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## DO STATE CAMPAIGN FINANCE REFORMS REDUCE PUBLIC CORRUPTION?

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by Adriana Cordis and Jeff Milyo



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**For press inquiries, please contact Jeff Milyo at [milyoj@missouri.edu](mailto:milyoj@missouri.edu).**

### **About the Authors**

Adriana Cordis  
Assistant Professor of Economics  
Department of Economics, University of South Carolina Upstate

Jeff Milyo  
Professor of Economics  
Department of Economics, University of Missouri

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### **Abstract**

The Supreme Court has long held that campaign finance regulations are permissible for preventing corruption or the appearance of corruption. Yet the implied hypothesis that campaign finance reforms are effective tools for combating public corruption has gone essentially untested. We conduct the first systematic evaluation of the effects of campaign finance laws on actual corruption rates in the states. We examine the effects of state reforms on both convictions and filings in public corruption cases over the last 25 years. Overall, we find no strong or convincing evidence that state campaign finance reforms reduce public corruption. Earlier research that employs similar methods also finds little support for the contention that state campaign finance regulations increase public trust and confidence in government. Together, these results call into question the legal rationale for campaign finance regulations.

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### **Keywords**

Political corruption, campaign finance

## Do State Campaign Finance Reforms Reduce Public Corruption?

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### I. Introduction

In the interest of preserving the basic constitutional freedoms of speech and association, the United States Supreme Court has long held that government restrictions on political campaign financing must be narrowly tailored to prevent “the actuality and appearance of corruption.”<sup>1</sup> This principle has been the basis for several court decisions that have reined in the scope of state and federal campaign finance regulations over the last 35 years.<sup>2</sup> For this reason, advocates for new and expanded restrictions on campaign financing maintain that such reforms are highly effective tools for addressing political corruption, preserving the integrity of democracy, and restoring public confidence in government.<sup>3</sup> Yet, despite this continual and intense focus on campaign finance reform as anticorruption policy, scholars have produced little work to evaluate whether campaign finance reforms actually reduce corruption or the appearance of corruption.

One explanation for the absence of systematic research on campaign finance reforms and corruption is that reforms themselves may be symptomatic of past corruption (Witko 2007).

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<sup>1</sup> Buckley v. Valeo, 424 U.S. 1 (1976).

<sup>2</sup> Recent examples include Randall v. Sorrell, 548 U.S. 230 (2006), striking low contribution limits in Vermont; Davis v. Federal Election Commission, 554 U.S. 724 (2008), striking differential contribution limits for candidates with self-financing opponents; Citizens United v. Federal Election Commission, 130 S. Ct. 876 (2010), striking prohibitions on corporate independent expenditures; Arizona Free Enterprise Club’s Freedom Club PAC et al. v. Bennett, 131 S. Ct. 2806 (2011), striking public matching funds for candidates with high-spending opponents; and American Tradition Partnership v. Bullock, 567 U.S. \_\_\_\_ (2012), which reaffirmed the application of *Citizens United* to the states.

<sup>3</sup> See Primo and Milyo 2006 for documented examples of such claims. For recent evidence, consider reactions to the *Citizens United* decision. For example, the *New York Times* editorial page characterized the majority decision as “radical” and as striking “a blow to the heart of democracy” in “The Court’s Blow to Democracy,” January 21, 2010, <http://www.nytimes.com/2010/01/22/opinion/22fri1.html>. In a subsequent weekly radio address, President Obama called for campaign finance reform, declaring, “What is at stake is no less than the integrity of our democracy”; see Barack Obama, “Giving Government Back to the American People,” weekly radio address, 2010, transcript available at <http://www.whitehouse.gov/blog/2010/05/01/weekly-address-giving-government-back-american-people>. For recent high-profile calls for campaign finance reform as a means specifically to address political corruption, see the Center for Public Integrity’s “State Integrity Investigation,” accessed July 13, 2012, <http://www.stateintegrity.org/>.

Moreover, it is difficult to disentangle the impact of federal reforms from other factors that may change coincidentally over time. But as Primo and Milyo (2006) and Milyo (2012) demonstrate, state campaign finance laws vary substantially both across states and over time; those authors exploit this state-level variation to identify the treatment effect of campaign finance reforms on public opinion about elections and government. In addition, a small but growing literature examines state-level data on public corruption convictions over time to analyze the causes and consequences of corruption in the states (e.g., Meier and Schlesinger 2002; Glaeser and Saks 2006; Cordis 2009 and 2012; Cordis and Warren 2012). Consequently, the states offer a laboratory for investigating the effects of campaign finance reforms on public corruption.

In this report, we conduct the first systematic analysis of the effects of state campaign finance reforms on corruption by state officials. We analyze corruption convictions among state government officials in every state from 1986 through 2010. This approach allows us to control for both time-varying and time-invariant state-specific factors, which in turn mitigates concerns about reverse causality from corruption to reform. As a further check on the endogeneity of reforms in the states, we examine the time trends in corruption leading up to episodes of reform. Finally, we examine time trends after reform as a check for delayed effects on corruption in the states.

We measure corruption using detailed data on both convictions and prosecutorial filings from the Transactional Records Access Clearinghouse at Syracuse University (TRACfed).<sup>4</sup> Whereas most previous studies of corruption cannot distinguish between federal, state, and local government officials, the TRACfed database permits us to focus on public corruption by state officials. We cannot observe the true corruption rate because of the hidden nature of corrupt activities, so convictions are at best a proxy for public corruption. Of particular concern is the

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<sup>4</sup> We obtained these data under license from TRACfed (<http://tracfed.syr.edu/>).

possibility that prosecutors are themselves tainted by local corruption and turn a blind eye to wrongdoing by government officials. A second concern is that legal standards and anticorruption efforts may vary across jurisdictions. However, we observe that federal district attorneys, who should be fairly insulated from local politics, prosecute nearly all public corruption cases. This system also ensures that attorneys pursue prosecutions under uniform legal standards. Further, the availability of corruption convictions among *federal* officials in a state provides a proxy for prosecutorial effort in the pursuit of corruption cases.

A final challenge to our analysis is the fact that public corruption convictions of state officials are quite rare. We observe no corruption convictions in about 60 percent of our state-year observations. We address the sporadic nature of corruption in several ways. First, we examine long-term trends in descriptive statistics within states as a first pass at uncovering any correlation between average corruption rates and campaign finance regulations in the states. We then conduct several different multivariate regression analyses and subject these to a battery of robustness checks.

We estimate both a conditional fixed-effects negative binomial regression on conviction counts and a random effects Tobit analysis of conviction rates. These estimation methods are well-suited to dealing with panel data that include many zero observations for the dependent variable, but neither model permits us to estimate true fixed effects. Consequently, we also estimate an ordinary least squares model with state fixed effects. All three of these approaches yield similar findings that are robust to a variety of alternative specifications. Overall, we find that state campaign finance reforms do not reduce state-level convictions (or filings) in public corruption cases.

## II. Background and Literature Review

There is a considerable scholarly literature on money in American politics; however, most studies focus on either the effects of campaign spending on election competitiveness or the effect of campaign contributions in shaping public policy. And while these questions certainly merit attention, it is quite amazing that some 35 years after *Buckley v. Valeo*, the fundamental issue of whether campaign finance laws reduce corruption has received little attention.

### *Money and Corruption*

Popular wisdom holds that money plays a dominant and corrupting role in American democracy, so it follows that campaign finance regulations might have a dramatic impact on political corruption. However, scientific research does not support this view of money in politics. For example, contrary to the popular concern that elective offices are essentially for sale to the highest bidder, several studies suggest only negligible effects of campaign spending on candidates' electoral success (e.g., Levitt 1994; Gerber 1998; Milyo and Groseclose 1999; Milyo 2001). Further, despite oft-stated fears that campaigns are awash in funds and that contributions are the functional equivalent of bribes, several studies argue that there is surprisingly little money flowing into American politics precisely because contributions do not appear to buy political favors (e.g., Milyo, Primo, and Groseclose 2000; Ansolabehere, de Figueiredo, and Snyder 2003). And finally, rather than alienating potential voters, campaign spending has long been associated with greater knowledge and interest in public affairs, as well as increased voter turnout (Coleman and Manna 2000).

Given such findings, it is by no means obvious that campaign finance reforms should have much impact on political corruption, unless, that is, one defines corruption by the presence

of money in politics. As Milyo (2012) notes, many reform advocates adhere to just such a “miasma theory” of corruption, wherein money exerts a nebulous corrupting influence on politics. In this view, any regulation that limits campaign finances is akin to “draining the swamp” and is therefore assumed to reduce corruption. However, the Supreme Court has not taken such a broad view of what constitutes public corruption, and instead has limited the legal focus of political corruption to criminal acts such as embezzlement, misappropriation of funds, bribery, and influence peddling.

Politics involves exchanges of all manner of favors. At what point do such exchanges cross the line and become bribes? Under the *Buckley* precedent, the majority of the court has consistently required an explicit quid pro quo in order for such exchanges to rise to the level of bribery or influence peddling. Consequently, most scholarly literature on money in American politics does not directly address whether campaign finance regulations satisfy the anticorruption rationale that the courts demand.

It is this understanding of what constitutes corruption that has led the court to declare unconstitutional many different types of federal and state campaign finance laws. For example, the government may limit the source and amount of contributions to candidates, but not total expenditures by candidates, since expenditure caps do not directly prevent quid pro quo exchanges but do limit candidate speech.<sup>5</sup> But the court has also struck down contribution limits for being too low (\$100 for state candidates in Vermont), since such low limits go beyond what is necessary to deter corruption (and hence are not “narrowly tailored” toward that end).<sup>6</sup> For a similar reason, the government cannot regulate self-financing by candidates; it is not possible for candidates to corrupt themselves with their own money. Likewise, state governments cannot

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<sup>5</sup> *Buckley v. Valeo*, 424 U.S. 1 (1976).

<sup>6</sup> *Randall v. Sorrell*, 548 U.S. 230 (2006).

limit the financing of initiatives and ballot measures, since the text of any such proposition is uncorrupted by spending for or against the proposition.

Similarly, in the recent and controversial *Citizens United* decision, the court affirmed that campaign expenditures made independent of any candidate are a kind of “safe harbor,” so that there is no anticorruption rationale for federal or state governments to prohibit corporations, unions, or other organizations from engaging in independent expenditures.<sup>7</sup> Most recently, the court reaffirmed that independent expenditures “do not give rise to corruption or the appearance of corruption.”<sup>8</sup> The court has also invalidated attempts at “leveling the playing field” between candidates that accept limited public funding for their campaigns and potentially high-spending opponents. Again, it has done so because laws that grant extra funding or differential contribution limits to publicly funded candidates facing stiff competition are not really aimed at preventing corruption, but instead punish high-spending candidates for engaging in “too much” political speech.<sup>9</sup>

### ***Campaign Finance Reform and Political Corruption***

Political economists have done little work to evaluate the impact of campaign finance reforms on corruption. One legitimate excuse for this shortfall is the inherent difficulty in identifying effects of federal reforms from other time-varying determinants of political corruption. But as Primo and Milyo (2006) and Milyo (2012) demonstrate, the states provide a laboratory much more amenable to this task, as there is substantial variation in state regulations both across and within states.

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<sup>7</sup> *Citizens United v. Federal Election Commission*, 130 S. Ct. 876 (2010).

<sup>8</sup> *American Tradition Partnership v. Bullock*, 567 U.S. \_\_\_\_ (2012).

<sup>9</sup> *Davis v. Federal Election Commission*, 554 U.S. 724 (2008); *Arizona Free Enterprise Club’s Freedom Club PAC et al. v. Bennett*, 131 S. Ct. 2806 (2011).



Even so, few studies even tangentially examine whether state campaign finance laws are associated with political corruption in the states. Of these, only an unpublished working paper by Maxwell and Winters (2005) uses data on actual corruption convictions, but this study examines just a single cross-section of data, so the authors cannot identify the within-state effects of reform. The remaining studies (Alt and Lassen 2003, 2008; Rosenson 2009) instead use Boylan and Long's (2003) survey of statehouse reporters to measure state-level corruption. However, the Boylan and Long survey data are only available for a single year, so these authors are also unable to identify within-state effects of campaign finance reforms.<sup>10</sup>

The corruption measures these studies employ are of dubious quality. Maxwell and Winters (2005) employ data on convictions from the Public Integrity Section (PIN) of the Department of Justice. This is by far the most common source social scientists employ for data on state-level public corruption.<sup>11</sup> The PIN data are based on a survey of federal prosecutors, so they contain some misreports and subjectivity in classifying cases (Boylan and Long 2003). Further, state officials account for less than 10 percent of all corruption convictions over the last 25 years, so that total convictions reported by the PIN are not highly correlated with observed corruption convictions among state officials (Cordis and Milyo 2013).

The shortcomings in the PIN survey data motivated Boylan and Long's (2003) survey of statehouse reporters. But this survey is also problematic in that it is based on the subjective opinions of a few journalists. Further, while statehouse reporters may have expertise in their own

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<sup>10</sup> Stratmann (2003) examines a single cross-section of 14 democratic countries to analyze the effects of national campaign finance laws on corruption. He finds more restrictive contribution limits are associated with higher levels of public corruption, as measured by the Transparency International Bribe Payers Index and the World Bank Corruption Index. Stratmann's is the only study of which we are aware that investigates the connection between campaign finance laws and corruption across countries.

<sup>11</sup> See, for example, Adsera, Boix, and Payne 2003; Alt and Lassen 2008; Cordis 2009; Dincer, Ellis, and Waddell 2010; Fisman and Gatti 2002; Glaeser and Saks 2006; Goel and Nelson 2011; Hill 2003; Johnson, LaFountain, and Yamark 2011; Leeson and Sobel 2008; Maxwell and Winters 2005; Meier and Holbrook 1992; Meier and Schlesinger 2002; and Nice 1983.

state political environments, it is less plausible that they would have much expertise in making comparisons to other states.<sup>12</sup>

Neither Maxwell and Winters (2005) nor Alt and Lassen (2003, 2008) set out to investigate campaign finance regulations as a determinant of public corruption. In fact, these authors examine only a single indicator for state campaign finance laws and only as an additional control variable in a subset of their statistical models. Further, the campaign finance regulation variable used in all three of these studies describes states with any restrictions on “campaign spending by or on behalf of candidates”; however, the landmark 1976 *Buckley* decision rendered mandatory spending restrictions unenforceable. Consequently, these authors appear to be measuring only the cross-sectional association between voluntary spending restrictions in some states and corruption. Controls for other prominent and more relevant features of state campaign finance regulatory regimes, such as contribution limits for different types of contributors, are absent.<sup>13</sup>

In contrast, Rosenson (2009) investigates specifically whether state campaign finance laws affect political corruption by examining the cross-sectional correlation between an index of major state campaign finance laws and statehouse reporters’ subjective evaluations of corruption in their own states. Rosenson also attempts to address the potential endogeneity of reforms by using an instrumental variables estimation procedure. This exercise is problematic for two reasons: (1) the first stage regression does not include all exogenous variables in the structural

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<sup>12</sup> A similar problem exists with the more recent “corruption risk report cards” assembled by the State Integrity Investigation (<http://www.stateintegrity.org/>), a coalition of media and advocacy organizations. For a more detailed discussion, see Cordis and Milyo (2013).

<sup>13</sup> Most states with voluntary spending ceilings for candidates offer public financing for candidates that abide by these limits; however, Maxwell and Winters (2005) and Alt and Lassen (2003, 2008) used an indicator that also includes states such as Colorado with purely voluntary spending limits. Further, this indicator does not distinguish between states that offer public financing to only gubernatorial candidates and those that also include state legislative candidates.

model, only the excluded instruments; and (2) the proposed instruments (government ideology, membership in Common Cause, and population) are themselves unlikely to be truly exogenous.

As a consequence of these shortcomings in both data and methods, the existing literature is uninformative about whether campaign finance reforms affect public corruption in the states. Even putting aside all such concerns, these studies offer no consistent evidence. Maxwell and Winters observe no significant relationship; Alt and Lassen find a negative association between voluntary spending restrictions and reporters' perceptions of corruption; and Rosenson finds a positive association between state campaign finance laws and reporters' perceptions of corruption.

Few other studies have made serious efforts to estimate the causal effects of state campaign finance laws on the “appearance of corruption,” or similar public-opinion-based measures of trust and confidence in government. In fact, only two studies examine the within-state effects of campaign finance laws on relevant public attitudes. Primo and Milyo (2006) find no strong evidence that reforms increase political efficacy, while Milyo (2012) finds no effect of campaign finance reforms on trust and confidence in government. These studies stand out for their implementation of best-practice evaluation methods, such as estimating treatment effects via difference-in-differences and performing multiple checks for the presence of time-varying unobservable factors that might confound identification in these models.

### **III. Data and Methods**

We seek to evaluate the treatment effect of state campaign finance reforms on the occurrence of public corruption. An immediate concern is that public corruption may cause state campaign finance reforms. We address this potential endogeneity in three ways. First, we examine the

raw data for any long-run relationship between (changes in) campaign finance laws and the levels of (or changes in) public corruption in the states. Second, we estimate regressions with state fixed effects to sweep out time-invariant unobservables and otherwise mitigate endogeneity bias (see, e.g., Levitt 1994).<sup>14</sup> Finally, we look for trends in state corruption in the years leading up to or just after episodes of campaign finance reform. While these methods are fairly standard in the evaluation literature, the challenge of measuring public corruption in the states complicates our task.

### ***Measuring Public Corruption in the States***

As noted previously, most empirical research on public corruption in the United States employs convictions data from the Public Integrity Section of the Department of Justice. However, among other problems, the PIN does not disaggregate state-level conviction data by type of government official, nor does it provide state-level breakdowns for cases filed versus convictions. For these reasons, we follow Cordis and Warren (2012) and Alt and Lassen (2011) in utilizing the TRACfed data archive. TRAC systematically employs the Freedom of Information Act (FOIA) to make available to the public large quantities of records from various federal agencies. Information on criminal cases based on administrative records from the Department of Justice is available beginning in 1986. Under license from TRAC, we collected data on all convictions and case filings classified by prosecutors as official corruption. From these data, we created annual series of state-level public corruption convictions and filings from 1986 through 2010.

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<sup>14</sup> In principle, we could use instrumental variable methods to address potential endogeneity, but we do not believe we can identify credible instruments to use. Previous studies that consider the determinants of state campaign finance regulations suggest variables like party control of government (Stratmann and Aparicio-Castillo 2006) or the presence of an initiative process in the state (Witko 2005); however, party control of government is also a likely determinant of corruption, while there is too little variation over time in the number of initiative states.

Figure 1 (page 39) plots corruption convictions for federal, state, and local officials over time. From this figure, it is apparent that convictions of state officials are relatively rare. So, while one advantage of TRACfed data is that they allow us to analyze corruption among state officials, the paucity of such convictions produces some challenges for our subsequent regression analyses. Figure 1 also raises the concern that federal prosecutors focus more on corruption among federal officials than nonfederal officials. However, Cordis and Milyo (2013) collect data on state and local prosecutions of public corruption from media reports and find that federal prosecutors handled 95 percent of all corruption cases from 1986 through 2010. Even so, in our subsequent statistical analyses, we augment the TRACfed data on corruption convictions among state officials resulting from federal prosecutions with these additional convictions from state and local prosecutions.

### ***Convictions versus Corruption***

The difficulty of observing illicit activities plagues any attempt to study such activities. Convictions may be a good proxy for illegal activity if legal standards and prosecutorial effort do not vary (or in our regression context, do not vary systematically with the independent variables of interest, state campaign finance reforms). Federal prosecutors in federal courts handle nearly all corruption cases in the United States, which goes a long way in addressing any concerns about variations in legal standards. Further, federal prosecutors are not beholden to state politicians, which also mitigates concerns that corruption among state officials compromises prosecutors and leads to a reduced effort to root out public corruption. Even so, prosecutorial effort may vary across states or over time within a state in a manner that confounds the identification of any treatment effect of campaign finance reforms on corruption. Another

advantage then of the TRACfed data is that we can use corruption convictions of federal officials in a state as a proxy for prosecutorial effort in corruption cases.

Yet another feature of the TRACfed data is the availability of prosecutorial filings in corruption cases disaggregated by state. Attorneys cannot demonstrate all corruption sufficiently in court to achieve a conviction, so prosecutorial filings give us another measure of the presence of state corruption. Further, there is less delay from acts of corruption to filings compared to convictions. Consequently, we perform all our statistical analyses using both convictions and filings (and with and without controls for prosecutorial effort).

### ***Delays in Case Filings and Convictions***

The TRACfed data archive includes information on the median and average time from initial referral of a public corruption case to filing or conviction. From 1986 through 2011, the median time from referral to case filing is 112 days and the average is 260 days; for convictions the median and average times are 386 days and 556 days, respectively. Consequently, measures of corruption based on filings and convictions will often lead the calendar year in which the associated corruption occurred.

To identify the effect of changes in state campaign finance laws, we need to account for the delay in observing corruption filings or convictions. We address this complication in several ways. As a first pass, we examine patterns in the raw data over decades in order to observe slow-moving trends. Second, in our multivariate analysis of annual data, we conduct several robustness tests to check for a delayed effect of state campaign finance reforms on our corruption measures. These checks include leading the indicators for state reforms by three or five years. We also estimate a series of indicators for one year after reform, two years after reform, and so

on. We additionally check for delayed effects by pooling our annual data into five nonoverlapping five-year waves, which permits us to examine the effects of state campaign finance regulations in year  $t$  on average corruption convictions for years  $t$  through  $t + 4$ . Finally, because filings do not exhibit the same delay, we check all our models using prosecutorial filings as the dependent variable.

### ***Public Corruption as a Limited Dependent Variable***

In order to compare corruption convictions and filings across states, we normalize them by the pool of government officials in the state. We define the corruption rate by the convictions or filings per 10,000 state government full-time equivalent civilian employees (FTEs). Table 1 (page 33) provides descriptive statistics for these four dependent variables: corruption convictions and prosecutorial filings, and the corruption and filing rates.

For all these measures, most state-year observations are zero (60 percent for convictions). Figure 2 (page 40) shows the histogram for convictions; it is apparent that corruption convictions are rare and idiosyncratic events. Given this finding, in our multivariate analysis, we estimate a negative binomial model of conviction counts in the states. For conviction rates, we have a similar problem that data on convictions are censored at zero. That is, there is likely some degree of corruption in every state, but not enough that it results in an observable conviction in most years. Consequently, we also estimate Tobit models of conviction rates to account for this censoring.

An alternative and simple method for analyzing sporadic corruption data is to pool them over a number of years and examine the cumulated or average incidence of corruption in the states. In figure 3 (page 41), we plot the average annual conviction rate in each state for

1991–2000 against the same for 2001–2010. The figure demonstrates that there is some degree of persistence in corruption convictions. Several states have either low corruption rates in both decades (e.g., South Dakota, Kansas, and Michigan) or high corruption rates in both decades (e.g., West Virginia, Tennessee, and Illinois). There also appears to be an upward drift in corruption rates in most states. Among those states that exhibit relatively large changes in corruption rates across these last two decades, more transition from low to high corruption (e.g., Connecticut, Rhode Island, and Alabama) than vice versa (e.g., Massachusetts).

As figure 3 indicates, pooling convictions data over time reduces the number of states with no observed corruption. For this reason, we examine the long-run correlation between average corruption rates and state campaign finance laws. In addition, while our primary multivariate analysis uses annual observations, we also check the robustness of our results by estimating our models using five nonoverlapping waves of five-year corruption rates.

### ***Panel Data Methods***

In our multivariate statistical models, we analyze a repeated cross-section of state-level observations, which allows us to take advantage of panel estimation methods. Accordingly, we estimate conditional fixed effects negative binomial regressions and random effects Tobits; however, neither estimator permits consistent estimation of true fixed effects.<sup>15</sup> For this reason, we also analyze ordinary least squares models for conviction rates in the states, since the linear model performs well in estimating marginal effects for limited dependent variables and permits estimation of within-state effects via inclusion of state indicators (Angrist and Pischke 2009). We

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<sup>15</sup> We estimate the conditional fixed effects negative binomial regression using the XTNBREG command in Stata 12 with the fixed effects option; however, fixed effects in this model refer to the dispersion within states, not true state-fixed effects. Likewise, we use the XTTOBIT command to estimate the random effects Tobit.



also adjust standard errors for clustering by state in our least squares models, so that the reported errors are both heteroscedastic-consistent and autocorrelation-consistent (Primo, Milyo, and Jacobsmeier 2007; Bertrand, Duflo, and Mullainathan 2004).

Finally, as an additional check for the presence of time-varying unobservables that may confound the identification of the treatment effect of state reforms on conviction rates, we estimate 11 separate indicators for each year before and after the implementation of a particular reform from  $t - 5$  to  $t + 5$ , as well as an indicator for the presence of that same reform for years  $t + 6$  and beyond. We then plot the estimated coefficients and 95 percent confidence interval for these indicators. This process allows us to easily observe any delayed impacts of reform as well as evidence of “reverse causality” from episodes of corruption to reform.

### ***State Campaign Finance Regulations***

We took all data on state campaign finance laws from Milyo (2012), who in turn relied on several sources, including the National Conference of State Legislatures, state government websites, and the Federal Election Commission. As Milyo (2012) and Primo and Milyo (2006) note, state campaign finance regulatory regimes fall into five broad and nested types: (1) no contribution limits, (2) limits on corporate contributions to candidates, (3) limits on corporate and individual contributions to candidates, (4) contribution limits and public funding of gubernatorial elections, and (5) contribution limits and public funding of gubernatorial and state legislative elections. Therefore, we create a campaign finance regulation index that ranges from 0 to 4, respectively. In addition to this simple index, we also examine the effects of the component laws by employing separate indicators for limits on corporate contributions, limits on individual contributions, and each type of public funding.

Table 2 (page 34) shows the number of states with each type of campaign finance law, as well as the average value of the campaign finance regulation index, by decade. Over the last 30 years, there has been a net increase in the number of states with contribution limits, and a smaller increase in the number of states that employ public funding of campaigns. However, because some states adopt, repeal, and then readopt campaign finance regulations (e.g., California and Missouri), the total number of changes is greater than the net change over time.

The state laws described above constitute the major features of the campaign finance regulatory landscape. For example, while states differ somewhat in their disclosure requirements for candidates (mainly in thresholds for disclosure and the timing and frequency of reports), over the time period that we examine, all states require public disclosure of contributor information. And while there is some variation in the treatment of independent expenditures across states, these have not been a particularly important source of campaign spending in most states for the time period that we examine (i.e., prior to *Citizens United*). Also, while maximum contribution limits vary somewhat among states with contribution limits, it is not readily apparent how to scale such limits. Finally, among states with public financing, most provide only partial matching funds up to a cap, but three states now provide “full funding” (albeit also up to a predetermined cap). Arizona and Maine implemented these so-called “clean money” reforms in the 2000 election cycle, but Connecticut only implemented its reform in 2010. Consequently, we only have two states with any post-clean election observations. For these reasons (and ease of exposition), we focus on the major features of campaign finance regulations listed in table 2. But for good measure, we also check the sensitivity of our findings to including variables for limits on independent expenditures and full public financing.

### ***Campaign Finance Reform and Public Corruption: A First Look***

In figure 4 (page 42), we illustrate the long-run association between average annual corruption conviction rates and the average state campaign finance regulation index over the last 20 years. Since many state campaign finance laws remain unchanged over this time period, this diagram should reveal any strong equilibrium relationship. However, there does not appear to be a negative (or positive) relationship between corruption convictions and campaign finance regulations.

It is possible that those states with a legacy of public corruption are the very same states that adopt reforms, so that reform-minded states exhibit high but declining rates of corruption. In that case, the contemporaneous snapshot in figure 4 could be misleading about the efficacy of reforms. For this reason, we present figure 5 (page 43), which compares the *change* in the average campaign finance index from the 1990s to the 2000s to the *change* in average annual corruption rates from the same periods. In this figure, it is apparent that among states that did not change their campaign finance laws, the average change in corruption was about zero. But the same is also true for the set of states that *did* change their campaign finance laws. Therefore, the long-term patterns in these raw data do not suggest that campaign finance reforms reduce public corruption.

However, some important determinants of state corruption may be spuriously correlated with campaign finance regulations, and so mask the true causal relationship in these figures. Consequently, we now consider multivariate models that include controls for potential confounding variables.

### *Control Variables in Multivariate Analyses*

We estimate three different types of statistical models (negative binomial, Tobit, and least squares) that relate campaign finance reforms to corruption in the states. All these models include year indicators, but only the ordinary least squares model also includes indicators for each state. Table 3 (page 35) provides descriptive statistics for all other control variables. Given the extensive list of control variables, we will estimate all our models with and without these controls. This process ensures that our findings are not sensitive to the particular set of control variables that we employ.

Our preferred regression specifications all include controls for time-varying state demographics and other state economic and political characteristics. The demographic controls include age composition of the state population, educational attainment, Hispanic ethnicity, the log of real per capita income, poverty status, race, and union membership. The economic and political controls include the state unemployment rate, the log of real state expenditures per capita, the log of state government employment, term limits, FOIA laws, and unified party control of state government.

Our preferred models also include the conviction rate for federal officials as a control for prosecutorial effort. However, convictions of federal officials are sporadic events just like convictions of state officials, so these two rates are not highly correlated in state-year data ( $r = 0.07$ ). Consequently, we smooth the conviction rate for federal officials by estimating a fifth order polynomial of the time trend in each state. We then use the predicted conviction rate for federal officials as our measure of prosecutorial effort.

Finally, as noted earlier, the negative binomial and Tobit models do not permit the inclusion of state fixed effects as control variables. This limitation raises the concern that our

findings in these models may result from excluded state characteristics that are spuriously correlated with both state campaign finance regulations and public corruption rates. To guard against this concern, we include controls in these models for both the judicial selection process and for whether the state permits direct legislation via ballot initiatives, as these are two prominent institutions that may lead to such confounding.

#### **IV. Results**

In this section, we present the results of three different types of multivariate regression analyses. The first analysis examines the effects of campaign finance regulations on corruption using a count model. The second uses a Tobit to examine corruption rates, and the third employs ordinary least squares to examine corruption rates. We then discuss the robustness of our results to different modeling assumptions. Finally, we estimate time trends before and after episodes of reform as a final check for reverse causality or delayed effects of reforms. In all cases, we report only the coefficients of interest; however, full regression results are available from the authors.

##### ***Negative Binomial Estimates***

In table 4 (page 36), we report the estimate incidence ratios and standard errors for selected independent variables of interest from a conditional fixed effects negative binomial regression on corruption convictions of state officials. All the models in columns 1–6 of table 4 include the full set of control variables listed in table 3, as well as year indicators. For these models, we focus on the limited nature of the dependent variable and put aside concerns about simultaneity that our control variables do not address.

We first model campaign finance laws as a simple additive index that ranges from zero to four (see column 1). The estimated incidence ratio indicates that a one point increase in this index yields a 6 percent *increase* in corruption convictions, although this effect is not statistically significant. Further, given that the average number of convictions of state officials in a given year is 1.12 and the standard deviation is 2.37, the point estimate is also substantively small. For example, moving from a completely unregulated regime to the most stringent regime would yield about one more corruption conviction over a three-year period (or a 30 percent increase in the annual incidence).

In column 2 of table 4, we estimate a model that includes an indicator for each type of major state campaign finance law. None of the incidence ratios for these indicators is statistically significant, nor are they jointly significant. The estimated coefficients for public financing are more sizable, but the effects of gubernatorial and legislative public financing are in opposite directions. Likewise, the estimates for corporate and individual contribution limits yield opposite effects on corruption counts. This pattern may be the result of correlation among these indicator variables.

In fact, the correlation between the contribution limit variables is 0.63 and the correlation between the two public financing variables is 0.67. Because the campaign finance indicators are highly correlated, we also estimate models where these indicators enter one at a time (see columns 3–7 of table 4). Once again, none of these indicators is statistically significant. Further, all the estimates are now smaller than in column 2, and all now have a *positive* effect on corruption.

The absence of significant effects from state campaign finance laws raises the concern that corruption counts are simply too noisy to observe any statistically significant determinants

of corruption. However, in every model, our proxy for prosecutorial effort (the smoothed federal conviction rate) is significantly related to corruption convictions among state officials. These incidence ratios imply that for every conviction per 10,000 federal officials in a state, there is a 25 percent increase in the number of state officials convicted. In addition, several of our other control variables have a significant effect (not shown in table 4). High school educational attainment ( $p < .01$ ) and unified party control of state government ( $p < .05$ ) are negatively associated with corruption convictions, while ballot measures ( $p < .01$ ) and the FOIA index ( $p < .05$ ) are positively associated with corruption convictions.

### ***Tobit Estimates***

In contrast to the count model above, we examine corruption rates in this section, again with a focus on the limited dependent variable. In table 5, we report the estimated coefficients and standard errors for selected independent variables of interest from random effects Tobit analysis of corruption conviction rates per 10,000 state officials. All the models in columns 1–6 of table 5 include the full set of control variables listed in table 3, as well as year indicators. All the models in table 5 are otherwise identical to those in table 4.

Starting in column 1, as in the previous set of regression equations, the index of state campaign finance regulations is positively associated with corruption convictions, although the estimated effect is small and statistically insignificant. The coefficient estimate in column 1 indicates that a one-point increase in the regulation index leads to 0.03 more convictions per 10,000 state FTEs. This effect is equivalent to just one-eighth of the standard deviation in the conviction rate (or about one-fourth of the mean conviction rate).

In columns 2–6, we report estimates when we unpack the campaign finance index into its component indicators. None of the campaign finance indicators is statistically significant, except for the relatively large coefficient on gubernatorial public financing in model 2 ( $p < .10$ ). This estimate suggests that gubernatorial public financing increases corruption convictions by 0.1 per 10,000 state FTEs, which is about 75 percent of the mean conviction rate and 40 percent of the standard deviation. However, the campaign finance indicators in model 2 are also not jointly significant.

As before, we observe other variables in the Tobit analysis that are significant determinants of corruption rates. For example, the federal conviction rate, our proxy for prosecutorial effort, is positively and significantly related to corruption. In addition, high school educational attainment ( $p < .01$ ), unified party control of state government ( $p < .05$ ), and legislative term limits ( $p < .05$ ) are all negatively related to corruption conviction rates. Finally, ballot measures ( $p < .01$ ) and partisan judicial elections ( $p < .01$ ) are each positively related to conviction rates.

### ***Ordinary Least Squares***

We now explicitly address remaining concerns about state-specific confounding variables by examining a difference-in-difference model. In table 6 (page 38), we report the estimated coefficients and standard errors for selected independent variables of interest from ordinary least squares regression with state fixed effects. All the models in columns 1–6 of table 6 also include the time-varying control variables listed in table 3, as well as year and state indicators.

In column 1, we report the coefficient on the campaign finance regulatory index. This index has a substantively and statistically insignificant impact on corruption conviction rates.



Likewise, when we unpack the index into its component indicator variables, they are not individually or jointly significant (columns 2–6). The effect of corporate contribution limits in model 2 is negative; the point estimate suggests that such limits reduce the conviction rate by about 70 percent of the mean and 36 percent of the standard deviation. This magnitude is similar to what we observed in the Tobit analysis, but again, in neither case are these effects statistically different from zero.

Once again, the proxy for prosecutorial effort is positively associated with the conviction rate for state officials ( $p < .01$ ). In addition, unified government ( $p < .05$ ) and the log of population ( $p < .05$ ) are associated with lower conviction rates; Hispanic ethnicity is positively associated with corruption ( $p < .01$ ).

### ***Robustness of Results***

We have estimated several variations of the models discussed above. In each case, we do not observe campaign finance laws to have a statistically significant impact on corruption counts or rates. For example, we re-estimated all the models above without controlling for the smoothed federal conviction rate. This does not substantively affect the estimates of interest (neither does replacing the smoothed conviction rate for federal officials with the observed annual rate).

Likewise, the substantive results remain unchanged when we drop all the control variables except those for year indicators and the log of state FTEs (and state indicators in the ordinary least squares model). Consequently, the particular set of control variables that we employ does not drive the failure to observe any significant reduction in corruption convictions.

Nor is it the case that the manner in which we describe state campaign finance laws drives our findings. For example, we have also examined each model that we estimate in tables

4–6 instead using the square of the campaign finance index or the log of one plus the index. These alternative specifications also do not yield significant results. For the models that employ binary indicators for campaign finance laws, we have substituted the number of years since 1976 that a law has been in place, or the log of the number of years. Again, this substitution does not alter our finding that campaign finance laws are not significantly related to corruption rates. Finally, we have also estimated our models including indicators for limits on independent expenditures and/or the “clean money” reforms that Arizona and Maine implemented in 2000. In no case are these additional variables individually or jointly significant.

We also re-estimated all our models in tables 4–6 using prosecutorial filings or filing rates per 10,000 state FTEs as the dependent variable. This does not substantively change the observed lack of a statistically significant association between campaign finance laws and corruption. We also re-estimated all our models after pooling the data into five nonoverlapping five-year waves. Again, we observe no significant effect of campaign finance laws on either measure of corruption (convictions or filings).

As an additional check on the possibility that reforms have a delayed impact on convictions, we also estimated each of the models reported in columns 2–6 of tables 4–6 but substituted indicators for campaign finance laws that have been in place for three or more years. Once again, we find no case in which state campaign finance laws are individually or jointly significant.

### ***Time Trends before and after Reform***

Up to this point, the only manner in which we have addressed the potential endogeneity of state campaign finance laws is through the use of multiple control variables and, in the ordinary least

squares model, state-level fixed effects. As noted earlier, we are not sanguine about the validity of the instrumental variables that Rosenson (2009) proposes. Therefore, we let the data speak for themselves regarding the presence of any time-varying trends in corruption before or after episodes of reform.

First, we re-estimate the models in column 2 of each table, but also include indicators for three years prior to each law being implemented, two years prior, and one year prior. For the negative binomial model and the Tobit model, these indicators are not significant; however, several of these lag variables are significant in the ordinary least squares regression.

Consequently, we conduct a more detailed analysis of time trends before and after episodes of reform using the ordinary least squares model with state fixed effects and indicators for each campaign finance law (i.e., the model in column 2 of table 6).

In order to check for the presence of confounding time trends changes in state corruption convictions, we create a set of time indicators for five-year leads and lags of a given reform. We examine each type of state law thusly in a separate regression. For example, when examining time trends around the implementation of corporate limits, we estimate the model in column 2 of table 6, but now we include separate indicators for five years prior to adopting corporate limits, four years prior, and so on up to five years after the adoption of limits. In addition, we estimate a common effect for six years out and beyond. We then repeat this exercise for each of the state laws in this model.

Figures 6–9 report the results of this exercise. We illustrate the estimated time paths for corruption conviction rates before and after the implementation of a specific reform, based on the estimated coefficients of the leads and lags. For example, in figure 6 (page 44), we show the time path for conviction rates in states that implement limits on corporate contributions. The solid line

indicates the estimated trend in convictions and the dotted lines indicate the 95 percent confidence interval. Because the confidence intervals always bound zero, none of the leading or lagging indicators is statistically significant (nor can we reject the null hypothesis that all the lead or lag indicators are jointly zero). Consequently, we are fairly confident that there are no unobserved trends that confound our estimate of the treatment effect of corporate contribution limits in this case.

Figure 7 (page 45) tells a similar story for individual contribution limits. However, figures 8 and 9 (pages 46–47) weakly suggest that episodes of corruption convictions lead to the adoption of public financing. In neither case do these reforms then lead to a significant decrease in corruption rates. Also, in none of these figures (6–9) do we observe a significant decrease in corruption after some period of years.

## **V. Discussion**

We conduct the first systematic and comprehensive test of the hypothesis that state campaign finance reforms reduce actual instances of public corruption. We employ several modeling strategies to overcome the time delay between acts of corruption and observations of corruption, as well as address the potential endogeneity of reforms and corruption. We do not observe any strong pattern between average corruption convictions and campaign finance regulatory regimes in the raw data (either in levels or changes). We do not observe any significant effects of state campaign finance laws on corruption in the negative binomial regression or the ordinary least squares regression, and we observe no significant decrease in corruption using a Tobit model. Finally, our analysis of time trends does not support the contention that reforms reduce

corruption, although there is some weak evidence that isolated episodes of corruption lead to the adoption of state reforms.

Nor is it the case that corruption data are simply too noisy to observe any systematic relationships. For example, several of our control variables have significant coefficients. Likewise, other studies find significant relationships between state-level corruption rates and other types of state institutions (Cordis 2009; Cordis and Warren 2012).

Consequently, we find no strong or consistent evidence that state campaign finance reform reduces public corruption. This finding is true regardless of whether the reform in question is a limit on corporate or individual contributions or some form of public financing. This finding is robust to a number of different specifications of the estimation model. In fact, campaign finance reforms are often positively correlated with corruption, and the only marginally significant estimate we observe across all our models suggests that gubernatorial public financing is associated with higher corruption rates. However, this anomalous result may exist because states that adopt gubernatorial public financing appear to have slightly higher corruption rates both before and after such reforms (as figure 8 shows).

Failure to reject a null hypothesis does not prove “no effect.” And given the size of the standard errors on many of our estimates, we cannot rule out the possibility that campaign finance laws may have some modest effect of reducing corruption. However, reforms could also have the opposite impact and instead increase corruption. These nonresults are substantively important because the courts effectively have placed the burden of proof on the government to show that campaign finance regulations reduce corruption.

Our findings are consistent with other research that demonstrates an absence of any treatment effect of state campaign finance regulations on public trust and confidence in state

government (Milyo 2012). While these results may not surprise scholars of American politics, they are at odds with the popular wisdom that many politicians, reform advocates, and media pundits espouse.<sup>16</sup> Further, the apparent impotence of campaign finance regulations in ameliorating the “actuality or appearance of corruption” has dramatic implications for the longstanding legal rationale for all existing campaign finance regulations. Heretofore, many judges and legislators have considered it self-evident that restrictive campaign finance regulations are a prophylactic for public corruption. We find that this presumption is baseless.

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<sup>16</sup> A recent *New York Times* news analysis of the effects of *Citizens United* nicely demonstrates the disconnect between the views of scholars of American politics and others. See David Kirkpatrick, “Does Corporate Money Lead to Political Corruption?” *New York Times*, January 21, 2010, <http://www.nytimes.com/2010/01/24/weekinreview/24kirkpatrick.html>.

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**Table 1. Public Corruption among State Officials, 1986–2010**

	N	Median	Mean	Standard Deviation
Convictions	1,250	0.00	1.12	2.37
Filings	1,250	0.00	1.27	3.10
Convictions per 10,000 FTEs	1,250	0.00	0.13	0.25
Filings per 10,000 FTEs	1,250	0.00	0.14	0.31

**Table 2. State Campaign Finance Regulations**

	1980	1990	2000	2010	Changes 1980– 2010	Changes 1986– 2010
States with contribution limits						
Corporate	35	37	44	44	15	14
Individual	25	28	36	36	17	16
States with public funding						
Gubernatorial	6	7	13	13	11	9
Legislative	4	3	6	7	7	5
Average CFR index	1.40	1.50	1.98	2.00		

Notes: CFR index is the sum of the indicators for each type of law present in a state. Changes include instances of repeals as well as the adoption of campaign finance regulations.

**Table 3. Descriptive Statistics for Control Variables (1986–2010;  $n = 1,250$ )**

	Mean	Standard Deviation
Time-varying controls:		
% Black	10.0	9.4
% Other race	6.0	9.6
% Hispanic	7.3	8.7
% Under age 18	25.6	2.3
% Age 65 and over	12.6	2.0
% High school degree	81.8	6.5
% College degree	23.4	5.2
% Poverty	12.9	3.7
% Union	13.1	6.0
Unemployment rate	5.5	1.8
Log (population)	15.0	1.0
Log (real per capita state expenditures)	8.5	0.3
Log (state government FTEs)	11.0	0.8
Log (real per capita income)	10.4	0.2
Republican X (unified control of state government)	0.18	0.39
Democrat X (unified control of state government)	0.24	0.43
Legislative term limits	0.26	0.44
FOIA index	6.0	2.5
Conviction rate per 10,000 federal FTEs (smoothed)	1.6	2.4
Time-invariant controls:		
Ballot measure state	0.48	0.50
Appointed judges	0.54	0.50
Partisan elected judges	0.16	0.37

Notes: All data on state demographics, government FTEs, and state expenditures are from the US Census; the state unemployment rate is from the Bureau of Labor Statistics. Data on legislative term limits are from the National Council of State Legislatures, ballot measures from the Initiative and Referendum Institute at USC, and judicial selection from the American Judicature Society. We constructed indicators for partisan control of state government from the archive of state data created by Carl Klarner at Indiana State University (<http://www.indstate.edu/polisci/klarnerpolitics.htm>).

**Table 4. Conditional Fixed Effect Negative Binomial Regression**

Corruption Convictions of State Officials, 1986–2010						
	(1)	(2)	(3)	(4)	(5)	(6)
Campaign finance regulation index	1.06 (0.09)					
Limits on corporate contributions		1.08 (0.32)	1.05 (0.22)			
Limits on individual contributions		0.98 (0.24)		1.02 (0.18)		
Gubernatorial public financing		1.50 (0.44)			1.35 (0.33)	
Legislative public financing		0.77 (0.32)				1.07 (0.36)
Log of state government FTEs	1.48 (1.09)	1.39 (1.04)	1.47 (1.09)	1.49 (1.10)	1.52 (1.12)	1.52 (1.13)
Federal conviction rate (smoothed)	1.25*** (0.06)	1.25*** (0.05)	1.25*** (0.06)	1.25*** (0.06)	1.24*** (0.06)	1.25*** (0.06)
Log likelihood	-1280.5	-1279.8	-1280.7	-1280.7	-1280.0	-1280.1

Notes: \*\*\*  $p < .01$ , \*\*  $p < .05$ , and \*  $p < .10$ ; standard errors are in parentheses. Coefficient estimates are incidence rate ratios, so a coefficient equals one under the null hypothesis. All models include controls for year and all state characteristics listed in table 3.

**Table 5. Tobit with Random Effects**

Corruption Conviction Rate per 10,000 State FTEs, 1986–2010						
	(1)	(2)	(3)	(4)	(5)	(6)
Campaign finance regulation index	0.03 (0.02)					
Limits on corporate contributions		-0.08 (0.08)	-0.00 (0.06)			
Limits on individual contributions		0.10* (0.06)		0.07 (0.05)		
Gubernatorial public financing		0.07 (0.07)			0.07 (0.06)	
Legislative public financing		0.00 (0.10)				0.05 (0.08)
Log of state government FTEs	-0.08 (0.19)	-0.06 (0.19)	-0.12 (0.19)	-0.08 (0.18)	-0.11 (0.19)	-0.10 (0.19)
Federal conviction rate (smoothed)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)
Log likelihood	-625.5	-624.2	-626.2	-625.3	-625.5	-626.1

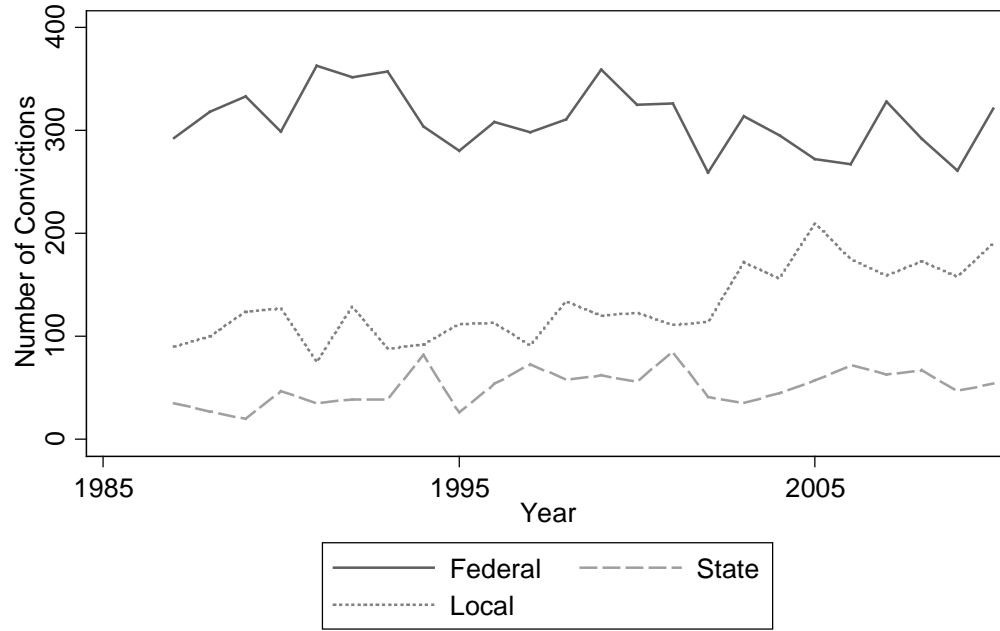
Notes: \*\*\*  $p < .01$ , \*\*  $p < .05$ , and \*  $p < .10$ ; standard errors in parentheses are adjusted for clustering within state. All models include controls for year and all state characteristics listed in table 3.

**Table 6. Ordinary Least Squares Regression with State Fixed Effects**

	Corruption Conviction Rate per 10,000 State FTEs, 1986–2010					
	(1)	(2)	(3)	(4)	(5)	(6)
Campaign finance regulation index	-0.00 (0.01)					
Limits on corporate contributions		-0.09 (0.06)	-0.03 (0.03)			
Limits on individual contributions		0.07 (0.05)		0.00 (0.03)		
Gubernatorial public financing		0.01 (0.05)			0.01 (0.04)	
Legislative public financing		0.02 (0.06)				0.01 (0.04)
Log of state government FTEs	0.04 (0.14)	0.05 (0.13)	0.04 (0.14)	0.04 (0.14)	0.03 (0.14)	0.04 (0.14)
Federal conviction rate (smoothed)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)
R <sup>2</sup>	.19	.19	.19	.19	.19	.19

Notes: \*\*\*  $p < .01$ , \*\*  $p < .05$ , and \*  $p < .10$ , standard errors in parentheses are adjusted for clustering within state. All models include controls for year and state, as well as all time-varying state characteristics listed in table 3.

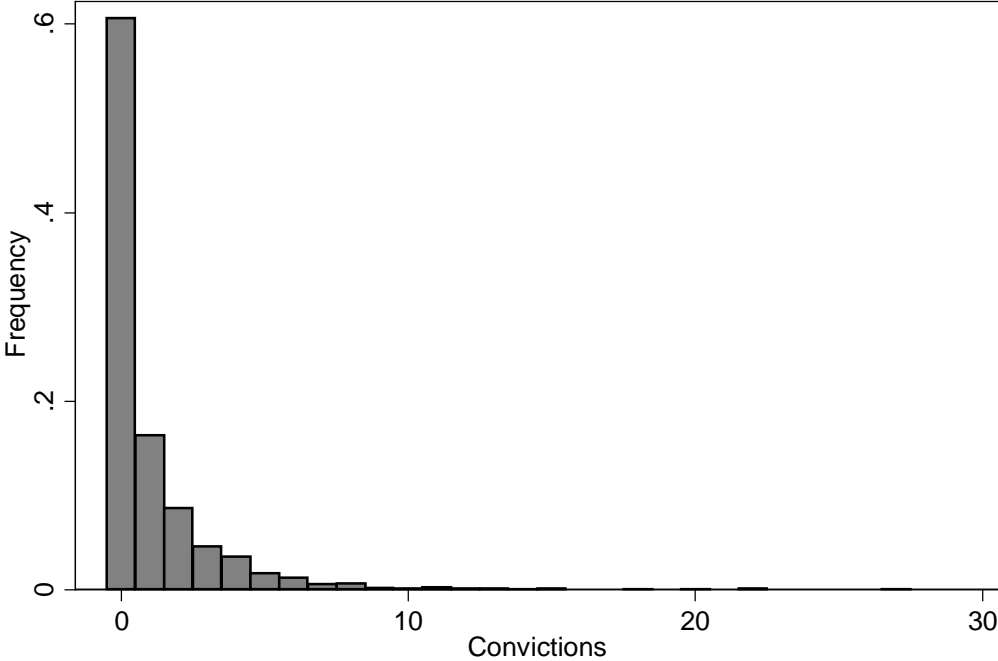
**Figure 1. Public Corruption by Level of Government**



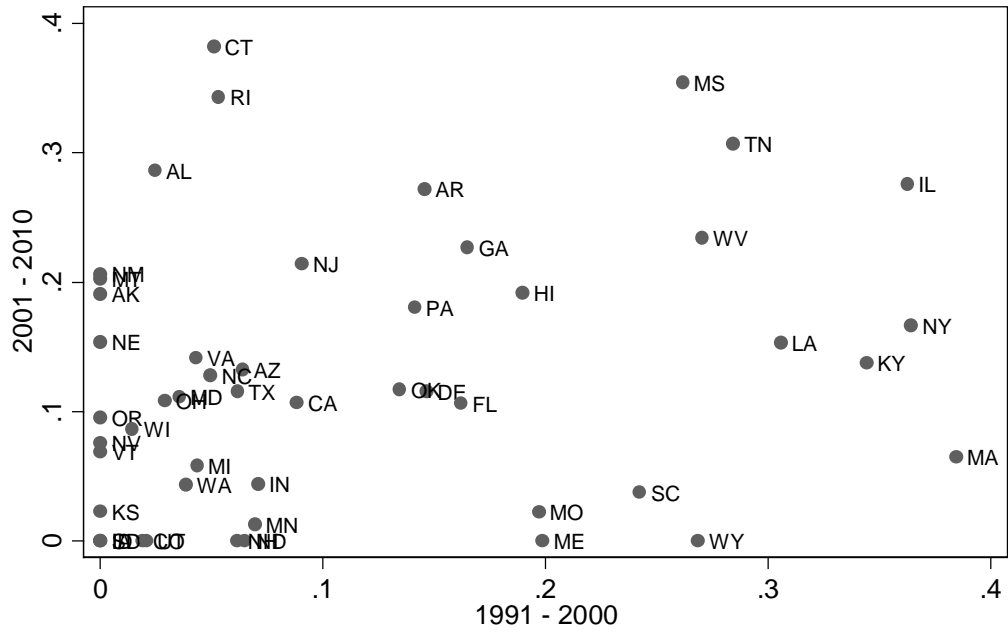
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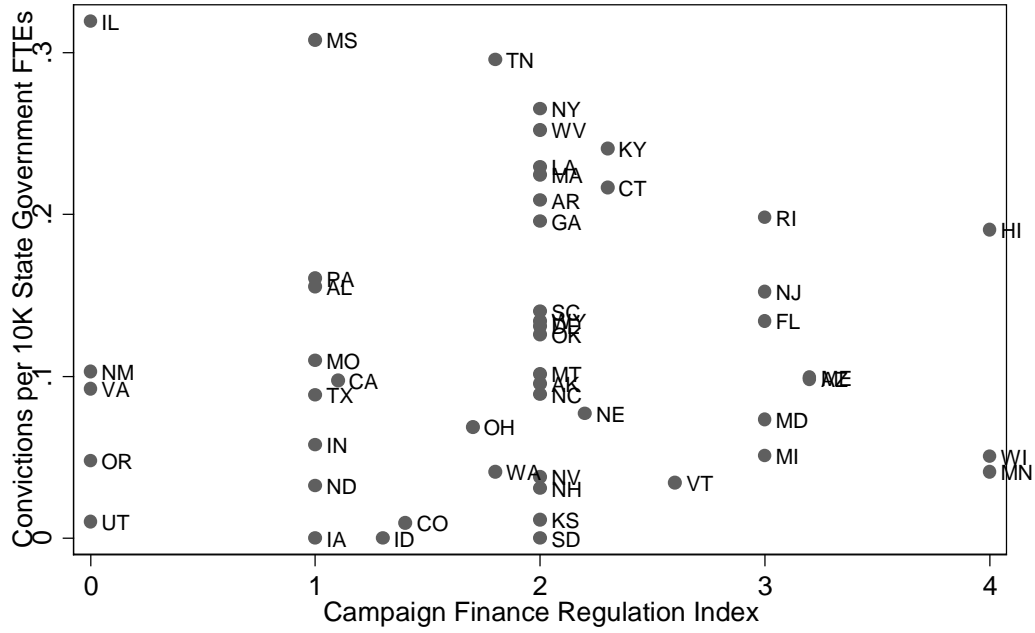
**Figure 2. Frequency Distribution of Convictions of State Officials**



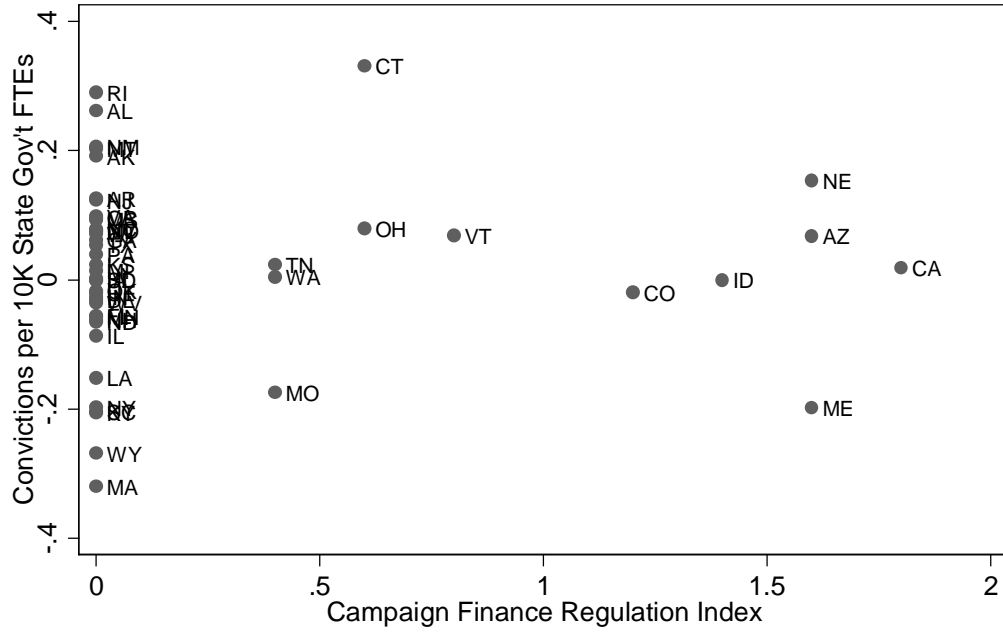
**Figure 3. Public Corruption in the States: State Officials Only, Average Annual Convictions per 10,000 State Government FTEs**



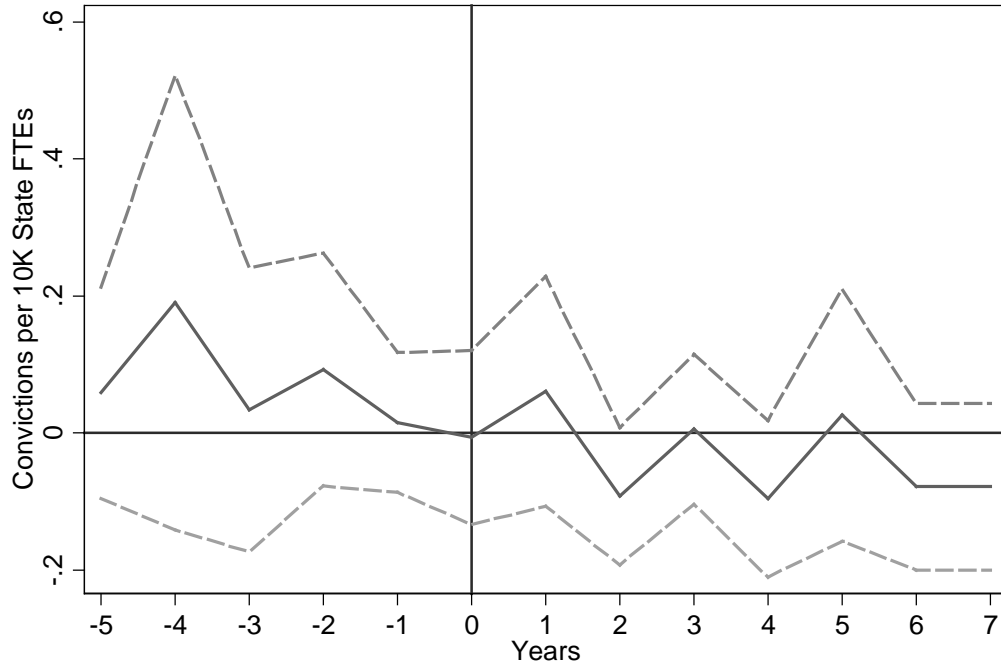
**Figure 4. Campaign Finance Regulations and Corruption: State Officials, Annual Averages, 1991–2010**



**Figure 5. Campaign Finance Reform and Corruption: State Officials, Difference in Annual Averages, 1991–2000 to 2001–2010**

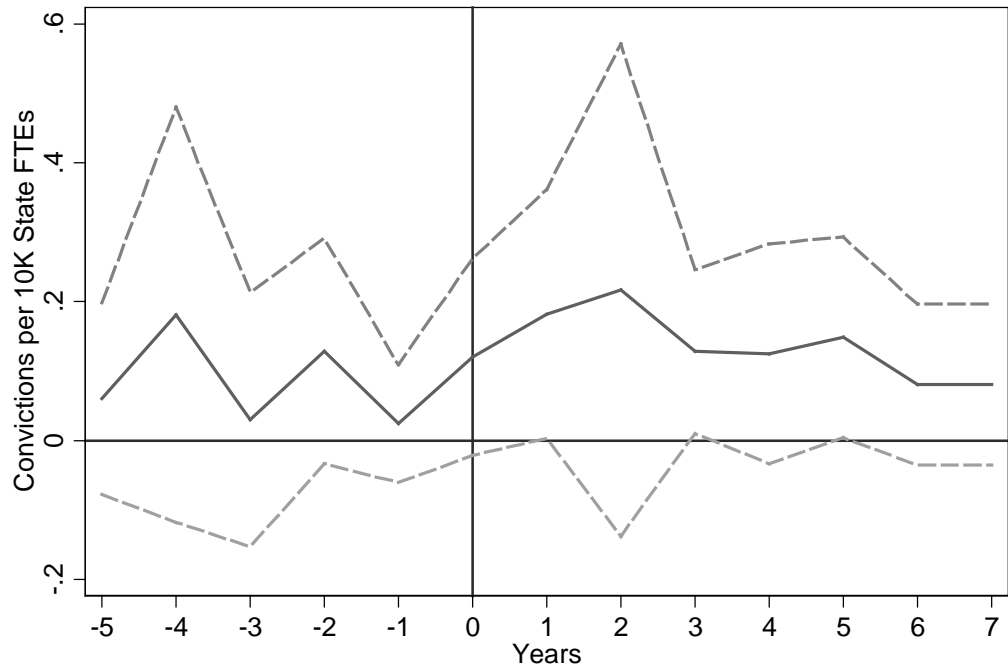


**Figure 6. Corporate Contribution Limits**



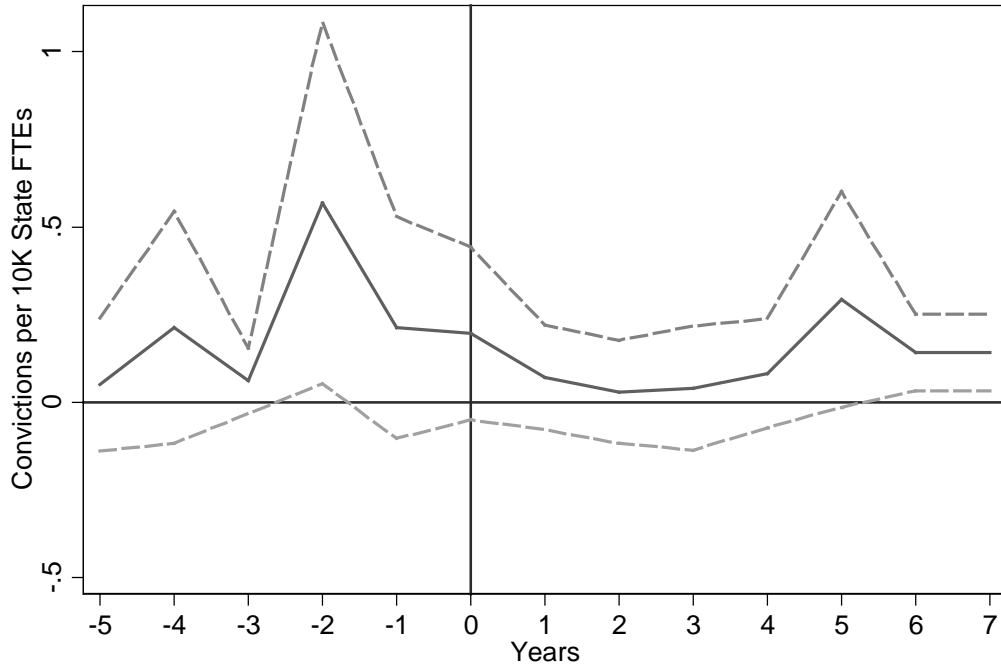
Notes: Based on ordinary least squares regression of convictions per 10,000 state government FTEs on indicators for campaign finance laws, year and state-fixed effects, and all time-varying state characteristics listed in table 3 (standard errors adjusted for clustering within state). The plot shows coefficient estimates and 95 percent confidence interval for time indicators from  $t - 5$  to  $t + 5$  (and a common indicator for  $t + 6$  and onward), where  $t = 0$  coincides with the implementation of corporate contribution limits.

**Figure 7. Individual Contribution Limits**



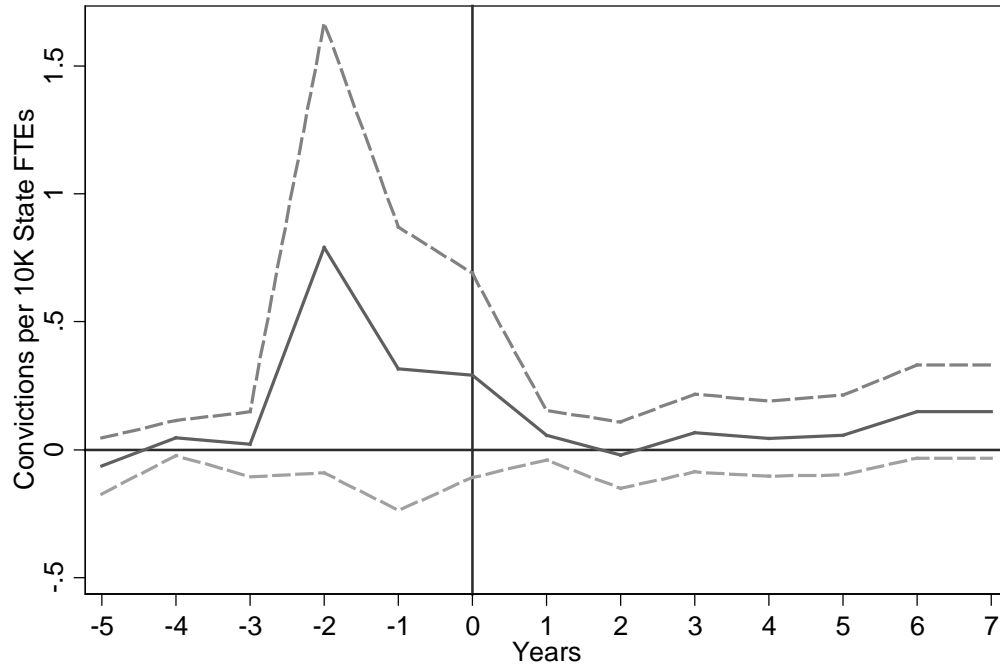
Notes: Based on ordinary least squares regression of convictions per 10,000 state government FTEs on indicators for campaign finance laws, year and state-fixed effects, and all other state characteristics listed in table 3 (standard errors adjusted for clustering within state). The plot shows coefficient estimates and 95 percent confidence interval for time indicators from  $t - 5$  to  $t + 5$  (and a common indicator for  $t + 6$  and onward), where  $t = 0$  coincides with the implementation of individual contribution limits.

**Figure 8. Gubernatorial Public Financing**



Notes: Based on ordinary least squares regression of convictions per 10,000 state government FTEs on indicators for campaign finance laws, year and state-fixed effects, and all other state characteristics listed in table 3 (standard errors adjusted for clustering within state). The plot shows coefficient estimates and 95 percent confidence interval for time indicators from  $t - 5$  to  $t + 5$  (and a common indicator for  $t + 6$  and onward), where  $t = 0$  coincides with the implementation of gubernatorial public financing.

**Figure 9. Legislative Public Financing**



Notes: Based on ordinary least squares regression of convictions per 10,000 state government FTEs on indicators for campaign finance laws, year and state-fixed effects, and all other state characteristics listed in table 3 (standard errors adjusted for clustering within state). The plot shows coefficient estimates and 95 percent confidence interval for time indicators from  $t - 5$  to  $t + 5$  (and a common indicator for  $t + 6$  and onward), where  $t = 0$  coincides with the implementation of legislative public financing.