

Do State Campaign Finance Reforms Reduce Public Corruption?

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Abstract:

The Supreme Court has long held that campaign finance regulations are permissible for the purpose of 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.

Do State Campaign Finance Reforms Reduce Public Corruption?

Adriana Cordis and Jeff Milyo*

1. 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 the financing of political campaigns must be narrowly tailored for the purpose of preventing “the actuality and appearance of corruption.”¹ This principle has been the basis for several court decisions that have reigned in the scope of state and federal campaign finance regulations over the last 35 years.² 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.³ Yet, despite this continual and intense focus on campaign finance reform as anti-

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¹ *Buckley v. Valeo*, 424 U.S. 1 (1976).

² Recent examples include: *Randall v. Sorrell*, 548 U.S. 230 (2006), striking low contribution limits in Vermont; *Davis v. FEC* (2008), striking differential contribution limits for candidates with self-financing opponents; *Citizens United v. FEC*, 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 *Citizens United* application to the states.

³ Examples of such claims are documented in Primo and Milyo 2006. 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 striking “a blow to the heart of democracy” (“The Court’s Blow to Democracy,” 2010). In a subsequent weekly radio address, President Obama called for campaign finance reform, declaring: “what is at

corruption policy, there has been very little effort by scholars to evaluate whether campaign finance reforms actually reduce corruption or the appearance of corruption.

One explanation for the absence of systematic research on the campaign finance reforms and corruption is that reforms themselves may be symptomatic of past corruption (Witko 2007). Moreover, it is difficult to disentangle the impact of reforms at the federal level from other factors which may change coincidentally over time. But as demonstrated in Primo and Milyo (2006) and Milyo (2012), there is substantial variation in state campaign finance laws 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 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 1986-2010. This approach also 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

stake is no less than the integrity of our democracy” (Obama 2010). 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” at: <http://www.stateintegrity.org/> (last viewed July 13, 2012).

episodes of reform. Finally, we also 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).⁴ Whereas most previous studies of corruption cannot distinguish between federal, state or local government officials, the TRACfed database permits us to focus on public corruption by state officials. Of course, the true corruption rate is unobserved by the very nature of corrupt activities, so convictions are at best a proxy for public corruption. Of particular concern is the possibility that prosecutors are themselves tainted by local corruption and turn a blind eye to wrong doing by government officials. A second concern is that legal standards and anti-corruption efforts may vary across jurisdictions. However, we observe that nearly all public corruption cases are prosecuted by federal district attorneys, who should be fairly insulated from local politics. This also ensures that prosecutions are pursued 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% 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.

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

We then conduct several different types of 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 includes 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.

2. 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 competitiveness of elections, 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, the fundamental issue of whether campaign finance laws reduce corruption has been largely ignored.

2.1 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 be expected to have a dramatic impact on political corruption. However, the popular view of money in politics is not well supported by scientific research. 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 the electoral success of candidates (e.g., Levitt 1994; Gerber 1998; Milyo and Groseclose 1999; and 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 surprising little money flowing into American politics precisely because contributions do not appear to buy political favors (e.g., Milyo, Primo and Groseclose 2000 and Ansolabehere, de Figueiredo and Snyder 2003). And finally, rather than alienate potential voters, campaign spending has long been known to be 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 reforms should have much impact on political corruption, ---- unless, that is, one defines corruption by the presence of money in politics. As noted in Milyo (2012), 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. Instead, it is criminal acts such as embezzlement, misappropriation of funds, bribery and influence peddling that constitute political corruption in the view of the Court.

Of course, 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 been consistent in requiring an explicit quid pro quo in order for such exchanges to rise to the level of bribery or influence peddling. Consequently, the great bulk of the scholarly

literature on money in American politics really doesn't directly address the issue of whether campaign finance regulations satisfy the anti-corruption rationale demanded by the courts.

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, 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.⁵ 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 (hence not "narrowly tailored toward that end").⁶ It is for a similar reason that self-financing by candidates cannot be regulated; it is not possible for a candidate to corrupt themselves with their own money. Likewise, state governments cannot limit the financing of initiatives and ballot measures, since the text of any such proposition is uncorrupted by spending for against the proposition.

Similarly, in the recent and controversial *Citizen United* decision, the Court affirmed that campaign expenditures made independent of any candidate are a kind of "safe-harbor", so that there is no anti-corruption rationale for federal or state governments to prohibit corporations, unions or other organizations from engaging in independent expenditures.⁷ And most recently, the Court re-affirmed that independent expenditures "do not give rise to corruption or the

⁵ *Buckley v. Valeo*, 424 U.S. 1 (1976).

⁶ *Randall v. Sorrell*, 548 U.S. 230 (2006).

⁷ *Citizens United v. FEC*, 130 S. Ct. 876 (2010).

appearance of corruption.”⁸ Finally, the Court has invalidated attempts at “leveling the playing field” between candidates that accept limited public funding for their campaigns and potential high-spending opponents. Again, this is 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.⁹

2.2 Campaign Finance Reform and Political Corruption

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

Even so, only a handful of 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; however, 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 and 2008; and 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 is only

⁸ *American Tradition Partnership v. Bullock*, 567 U. S. ____ (2012).

⁹ *Davis v. FEC* (2008); and *Arizona Free Enterprise Club’s Freedom Club PAC et al. v. Bennett*, 131 S. Ct. 2806 (2011).

available for a single year, so these authors are also unable to identify within-state effects of campaign finance reforms.¹⁰

Apart from this, the corruption measures employed in these studies 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 of data on state-level public corruption employed by social scientists.¹¹ The PIN data is based on a survey of federal prosecutors, so it contains some misreports and subjectivity in classifying cases (Boylan and Long 2003). Further, state officials account for less than 10% 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, in progress).

The shortcomings in the PIN survey data were the motivation for 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 small number of journalists. Further, while statehouse reporters may have expertise in their own state political environment, it is less plausible that they would have much expertise in making comparisons to other states.¹²

¹⁰ 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. This is the only study of which we are aware that investigates the connection between campaign finance laws and corruption across countries.

¹¹ For example, Adser et al. (2003), Alt and Lassen (2008), Cordis (2009), Dincer et al. (2010), Fisman and Gatti (2002), Glaeser and Saks (2006), Goel and Nelson (2011), Hill (2003), Johnson et al. (2011), Leeson and Sobel (2008), Maxwell and Winters (2005), Meier and Holbrook (1992), Meier and Schlesinger (2002), and Nice (1983).

¹² 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 (in progress).

Neither Maxwell and Winters (2005) nor Alt and Lassen (2003 and 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 even so, 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, mandatory spending restrictions were rendered unenforceable by the landmark 1976 *Buckley* decision. Consequently, these authors appear to be measuring only the cross-sectional association between voluntary spending restrictions in some states and corruption, absent controls for other prominent and more relevant features of state campaign finance regulatory regimes, such as contribution limits for different types of contributors.¹³

In contrast, Rosenson (2009) undertakes specifically to investigate the question of 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 state. Rosenson also attempts to address the potential endogeneity of reforms by using an instrumental variables estimation procedure. However, this exercise is problematic for two reasons: 1) the first stage regression does not include all exogenous variables in the structural 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.

¹³ Most states with voluntary spending ceilings for candidates offer public financing for candidates that abide by these limits; however, the indicator used by Maxwell and Winters (2005) and Alt and Lassen (2003 and 2008) 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 versus those that also include state legislative candidates.

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. However, 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.

There have been likewise few 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.

3. 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 state campaign finance reforms may themselves be caused by the presence of public corruption. We address this potential endogeneity in three ways. First, we examine the raw data for any long-run relationship between the levels of (or 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 (e.g. Levitt 1994).¹⁴ Finally, we look for trends in state corruption in the years leading up to or just after episodes of campaign finance reform. These methods are fairly standard in the evaluation literature; however, our task is complicated by the challenge of measuring public corruption in the states.

3.1 Measuring Public Corruption in the States

As noted above, most empirical research on public corruption in the U.S. employs convictions data from the Public Integrity Section of the DOJ. However, among other problems, the PIN does not disaggregate state-level conviction data by type of government official, nor do they 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 compiled by the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. TRAC systematically employs the Freedom of Information Act 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 this data, we created annual series of state level public corruption convictions and filings from 1986-2010.

¹⁴ In principle we could use instrumental variable methods to address potential endogeneity, but we are at a loss for credible instruments. Previous studies that consider the determinants of state campaign finance regulations suggest variables like party control of government (Stratmann and Aparacio-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 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 it allows 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 non-federal officials. However, Cordis and Milyo (in progress) collect data on state and local prosecutions of public corruption from media reports; they find that 95% of all corruption cases from 1986-2010 were handled by federal prosecutors. 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.

3.2 Convictions versus Corruption

Of course, any attempt to study illicit activities is plagued by the problem that it is not easy to observe such activities. Convictions are 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). The fact that nearly all corruption cases in the United States are handled by federal prosecutors in federal courts goes a long way to address any concerns about variations in legal standards. Further, because federal prosecutors are not beholden to state politicians, this also mitigates concerns that official 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. Not all corruption can be demonstrated 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 of our statistical analyses using both convictions and filings (and with and without controls for prosecutorial effort).

3.3 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 1986-2011, 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.

In order to identify the effect of changes in state campaign finance laws, it is therefore necessary to take some account of 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 the course of 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 and so on. We also check for delayed effects by pooling our annual data into five non-overlapping five-year waves; this 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 of our models using prosecutorial filings as the dependent variable.

3.4 Public Corruption as a Limited Dependent Variable

In order to compare corruption convictions and filings across states, we normalize these 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). In Table 1, we provide descriptive statistics for these four dependent variables: corruption convictions and prosecutorial filings, and the corruption and filing rates.

For all of these measures, most state-year observations are zero (60% for convictions). The histogram for convictions is shown in Figure 2; it is apparent that corruption convictions are rare and idiosyncratic events. Given this, 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 is 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, 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 indicated by Figure 3, 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 is conducted using annual observations, we also check the robustness of our results by also estimating our models using five non-overlapping waves of five-year corruption rates.

3.5 Panel Data Methods

In our multivariate statistical models, we analyze a repeated cross-section of state-level observations; this allows us to take advantage of panel estimation methods. Accordingly, we estimate conditional fixed effects negative binomial regressions and random effects tobits; however, neither of these estimators permits consistent estimation of true fixed effects.¹⁵ For this reason, we also analyze ordinary least squares models for conviction rates in the states,

¹⁵ 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.

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 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 et al. 2007 and Bertrand et al. 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 eleven 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% confidence interval for these indicators. This allows us to easily observe any delayed impacts of reform, as well as evidence of “reverse causality” from episodes of corruption to reform.

3.6 State Campaign Finance Regulations

All data on state campaign finance laws are taken from Milyo (2012), who in turn relied on several sources, including the National Council of State Legislatures, state government websites and the Federal Election Commission. As noted in Milyo (2012) and Primo and Milyo (2006), state campaign finance regulatory regimes fall into five broad and nested types: i) no contribution limits, ii) limits on corporate contributions to candidates, iii) limits on corporate and individual contributions to candidates, iv) contributions limits and public funding of gubernatorial elections, and v) contributions limits and public funding of gubernatorial and state legislative elections. Therefore, we create a campaign finance index (CFR) 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 describes 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 adopt campaign finance regulations over time (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, maximum contribution limits vary somewhat among states with contribution limits; however, 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 pre-determined cap). However, while Arizona and Maine implemented these so-called “clean money” reforms in the 2000 election cycle, 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.¹⁶

3.7 Campaign Finance Reform and Public Corruption: A First Look

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

Of course, it is still 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, which compares the *change* in the average campaign finance index from the 1990's to the 2000's 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.

¹⁶ We obtained data on state regulations limiting independent expenditures from Klump, Mialon and Williams (2012).

However, it may be the case that some important determinants of state corruption are 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.

3.8 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 of these models include year indicators, but only the ordinary least squares model also includes indicators for each state. Descriptive statistics for all other control variables are provided in Table 3. Given the extensive list of control variables, we will estimate all of our models with and without these controls; this assures 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, 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 above, the negative binomial and tobit models do not permit the inclusion of state fixed effects as control variables. This raises the concern that our findings in these models may be artifacts of excluded state characteristics that are spuriously correlated with both state campaign finance regulations and public corruption rates. To guard against this, we include controls in these models for both the judicial selection process and whether the state permits direct legislation via ballot initiatives, as these are two prominent institutions that may lead to such confounding.

4. 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.

4.1 Negative Binomial Estimates

For now, we focus on the limited nature of the dependent variable and put aside concerns about simultaneity not addressed by our control variables. In Table 4 we report the

estimated 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 of 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.

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% *increase* in corruption convictions, although this effect is not statistically significant. Further, given 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% 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 are statistically significant, nor are they jointly significant. The estimated coefficients for public financing are more sizable, but they 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 are statistically significant. Further, all of 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% increase in the number of state officials convicted. In addition, several of our other control variables have a significant effect, as well (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.

4.2 Tobit Estimates

In contrast to the count model above, we examine corruption rates in this section, and 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 of 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 of the models in Table 5 are otherwise identical to those in Table 4.

Starting in column 1, as in the previous exercise, the index of state campaign finance regulations is positively associated with corruption convictions, albeit 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 are 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% of the mean conviction rate and 40% of the standard deviation. However, the campaign finance indicators in model 2 are also not jointly significant.

As before, we do 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.

4.3 Ordinary Least Squares

We now explicitly address remaining concerns about state-specific confounding variables by examining a difference-in-difference model. In Table 6, we report the estimated coefficients and standard errors for selected independent variables of interest from ordinary

least squares regression with state fixed effects. All of 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; the 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). However, it should be noted that the effect of corporate contribution limits in model 2 is at least negative; the point estimate suggests that such limits reduce the conviction rate by about 70% of the mean and 36% of the standard deviation. This magnitude is similar to what was observed in the Tobit analysis, but again, in neither case are these effects statistically different than 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 ($p < .01$).

4.4 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 of 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 of

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 failure to observe any significant reduction in corruption convictions is not driven by the particular set of control variables that we employ.

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 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 implemented in 2000 by Arizona and Maine; in no case are these additional variables individually or jointly significant.

We also re-estimated all of 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-estimate all of our models after pooling the data into five non-overlapping 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. But once again, we find no case in which state campaign finance laws are individually or jointly significant.

4.5 Time Trends Before and After Reform

To this point, the only manner in which we have addressed the potential endogeneity of state campaign finance laws is through the use multiple control variables and, in the ordinary least squares model, state-level fixed effects. As noted above, we are not sanguine about the validity of the instrumental variables proposed by Rosenson (2009); therefore, we let the data speak for itself 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 7).

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. Each type of state law is examined 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 7, 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.

The results of this exercise are reported in Figures 6-9; 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, we show the time path for conviction rates in states that implement limits on corporate contributions. The estimated trend in convictions is shown in the solid line and the dotted lines indicate the 95% confidence interval. The fact that the confidence intervals always bound zero indicates that none of the leading or lagging indicators is statistically significant (nor can we reject the null hypothesis that all of 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 tells a similar story for individual contribution limits. However, Figures 8 and 9 weakly suggest that episodes of corruption convictions lead to the adoption of public financing. However, 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.

5. 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 addressing 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 that no strong or consistent evidence that state campaign finance reform reduces public corruption. This 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 significantly estimate we observe across all of our models suggests that gubernatorial public financing is associated with higher corruption rates. However, this anomalous result may be attributable to the fact that states that adopt gubernatorial public financing appear to have slightly higher corruption rates both before and after such reforms (as seen in Figure 8).

Of course, 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, it is equi-likely that reforms have the opposite impact and instead increase corruption. These non-results are substantively important because the Courts effectively have placed the burden of proof on 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). And while these results may be unsurprising to scholars of American politics, they are wildly at odds with the popular wisdom espoused by many politicians, reform advocates and media pundits.¹⁷ Beyond this, 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 demonstrate that this presumption is baseless.

¹⁷ The disconnect between the views of scholars of American politics and others is nicely demonstrated in a recent *New York Times* news analysis of the effects of *Citizens United* (Kirkpatrick 2010).

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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 10K FTEs	1,250	0.00	0.13	0.25
Filings per 10K 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*Unified Control of State Government	.18	.39
Democrat*Unified Control of State Government	.24	.43
Legislative Term Limits	.26	.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	.48	.50
Appointed judges	.54	.50
Partisan elected judges	.16	.37

Notes: All data on state demographics, government FTE's and state expenditures are from the U.S 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. Indicators for partisan control of state government indicators were constructed 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 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 ²	.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:

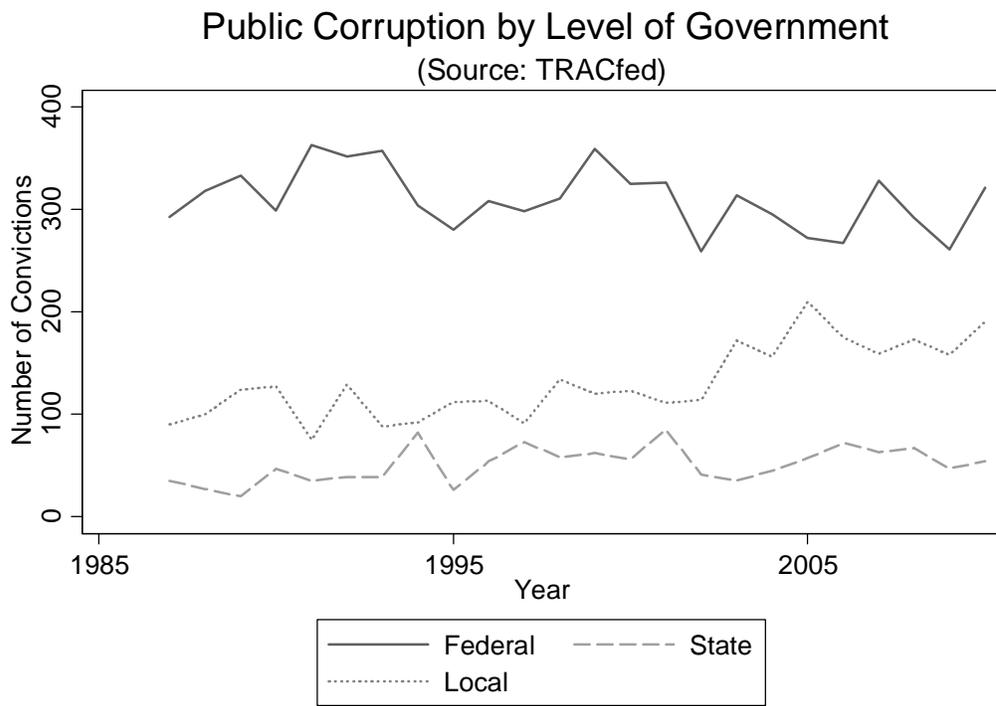


Figure 2:

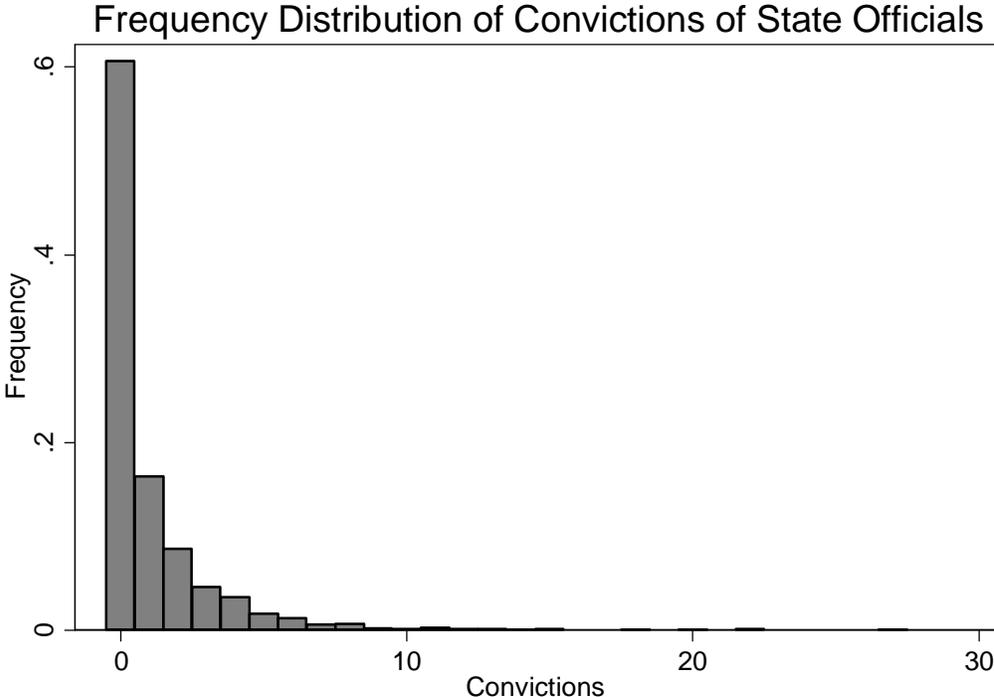


Figure 3:

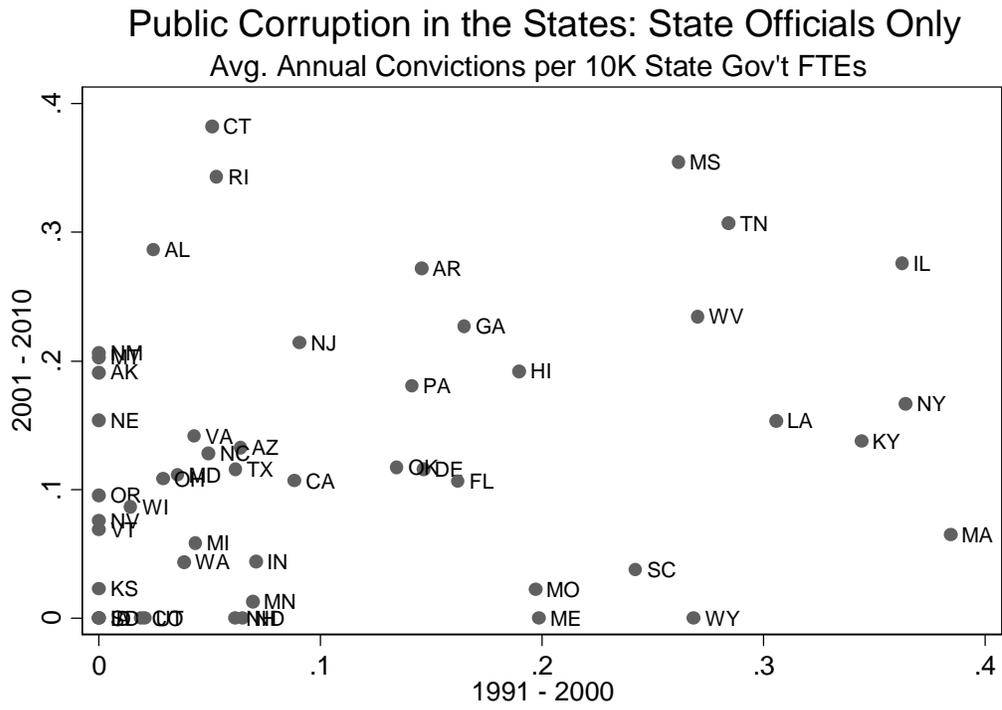


Figure 4:

Campaign Finance Regulations and Corruption: State Officials

Annual Averages from 1991-2010

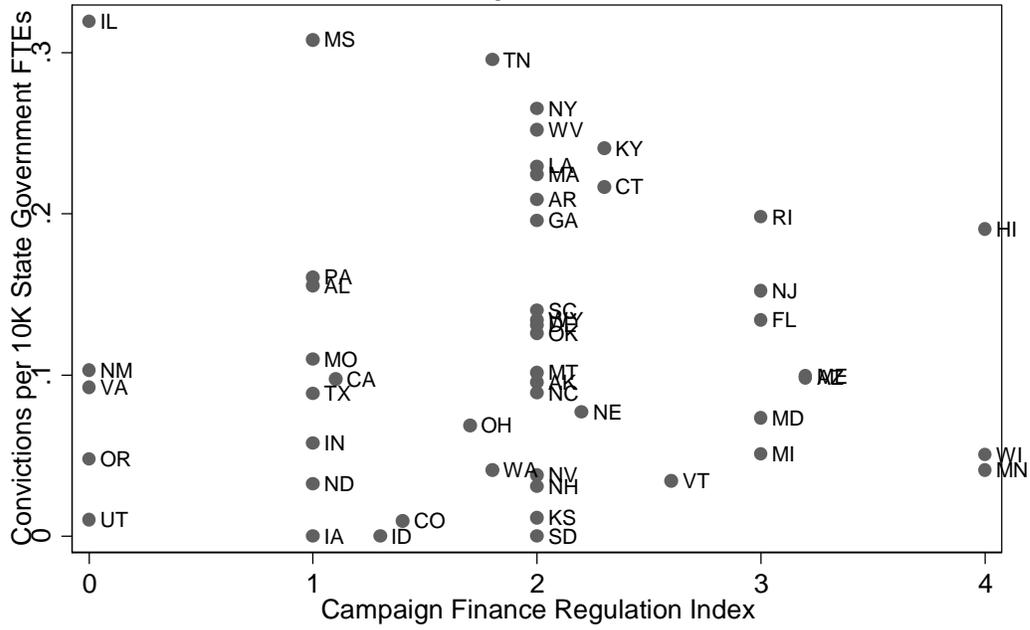


Figure 5:

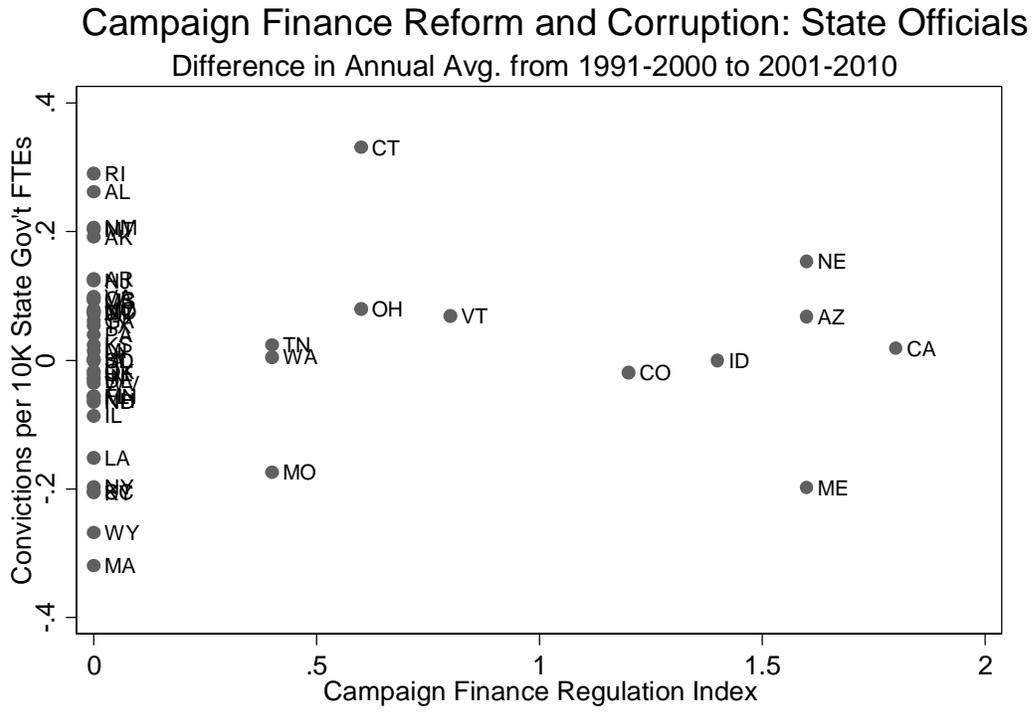
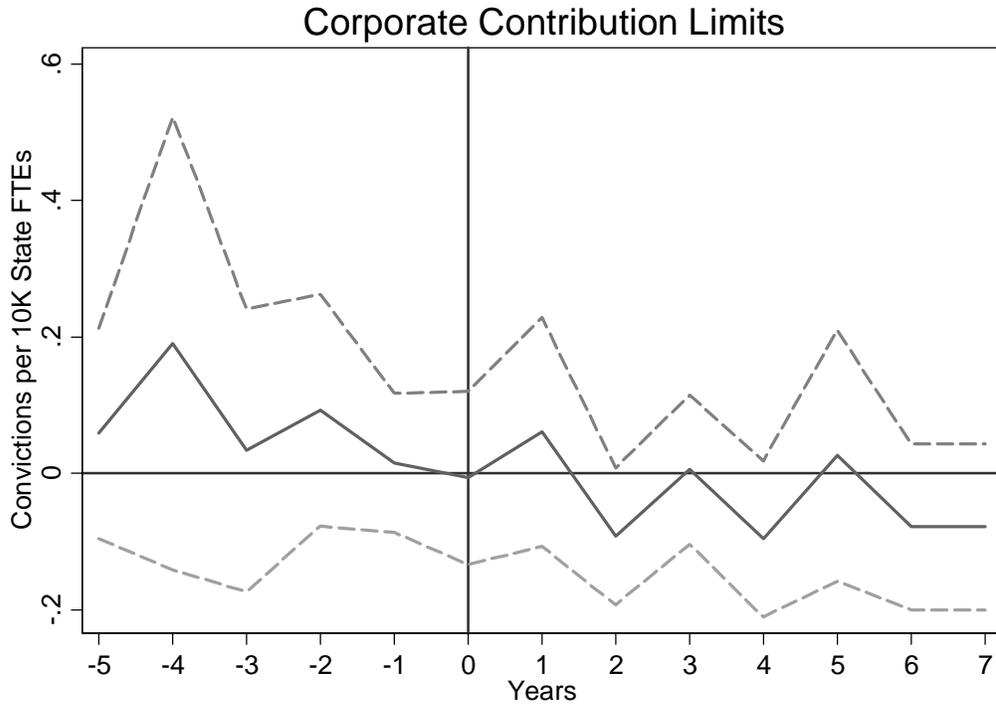
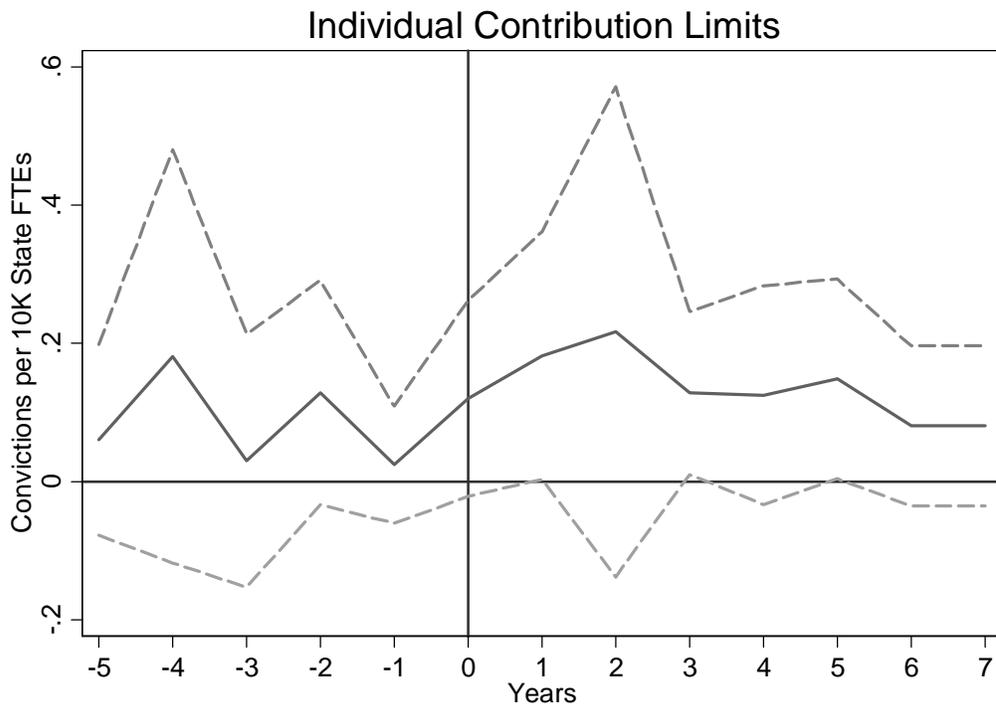


Figure 6:



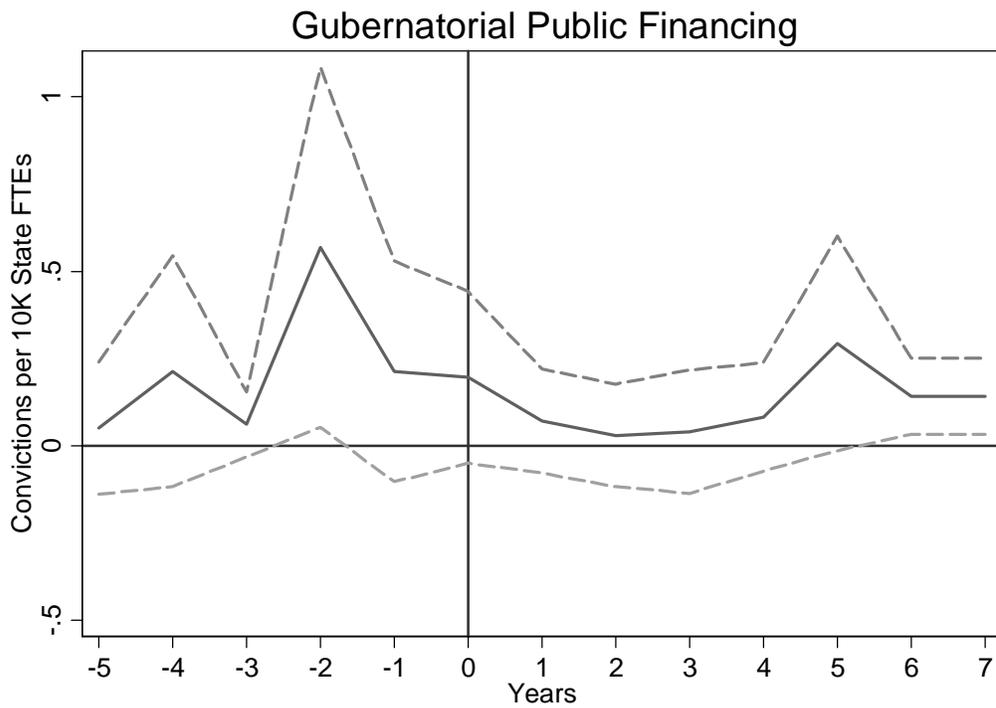
Notes: Based on ordinary least squares regression of convictions per 10,000 state government FTEs on indicators for campaign finance laws, year and state-fixed effects and all time-varying state characteristics listed in Table 3 (standard errors adjusted for clustering within state). The plot shows coefficient estimates and 95% 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:



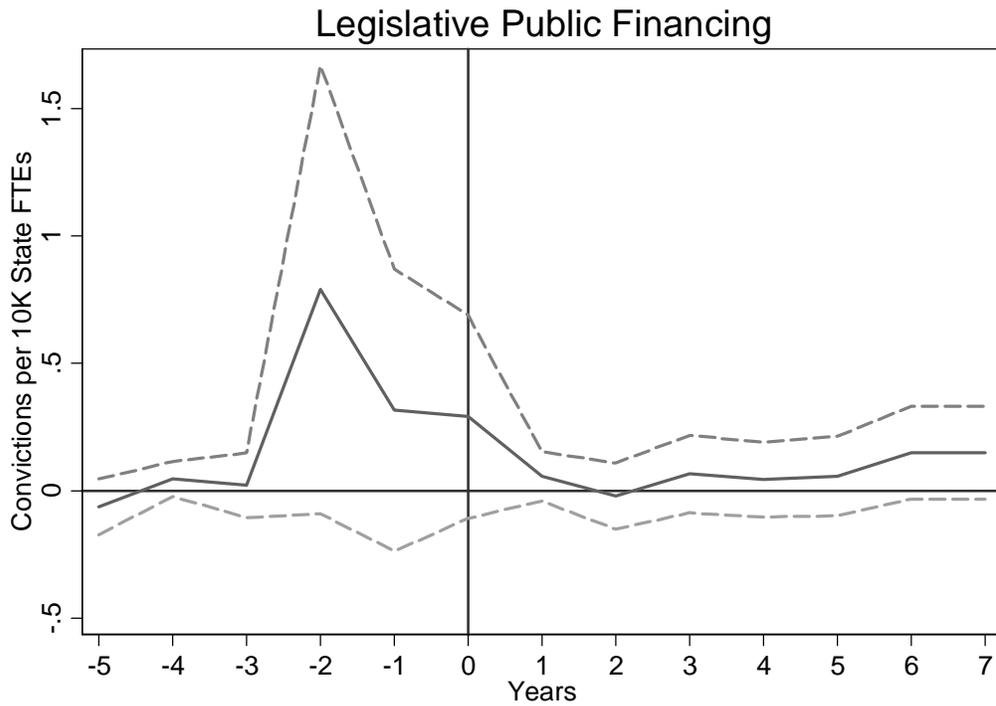
Notes: Based on ordinary least squares regression of convictions per 10,000 state government FTEs on indicators for campaign finance laws, year and state-fixed effects and all other state characteristics listed in Table 3 (standard errors adjusted for clustering within state). The plot shows coefficient estimates and 95% 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:



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% 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:



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% 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.