

## Campaign Finance Laws and Political Efficacy: Evidence from the States

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A failure to regulate the arena of campaign finance allows the influence of wealthy individuals and corporations to drown out the voices of individual citizens . . . causing the public to become disillusioned with and mistrustful of the political system.

—*Jacobus v. Alaska*<sup>1</sup>

**D**O CAMPAIGN FINANCE REGULATIONS affect how citizens view their government? This question is both theoretically important and policy-relevant. A central argument for more restrictive campaign finance laws at both the state and federal levels is that tighter rules will restore trust in the government and make citizens once again feel that they can affect the political process. Supreme Court decisions upholding the constitutionality of campaign finance regulations acknowledge that the maintenance of confidence in government can sometimes override free-speech concerns. Yet the purported link between campaign finance law and perceptions of government has never been established systematically. In fact, there is good reason to doubt the existence of such a link, as both Primo (2002) and Coleman and Manna (2000) argue that there is little or no relationship between total campaign spending in fed-

eral elections and average trust in government at the national level. In fact, Freedman, Franz, and Goldstein (2004) find that campaign advertising (and, therefore, campaign spending) increases interest levels, knowledge, and turnout, suggesting that spending may in fact be a net positive for democracy.<sup>2</sup>

Given the central role that trust and efficacy play in scholarly and policy discussions about campaign finance reform, it is surprising that no study has directly examined the connection between existing campaign finance laws and how citizens view their government.<sup>3</sup> Nevertheless, institutional reforms are widely thought to be a potential policy lever for strengthening democracy. For example, Hibbing and Theiss-

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<sup>1</sup> 338 F.3d 1095, 1107 (9th Cir. 2003).

<sup>2</sup> This point is also made by Coleman and Manna (2000).

<sup>3</sup> One reason for the dearth of such evidence is that until recently, most rigorous academic work on campaign finance has been limited to attempts at measuring the effect of either campaign spending on electoral outcomes or campaign contributions on policy outcomes. (On the effects of campaign spending and electoral outcomes, see especially Levitt [1994] and Gerber [1998]; on the effects of campaign contributions on roll-call votes, see especially Stratmann [1998], Bronars and Lott [1998], and Levitt [1998].) In addition, these efforts focus on the federal level; since federal campaign finance laws have been relatively stable over the past three decades, there has been little opportunity for systematic analysis regarding the impact of these laws.

Morse (1995) discuss possible reforms within Congress that might increase public support for the institution. These include term limits (which have since been ruled unconstitutional for Congressional offices), a balanced budget rule, and what the authors term “serious” campaign finance legislation (Hibbing and Theiss-Morse 1995, 62–83). Weber argues that if several state-level reforms are implemented, including the creation of independent commissions to oversee campaign finance in the states, then “the result will be a restoration of public confidence in state legislatures” (Weber 1999, 625–626). Goidel, Gross, and Shields (1999, 12) also argue that campaign finance reforms will “improve the quality of American democracy,” though none of these authors explicitly test the hypothesis that campaign finance laws are related to Americans’ perceptions of their political efficacy.<sup>4</sup> Despite these positives, Gross and Goidel claim (but do not demonstrate) that reform may also have a negative side:

. . . the consideration of reform temporarily appeased an increasingly cynical and alienated public while feeding the media establishment with sound bites on the evils of money. But, in the long run, it *undoubtedly* increased public cynicism and added to public alienation, even as it protected individual members of Congress who could claim to be in favor of reform without ever having to abide by its provisions (Gross and Goidel 2003, ix, emphasis added).

A few studies have attempted to empirically connect campaign finance and views on government. Grant and Rudolph (2004) adopt an experimental approach to study the foundations of public support for campaign finance reform, especially the tension between the values of liberty and equality. Persily and Lammie (2004), using descriptive data and indirect tests, conclude that the likely link between federal campaign finance law and trust in government is weak. Like Primo (2002), they point out that campaign finance law is unique in jurisprudence because public opinion is deemed directly relevant for determining whether or not

a law is constitutional. The authors go on to note that the evidence brought to bear in court cases tends to be of two forms: (1) anecdotal (e.g., news accounts) or (2) data showing that the public is distrustful of government and would like to see more campaign finance reform.<sup>5</sup> What is missing, as we show below, is evidence that reform will lead to improved trust in government or perceptions of political efficacy.

Two recent studies (Goidel, Gross, and Shields 1999; Coleman and Manna 2000) examine a related issue: the relationship between spending in U.S. House elections and individual survey responses to NES questions on efficacy or knowledge. These authors reach nearly opposite conclusions, with Coleman and Manna finding some evidence that campaign spending may improve knowledge and perceived efficacy, and with Goidel et al. finding some evidence to the contrary. These disparate findings may be attributable to the different methodological approaches taken by the authors. For example, Goidel et al. do not examine the potential endogeneity of campaign spending and efficacy or knowledge, while Coleman and Manna do.<sup>6</sup>

The public debate over state and federal campaign finance reform has likewise generated frequent and unsupported assertions that such reforms will restore faith in democracy. For example, Common Cause president Don Simon, commenting on the 2002 Bipartisan Campaign Reform Act (BCRA), stated, “I think it makes a huge difference that the world of soft money has come to an end. The law took a half a billion dollars off the political table, and it makes a whole lot of difference in the way the process works, and for the con-

<sup>4</sup> A recent review of the literature on money in American politics and its lessons for policy does not address whether campaign finance laws influence political efficacy or trust, underscoring the lack of evidence on this question (Mann 2003).

<sup>5</sup> There is reason to think that news accounts, by focusing on extreme examples of spending or corruption, may in fact be part of the problem. See Ansolabehere, Snyder, and Snyder (2005).

<sup>6</sup> Coleman and Manna employ an instrumental variables regression to estimate the effect of campaign spending; however, their instruments, which include incumbent and challenger quality and lagged vote share, may also be endogenous.

fidence the American people have in the system" (Vlahos 2003). But perhaps no example is as telling as when Senator John McCain (R-AZ) took to the Senate floor to argue in support of reform:

The Senator from Kentucky [Mitch McConnell] said the other day that there is no evidence, no polling data, no indication at all that the people's estrangement from Congress would be repaired by campaign finance reform. He is correct, there is no such evidence (*Congressional Record* 1999).

McCain then went on to explain that he had a "hunch" that reform would improve citizens' views of the Congress. But if there is no evidence that campaign finance reforms improve trust and political efficacy, why do proponents of reform frame their arguments in this manner? The answer is found in the rationale provided by the Supreme Court for permitting restrictions that otherwise infringe on First Amendment freedoms.

In the landmark decision *Buckley v. Valeo*, 424 U.S. 1 (1976), the Court specifically refers to confidence in government and the dangers unchecked campaign contributions pose for representative government:

Of almost equal concern as the danger of actual quid pro quo arrangements is the impact of the appearance of corruption stemming from public awareness of the opportunities for abuse inherent in a regime of large individual financial contributions. In *CSC v. Letter Carriers* [413 U.S. 548 (1973)] . . . the Court found that the danger to 'fair and effective government' posed by partisan political conduct on the part of federal employees charged with administering the law was a sufficiently important concern to justify broad restrictions on the employees' right of partisan political association. Here, as there, Congress could legitimately conclude that the avoidance of the appearance of improper influence 'is also critical . . . if confidence in the system of representative Government is not to be eroded to a disastrous extent.'

This line of reasoning is reiterated in *Nixon v. Shrink Missouri Government PAC*, 528 U.S. 377 (2000), although the majority opinion also explicitly notes the absence of evidence that campaign finance laws influence public trust in government.<sup>7</sup> The Ninth Circuit Court of Appeals has also referenced the presumed connection between campaign finance reform and trust in government (see the epigraph to this Introduction).

Most recently, in *McConnell v. FEC*, 540 U.S. 93 (2003), the Supreme Court repeatedly appealed, often in reference to past decisions, to the "eroding of public confidence in the electoral process through the appearance of corruption" as a justification for limits on large contributions to political parties (540 U.S., at 95, 136). Limiting the appearance of corruption is also deemed to be of "'almost equal' importance" (540 U.S., at 96, 143) as limiting actual corruption for preserving the "integrity of the electoral process" (540 U.S., at 119). Interestingly, while the Court cites "common sense" (540 U.S., at 145) and several anecdotes regarding contributions and access to policy makers as support for the claim that campaign finance is critically linked to political efficacy and trust in government, no evidence is cited regarding actual (i.e., criminal) corruption, nor is there any evidence cited regarding the presumed pernicious impact of campaign contributions on public opinion. Overall, the Court does not offer any evidence that legal reforms influence public opinion about the integrity of the democratic process, perhaps because the link is taken to be self-evident.<sup>8</sup> Justice Stephen Breyer, in a series of lectures at Harvard University, commented, "[Campaign finance laws] seek to democratize the influence that money can bring to bear upon the electoral process, thereby building public confidence in that process . . ." (2005, 47).

Our discussion thus far establishes two facts: (1) policy makers, scholars, and judges assert

<sup>7</sup> Writing for the majority, Justice Souter argues: "The state statute is not void, however, for want of evidence" (528 U.S., at 391).

<sup>8</sup> See Persily and Lammie (2004) for further discussion of how the courts view the appearance of corruption rationale justifying campaign finance reform.

that campaign finance reform will improve popular perceptions of the democratic process, and (2) there is no systematic evidence that directly supports or contradicts this claim. In what follows, we address this disconnect by conducting the first test of the effect of campaign finance reforms on perceived political efficacy. In particular, we study the connection between state campaign finance laws over the last half century and responses to survey questions about political efficacy.

The states are an ideal area to probe this relationship, since laws vary considerably both across the states and over time. In the campaign finance literature others have used this variation to study the electoral effects of campaign finance laws, and recent findings lend credibility to the claim that campaign reforms may influence citizens' perceived efficacy by improving the quality of elections.

For example, Stratmann and Aparicio-Castillo (2006) examine the effects of campaign finance laws on legislative elections in 45 states over twenty years. Because these authors have a lengthy panel data set (unlike previous studies), they are able to control for unobserved state-specific effects, greatly reducing concerns that state campaign finance regulations are spuriously correlated with measures of electoral competitiveness.<sup>9</sup> They find that limits on campaign contributions serve to reduce the winning margins of incumbents. However, they do not explore the effects of other types of campaign finance regulations.

We explore the relationship between perceived citizen efficacy and campaign finance laws using individual survey responses from the National Election Studies (NES) from 1952 to 2000. We conduct a contextual analysis by combining these responses with state-level variables that describe campaign finance regulations and other relevant state political institutions. We find some evidence that public disclosure and restrictions on contributions from organizations improve perceived political efficacy, but fail to find such evidence for other types of campaign finance regulations. In fact, public financing tends to be associated with a decline in perceived efficacy. Overall, no state campaign finance laws appear to have a substantively large impact on the public's perceptions of govern-

ment, one way or the other. These findings are reinforced by a novel methodological approach to studying state-level trust in government, which is presented in a technical appendix.

## POLITICAL EFFICACY

A voluminous literature addresses the validity and reliability of survey measures for trust and perceived political efficacy (e.g., Campbell, Gurin, and Miller 1954; Miller 1974; Citrin and Green 1986; Craig, Niemi, and Silver 1990; see Robinson et. al. 1999 for summaries of various measures). In general, there is a consensus that trust, internal efficacy, and external efficacy are three distinct concepts. There is less agreement on the proper ways to measure these concepts. Trust refers to the faith individuals have in the government (Citrin and Muste 1999). Political efficacy<sup>10</sup> refers to the belief that one can have an influence on the political process (Campbell, Gurin, and Miller 1974), although Balch (1974) and others further subdivide political efficacy into "external" and "internal" components. Internal efficacy taps beliefs about whether one can influence the political process, and external efficacy reflects beliefs about whether elected officials are sufficiently responsive to constituents. The upshot of these debates is that measures of efficacy may be "noisy," and this should be kept in mind as the results are presented.

Much political hay has been made of the sharp decline in trust and political efficacy that occurred from the 1950s to the present. For instance, consider one of the most-cited NES questions, used to measure political trust: "How much of the time do you think you can trust the government in Washington to do what is right—just about always, most of the time or only some of the time?" "Trust in government" is defined as answering "just about always" or "most

<sup>9</sup> Gross and Goidel (2003) are also working with a panel data set, but they do not address unobserved state-specific effects in their analysis.

<sup>10</sup> The surveys measure respondents' perceived efficacy, not their actual efficacy. For convenience, the literature drops the word "perceived," and we follow that practice in the remainder of this article.

of the time.” In 1964, when the question was first asked on a regular basis, 76 percent of respondents trusted the government. By 2000, that number had dropped to 44 percent. There are many culprits identified for the temporal decline in trust at the federal level, including a general increase in cynicism (Alford 2001) and the Vietnam War and Watergate (Orren 1997).

Whether or not declines in trust and efficacy materially affect democratic rule is a separate question. For instance, Citrin (1974) and Rosenstone and Hansen (1993) do not find that trust in government influences turnout. In fact, one can argue (as Citrin and Luks [2001] and others do) that some level of mistrust is a net positive for democracy. On the other hand, Brehm and Rahn (1997) posit links between interpersonal trust, civic engagement, and confidence in government, suggesting that low levels of confidence in government may harm interpersonal trust as well as civic engagement.

Direct links between participation and trust or efficacy are only part of the story. Another argument emphasizes the relevance of trust and efficacy’s symbolic aspects. Low levels of trust and efficacy are, by virtue of being below some threshold level, threats to self-rule. Therefore, laws should be structured to improve citizen perceptions of government. For the purposes of analysis, we assume that positive perceptions of government are desirable for democratic governance and seek to ascertain whether campaign finance laws influence these perceptions. Only if some substantively meaningful statistical relationship is present does the campaign finance debate need to further probe the links among trust, efficacy, and democracy.

**CAMPAIGN FINANCE LAWS AND POLITICAL EFFICACY: THEORETICAL LINKS AND EMPIRICAL IMPLICATIONS<sup>11</sup>**

The literature on electoral competition, campaign advertising, political efficacy, and trust in government suggests several causal pathways by which campaign finance laws might influence citizens’ perceptions about the workings of democracy. To the extent that campaign finance regulations influence electoral competition, or the amount and tone of advertising,

the effects of such regulations will reverberate throughout the larger system of relationships involving competition and advertising. Consequently, voter turnout, trust in government, political interest and even candidate emergence may all ultimately be affected in some way by changes in campaign finance laws.

In this section, we provide a highly simplified structural model of some of these relationships. Our purpose is to provide enough detail to motivate the subsequent empirical approach.

Define:

- **X** = a vector of individual characteristics (e.g., age, race, party ID)
- **Z** = a vector of characteristics of state institutions (e.g., campaign finance laws, term limits)
- *EFFICACY* = an individual’s perception of political efficacy or trust in government
- *EXPOSURE* = an individual’s exposure to campaign messages
- *INTEREST* = an individual’s interest in or knowledge of politics
- *VOTE* = an individual’s vote choice/turnout decision
- *COMPETITION* = competitiveness of elections taking place at the time of a survey
- *SPENDING* = campaign spending by candidates

A stylized structural model of political efficacy for  $i = (1, \dots, n)$  individuals residing in  $s = (1, \dots, k)$  states may then be articulated as:

$$\begin{matrix} EFFICACY_i = f_1(\mathbf{X}_i, \mathbf{Z}_s; \\ EXPOSURE_i, INTEREST_i, \\ COMPETITION_s, SPENDING_s) \end{matrix} \quad (1)$$

$$\begin{matrix} EXPOSURE_i = f_2(\mathbf{X}_i, \mathbf{Z}_s; \\ EFFICACY_i, INTEREST_i, \\ COMPETITION_s, SPENDING_s) \end{matrix} \quad (2)$$

$$\begin{matrix} INTEREST_i = f_3(\mathbf{X}_i, \mathbf{Z}_s; \\ EXPOSURE_i, EFFICACY_i, \\ COMPETITION_s, SPENDING_s) \end{matrix} \quad (3)$$

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<sup>11</sup> This section is technical in nature and can be skipped by readers interested only in the main findings.

$$\begin{aligned} VOTE_i = f_4(\mathbf{X}_i, \mathbf{Z}_s; EFFICACY_i, \\ INTEREST_i, EXPOSURE_i, \\ COMPETITION_s, SPENDING_s) \end{aligned} \quad (4)$$

$$COMPETITION_s = f_5(\mathbf{Z}_s; SPENDING_s) \quad (5)$$

$$SPENDING_s = f_6(\mathbf{Z}_s; COMPETITION_s) \quad (6)$$

Obviously, we have simplified matters greatly by abstracting away from issues such as the relative amounts of campaign spending by particular candidates or the causes and consequences of candidate quality, tone of advertising, and the like. Even so, the model suggests that there is a complicated and potentially intractable set of interrelationships in this simultaneous system. In order to fully identify the effects of campaign finance laws (elements of  $\mathbf{Z}$ ) on efficacy, exposure, interest, etc., it would be necessary to impose numerous exclusion restrictions on this system. For instance, campaign finance laws likely do not directly influence vote choice, but they do exert an indirect influence on this endogenous variable by influencing campaign spending and competition. Therefore, one exclusion restriction might be to leave campaign finance laws out of equation (4).

We are not sanguine about the prospects of identifying the relationships characterized in the model above.<sup>12</sup> However, even if such identification were in principle possible, values for some of the variables listed above (e.g., exposure) may not be available over a long time period or for all states. This absence of data availability also hinders the ability of researchers to probe the effects of political institutions that change infrequently. But all is not lost; the system above can be solved for each of the endogenous variables in terms of the exogenous variables.<sup>13</sup>

In other words, a direct implication of the stylized model is that:

$$EFFICACY_i = g(\mathbf{X}_i, \mathbf{Z}_s) \quad (7)$$

In fact, each of the endogenous variables in our stylized structural model can be expressed as functions of only  $\mathbf{X}_i$  and  $\mathbf{Z}_s$ . Further, these reduced form specifications describe the net effect of campaign finance laws (or other elements of  $\mathbf{X}$  and  $\mathbf{Z}$ ) on efficacy (or other

endogenous variables). Therefore, we test the claim that campaign finance reforms increase political efficacy by estimating the reduced-form specification in equation (7). Such estimates are unbiased (or in the case of maximum likelihood, consistent).<sup>14</sup> This approach also has the practical advantage that limited data for some endogenous variables (e.g., exposure) have no bearing on our ability to estimate the reduced form in equation (7) over a long time period. Finally, for our purposes, the reduced-form approach is desirable because it allows us to exploit the long run variation in state campaign finance laws in order to test the claims made by policy makers and scholars that such reforms have a salutary effect on political efficacy. The drawback of this method is that we cannot study the potential causal pathways by which reform impacts efficacy. We return to this issue in the discussion.

## CAMPAIGN FINANCE LAWS IN THE STATES

Campaign finance laws have changed dramatically in the states in recent decades. For instance, in 1950 few states had restrictions on

<sup>12</sup> We are not alone. Rarely are complete systems estimated. For example, Box-Steffensmeier, Grant, and Rudolph (2003) study the impact of campaign finance attitudes on turnout and vote choice but only specify part of what is certainly a larger system of equations.

<sup>13</sup> To give a simple example, consider a two-equation (identified) system as follows:  $y_1 = \beta_0 + \beta_1 X_1 + \gamma_1 y_2 + \varepsilon_1$  and  $y_2 = \beta_2 + \beta_3 X_2 + \gamma_2 y_1 + \varepsilon_2$ . The reduced form can then be written as follows:

$$\begin{aligned} y_1 &= \pi_0 + \pi_1 X_1 + \pi_2 X_2 + v_1 \text{ and } y_2 = \pi_3 \\ &+ \pi_4 X_1 + \pi_5 X_2 + v_2, \text{ where } \pi_0 = \frac{\beta_2 \gamma_1 + \beta_0}{1 - \gamma_1 \gamma_2}, \\ \pi_1 &= \frac{\beta_1}{1 - \gamma_1 \gamma_2}, \pi_2 = \frac{\beta_3 \gamma_1}{1 - \gamma_1 \gamma_2}, \pi_3 = \frac{\beta_0 \gamma_2 + \beta_2}{1 - \gamma_1 \gamma_2}, \\ \pi_4 &= \frac{\beta_1 \gamma_2}{1 - \gamma_1 \gamma_2}, \pi_5 = \frac{\beta_3}{1 - \gamma_1 \gamma_2}, \end{aligned}$$

and  $v_1$  and  $v_2$  are linear combinations of the error terms. Because the right-hand side variables are assumed to be uncorrelated with a typical error term, the reduced-form parameters (the  $\pi$ 's) can be estimated in the usual fashion.

<sup>14</sup> For further details, see Maddala (1983) and Kennedy (2003).

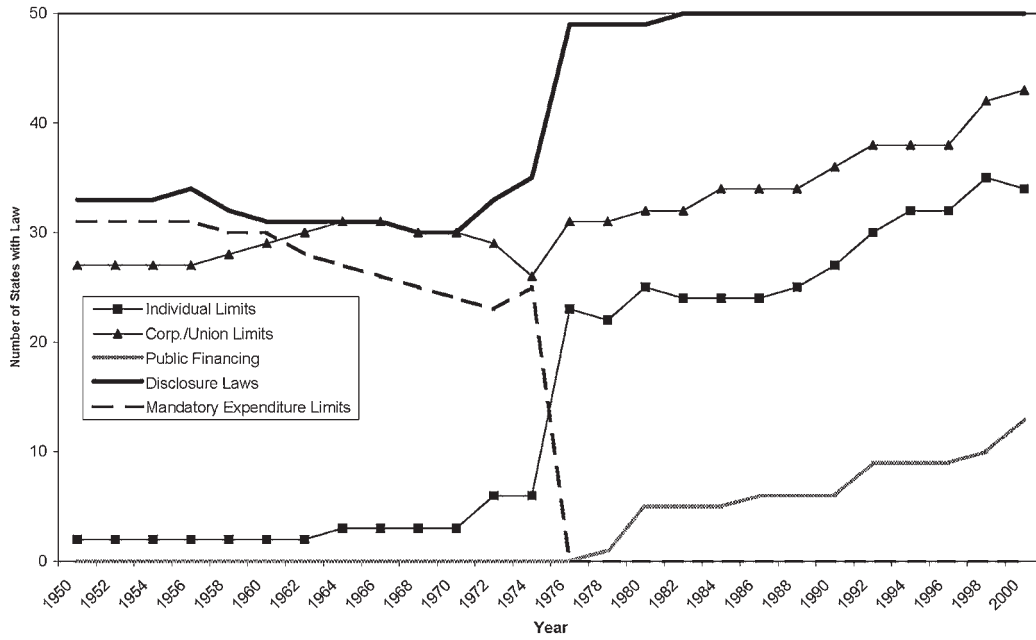


FIG. 1.

contributions by individuals, but by 2000 limits on contributions from both individuals and organizations (i.e., corporations, unions, and PACs) were the norm. The trend in state reforms mirrors that at the federal level, where a major wave of changes occurred in the 1970s. State reforms also picked up steam in the 1990s, with more than one-third of states altering their laws during this period (Malbin and Gais 1998). We are in what might be called an era of “mature” campaign finance regulation, since all states have disclosure laws on the books, and most states have some restrictions on contributions.

We focus here on contributions to candidates rather than to parties, as information on the latter is not readily available for the full time period under study. We consider disclosure laws, contribution limits on organizations, contribution limits on individuals, the presence of public financing tied to voluntary expenditure limits, and mandatory expenditure limits in place prior to the Supreme Court’s ruling that such limits were unconstitutional. Figure 1 depicts the number of states over time with each type of campaign finance law.

While there are several ways to categorize and measure state-level laws, in this case simpler is better. We measure the presence or absence of particular types of laws, such as con-

tribution limits and public financing. Using specific dollar amounts leads one into a morass, in part because states greatly differ in many respects, including cost-of-living, wealth, and the cost of media markets. Put concretely, does a \$1000 limit on individual contributions to a candidate mean the same thing in Arkansas as it does in California? If not, how would one compare specific limits across states? Other aspects of campaign finance law, such as enforcement quality, suffer from similar problems. The presence or absence of particular laws, on the other hand, can be clearly measured and is directly comparable across states.

DATA AND METHODS

The biennial National Election Studies has been asking questions on trust and efficacy for five decades.<sup>15</sup> We focus on political efficacy because the battery of NES trust questions asks specifically about the federal government, while questions regarding efficacy ask about government generally. This paper therefore

<sup>15</sup> Importantly, these are not panel data, since the respondents differ each time the survey is administered. Rather, they are pooled cross-sectional data.

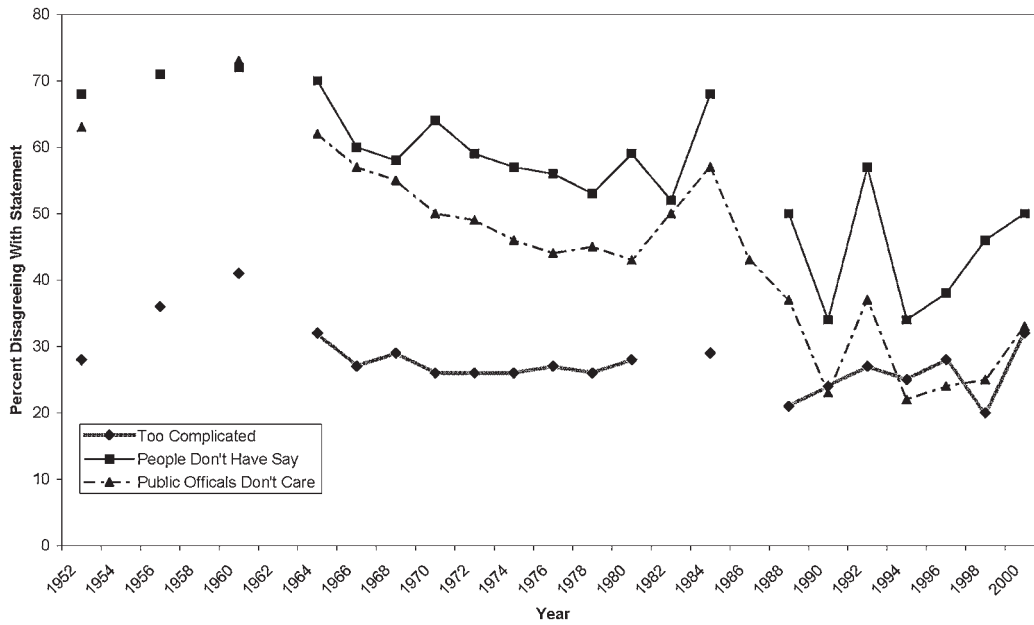


FIG. 2.

uses the best available temporal data.<sup>16</sup> Unfortunately, this data limitation means that we cannot reach definitive conclusions with respect to trust. This does not alter the importance of our findings, since concepts like trust, faith, confidence, and efficacy are often used interchangeably in popular (and legal) usage.<sup>17</sup>

Individual-level data are from the 1948–2000 NES Cumulative Data File. State-level data are taken from *The Book of the States, Campaign Finance Law*, and other public sources. Using state-level variables when working with individual-level variables requires caution. For instance, because the National Election Studies does not include representative state samples, it is not possible to make claims about a specific state and how it has changed over time. Rather, residing in a state should be viewed as a “treatment” on the individuals, with state institutional features, including campaign finance laws, representing treatment effects similar to those we might observe in medical experiments. In this way, we can ascertain whether living in a state with particular campaign finance laws influences one’s perceptions of government.<sup>18</sup>

We utilize three questions about efficacy that have been asked in the National Election Studies over time. The dependent variables are dichotomous and are coded such that a value of

one indicates a more favorable disposition toward government (i.e., disagreeing with the statements). The percent disagreeing for each question appears in parentheses. The first two questions, which tap external efficacy, are most directly relevant for the issue of campaign finance reform. Figure 2 summarizes the year-to-year responses for the entire NES sample.

- People like me don’t have any say about what the government does. [60% disagree]
- Public officials don’t care much what people like me think. [50% disagree]
- Sometimes politics and government seem so complicated that a person like me can’t really understand what’s going on. [30% disagree]

<sup>16</sup> One other drawback of the data is that the dependent variable reflects questions asking about government generally, not state government specifically, thereby creating a noisy dependent variable.

<sup>17</sup> We do, however, attempt to capture state-level trust using the 1996 NES. See the appendix.

<sup>18</sup> NES-provided weights ensure that the analysis of campaign finance laws is accurate with respect to subgroups. Since in the NES sample the number of individuals surveyed in a state is typically related to that state’s population, we have also verified that there is a negligible relationship between the population of a state and the presence of campaign finance laws.



Because we are working with binary dependent variables, probit models are estimated.<sup>19</sup> These models are utilized when working with dependent variables that can take on two values. When appropriately converted, the coefficients on the independent variables tell us how much a one-unit change in the independent variable influences the likelihood of responding positively to a question.<sup>20</sup> In our case, we are interested in the impact of implementing a campaign finance law, and in the results section we discuss the impact of these laws on the three efficacy measures.

Several state-level variables are included in the analysis to control for other features of a state besides changes in campaign finance law that may influence citizen perceptions. These include indicators for the presence of the citizen initiative, gubernatorial or legislative term limits, whether a poll tax or literacy tests are necessary to vote, and whether easy voting registration is present (i.e., same day or no advance registration required).<sup>21</sup> We also include state dummy variables to control for unobserved heterogeneity across states; this is particularly important given that state campaign finance laws may be passed precisely in those states with chronically low levels of trust or efficacy. Year dummy variables are included in the analysis to control for features of particular survey years that may influence survey responses.<sup>22</sup>

Five dichotomous campaign finance variables represent the laws in each state.<sup>23</sup> These are indicators for the presence of the following:

1. Public disclosure of campaign contributions
2. Limits on contributions by organizations only
3. Limits on contributions by organizations and individuals
4. Public subsidies to candidates that abide by expenditure limits
5. Mandatory expenditure limits in place prior to the 1976 *Buckley v. Valeo* decision outlawing such limits.

To investigate whether there is a delay before campaign finance laws affect efficacy, we examine models that include four-year lags of these laws. We also include four-year leads of laws in some specifications to see whether ef-

ficacy is related to future campaign finance laws, which is possible if low levels of efficacy usher in reform.

Individual-level controls include education (grade school, high school, some college, college or more), age, age squared, income (measured in percentiles, with a coding of 1 representing the 1st to 16th percentile, 2 the 17th to 33rd percentile, 3 the 34th to 67th percentile, 4 the 68th to 95th percentile, and 5 the 96th to 99th percentile), unemployment, race, gender, and the strength of one's partisan affiliation (ranging from 1 to 4).<sup>24</sup> We include strength of affiliation because we expect that individuals with strong partisan ties are more likely to feel efficacious, regardless of whether they are Re-

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<sup>19</sup> Because multiple survey respondents from each state and year in the sample receive the same "treatment" in the form of campaign finance and other state-level laws, the observations from each state are unlikely to be independent. This may cause standard errors to be underestimated. We therefore adjust standard errors for the clustering of observations within state and year using a modification of White's (1980) standard errors developed by Rogers (1993).

<sup>20</sup> The coefficients are not directly interpretable without a transformation.

<sup>21</sup> Poll taxes and literacy tests are of course now outlawed; however, they were present for some years in our sample.

<sup>22</sup> When working with data of this type, it is also possible to use hierarchical linear modeling (HLM), which enables the researcher to offer theoretical explanations for differences in behavior across levels. However, HLM requires many more assumptions than standard regression or maximum likelihood techniques, and it is more sensitive to measurement error (Bryk and Raudenbush 1992; Steenbergen and Jones 2002). For our purposes, then, HLM is not the best method.

<sup>23</sup> By defining the variables in this way, we avoid concerns about multicollinearity, especially between limits on contributions to individuals and organizations. This was verified by conducting a variety of diagnostic tests for the campaign finance variables. We do not create an index of laws because we do not expect their effects to be additive. This is borne out in the empirical analysis.

<sup>24</sup> A host of other variables might be expected to have a direct influence on efficacy (e.g., campaign spending, financial situation, exposure to news sources); however, such variables are either not in the dataset for the entire period or are in turn functions of our included covariates. In other words, where other authors, such as Coleman and Manna (2000) have sought to estimate the structural equation relating campaign spending to efficacy, we estimate the reduced form effect of state campaign finance laws. Consequently, we include only exogenous control variables in our model specification. See section 3 for further details.

TABLE 1. SUMMARY STATISTICS FOR EFFICACY ANALYSIS

<i>Variable</i>	<i>Mean</i>
Have a Say?	0.60
Officials Care?	0.50
Complicated?	0.30
Public disclosure of contributions	0.89
Contribution limits (organizations only)	0.40
Contribution limits (orgs. and ind.)	0.29
Public funding	0.08
Mandatory expenditure limits	0.24
Income = 2	0.17
Income = 3	0.33
Income = 4	0.29
Income = 5	0.05
Unemployed	0.06
High School	0.48
Some College	0.20
College	0.17
Age	44.90
Age Squared	2302.39
Nonwhite	0.15
Female	0.55
Partisan Strength	2.86
Democrat	0.53
Republican	0.36
Unified Dem. Govt.	0.36
Unified Rep. Govt.	0.14
Dem. × Unified Dem.	0.21
Dem. × Unified Rep.	0.07
Rep. × Unified Rep.	0.06
Rep. × Unified Dem.	0.11
Citizen Initiative	0.44
Gub. Term Limits	0.53
Leg. Term Limits	0.08
Poll Tax	0.03
Literacy Test	0.08
Easy Registration	0.02

The NES was administered every two years beginning in 1948, with the exception of 1950. The “have a say” question was not asked in 1948, 1954, 1958, 1962, and 1986;  $N = 30,034$ . The “officials care” question was not asked in 1948, 1954, 1958, and 1962;  $N = 30,189$ . The “complicated” question was not asked in 1948, 1954, 1958, 1962, 1982, 1986;  $N = 28,810$ . 1966 is dropped from the analysis because the question about employment status was not asked in that year. The following states were not included in the analysis due to a lack of observations: Alaska, Hawaii, Montana, and Vermont. In addition, Rhode Island was not included in the “have a say” or “complicated” analyses due to a lack of observations. Observations with missing data are excluded from the analysis. The summary statistics presented for the independent variables ( $N = 30,034$ ) are based on the “have a say” specification; they do not change in any meaningful way in the other specifications. We present only the means because all variables (except for age, age squared, and partisan strength) are dichotomous. The standard deviations for the exceptions are as follows: age (16.91), age squared (1675.98), and partisan strength (0.97).

publicans or Democrats. In addition, we include controls for Republican and Democratic party identification. We also interact party identification and year to assess whether there

are any national-level partisan trends that may cause Democrats or Republicans to feel less efficacious. Additional variables include unified state Democratic government and unified state Republican government, as well as interaction terms for Democratic respondent living in a unified Democratic state, Democratic respondent living in a unified Republican state, Republican respondent living in a unified Republican state, and Republican respondent living in a unified Democratic state. These interactions capture whether affiliating with an out-of-power party makes one feel less efficacious. Summary statistics and details regarding the years and states included in each specification appear in Table 1.

## RESULTS

In Table 2, we present the estimated probit coefficients for each question on political efficacy. Year and state dummy variables are both jointly significant. The importance of the state dummy variables suggests that they are controlling for factors that might otherwise bias our estimates of the effects of campaign finance laws.<sup>25</sup> Neither lags nor leads of the campaign finance variables are jointly significant at the 0.10 level (using Wald tests in separate specifications), thereby mitigating concerns about endogeneity or delayed effects for those variables.

In general, the individual-level controls are statistically significant. The more educated tend to have a more favorable view of government. The same positive relationship exists for income and strength of partisanship. Gender has mixed effects, while nonwhites are less likely to feel efficacious than whites. State-level institutional controls have mixed effects on efficacy, and these effects are typically not statistically significant. Overall, the explanatory power of the controls is modest, leaving much variation for campaign laws to explain.<sup>26</sup>

<sup>25</sup> We also examined the coefficients on the state dummy variables for patterns that suggest that “reform” states were systematically different from laissez-faire states, but none emerged.

<sup>26</sup> The pseudo- $R^2$  measure, while imperfect, is on the order of 0.10 in most cases when just controls are included in the estimation.

TABLE 2. POLITICAL EFFICACY AND CAMPAIGN FINANCE LAWS

Variable	Have a say?		Officials care?		Complicated?	
	Coef.	z-stat	Coef.	z-stat	Coef.	z-stat
Public disclosure of contributions	0.074	1.72	0.074	1.86	0.004	0.10
Contribution limits (organizations only)	0.099	2.41	0.21	0.55	0.050	1.13
Contribution limits (orgs. and ind.)	-0.032	0.80	0.017	0.44	-0.033	0.80
Public funding	-0.130	2.64	-0.104	2.17	-0.065	1.33
Mandatory expenditure limits	-0.019	0.51	-0.009	0.25	-0.052	1.40
Income = 2	0.079	2.90	0.058	2.16	0.055	1.58
Income = 3	0.167	6.39	0.171	6.26	0.080	2.40
Income = 4	0.256	8.64	0.265	8.94	0.193	5.59
Income = 5	0.375	8.77	0.484	11.91	0.370	7.73
Unemployed	-0.080	2.15	-0.066	1.83	0.025	0.62
High School	0.380	13.49	0.427	16.16	0.393	11.98
Some College	0.697	20.36	0.676	20.72	0.836	22.09
College	0.918	25.96	0.929	28.29	1.136	28.91
Age	0.004	1.51	-0.001	0.22	0.006	1.62
Age Squared	-0.000	2.04	-0.000	1.02	-0.000	2.25
Nonwhite	-0.074	2.86	-0.145	5.59	-0.026	0.89
Female	-0.035	2.10	0.045	2.64	-0.363	21.04
Partisan Strength	0.093	7.91	0.109	9.14	0.087	6.96
Democrat	-0.045	0.37	0.300	1.47	0.108	0.88
Republican	0.057	0.40	-0.147	1.27	-0.260	1.79
Unified Dem. Govt.	-0.007	0.13	-0.121	2.08	0.013	0.21
Unified Rep. Govt.	0.043	0.52	0.012	0.15	-0.071	0.83
Dem. × Unified Dem.	-0.012	0.20	0.138	2.30	-0.045	0.69
Dem. × Unified Rep.	0.051	0.56	0.016	0.18	0.075	0.80
Rep. × Unified Rep.	0.058	0.59	0.108	1.18	0.114	1.14
Rep. × Unified Dem.	0.036	0.57	0.059	0.93	0.042	0.63
Citizen Initiative	-0.032	0.47	-0.048	0.71	-0.096	1.77
Gub. Term Limits	-0.056	1.09	-0.143	3.02	0.009	0.20
Leg. Term Limits	-0.062	1.30	-0.037	0.85	0.053	1.18
Poll Tax	-0.032	0.50	-0.199	2.71	-0.071	1.15
Literacy Test	-0.043	0.93	-0.036	0.74	-0.076	1.52
Easy Registration	-0.015	0.19	0.018	0.24	0.017	0.18
Constant	-0.923	5.21	-1.70	8.27	-1.34	7.28
N	30,034		30,189		28,810	

All probit specifications include state and year dummy variables with robust standard errors adjusted for clustering within state and year, as well as interaction terms for year and party identification. Absolute values of z-statistics are presented in the table. "Have a say?" refers to whether or not individuals believe that they have a say in government; "Officials Care?" refers to whether individuals believe that officials care what people like them think; and "Complicated?" refers to whether individuals find politics to be too complicated. In all cases, the dependent variable is coded 1 for a response indicating that an individual feels efficacious, and 0 otherwise.

The first efficacy measure is "Do people have a say?" Public disclosure has a positive and statistically significant coefficient, as does the presence of organizational but not individual limits. Adding individual limits in a state decreases efficacy, though this result is not statistically significant. The same is true for mandatory expenditure limits. Public funding, however, has a statistically significant negative effect on efficacy.

For the question "Do officials care?," public disclosure is once again positively and significantly associated with a positive response, and public funding is negatively and significantly

associated with feeling efficacious. No other campaign finance variables have a statistically significant effect in this specification. Finally, the third measure, "Is politics too complicated?," yields no statistically significant campaign finance variables.

In order to illustrate the substantive importance of campaign finance laws, we use our model to calculate the change in the probability that an individual reports a favorable view of government. Table 3 includes estimates of the effects of campaign finance laws on the change in the probability that an individual has a favorable view of government. This exercise

TABLE 3. MARGINAL EFFECTS OF CAMPAIGN FINANCE LAWS ON POLITICAL EFFICACY

<i>Variable</i>	<i>Have a say?</i>	<i>Officials care?</i>	<i>Too complicated?</i>
Mean of dependent variable	0.60	0.50	0.30
Change in probability from the presence of campaign finance laws			
Public disclosure of campaign contributions	0.03*	0.03*	0.00
Limits on contributions from organizations only	0.04**	0.01	0.02
Limits on contributions from organizations and individuals	-0.01	0.01	-0.01
Public funding of candidates conditional on expenditure limits	-0.05***	-0.04**	-0.03
Mandatory expenditure limits (pre-Buckley)	-0.01	0.00	-0.02

The change in probability of a favorable response from the implementation of a particular law (or set of laws) is derived from the estimated coefficients in Table 2, where all changes are calculated at the mean of the dependent variable.

\* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

requires the selection of a baseline probability for comparison; we consider the hypothetical case of an individual with a baseline probability of responding favorably to each efficacy measure that is equal to the mean for that measure. For example, in column one of Table 3, the addition of a public disclosure law raises the probability that our hypothetical individual will respond that people “have a say” from 60 percent to 63 percent.

The results in Table 3 show that none of the campaign finance variables consistently has a statistically significant effect on every measure of political efficacy. There may be some modest improvement in efficacy from disclosure laws, and perhaps even limits on contributions from organizations. However, the evidence showing a negative effect of public financing is at least as strong. Finally, there is no evidence that either mandatory expenditure limits or limits on individual contributions have any appreciable impact on efficacy.<sup>27</sup>

## DISCUSSION

Judicial and legislative actors have long assumed that campaign finance reforms can and do influence public perceptions about the workings of democracy. Until now, there has been no systematic study of the presumed link between campaign finance regulations and

public opinion. While ours is surely not the last word on the subject, our findings are in stark contrast to what has been accepted as the common sense and self-evident connection between campaign reform and the perceived integrity of the American democratic process.

This is not to say that the causal relationship identified by policymakers does not exist. But even if it does, several recent studies suggest that there may be important countervailing salutary influences of campaign spending, as well. Consequently, the net effects of campaign finance regulations on political efficacy appear muted or even contrary to expectations.<sup>28</sup> These countervailing forces may explain the two apparent puzzles in our findings. First, why might disclosure laws have such a positive influence on efficacy while public financing has negative effects? Second, why might organizational contribution limits improve efficacy while individual limits have little influence or perhaps even negatively impact efficacy?

<sup>27</sup> We also conduct a robustness check by creating an index of responses to the dependent variables, from 0 to 3, and estimating an ordered probit. In this specification, organizational contribution limits have a positive and statistically significant effect on efficacy, and public financing has a negative and statistically significant effect. No other laws have statistically significant effects. As in the main specification, all effects are substantively modest.

<sup>28</sup> This also suggests the importance of further work probing the structural system discussed earlier.

The “Brandeis view,” that sunshine is the best disinfectant, suggests that states with disclosure are providing their citizens with the necessary information to be better citizens. Disclosure laws probably do not reduce overall campaign spending, so they do not reduce the positive aspects of political advertising. On the other hand, public financing schemes are typically devised to limit overall expenditures, so they may have a greater negative impact on the beneficial aspects of political expenditures. In addition, public financing may be predicated on false promises for a better democratic process. When the smoke clears and “politics as usual” returns after reform, individuals may become even more disenchanted with their government.<sup>29</sup> Therefore, the apparent counterintuitive finding that disclosure and public financing work in opposite directions on political efficacy is quite plausible. This is potentially the most policy-relevant finding, as well, since public financing is the type of reform most likely to be implemented in jurisdictions with “mature” regulatory regimes.

Our results regarding the effects of contribution limits are less robust, but likewise suggest the presence of countervailing influences of campaign finance on political efficacy. The findings suggest that individual limits do not improve efficacy (and may harm efficacy), while contribution limits on organizations appear to have a nonnegative influence on efficacy. One reason for this relationship may be that corporate or union contributions represent “interested money,” whereas individual contributions are viewed as more benign. Hence, when corporate contributions are limited, individuals cheer, but when individual contributions are limited, individuals jeer. Another possibility is that the increased competitiveness in states with limits on organizational contributions may in turn increase efficacy. Because not all laws associated with increased competitiveness improve efficacy, this is clearly only part of any complete explanation. And importantly, the results for contribution limits, even when statistically significant, are substantively modest.

An alternative perspective is that political efficacy is not greatly improved by public financing or limits on contributions because we have not enacted the right types of limits or public financing systems. For instance, Acker-

man and Ayres propose the creation of “Patriot Dollars” and a secret “donation booth” to overcome the problems inherent in existing campaign finance regimes (2002). Under their plan, citizens would be given funds to give to candidates of their choice, secretly, and similarly individuals could give unlimited contributions to candidates of their choice, but only anonymously. Ackerman and Ayres suggest that this improves on the existing system because it avoids the inevitable attempts to skirt limits that have arisen after nearly all reforms to date. Of course, there is no guarantee that their plan (or any other, for that matter) will work as intended. In fact, it is not an exaggeration to state that most of the consequences of campaign finance law since the 1970s have been unintended rather than the result of careful planning.<sup>30</sup>

Another criticism of our findings might be that because citizens have displayed a remarkable lack of interest in and knowledge of campaign finance reform (Primo 2002; Mayer 2001), it should not be surprising that there is little relationship between laws and levels of efficacy or trust. This argument has intuitive appeal but fails to acknowledge that (a) the laws-trust-efficacy link has been used extensively in jurisprudence and is viewed as self-evident and (b) more than just knowledge of the laws may influence efficacy. Whatever influence the laws have on the political system in a state is captured in the statistical analysis by the law variables. Knowledge of the laws is not a necessary condition for the laws to have an effect, as our earlier system of equations indicates. For instance, campaign finance laws may affect campaign spending, which in turn influences exposure to advertising, which in turn influences efficacy. Further, Primo (2002) has shown that attentive individuals tend to be more skeptical of campaign financing than those who pay little attention to money in politics, and Ansolabehere, Snowberg, and Snyder (2005) show that more attentive in-

<sup>29</sup> Of course, it is possible that states with low trust and efficacy adopt campaign finance reforms. However, our analysis accounts for this possibility both by incorporating state indicators and by examining the lags and leads of the laws.

<sup>30</sup> A parallel can be drawn to ethics laws. See Rosenson (2003) for a discussion.

dividuals overestimate the amount of money in politics. If anything, then, ignorance of the laws may very well be bliss.

This study suggests profitable future directions for research. Further work should probe the complex web of interrelationships in campaign finance to consider possible causal pathways that lead to these small effects. In addition, a limited number of surveys ask directly about state-level trust or efficacy. A study based on these surveys would trade off a longer time span of data for a less noisy dependent variable, thereby offering another way to study the question addressed in this article.

In closing, this article makes a theoretical contribution to the study of attitudes toward government and offers the first systematic test of the link between campaign finance laws and citizen perceptions of democratic rule. We find that the effect of campaign finance laws is sometimes perverse, rarely positive, and never more than modest. Given the importance placed on public opinion for the development of campaign finance law, it is remarkable that we have found so little evidence that citizens are influenced by the campaign finance laws of their state.

#### APPENDIX: AN ALTERNATIVE APPROACH

One criticism of the test in the main text is that the questions are not directly asking about state government. To address this, we develop a method for estimating a state-specific trust measure using a question from the NES. Consider two regression equations estimating the effect of campaign finance laws on trust in the state and federal governments, where each error term includes a common individual-level fixed effect,  $fe$ , capturing the tendency to mistrust government at all levels. Let  $Fedtrust$  be a latent variable measuring trust in the federal government and  $Statetrust$  be a latent variable measuring trust in the state government. Let  $i$  index the  $k$  covariates appearing in both equations, and  $j$  index the campaign finance laws that may affect trust in the state government. (By construction, we assume that state campaign finance laws have no effect on trust in the federal government.) Finally, let  $y^*$  be a con-

tinuous latent variable that measures the difference in an individual's trust in the federal and state governments.

$$Fedtrust = a_0 + \sum_{i=1}^k a_i x_i + v_1 \quad (A1)$$

$$Statetrust = b_0 + \sum_{i=1}^k b_i x_i + \sum_{j=1}^m b_j statelaw_j + v_2 \quad (A2)$$

where  $v_1 = \varepsilon_1 + fe$ ,  $v_2 = \varepsilon_2 + fe$ ,  $cov(\varepsilon_1, \varepsilon_2) = 0$ , and  $\varepsilon_1$  and  $\varepsilon_2$  are distributed i.i.d. normal. Subtracting equation A1 from equation A2 gives

$$Statetrust - Fedtrust = (b_0 - a_0) + \sum_{i=1}^k (b_i - a_i) x_i + \sum_{j=1}^m b_j statelaw_j + (\varepsilon_2 - \varepsilon_1) \quad (A3)$$

TABLE A1. SUMMARY STATISTICS FOR TRUST ANALYSIS

Variable	Mean
Relative state trust	0.62
Contribution limits (organizations only)	0.15
Contribution limits (orgs. and ind.)	0.50
Public funding	0.23
Income = 2	0.16
Income = 3	0.37
Income = 4	0.25
Income = 5	0.05
Unemployed	0.07
High School	0.42
Some College	0.27
College	0.27
Age	47.64
Age Squared	2578.90
Nonwhite	0.23
Female	0.55
Partisan Strength	2.93
Democrat	0.53
Republican	0.39
Unified Dem. Govt.	0.12
Unified Rep. Govt.	0.26
Dem. × Unified Dem.	0.07
Dem. × Unified Rep.	0.13
Rep. × Unified Rep.	0.11
Rep. × Unified Dem.	0.04
Citizen Initiative	0.41
Gub. Term Limits	0.73
Leg. Term Limits	0.38
Easy Registration	0.05

Summary statistics are from the 1996 National Election Study. Observations with missing data are excluded from the analysis.  $N = 1316$ . We present only the means because all variables (except for age, age squared, and partisan strength) are dichotomous. The standard deviations for the exceptions are as follows: age (17.59), age squared (1852.32), and partisan strength (0.94).

TABLE A2. TRUST IN GOVERNMENT AND CAMPAIGN FINANCE LAWS

<i>Variable</i>	<i>Coef.</i>	<i>z-stat</i>
Contribution limits (organizations only)	0.169	1.03
Contribution limits (orgs. and ind.)	0.077	0.62
Public funding	-0.110	1.00
Income = 2	0.317	2.18
Income = 3	0.371	3.34
Income = 4	0.314	2.50
Income = 5	0.411	2.29
Unemployed	-0.087	0.48
High School	-0.168	0.71
Some College	-0.227	1.12
College	-0.303	1.36
Age	-0.021	1.41
Age Squared	-0.000	1.33
Nonwhite	0.010	0.12
Female	-0.013	0.14
Partisan Strength	-0.121	2.61
Democrat	-0.033	0.17
Republican	0.746	4.38
Unified Dem. Govt.	-0.284	0.65
Unified Rep. Govt.	0.490	1.26
Dem. × Unified Dem.	0.043	0.11
Dem. × Unified Rep.	-0.685	1.83
Rep. × Unified Rep.	-0.754	1.78
Rep. × Unified Dem.	-0.011	0.02
Citizen Initiative	0.027	0.25
Gub. Term Limits	-0.024	0.27
Leg. Term Limits	0.043	0.35
Easy Registration	0.320	2.51
Constant	0.769	1.71

The probit specification includes region dummy variables with robust standard errors adjusted for clustering within state; absolute values of z-statistics are presented in the table. The dependent variable is coded 1 if individual trusts state government more than the federal government (mean = 0.62).  $N = 1316$ .

The dependent variable is now equivalent to  $y^*$ . Substituting gives

$$y^* = (b_0 - a_0) + \sum_{i=1}^k (b_i - a_i)x_i + \sum_{j=1}^m b_j \text{statelaw}_j + (\varepsilon_2 - \varepsilon_1) \quad (\text{A4})$$

If we are able to observe whether  $y^*$  is positive or negative, then this equation can be estimated using a standard probit or logit, with the dependent variable being coded one if  $y^* > 0$  and zero otherwise. This will allow us to estimate directly the effects of state laws on trust in government, since  $\sum_{j=1}^m b_j$  and the associated standard errors can be estimated via the combined equation. The remaining coefficients capture the effect of an independent variable on the difference between state-level and federal-level government trust. Therefore, in this esti-

imation, we are only able to measure the direct effect of campaign finance law (and other state-level institutions) on trust in state government. Standard errors need to be adjusted to account for clustering in the data (i.e., multiple observations from the same state). To implement this method, all we need is a survey question that asks respondents to rank-order their level of trust in the state and federal governments. Fortunately, such a question is available.

The 1996 National Election Studies asks two questions regarding relative levels of trust in the state and federal governments. These questions are used to assess the effects of campaign finance laws on trust in state government.

- We find that people differ in how much faith and confidence they have in various levels of government in this country. In your case, do you have more faith and con-

fidence in the national government, the government of this state, or in the local government around here?

- Which level of government do you have the least faith and confidence in—the national government, the government of this state, or the local government around here?

These two questions allow us to categorize individuals by whether they trust the state or the federal government more. There are six possible orderings for trust (denoting state by S, federal by F, and local by L), with  $>$  representing a strict ordering:  $S > F > L$ ;  $S > L > F$ ;  $L > S > F$ ;  $F > S > L$ ;  $F > L > S$ ; and  $L > F > S$ . Each of these is captured by the two questions above. For instance, if an individual answers that they have the most confidence in the state government and the least in the local government, then they must rank the federal government second (i.e.,  $S > F > L$ ).

Recall that the estimation requires that we know the sign of  $\gamma^*$ , a continuous latent variable that measures the difference in an individual's trust in the federal and state governments. The above orderings allow us to determine such a sign. When the state is trusted more than the federal government, this implies that  $\gamma^* > 0$ . This will be true for the first three orderings. The opposite will hold for the latter three. Thus, for each individual who responds to both of the above questions, we know whether  $\gamma^* < 0$  or  $\gamma^* > 0$ . Using this definition, 62 percent of respondents trust the state government more than the federal government.

The results comport with the findings in the main analysis, though none of the campaign finance variables are statistically significant.<sup>31</sup> (For summary statistics and results, see Tables A1 and A2.) The substantive effect of public financing is negative. The presence of organizational limits increases an individual's likelihood of trusting state government, but this effect dissipates if individual limits are introduced alongside those organizational limits.

<sup>31</sup> Disclosure is omitted because it is present in all states, and other state-level institutions, such as the presence of the citizen initiative or term limits, are also omitted due to perfect collinearity.

This implies that trust declines when limits on individuals are instituted, reinforcing the finding that moving beyond basic campaign finance laws may in fact harm perceptions of government.

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