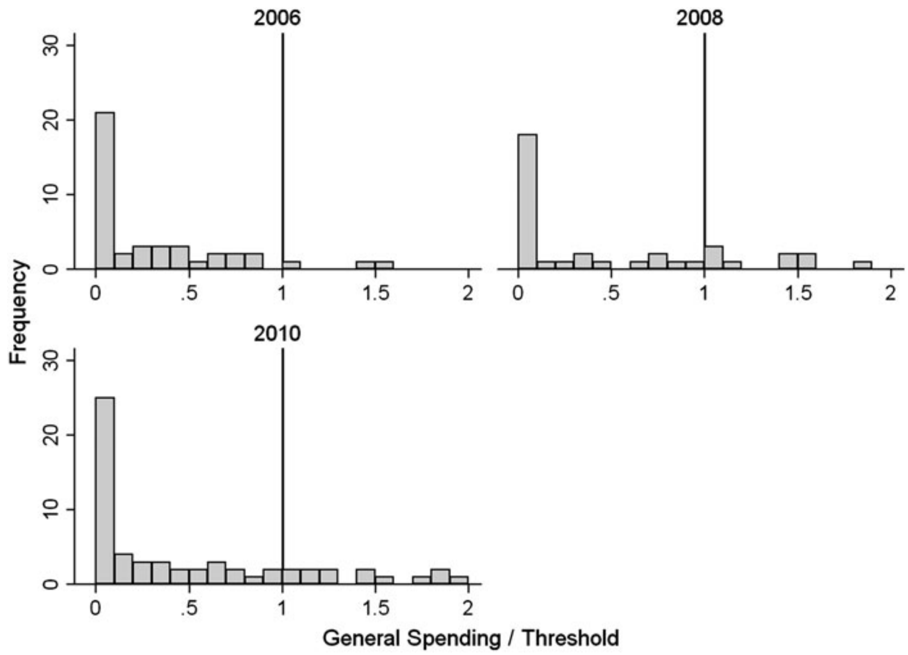

FIG. 1. Distribution of spending by privately funded (nonparticipating) state legislative candidates facing publicly funded opponents in Arizona (General Elections, 2006–2010). *Note:* The figure shows the distribution of campaign spending by privately funded state legislative candidates in Arizona who faced a publicly funded opponent. Spending is re-scaled so that the triggering threshold is 1. Under the matching provisions in 2006 and 2008, we see no evidence that candidates clustered their spending below the triggering threshold. In 2010 after the matching provisions were out-of-effect, the distribution looks almost identical.

candidate. For each nonparticipating candidate we divided his or her expenditures by the applicable statutory triggering threshold ($17,918 for 2006, $19,382 for 2008, and $21,479 for 2010). This means that a candidate that spent exactly as much as the triggering threshold (for example, $17,918 in 2006) receives a value of 1.

F1 ▶ Figure 1 shows the distribution of spending for privately funded (nonparticipating) candidates who faced a publicly funded opponent in each of the three election cycles we analyze.[8] The horizontal axis displays the converted spending figures (nonparticipating candidate spending in the general election divided by the triggering threshold), while the vertical axis displays the number of nonparticipating candidates that fall into each bin.[9] The triggering threshold is denoted by the solid vertical line. Most nonparticipating candidates spend well below the threshold. The most obvious feature of this figure is the clustering near zero dollars in spending. Many of these candidates were probably either likely to win by a large margin, uncompetitive, or unable to qualify for public funding. The area of interest in these figures is near the vertical line. Do candidates cluster around the line?

We see no evidence that nonparticipating candidates intentionally spend below the threshold. If nonparticipating candidates were intentionally spending below the threshold, we might expect a cluster of spending just to the left of 1 (the vertical line). However, there are just as many nonparticipating candidates spending barely above the threshold as there are nonparticipating candidates spending just below. Moreover, in no year is there a large amount of candidates around the threshold (just below or above it).

A more detailed examination of the data reveals a pattern that is strikingly unsupportive of the assertion that candidates were withholding their spending. For 2006, 2008, and 2010 there were 153 nonparticipating candidates for state legislature running against a participating opponent. Of those, 91

[8]For visual purposes, we do not show the candidates (less than 10% of the sample) who spent more than two times the threshold.

[9]Each bin is equivalent to one-tenth of the spending threshold, such that the bin to the left of the vertical line includes candidates who spent somewhere between nine-tenths of the spending limit and exactly the spending limit. The size of the bins is immaterial to the conclusions we draw (results available upon request).

nonparticipating candidates spent less than $10,000, which is well below the triggering threshold. These candidates were not deterred by matching funds because they did not raise or spend enough funds for the threshold to be a relevant consideration. For example, in 2008, these candidates could have spent an extra $9,382 without triggering matching funds. Of the remaining 62 candidates, 23 spent more than $30,000, well above the triggering threshold. These candidates also do not appear to be deterred by matching provisions. That leaves less than one-fourth of nonparticipating candidates whose spending could have been plausibly deterred by the matching provisions. Among these candidates, we see no evidence that they strategically stop their spending below the threshold.

This analysis alone does not establish the causal effect of the law. While the distribution of spending is strongly inconsistent with the claim of chilled spending, we cannot possibly account for all of the many factors that contribute to the shape of the distribution of spending. However, the Court's injunction in 2010 gives us the first opportunity to explicitly test the causal effect of the law. If the law had an effect on candidate spending, then we would expect candidates to behave differently in 2010 than 2006 or 2008. Yet, Figure 1 suggests that the distribution of spending is similar for all three years, and there is no evidence of clustering below the triggering threshold in any year. This analysis would not have been possible without the court's injunction, which mimics an experimental intervention.

As an additional test, we conduct a parallel analysis using general election campaign spending data for privately funded (nonparticipating) state legislative candidates who faced publicly funded opponents in Maine between 2000 and 2010. Maine has a similar, although not identical, public financing program with a matching funds provision for state legislative candidates. With the inclusion of Maine in our analysis, we are moving towards a more robust test of the relationship between the law and candidate spending. When we only examined Arizona, the pattern expected by the petitioners might not emerge because of some oddity of Arizona that we cannot account for. By examining another state with a similar law, we are, essentially, giving the petitioners two opportunities to prove their case.

Figure 2 presents the spending distributions among these candidates in Maine for each election F2

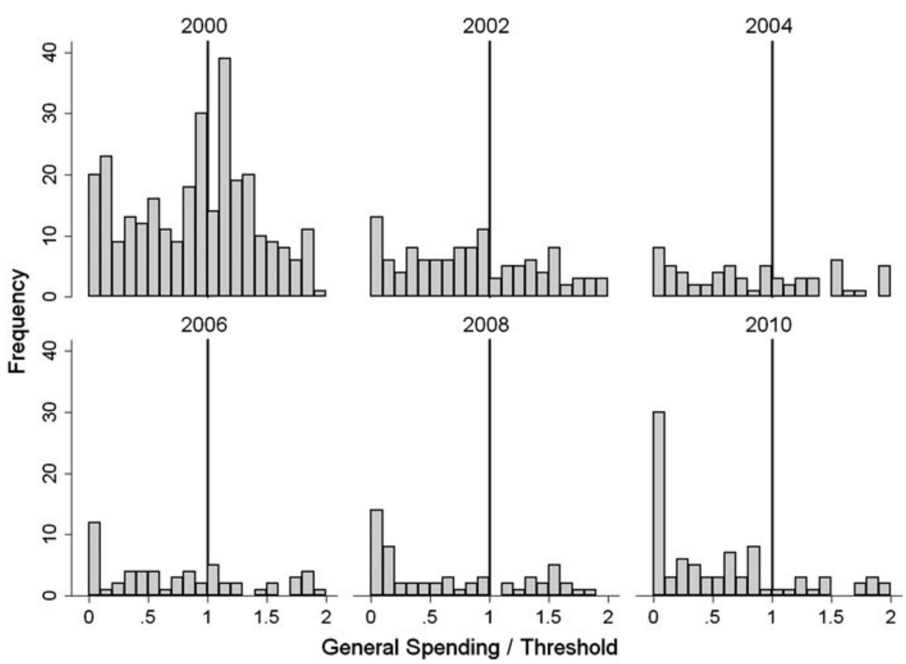

FIG. 2. Distribution of spending by privately funded (nonparticipating) state legislative candidates facing publicly funded opponents in Maine (General Elections, 2000–2010). *Note:* The figure shows the distribution of campaign spending by privately funded state legislative candidates in Maine who faced a publicly funded opponent. Spending is re-scaled so that the triggering threshold is 1. Matching provisions were in effect for all six elections shown. We see no evidence that candidates clustered their spending below the triggering threshold in any of the elections.

cycle. As can be seen by the decreasing frequency of candidates in each graph, moving from 2000 to 2010, the number of nonparticipating candidates has decreased over time, presumably because more and more candidates have seen the program as beneficial and chosen to take part. As in Arizona, we can look for visual evidence of clustering below the corresponding triggering thresholds—with candidate spending spread around the distribution, no clear visual evidence emerges. However, we do not have to rely on visual evidence alone. Below we proceed to a more rigorous statistical analysis that allows us to test for the presence of patterns that may not be apparent to the naked eye.

The most rigorous statistical test for clustering, proposed by McCrary (2008), tests for a discontinuity of the density function at the threshold by conducting local linear regression on both sides of the threshold.[10] This procedure builds on previous work applying the principles of regression to the estimation of a density function at a boundary or threshold (Cheng, Fan, Marron 1993). Appendix Table 1 presents statistical estimates for the extent of clustering around the threshold for the analysis of candidate spending in Arizona. The coefficients we report can be interpreted as the difference between the number of nonparticipating candidates spending just below the threshold and the number of nonparticipating candidates spending just above the threshold. Positive values would indicate clustering below the threshold and negative values would indicate clustering above the threshold. This analysis reveals no statistically significant evidence of clustering in Arizona or Maine. Positive coefficients would serve as evidence of clustering. The clustering coefficients are all substantively small and statistically indistinguishable from zero (the smallest *p*-value is .33).

We can again look at 2010 as an experiment that allows us insight into the causal effect of the law. The coefficients for 2006–2008 and 2010 are very similar. This indicates that, even with the law not in effect in 2010, in terms of spending the candidates behaved in a very similar manner to the 2006–2008 period when the law was in effect. This is evidence that the law had no discernible effect causing candidates to hold back their spending.

In short, both the visual and statistical analyses for clustering around the triggering thresholds in Arizona and Maine do not reveal evidence that candidate spending is strategic around the triggering thresholds. These empirical results lead us to conclude matching provisions do not chill spending.[11] That this threshold was set by an agent other than candidates themselves, provides for an opportunity to assess the effect of the law. If this law created a distribution of spending that would not otherwise exist, it should be in relation to the threshold set by the law. In this sense, the law itself provides an opportunity to test for the effect of the law, much like an experimenter intervening in a process and seeing if the results are different than if there was no intervention. However, for reasons already discussed, there are limitations to what we can learn about the causal effects of this law from this approach. We need something that more closely approximates an experiment. Next we turn more fully to the natural experiment provided by the Court.

THE 2010 INJUNCTION DID NOT ALTER CANDIDATE OR INDEPENDENT GROUP SPENDING

As discussed, the Court's 2010 injunction serves as a natural experiment, allowing us to test for the causal effect of matching provisions. The Court only intervened in one state, making Arizona a treatment group, while the other states serve as a control. The petitioners claimed that the law caused less spending in Arizona than would have occurred without the law. If the law did influence spending, then we should see a change in Arizona, relative to other states, after the Court's intervention. If matching provisions had previously had a chilling effect on spending by nonparticipating candidates, we should see their spending increase dramatically in Arizona relative to other states in 2010.

We return to spending data for nonparticipating state legislative candidates to test this hypothesis. In 2010, candidates in Arizona decided whether or not to participate in public funding before the injunction occurred. This means that their choice to participate in the election could not have been influenced by the injunction, suggesting that the

[10]This method requires arbitrary decisions to be made about the bin size and the window of analysis. However, the results we present here are insensitive to changes in these specifications.

[11]This conclusion is consistent with those made by Gierzynski (2011), in his own analysis of the effect of Maine's public funding program on fundraising and spending.

types of candidates participating should be similar for 2006, 2008, and 2010. As such, this allows for a valid comparison of the spending of candidates across those years and the test approximates a randomized experiment in which subjects would be assigned to the treatment and control conditions by the researcher to ensure the validity of comparison.

At first glance, 2010 was not an extraordinary year in terms of campaign spending. Among privately funded state legislative candidates who ran against a publicly funded opponent, 2010 spending was similar to that in 2008 ($15,223 per candidate versus $16,172). This initial comparison suggests that the injunction did not have a significant effect on the spending behavior of state legislative candidates. The magnitude of spending was no different under a system of matching provisions than it was when the provisions were removed. In fact, spending was even lower in 2010 than it was in 2008. This is evidence that matching funds did not chill the spending of nonparticipating candidates; if matching funds had chilled spending, we should observe more spending in 2010 when the injunction had been issued.

However, there are many factors that affect spending year to year—for example, a lack of public interest in the campaign that year—so perhaps spending in Arizona would have been exceptionally low in 2010, but the injunction caused spending to return to normal levels. We can try to account for this by comparing spending in Arizona to other states over time, with these states serving as a "control" group. Of course, every election year is different, and the magnitude of spending will vary with the political climate of the time. For example, spending in states holding elections for high-profile, statewide offices, like governor, will likely be higher than in elections in states without statewide contests. However, states should exhibit similar trends in campaign spending over time. Therefore, we can compare changes in spending in Arizona in 2010 to the spending changes in other states to obtain a better estimate of the effect of the injunction.

As we note above, Maine's campaign finance program is similar to Arizona's, and both states have similar levels of per capita campaign spending. The crucial difference is that the Court did not issue an injunction in Maine, making Maine a natural control state to compare with Arizona for an initial cross-state analysis. Figure 3 shows the trends in campaign spending for all state legislative and statewide candidates from 2002 to 2010 who opted out of public funding. Both states exhibit a similar trend in spending from 2002 to 2008. If matching provisions chill political spending, we should expect that the removal of these provisions in Arizona in 2010 would cause spending in Arizona to increase more precipitously relative to spending in Maine. However, the evidence shows that per capita spending in Maine actually increased more so than in Arizona between 2008 and 2010. Over this period, nonparticipating candidates in Maine increased their spending by 7.4 dollars per person, while Arizona's nonparticipating candidates only increased spending by 1.3 dollars per person. This is evidence that the injunction, that is the treatment of removing the law, did not have any effect on spending, indicating that the law itself was not affecting spending.

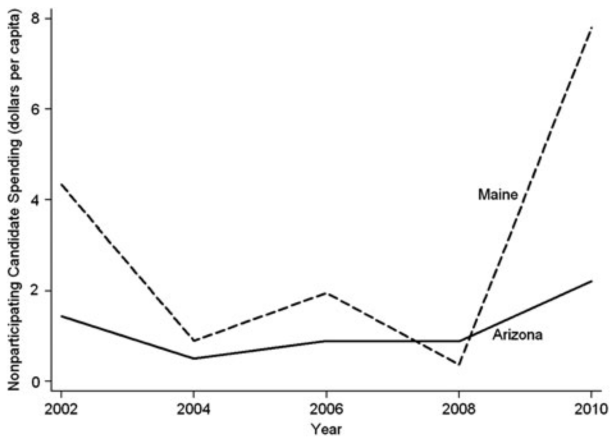

FIG. 3. Spending trends for privately funded (nonparticipating) candidates in Arizona and Maine (2002–2010). *Note:* The figure shows the level of campaign spending by privately funded state legislative candidates in Arizona and Maine between 2002 and 2010. The 2010 Court injunction took matching provisions out-of-effect in Arizona in 2010. If these provisions inhibit spending, we should see spending increase dramatically in 2010 in Arizona relative to Maine over the same time period. However, we find no such effect.

Of course, a control group of only one state is problematic. There may be characteristics of Maine, which we do not account for, that caused their spending to rise. For a stronger test, we collected data on general election campaign spending between 2000 and 2010 for as many states as possible. For each state-election, we obtained two figures: (1) the total amount of spending by statewide and state legislative candidates and (2) the total amount of spending by PACs and interest groups.[12] Appendix Table 2 shows the state elections for which this data has been collected. All of these states hold their gubernatorial and statewide races at the same time, so for the purposes of our analysis, we should expect that these states exhibit similar trends in campaign spending over time.

Since we are using many states, rather than just Maine as our only control state, we can employ a statistical technique called synthetic control (see Abadie, Diamond, and Hainmueller 2010). This technique creates a composite of the states to serve as a baseline against which to compare spending changes in Arizona. This approach allows us to find the weighted average of other states that most closely mirrors Arizona in terms of campaign spending, population, and population growth, and other demographic or political variables. This presents us with a state that is well balanced as a control group to test against Arizona, which is what we would want to obtain in a setting where we could use a true randomized experiment to assign states to treatment and control. It also allows us to account for objections voiced by Justice Kennedy during oral arguments that the change in population in a state had to be accounted for when considering changes in spending.[13] We do that directly here by including population change in our regression models.

Through multiple tests and specifications, we find no evidence that campaign spending increased in Arizona in 2010 relative to the synthetic control of other comparable states. If anything, the increase in spending in Arizona between 2008 and 2010 was *smaller* than the increases in other states.

F4 ▶ Figure 4 provides one example of such a synthetic control analysis. For all states where we could obtain candidate spending data for 2006, 2008, and 2010 (CA, ME, MI, and TN), we estimated the control group that best matches Arizona in terms of per capita candidate spending in 2006 and 2008 and population growth. This particular synthetic control group is a weighted average of Tennessee (weight = .93) and California (weight = .07). Between 2008 and 2010, spending in the synthetic control group increased by 4.0 dollars per person, while spending in Arizona increased by only 1.8 dollars per person. This tells us that even when looking at very similar states, we find hardly any evidence that the injunction changed spending by candidates.

As a final test of the effect of the 2010 injunction, we can employ difference-in-differences methods

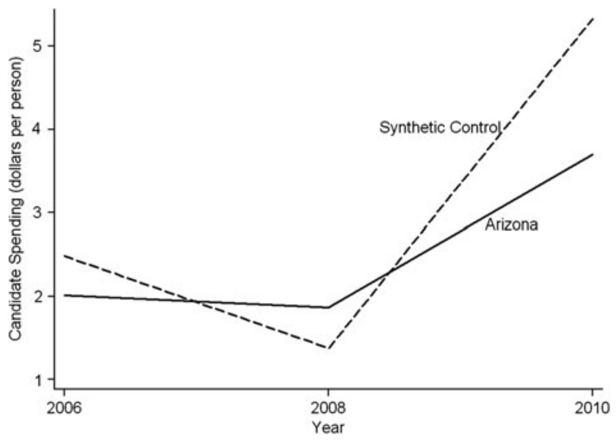

FIG. 4. Candidate spending in Arizona and synthetic control group (2006–2010). *Note:* The figure shows the level of campaign spending by privately funded state legislative candidates in Arizona and a synthetic control unit between 2006 and 2010. The synthetic control unit is the optimal weighted average of other states that most closely mirrors Arizona along numerous political and demographic characteristics. The 2010 Court injunction took matching provisions out-of-effect in Arizona in 2010. If these provisions inhibit spending, we should see spending increase dramatically in 2010 in Arizona relative to the synthetic control unit over the same time period. However, we find no such effect.

to test whether spending in Arizona in 2010 increased more than would have been expected in the absence of the injunction. A difference-in-differences method simply means we look at the differences in changes in spending between sets of states. If the injunction exerted an effect, we would expect the difference between Arizona in 2010 and previous years to be greater than the difference between 2010 and previous years for other states. This is a rigorous way to control for any preexisting differences in spending between states. At this point we are coming close to approximating a laboratory experiment—one in which an experimenter would take identical groups, measure their condition before administering a treatment, and then measure their condition after administering a treatment.

In Appendix Table 3, we present the results of six regression analyses designed to systematically test

[12]Each state has different reporting requirements and differing degrees of data accessibility. However, for the purposes of our analysis, it is most important that the data is consistent within each state. This is because the difference-in-differences design used below compares differences within states, which effectively controls for any other differences between states.

[13]Oral Arguments. Transcript page 32, line 16.

for such an effect. The regression models test for the effect of the 2010 injunction in Arizona while controlling for different average levels of spending in each state and for different spending levels in each year (captured through the use of state and year fixed effects variables in the models). The table reports the estimated effect of the injunction on three types of spending separately: (1) total candidate spending, (2) total spending of privately funded, nonparticipating candidates, and (3) total spending by PACs and independent groups. The estimates are reported as both units of dollars spent per person living in the state and as logs of those spending levels. The table also presents the standard errors associated with each estimate, indicating whether the estimates are statistically different from zero.

If matching provisions chilled campaign spending before 2010, we would expect a large, positive effect of the injunction on spending. For all three types of spending, we fail to estimate a statistically significant or substantively meaningful effect of the injunction. In fact, the models indicate that candidate spending actually decreased after the injunction in 2010. This is the opposite of what should happen if spending was chilled. Focusing on the analysis of private and independent spending, we estimate that nonparticipating candidates spent about 3 dollars per person less than expected and independent groups spent 25 cents per person more than expected on average. Neither result, however, is statistically significant or substantively meaningful. In sum, we fail to find any evidence that the injunction increased spending or that matching provisions inhibited spending.[14] The lack of effect on independent spending is noteworthy, considering the attention devoted to it by the Court. On average, Arizona candidates appear to have spent just as much in the presence of matching provisions as they would have in the absence of such a policy.

CONCLUSION

We conducted more than ten statistical analyses to determine whether there is empirical support for the claim that public financing matching provisions chill spending, and thereby speech, in elections. The evidence we present consistently points in the same direction and leads us to conclude there is no systematic, empirical evidence of such a chilling effect. Moreover, our estimates are not plagued by omitted variables or other problems because we exploit a natural experiment in which the law was exogenously manipulated. This is a strong assessment of the causal effect of matching funds in Arizona (and Maine). We have shown, despite the claims of the Chief Justice, that a negative effect could be shown, with a high degree of scientific rigor.

We emphasize that the tests we conducted could have yielded a different answer. We could have found that the Arizona law did significantly decrease candidate and outside group spending. The interpretation of this fact would have been left to the Court. By conducting rigorous and methodologically sound tests, we were open to the possibility of finding evidence that supported either side of this far-reaching case. The argument used by the majority, that their expectation is simply "evident and inherent" could, of course, be used by either side of any argument. However, there would be no objective way to adjudicate between competing claims of inherent truths.

Prior studies have demonstrated that the Court often has difficulty interpreting and harnessing social scientific research (Rustad and Koenig 1993), so, perhaps, the majority's dismissal of empirical evidence should have been expected. However, even if the majority had chosen to engage the evidence weighing against the petitioners' claim, the essential empirical question, that of a causal effect of the law on candidate speech, may still have been difficult to answer. Establishing causal claims are among the most vexing problems in all of scientific inquiry, but we have shown that, in this case, the Court was distinctly able to address this question of causality. This can serve as a model for the potential investigatory power of judicial injunctions. When the Court chooses to entertain empirical evidence, its own actions can create a uniquely powerful social experiment.

[14]For an even simpler test of the hypothesis of a chill, we can estimate these same regression models without controlling for different average levels of spending in each state (state fixed effects) and for different spending levels in each year (year fixed effects). These specifications also fail to detect any statistical or substantive evidence for increased spending after the Court's injunction.

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(*Appendix follows*→)

APPENDIX

Appendix Table 1. Statistical Tests for Clustering Around the Triggering Threshold

	Coefficient	*Standard Error*	*p-value*
Arizona 2006–2008	−0.30	0.94	0.75
Arizona 2010	−0.42	0.43	0.33
Maine 2002–2010	−2.90	7.10	.69

Note: Following the recommendations of McCrary (2008), the table provides explicit statistical tests for clustering around the triggering threshold in three sets of elections. The coefficients can be interpreted as the number of candidates clustering just below the threshold. In all three cases, the negative coefficients indicate clustering *above* the threshold, exactly opposite what we would see if candidates strategically chilled their spending in response to the matching provisions. The coefficients are statistically and substantively indistinguishable from zero, suggesting that candidates do not strategically chill their spending as a result of the campaign finance law.

Appendix Table 2. Available Campaign Finance Data by State and Year

	2000	*2002*	*2004*	*2006*	*2008*	*2010*
AK		2		2		2
AZ	2	2	2	2	2	2
CA		1	1	1	1	1
CO		2		2		2
CT		1		1		2
FL		2		2		2
ME		2	2	2	2	2
MI		2	2	2	2	2
OR	1	1	1			
TN			2	2	2	2

Note: A value of 2 indicates that both candidate and independent spending data is available for this election. A value of 1 indicates that only candidate spending is available. No entry indicates that no data was available.

Appendix Table 3. The Effect of the 2010 Injunction in Arizona on Campaign Spending

	Candidate Spending		*Private Spending*		*Independent Spending*	
	per capita	*log*	*per capita*	*log*	*per capita*	*log*
Injunction	−5.45 (3.90)	−0.43 (0.38)	−2.62 (2.22)	−0.06 (0.33)	0.25 (1.81)	0.63 (0.47)
Constant	8.21 (1.64)*	14.95 (0.22)*	10.12 (1.25)*	15.39 (0.22)*	7.63 (0.81)*	14.70 (0.23)*
Observations	40	40	37	37	30	30
R-squared	0.64	0.90	0.76	0.93	0.86	0.86
SER	4.04	0.50	2.54	0.50	1.95	0.86

Note: Ordinary least squares (OLS) regression coefficients with state-clustered standard errors in parentheses. All models include state and year fixed effects. *Denotes statistical significance at the $p < .05$ level. The coefficient on "Injunction" represents the effect of the 2010 Court injunction in Arizona on the dependent variable. In all six models, we find no substantively or statistically significant evidence that the injunction (i.e., the removal of matching funds) altered the spending behaviors of independent groups and privately funded candidates.