

**The Effects of Campaign Finance Spending Bans on Electoral Outcomes:
Evidence From the States about the Potential Impact of *Citizens United v. FEC***

Raymond J. La Raja
University of Massachusetts, Amherst
laraja@polsci.umass.edu

Brian F. Schaffner
University of Massachusetts, Amherst
schaffne@polsci.umass.edu

This paper seeks to understand the effect of campaign finance laws on electoral outcomes. Spurred by the recent Supreme Court decision, *Citizens United v. Federal Election Commission* (2010), which eliminated bans on corporate and union political spending, the study focuses on whether such bans generate electoral outcomes that are notably different from an electoral system that lacks such bans. We look to two key electoral dynamics that such bans might influence: the partisan balance of power and the success of incumbents. Using historical data on regulations in 49 American states between 1968 and 2009 we test alternative models for evaluating the impact of corporate spending bans put in place during this period. The results indicate that spending bans appear to have limited effects on election outcomes.

Political reformers place a great deal of hope in the expectation that laws preventing corporations from funneling large sums of money into politics will curtail their influence in government. A recent landmark decision, *Citizens United v. Federal Elections Commission (FEC)* (2010), which allows both corporations and unions to spend unlimited funds on electioneering, raises the specter that such groups will now have more leverage than ever to shape the nation's politics and policy.¹ The reaction to this ruling was swift and chiefly alarmed about the prospect of amplified political influence by corporations. The editors of the *New York Times* echoed hundreds of news editorials throughout the nation when they called *Citizens United* a “radical decision, which strikes at the heart of democracy” (New York Times 2010). The chief concern of critics is that absent the restrictions on campaign spending, corporate wealth will distort American politics (Dworkin 2010; Hasen 2010).

While concerns over the potential effects of nullifying spending bans are understandable, such concerns also seem to be based on the assumption that campaign finance restrictions are relatively effective at limiting influence. Yet, we possess remarkably limited knowledge about the efficacy of laws that regulate corporate or union expenditures. The vast majority of research on campaign finance focuses on the effect of *candidate* spending on congressional election outcomes (e.g., Jacobson 1978; 1980; 1990; Krasno and Green 1988). Additionally, scholars tend to focus on the consequences of *contribution* limits to U.S. candidates, but no studies to our knowledge have observed how *spending* restrictions might affect macro political outcomes.

Constraints on political spending by selected groups may have any number of notable outcomes for the conduct of elections and the making of public policy. While all of these potential outcomes merit attention, in this paper, we take the first step of examining whether and to what

¹ See *Citizens United v. Federal Elections Commission* 558 U.S. 310 (2010). The Court ruled that domestic corporations and labor unions may spend unlimited sums to influence elections, except for donating money to candidates and parties.

extent laws aimed at restricting corporate spending affect electoral outcomes.² In other words, we ask a question of fundamental importance for the democratic system: do bans on unlimited corporate spending affect which candidates receive more votes and which party wins more elections? To answer this question, our analysis exploits the fact that prior to the 2010 Court decision, states were given the authority to regulate whether corporations could engage in unlimited campaign spending to influence elections for state offices. Immediately prior to *Citizens United*, 28 states permitted corporations to spend money independently in state elections. Even more important is that many of the states that did have corporate and/or spending bans in place prior to the 2010 decision had enacted those bans during a period for which we have political data. Thus, state-level variation makes it possible to compare outcomes before and after such prohibitions were enacted.

Our approach allows us to contribute to the discipline's broader understanding of the efficacy of campaign finance laws by focusing on rules that constrain election spending by interest groups. Moreover, it addresses directly the likely effects of a highly controversial Supreme Court case, *Citizens United v. FEC*, which potentially affects national, state and local elections. Based on theoretical expectations about partisan preferences of corporations, we expected the presence (or absence) of bans to shape partisan control of legislatures and incumbent electoral advantages. However, we find that spending bans have little or no impact on these outcomes, confirming some previous findings about the marginal impact of campaign finance rules on various political outcomes (see, for example, Ansolabehere, deFigueiredo, and Snyder 2003). The paper concludes with some

² While we initially sought to assess the impact of a ban on labor union spending, the limited variation of campaign finance laws regarding unions makes this effort exceedingly difficult. Only one state, New Hampshire, had a spending ban on unions but not on corporations. In contrast, 9 states had spending bans on corporations but none on unions. The consequence is that we cannot distinguish the independent effect of a spending ban on labor unions from contexts in which a spending ban exists for both labor unions and corporations.

insights for why political spending bans may not have shaped these particular outcomes in the American states, while pointing to reasons why interest groups may continue to spend money in pursuit of certain goals.

Background on Campaign Finance Restrictions on Corporations and Labor Unions

Efforts to restrain the ability of corporations to finance politics began at the turn of the 20th century as progressive reformers sought to curtail the influence of large corporate trusts. Congress passed the Tillman Act of 1907, which barred corporations from making contributions in connection with a federal election. Laws prohibiting labor union contributions came in 1943 under the Smith-Connally Act, which spurred labor unions to innovate by setting up political action committees (PACs) to collect individual contributions from members for political purposes. A few years later, the Taft-Hartley Act of 1947 enshrined the temporary wartime provisions of Smith-Connally and went further, by declaring that both corporations and labor unions could not *spend* funds from their general treasuries for federal electioneering.

The Watergate scandal involving, among other things, slush funds from corporations and wealthy interests for President Nixon's reelection campaign led to a series of amendments in 1974 to the Federal Election Campaign Act (FECA) of 1971. These new rules set strict limits on how much PACs and individuals could contribute to candidates and parties, and underscored existing prohibitions on corporate and union financing of federal elections. However, under many state laws the national party committees could raise corporate and union money – commonly called soft money -- for party-building activity. Congress eventually banned party soft money under the Bipartisan Campaign Reform Act (BCRA) of 2002. This legislation spurred the creation of various political organizations, established under sections 527 and 501(c)4 of the federal tax law, which could accept corporate and union contributions. Under BCRA, these 527 and 501(c)4 organizations

-- financed by wealthy individuals, ideological groups, corporations and unions – were free to advocate for political issues so long as they did not explicitly call for the election or defeat of a federal candidate, or invoke the name of federal candidates in the weeks before an election.

The decision in *Citizens United v FEC* put an end to these restrictions on outside spending in elections, both at the federal and state level. In 2008, a non-profit corporation called Citizens United released a documentary criticizing Hillary Clinton, who was then running for president. When the organization advertised its documentary in broadcast outlets the FEC claimed it violated the electioneering provisions of the BCRA. The case made its way to the Supreme Court, in which a 5-4 majority said there was no practical way to distinguish between media corporations (which were exempt from BCRA) and other corporations. More to the point, it argued that corporations – and labor unions -- were covered by the First Amendment. Henceforth, corporations and labor unions could spend freely to influence elections, so long as they did not coordinate their activity with candidates and political parties.

The Effect of Campaign Finance Restrictions on Electoral Outcomes

The contentious regulatory history over keeping corporate money and labor union out of politics makes this study especially relevant. Thus, we examine whether the presence or absence of prohibitions on political spending influences electoral outcomes. Robust debates continue to roil journals in the profession about whether campaign finance restrictions have an impact on political outcomes (see Mann 2003, for a review). Empirical work has focused largely on the consequences of *candidate* spending on individual election outcomes, rather than the effect of spending activity by corporations or unions on aggregate political outcomes. This is a curious gap in the literature because even prior to the *Citizens United* decision, U.S. interest groups possessed strong protections on free speech, which enabled them to engage in a variety of activities to influence elections,

including issue advocacy and voter mobilization. While much work has traced the strategies of outside groups in elections (Franz 2008; Magleby, Monson, and Patterson 2007; Skinner 2007) there has been none to our knowledge that assesses whether regulations on groups affect their capacity to influence aggregate electoral outcomes.

For spending bans to influence electoral outcomes it must be the case that corporations and unions want to influence election outcomes with their spending and that they are capable of doing so when spending bans are not in place. We might expect corporations and unions to be motivated to influence two outcomes in particular—the success of parties in winning votes and the success of incumbents in securing reelection. It is reasonable to assume that corporations and unions wish for their spending to have partisan consequences because the goals of these entities are often aligned with those of particular parties. To be sure, there is some evidence that corporations tend to hedge their contribution strategies by giving to incumbents, regardless of party, to ensure that they get sympathetic votes for specific pieces of legislation (Stratmann 2002). However, in open seat races and increasingly even in elections featuring incumbents, corporations tend to favor Republicans, while labor unions consistently support Democrats (Brunell 2005). Based on these patterns, it seems to be the case that corporations and unions wish to influence the electoral process in a way that elects more members from their preferred parties.

Corporations and unions may also have clear preferences when it comes to support for (or opposition to) incumbent legislators. Indeed, we know that corporations tend to support incumbents with their campaign *contributions* because they are primarily interested in gaining access to policymakers (Lowery and Brasher 2004). This finding suggests that restrictions on the political activity of corporations should benefit challengers who suffer financial disadvantages (Abramowitz 1991). However, more recent work suggests a nuanced strategy on the part of corporations. As noted above, corporations and unions have an ideological affinity for one party or the other and

they understand the importance of majority party status for gaining their preferred policy outcomes. For this reason, they are often willing to risk supporting challengers from the party of their preference, even if they simultaneously support incumbents from both parties who are friendly to their cause (Brunell 2005).

From this perspective, restrictions on political spending by corporations might hurt challengers. After all, corporations and labor unions tend to support quality challengers who have a real shot at winning, while the vast majority of incumbents they support may not be genuinely threatened electorally (Brunell 2005). In other words, corporations appear to pursue an *electoral* strategy with challengers, and an access strategy with relatively safe incumbents. Thus, restrictions on spending should disproportionately affect contests with viable challengers, which might increase the advantages of incumbents in these contests (Meirowitz 2008).

Overall, we can stipulate that corporations would tend to be motivated to spend money on incumbents and strong Republican challengers, while labor unions would largely spend money on Democrats. Of course, for corporate or union spending to matter it is not enough that these entities spend on behalf of their preferred party's candidates; it also must be the case that this spending translate into votes. There is a large body of research examining the effect of campaign spending on electoral outcomes (Gerber 1998; Jacobson 1980; Levitt 1994). One of the enduring insights from this work is that while campaign spending does affect electoral outcomes, the effect of each additional dollar spent on an election tends to have a diminishing return in terms of the outcome. Figure 1 illustrates this effect using data from the 2010 congressional elections.³ Note that the first quarter million in spending by either candidate has a strong affect on the outcome of the election. However, after each candidate has spent about \$1 million on the election, there is only a very small

³ This plot was produced from a simple model regressing the percentage of the two party vote won by the Democratic candidate on the natural log of Democratic spending on the race, the natural log of Republican spending, and the two-party vote for Obama in the district.

payoff from additional spending.

[FIGURE 1 ABOUT HERE]

While the shape of the relationship will vary for different offices at different levels of government, it is most often the case that the first dollar spent on an election has much more value than the millionth dollar spent. This fact provides us with a reason to doubt that unlimited spending from corporations and labor unions will have a major effect on election outcomes. After all, corporations and labor unions are most likely to invest their spending in competitive races where they believe their investment may be capable of influencing the outcome. Yet, it is precisely in those races where a great deal of money will have already been spent by the candidates, parties, and other interest groups. If one were to assume that the relationship between campaign spending and election outcomes was linear, then it could matter a great deal that corporations and unions were allowed to spend unlimited amounts of money on races that had already attracted a great deal of investment from other sources. In this situation, a corporation could spend \$100,000 on an election where \$1 million had already been spent and expect to see just as much value as if they had spent the only money in the race. But since increased spending has diminishing returns, there is good reason to think that allowing such spending in that type of race may have little effect on the outcome of that election.

Thus, even if corporations wish to influence election outcomes by spending large amounts of money independently, there is still good reason to believe that their influence will be minimal. And if the influence of such spending is minimal, then we would not expect to see dramatic differences in election outcomes in states where such spending is banned compared to those where it is allowed. Accordingly, on one hand, there is reason to expect that bans on corporate spending

may have few consequences for who wins or loses elections. On the other hand, others have argued that the absence of such bans will have important electoral consequences and even if such spending has a marginal effect, it may be sufficient to swing enough election outcomes so that there are observable advantages for partisans and incumbents in states where such spending is permitted.⁴

Methodology

To examine whether corporate spending bans influence electoral outcomes, we analyze the effects that these bans have had in the American states. For this analysis, we collected data on campaign finance laws for each state beginning in 1968 and running through the 2008 election cycle. The dates for when a state passed laws banning corporate or union spending came from the National Conference of State Legislatures. The National Conference of State Legislatures cited the specific laws in each state that banned either corporate or labor union spending.⁵ We then researched these laws to determine the date when the law was passed. When a law was passed

⁴ See for example, Fred Wertheimer, “An Electoral Catastrophe” (in Room For Debate: How Corporate Money Will Reshape Politics) *New York Times*, January 21, 2012. Available at <http://roomfordebate.blogs.nytimes.com/2010/01/21/how-corporate-money-will-reshape-politics/>

⁵ Available at <http://www.ncsl.org/legislatures-elections/elections-campaigns/citizens-united-and-the-states.aspx> (Accessed January 20, 2011). While the NCSL data provides a list of laws that were in place when the Citizens United decision was announced, such a list would exclude any states that might have previously had a spending ban but had repealed that ban prior to *Citizens United*. We took multiple steps to determine whether any states had previously banned corporate spending on elections. First, we consulted several campaign finance historians; each of these historians indicated that they were not aware of any state that had previously had a ban and repealed it. Second, we identified a group of states that would have been most likely to have had such a ban in the earlier period. Specifically, since we found that states that banned corporate spending in the contemporary period also always banned corporate contributions, we used a separate dataset on corporate contribution regulations to identify states that earlier in the period had contribution bans but then removed those bans prior to *Citizens United*. These are the states that would have been most likely to have previously had a corporate spending ban as well. The states included AL, HI, IN, LA, MS, MO, NH, OR and UT. We contacted experienced officials in Secretary of State offices (or the appropriate agency) at each of these state governments and in each case the officials we contacted said that their states had not previously had bans on corporate spending. Thus, we can find no evidence that any state had a ban on corporate spending in elections that it had repealed prior to Citizens United.

during an election year, we coded its impact as beginning in the following election year. Twenty-one states had a corporate spending ban in place for at least part of the period, and all but three of those states enacted that ban during this period. Thus, we have eighteen interventions where states enacted a corporate spending ban when they had not previously had one in place. We add to this group New York, which enacted restrictive limits on corporate spending during the period of the study. Specifically, in 1976, New York enacted a law that limited corporate spending in elections to \$5,000. We treat this as a quasi-spending bans since the spending limits are strict enough to severely limit corporate influence. Thus, we examine a total of nineteen corporate spending ban interventions, which provide particularly strong leverage for determining the effects of these types of bans.

We focus on four dependent variables that we expect to be influenced by the existence of a corporate spending ban—the share of legislative seats won by Republicans in a particular election, the share of the total vote in state legislative races that was won by Republicans, the share of incumbents who won campaigns for re-election, and the share of the total legislative vote won by incumbents. The vote share data comes from the State Legislative Election Returns dataset (Klarner, Berry, Carsey, Jewell, Niemi, Powell, and Snyder 2013). These data cover elections held between 1967 and 2008; therefore, our analysis of these dependent variables is limited to that time period. We calculate the Republican share of the vote for state legislative races by taking the total number of votes cast for Republican candidates running in all general elections for the state legislature and dividing by the total number of votes cast for all candidates (of any party) running in the general election. Since corporations tend to be more supportive of Republican candidates, we expect that Republicans will not perform as well when a state has instituted a corporate spending ban. The incumbency vote share is calculated simply as the percent of the vote won by incumbent candidates who ran for either chamber of the state legislature in that year (this calculation only includes races in which there was an incumbent running for re-election). We expect that in states

that enact spending bans, incumbents will win a smaller share of the vote compared to states where such bans do not exist.

While the intervention we are most interested in is the implementation of a corporate spending ban, it is important to be aware of other types of campaign finance laws that may serve to enhance or diminish the effect of enacting such a ban. This information about other campaign finance regulations is also important for contextualizing what our analysis can and cannot say about corporate spending bans. Table 1 indicates the frequency with which corporate spending bans occur in our dataset and the extent to which they exist in conjunction with other regulations. Several patterns are worth noting. First, corporate spending bans were not particularly widespread during the period of our study. In fact, such bans were only in place in 300 state-election year dyads, or about one-fifth of all cases. Second, while many states had limits on contributions from organizations but no corporate spending ban, the opposite was never true.⁶ In all 300 cases where a state had enacted a corporate spending ban, limits on contributions from organizations were also in place. Thus, in analyzing our data, it is impossible to determine the effects of a corporate spending ban absent contribution limits. Importantly, contribution limits on political groups were never enacted at the same time as corporate spending bans; therefore, our analysis should be able to distinguish the effects of the latter and the former.⁷

A third important point regarding contribution laws is that all states that placed a ban on corporate spending in election campaigns also had corporate contribution bans in place. Alaska and Colorado introduced bans on corporate spending and contributions at the same time and New York instituted a limit on corporate contributions at the same time that the state introduced its limit on

⁶ The data on contribution limits on organizations comes from Primo and Milyo (2006).

⁷ It is also possible that the effects of corporate spending bans are conditional on the presence of other types of limits or bans in a state. We tested this possibility by incorporating interaction terms into the cross-sectional time series models presented in the appendix, but in every case the interaction terms were small and did not approach statistical significance.

corporate spending. However, the other 16 states that instituted corporate spending bans did so after they had already had a corporate contribution ban in place during previous election cycles. Thus, for most states in our analysis, the effects we are estimating are those of instituting a corporate spending ban once a corporate contribution ban is already in place.

The final type of regulation that we are careful to take note of is the presence or absence of spending bans on labor unions. Since such spending bans would serve to curtail traditional sources of Democratic support, it is important to be aware of whether such bans existed in the states we are studying. As Table 2 notes, approximately half of the instances where a state had a ban on corporate spending it also had a ban on union spending. Just fifteen states had a union spending ban in place for at least part of the period, none of which had the ban for the entire period for which we have data. Fortunately, there are many instances where a corporate spending ban was in place while a similar ban was not applied to unions. For example, six states (Connecticut, Iowa, Kentucky, Massachusetts, Minnesota, and Tennessee) introduced a ban on corporate spending during the period of our study but did not introduce a ban on union spending. Additionally, New York implemented severe limits on corporate spending without enacting similar limits on spending by labor unions. Another fourteen states introduced bans on both corporate and union spending at the same time. In the analysis that follows, we consider these two groups of states separately in order to properly contextualize our results. We would expect larger effects for the corporate spending ban intervention in states that did not also introduce a union spending ban.

Figure 2 provides a more detailed account of our key variables by showing the years during which each state had corporate and union spending bans in place. The shaded area indicates the years during which each of these states had a corporate spending ban in place. The darker shading indicates that both corporate and union spending bans were in place while the lighter shading is used for states that only implemented a ban on spending by corporations. For each state, the figure

indicates when the ban was enacted and it also tracks the values of the four dependent variables we examine.

[FIGURE 2 ABOUT HERE]

Estimation Approach

It is commonplace for scholars to use cross-sectional time series models when analyzing data on states over time. However, many scholars have warned about the difficulties entailed in constructing cross sectional time series models (e.g., Achen 2000; Beck and Katz 1995; Stimson 1985; Wilson and Butler 2007). Given the “exceedingly frail” (Wilson and Butler 101, p. 121) nature of time-series cross-section analysis, we instead utilize an intervention analysis that estimates a separate ARIMA time series model for each state that implemented a ban during the time period for which we have data available. This approach allows us to sidestep the problems inherent in pooling states into a single cross-sectional time series analysis by estimating separate effects for each state that enacted a corporate spending.⁸ By focusing narrowly on states that changed their campaign finance laws during this period, we are able to gain more leverage in identifying the causal effects of the intervention. We separate the states into two groups—states that enacted a corporate spending ban without a union spending ban and states that enacted both types of bans.

For each state, we estimate a separate ARIMA model for each of the four dependent variables, amounting to 76 models in all. We specify an AR(1) process for each of the models, but alternative specifications did not change the substantive results we present here. We also incorporate a first difference of the dependent variable to account for the fact that in many cases there were trends present in the series. For example, Republican Party strength steadily increased in southern

⁸ We also ran the cross-sectional times series models with similar results, as shown in Appendix 2.

state legislatures during this period. Likewise, incumbent re-election rates increased during the second half of the 20th century in most of the states in our study.⁹ Finally, each model includes a measure of the national policy mood (Stimson 1991). The mood variable captures the extent to which the American public supports more liberal or conservative government programs and it has been shown to play an important role in explaining election outcomes (Erikson, MacKuen, and Stimson 2002). We include the measure in our state-level models to control for any partisan swings in the states that may occur simply because of a shifting policy mood.¹⁰

Appendix 1 compiles the full results from each of these models. For ease of presentation, in the discussion that follows we present plots of the coefficients for the corporate spending ban intervention. We also summarize the results for each grouping of states using meta-analysis to produce a weighted mean. The weighted mean we present is generated by giving more weight to coefficients that are estimated with more precision (i.e. those with smaller standard errors). However, we mostly focus on analyzing the state-by-state effects.

Results

Figure 3 presents the coefficient plots for the models estimating the effect of a corporate spending ban on the share of lower chamber seats won by Republicans. Two separate plots are

⁹ The differencing makes an important difference in our substantive results for the incumbency models, but does not change the results for the other dependent variables. When we do not difference the incumbent re-election rate, we find that enacting a corporate spending ban consistently and significantly increases the incumbent re-election rate. However, this is attributable to the fact that re-election rates were higher later in the series than earlier in the series.

¹⁰ While we are limited to measuring mood at the national level rather than state-by-state, we do not think that this poses a major problem for our analyses. While the level of the mood variable would likely vary across states, we expect the movement in that variable to be relatively similar across states over time. In other words, Texas would likely always register as more conservative on the mood variable than Massachusetts, but both were likely trending more conservative in the late 1970s and more liberal in the mid-2000s. Since our analyses are conducted state-by-state, the differing levels across states are of no consequence and it is the movement in the variable over time that is most important for allowing us to control for time-specific trends.

presented in this figure. The first plot includes the seven states that implemented a corporate spending ban (or limit in the case of New York) during the period for which we have data, but did not implement a union spending ban at the same time. Thus, these states provide us with leverage for discerning the impact of a corporate spending ban independent of a union spending ban. The second plot includes 12 states that implemented both types of bans concurrently. The plots include the coefficient estimate and 95% confidence interval for each state as well as the amount of weight each state was given in calculating the summary estimate. For example, in the first plot, the figure shows that the coefficient for the corporate spending ban intervention was $-.01$ for the model estimated with data from Connecticut. However, the confidence interval for this estimate is quite large, which means that Connecticut is only given $.23\%$ of the total weight in calculating the summary figure. In contrast, the estimate for Kentucky was much more precise, which meant that more weight was placed on that estimate when calculating the summary score at the bottom of the figure. While the weighted average is a convenient summary statistic, we caution against assigning too much importance to it. Thus, in our discussion below, we are careful to focus mostly on the effect of the intervention in each state.

[FIGURE 3 ABOUT HERE]

Of the seven coefficients plotted in the first panel of Figure 3, two are in the (expected) negative direction, while five have a positive sign. However, the coefficients were quite small in all eight states, and in only one case—Kentucky, where the coefficient was actually positive—could we be 95% confident that the coefficient was different from zero. In Kentucky, the introduction of a corporate spending ban appeared to actually coincide with an increase in Republican success at

winning seats to the state legislature. The model estimates that the ban accounted for a 7% increase in the number of seats won by Republicans.

The second plot in Figure 3 shows the effect that implementing both a corporate and union spending ban at the same time had on the Republican share of the lower chamber in each state. There is little in the way of clear patterns from this plot—seven of the coefficients are positive while five are negative. Only two of the twelve coefficients were statistically distinguishable from zero; in both cases, the coefficients were positive (meaning the bans helped Republicans). In Oklahoma, the bans on corporate and union spending appeared to produce a 3% increase in the share of seats won by Republicans. In Wisconsin, the introduction of the bans led to a 5% increase in Republican seats. Overall, there is little in the way of consistent evidence that spending bans produce any partisan bias in election outcomes for state legislature. In the rare instances where there was a statistically distinguishable effect (Kentucky, Oklahoma, and Wisconsin), it was in favor of the Republican Party.

In examining the state-by-state coefficients in Figure 3, it is also worth considering which cases might provide the best comparison to the national government. Among this set of states there are several highly professionalized legislatures, including in New York (second most professional legislature), Wisconsin (third), Massachusetts (fifth), Michigan (sixth), and Ohio (seventh). Only in Wisconsin did the coefficient for the intervention differ significantly from zero, and in that instance Republicans actually performed better (not worse) when corporate and union spending was banned.

While Republican success in winning legislative elections is one measure of the possible partisan consequences of campaign spending bans, another way of measuring the partisan effects of these laws is by examining whether Republican candidates received fewer votes when corporate spending was banned. Figure 4 presents the same type of plots except in this case the dependent variable is the proportion of the total two-party vote won by Republican candidates for the

legislature. In this case, the null results are even more definitive—there is not a single state in which the introduction of a corporate spending ban produced a statistically significant increase or decrease in the Republican share of the vote for state legislature. Indeed, the weighted average for each plot is approximately zero.

[FIGURE 4 ABOUT HERE]

Overall, the results presented in Figures 3 and 4 provide little support for the hypothesis that allowing unlimited corporate spending in elections will benefit Republican candidates (or, conversely, that banning such spending will disadvantage Republicans). We also failed to find a significant effect for the corporate spending ban in our cross-sectional time series models (presented in Appendix 2).

While our analyses reveal little in the way of partisan electoral consequences for campaign spending bans, we also hypothesized that allowing corporations to spend unlimited amounts on elections may help to entrench incumbents in office. To examine this possibility, Figure 5 presents the summary of coefficients from the models testing the effects of corporate spending bans on the proportion of incumbents winning reelection to the state legislature. Note that each of the nineteen coefficients plotted in the figure has a confidence interval that overlaps with zero. In other words, in the nineteen states in which a corporate spending ban was introduced during the period of our study there was not a single instance where that ban produced a statistically significant increase or decrease in the incumbent reelection rate.

[FIGURE 5 ABOUT HERE]

Finally, Figure 6 plots the spending ban coefficients for the models where the dependent variable was the proportion of the total vote won by incumbent candidates for the legislature. In only one of the nineteen states did we see a statistically significant coefficient for the corporate spending ban. This significant coefficient was observed in Massachusetts, where the introduction of a ban on corporate spending appeared to produce a 10 percentage point drop in the share of the vote earned by incumbent candidates. This effect would be consistent with our expectation that corporate money is more likely to be spent on behalf of incumbents and that a ban on such spending would hurt incumbent candidates. However, we hesitate to make much of this result given that we do not observe a similar effect in any of the other 18 states that banned corporate spending during this period. These non-findings are consistent with our cross sectional time series analysis as well.

[FIGURE 6 ABOUT HERE]

Overall, the results from our models indicate that corporate spending bans have little effect on who wins elections (at least when the “who” is defined by incumbency or partisanship). Republicans did not fare worse in elections following the implementation of a corporate spending ban, the corollary of course being that they did not perform better when such a ban did not exist. Likewise, incumbent re-election rates did not decrease when a corporate spending ban was enacted.

Conclusion

Drawing on decades of data on campaign finance laws in the American states, our largely null findings cast doubt on several forecasts about the consequences of *Citizens United* with respect to electoral outcomes. Spending bans did not appear to be effective in altering the partisan balance in

legislatures nor did they influence the incumbent re-election rates. Yet, while we believe that the data convincingly demonstrate that spending bans are generally ineffective at influencing the outcomes we focus on, we think it is important to make note of what this paper cannot say. First, this study focused on just two outcomes – partisan control of government, and incumbency reelection rates. While these measures allow us to gain a sense for how spending bans might affect partisan balance and turnover in government, we also acknowledge a wide range of potential effects that we are not able to examine. For example, the presence of spending bans may increase the extent to which citizens trust their governments, lead to more responsiveness and accountability from elected officials, or influence any number of policy outcomes. While some of these dynamics (e.g. trust in government) likely cannot be addressed at the state level because of the lack of temporal state-by-state data, future research may focus on gathering comprehensive over-time data on other policy areas that may be subject to corporate or union influence.

A second limitation of our study is that campaign finance dynamics at the state-level may not be generalizable to the federal government. Indeed, given the stakes involved in national policymaking, there is a greater incentive for corporations and unions to take full advantage of the absence of spending bans by pouring much more money into federal elections than they might to influence state election outcomes. If this is true, then the influence of spending bans may actually be consequential for national politics and policymaking even though we do not find them to be important at the state level. However, it bears mentioning that spending bans almost never had an effect on state politics in this study, even in the states with larger economies and more professional legislatures. We might also point out that a preliminary analysis of the 2012 congressional elections, which experienced an estimated \$140 million in independent spending by outside groups in 25 toss

up races, did not affect the likelihood of winning.¹¹

Despite these limitations, our findings are important for understanding the role of campaign finance restrictions on macro political outcomes. While previous studies have looked at the relationship between candidate spending and electoral performance, they have not examined how laws regulating interest group spending might affect electoral results. Well before the *Citizens United* decision, interest groups have been engaged directly in political campaigns (Heard 1960; Overacker and West 1932). At the federal level, such engagement appears to have increased since the 1980s (Magleby and Corrado 2011; West 2010). Previous studies, which focused on candidate financing and electoral outcomes may miss the broader dynamics of campaign effects by non-candidate organizations.

We offer several explanations for why the laws we examined likely have minimal impact. First, as we stated at the outset candidates can raise money from several sources and there are significant diminishing effects of additional spending in a race. In Congress, the first half-million can be influential, but the next half million may not matter much at all, or only very little. For this reason, it would be difficult for corporations or unions to spend large sums to change systematically the outcome of elections, so long as candidates have plentiful access to campaign funds. Nonetheless, it is somewhat surprising that campaign spending might not have a more linear effect in state legislative races where candidates are less well known. On the other hand, districts are smaller in state legislative races and personal relationships or grassroots strategies may matter more than expensive mass communications.

A second plausible explanation is that interest groups are highly adaptable. Research shows

¹¹ See Lee Drutman, “How Much Did Money Really Matter in 2012,” Sunlight Foundation Blog, November 9, 2012. Available at <http://sunlightfoundation.com/blog/2012/11/09/how-much-did-money-matter/>

they routinely adjust to changes in campaign finances laws to pursue political objectives. For example, when laws restrict contributions to candidates, business and labor interests turn to other forms of electioneering, including direct contact with voters, endorsements and contributions to political parties (Hogan 2005; Magleby, Monson, and Patterson 2007; Malbin and Gais 1998). Similarly, laws that ban direct political spending may simply encourage these groups to increase political contributions and other forms of electoral support that are more difficult to observe.¹² The capacity of political interests to adapt to regulations suggests that efforts to cordon off the flow of money in politics may be a fruitless undertaking. The hydraulic metaphor has been aptly applied to a situation in which money, like water, seeks its own level (Issacharoff and Karlan 1998-1999). For this reason, whether a spending ban exists or not, political interests find a way to maintain influence and the likely outcome is the status quo.

A third and related reason that our minimal effects finding is not surprising is that political interests vying for influence may end up neutralizing each other, even though they end up spending more on politics than previously. When one side appears especially active or gaining an advantage, the threatened rivals tend to counter-mobilize (Walker 1991). The ongoing quest for influence results in an arms race (another apt metaphor for campaign finance) in which infusions of political money from contending parties offset expected gains from additional spending (Gray and Lowery 1997). To be sure, this dynamic has potentially detrimental costs for democracy. The spiraling sums of money could raise the cost of politics, which makes it harder for less wealthy and new groups to get involved. It also makes fearful politicians spend more time raising money in anticipation of being targeted by outside groups.

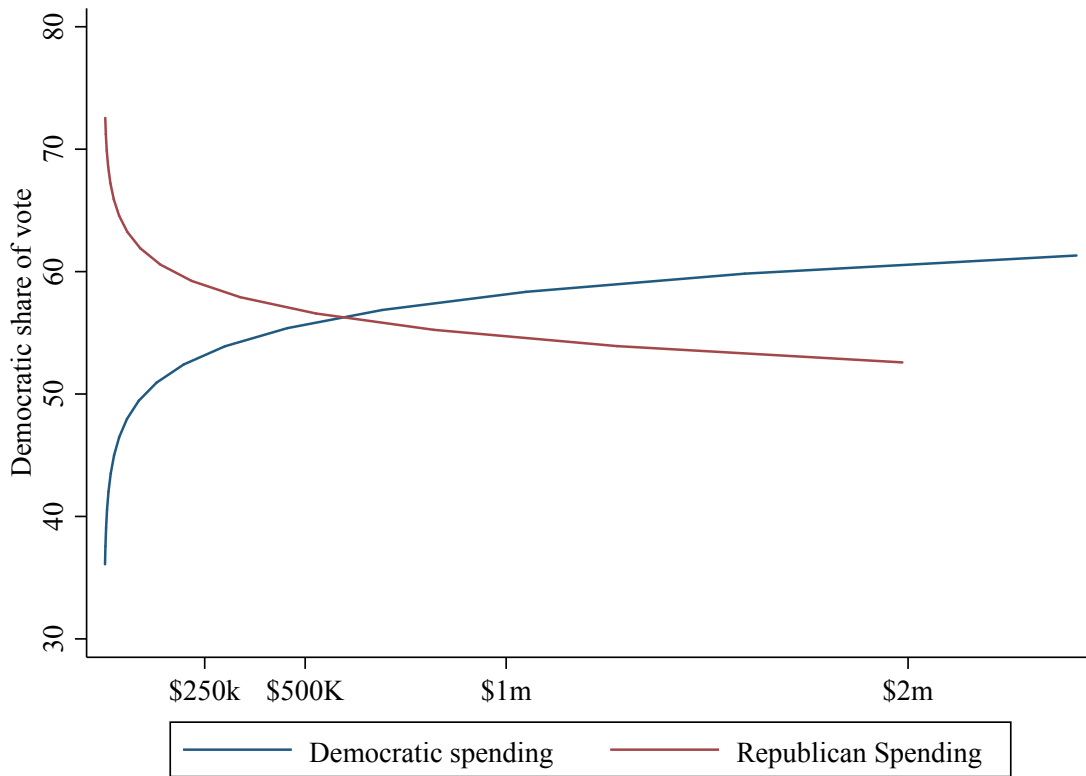
¹² In Colorado, for example, interest groups in the wake of Amendment 27 (2002) established so-called “education” committees that could spend unrestricted amounts in state elections. See Arthur Kane, “Politicos scouring laws for loopholes Colorado offers test for new fund limits,” *Denver Post*, December 1, 2002.

Despite these non-effects for electoral outcomes, corporations and unions appear to have other good reasons for spending in elections. Campaign spending, for example, might have a greater effect in primaries where party cues are absent. Moreover, such spending may instill fear in incumbents who stray from group preferences (Masket 2009), or pave the way for greater access to officeholders who believe that group spending aided their victory. For some or all of these reasons, in the aftermath of the *Citizens United* decision we fully expect business and labor interests to exploit opportunities to spend money in campaigns even if their electoral influence is marginal.

Table 1: Co-occurrences of Campaign Finance Regulations in Dataset

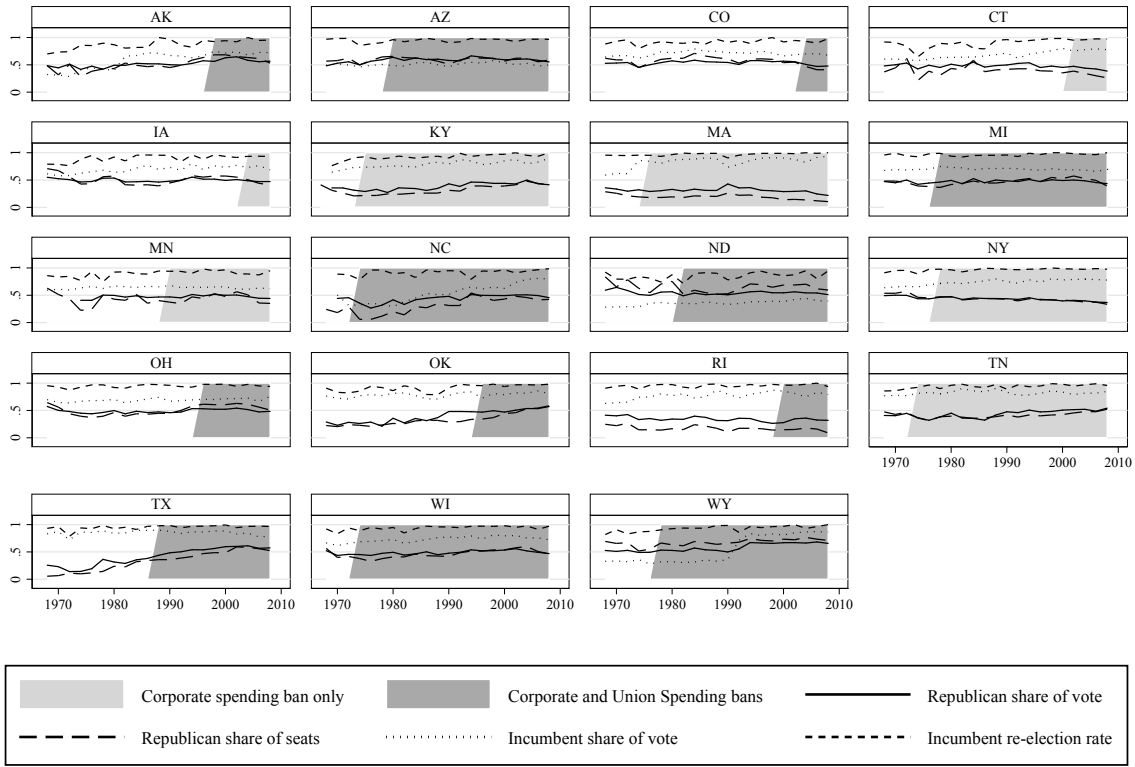
Situation	Total Cases	Cases with union spending ban in place	Cases with group contribution limits in place
Corporate spending ban	310 (21.1%)	162 (52.3%)	300 (96.8%)

Figure 1: The Diminishing Returns of Additional Campaign Spending on Election Outcomes



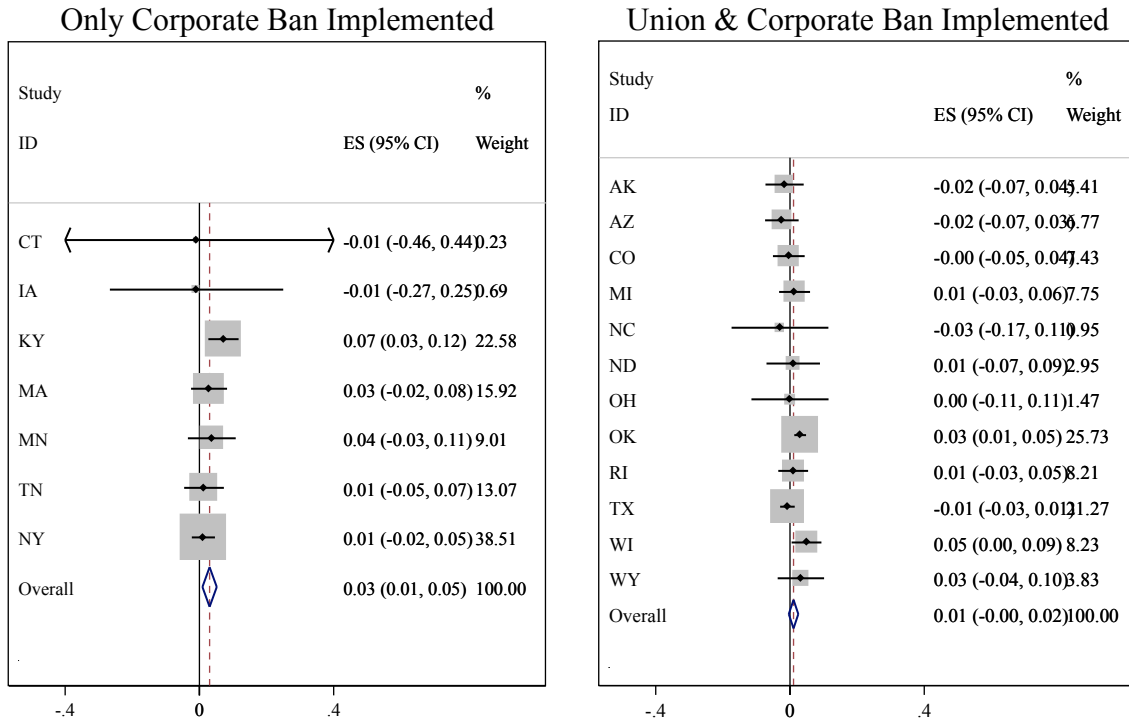
This figure plots the non-linear relationship between candidate spending and final vote share in the 2010 Congressional Elections. These effects are based on a simple ordinary least squares regression model using the percentage of the two-party vote won by the Democratic candidate as the dependent variable. The independent variables were the natural log of the amount spent by the Democratic (plotted above), the natural log of the amount spent by the Republican candidate (plotted above), and the percentage of the two-party vote for president won by Obama in the district in 2008.

Figure 2: Plots Tracking Finance Laws and Measures of Electoral and Policy Influence Over Time



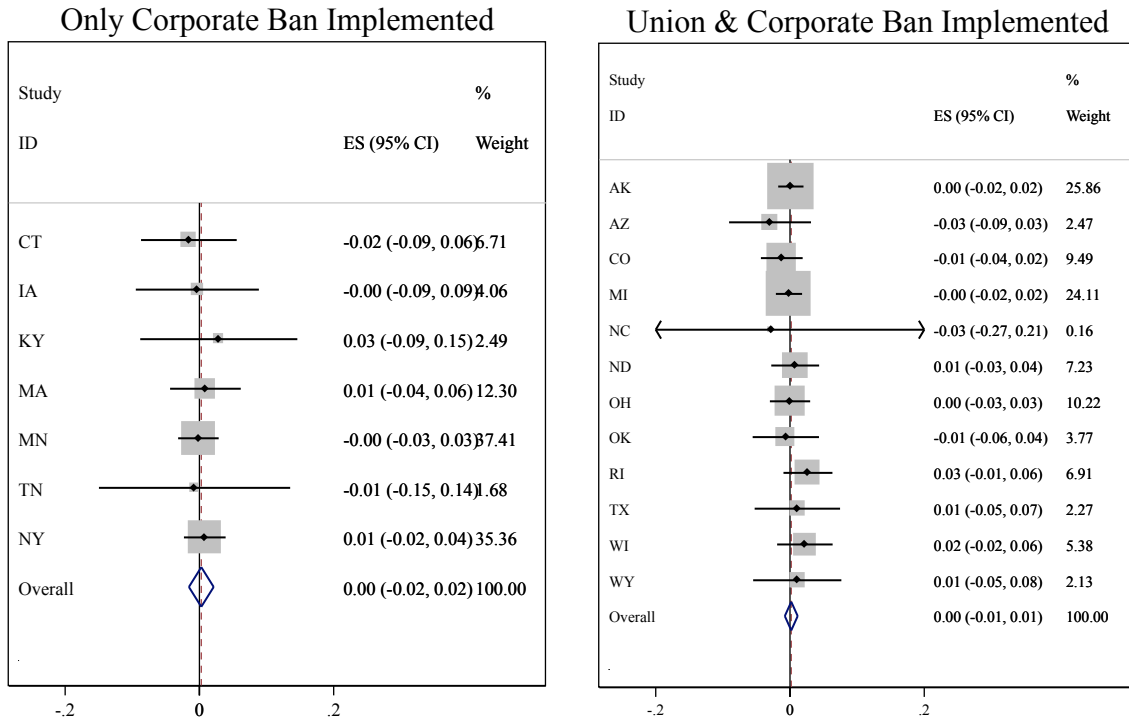
Light shaded areas denote periods during which a state implemented a corporate spending ban but no union spending ban. Dark shaded areas denote periods during which both types of bans were in effect.

Figure 3: Summary of Effects of Corporate Spending Bans on Republican Share of Legislative Seats From State-by-State ARIMA Models



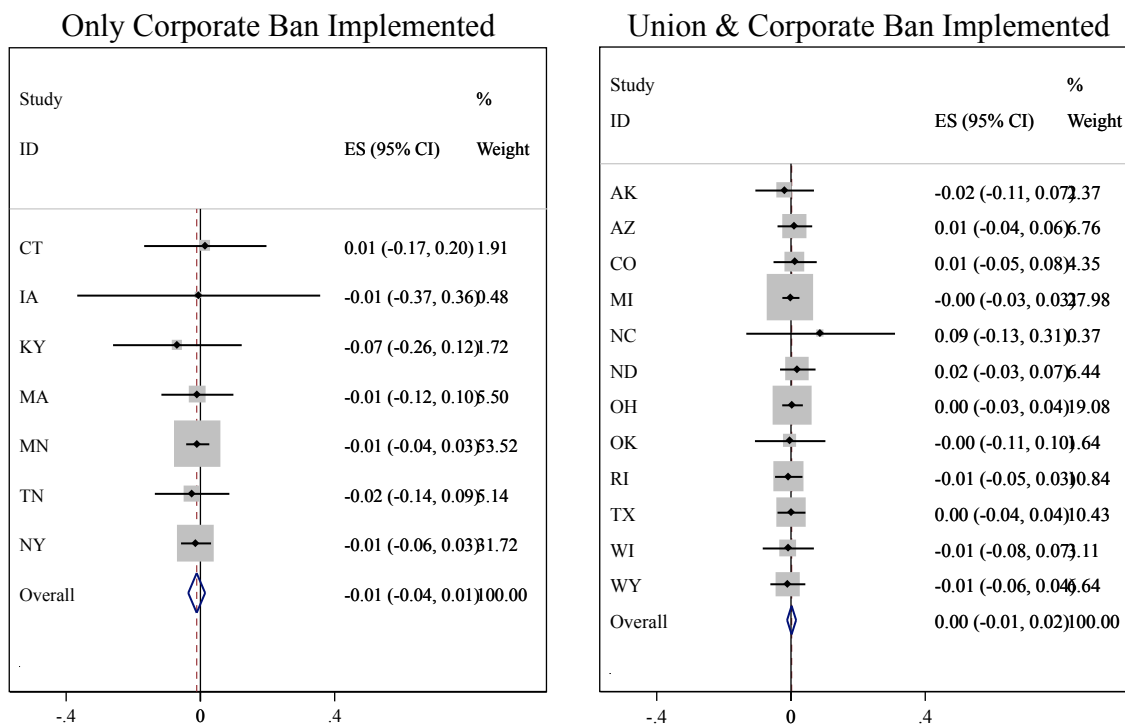
Plots show the size of the coefficient and the 95% confidence interval for the corporate spending ban intervention in each state, based on the ARIMA models presented in the Appendix. Shaded boxes indicate the amount of weight given to each state in estimating the average effect. Dependent variable is number of Republican candidates winning elections for the state legislature divided by the total number of Republican and Democratic candidates winning elections.

Figure 4: Summary of Effects of Corporate Spending Ban on Republican Share of Legislative Two-Party Vote From State-by-State ARIMA Models



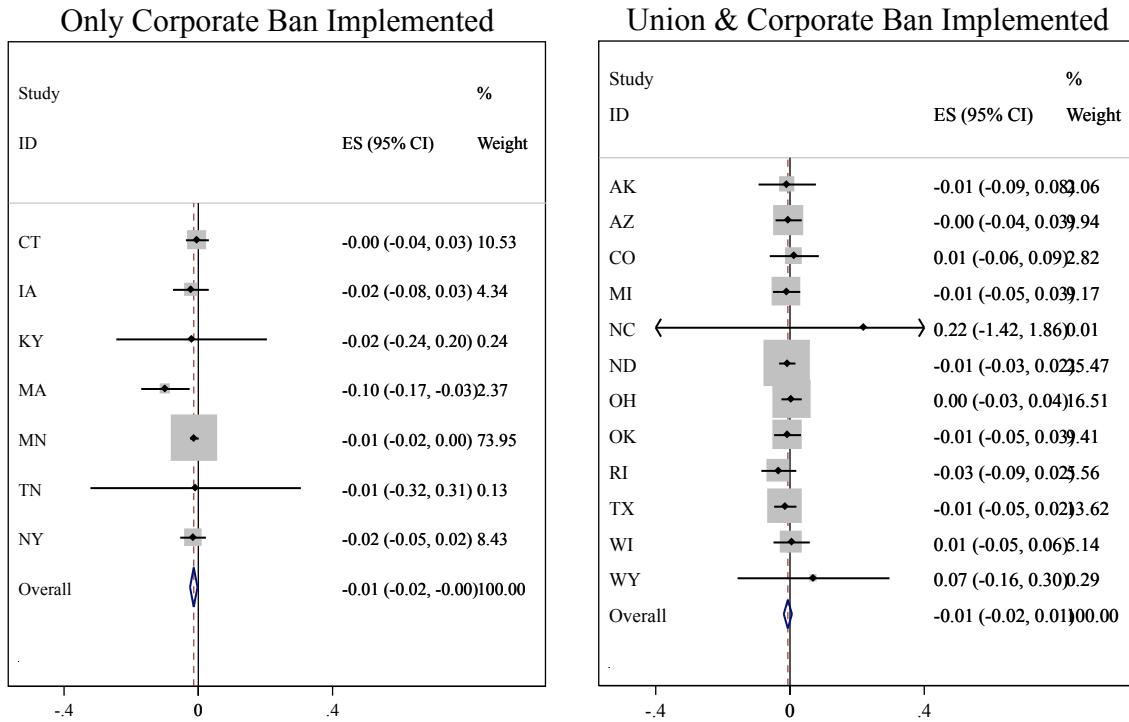
Plots show the size of the coefficient and the 95% confidence interval for the corporate spending ban intervention in each state, based on the ARIMA models presented in the Appendix. Shaded boxes indicate the amount of weight given to each state in estimating the average effect. Dependent variable is number of votes cast for Republican candidates for the legislature divided by the total votes cast for Democratic and Republican candidates for the legislature.

Figure 5: Summary of Effects of Corporate Spending Bans on Share of Incumbents Re-elected to Office from State-by-State ARIMA Models



Plots show the size of the coefficient and the 95% confidence interval for the corporate spending ban intervention in each state, based on the ARIMA models presented in the Appendix. Shaded boxes indicate the amount of weight given to each state in estimating the average effect. Dependent variable is the number of incumbents winning re-election to the state legislature divided by the number of incumbents running for re-election.

Figure 6: Summary of Effects of Corporate Spending Bans on Proportion of the Vote Won by Incumbent Candidates for State Legislature from State-by-State ARIMA Models



Plots show the size of the coefficient and the 95% confidence interval for the corporate spending ban intervention in each state, based on the ARIMA models presented in the Appendix. Shaded boxes indicate the amount of weight given to each state in estimating the average effect. Dependent variable is number of votes cast for incumbent candidates for the legislature divided by the total votes cast in races where an incumbent was running for re-election.

Appendix 1: Full Estimates from ARIMA Spending Ban Intervention Models

Table A1: ARIMA Models Estimating Republican Share of Legislative Seats

State	Intervention	SE	Mood	SE	Intercept	SE	Lag	SE	Sigma	SE	N	LL
CT	-0.009	0.228	-0.005	0.006	0.265	0.374	-0.608	0.263	0.100	0.013	20.000	17.454
IA	-0.009	0.132	-0.006	0.005	0.331	0.287	0.205	0.343	0.062	0.010	20.000	27.170
KY	0.072	0.023	-0.002	0.002	0.040	0.114	-0.317	0.299	0.029	0.006	20.000	42.212
MA	0.029	0.027	0.000	0.002	-0.039	0.116	-0.333	0.358	0.021	0.004	20.000	48.647
MN	0.037	0.036	-0.016	0.005	0.880	0.265	-0.495	0.281	0.082	0.015	20.000	21.557
TN	0.014	0.030	-0.001	0.003	0.023	0.204	-0.223	0.177	0.038	0.008	20.000	37.013
NY	0.012	0.018	-0.001	0.003	0.029	0.152	-0.339	0.334	0.029	0.006	20.000	42.522
AK	-0.016	0.029	-0.008	0.003	0.451	0.151	-0.780	0.117	0.055	0.012	20.000	28.997
AZ	-0.024	0.026	-0.004	0.003	0.242	0.203	-0.324	0.287	0.047	0.013	20.000	32.908
CO	-0.003	0.024	-0.009	0.002	0.491	0.135	-0.453	0.225	0.044	0.011	20.000	34.199
MI	0.014	0.023	-0.005	0.002	0.285	0.107	-0.344	0.301	0.036	0.008	20.000	37.868
NC	-0.030	0.074	-0.008	0.007	0.478	0.455	-0.384	0.215	0.083	0.014	20.000	21.226
ND	0.010	0.041	-0.004	0.005	0.208	0.273	-0.363	0.277	0.092	0.017	20.000	19.338
OH	0.000	0.059	-0.009	0.004	0.483	0.204	0.206	0.211	0.040	0.007	20.000	35.923
OK	0.030	0.009	-0.002	0.001	0.096	0.084	-0.408	0.269	0.023	0.006	20.000	47.103
RI	0.010	0.023	-0.004	0.002	0.234	0.140	-0.384	0.272	0.041	0.011	20.000	35.586
TX	-0.008	0.011	-0.005	0.001	0.300	0.081	-0.434	0.262	0.029	0.006	20.000	42.066
WI	0.049	0.023	-0.005	0.003	0.261	0.201	-0.415	0.231	0.042	0.009	20.000	34.911
WY	0.032	0.035	-0.003	0.004	0.172	0.222	-0.349	0.289	0.047	0.010	20.000	32.806

These models produce the estimates reported in Figure 3. Each model makes up a separate row of estimates.

Table A2: ARIMA Models Estimating Republican Share of Legislative Vote

State	Intervention	SE	Mood	SE	Intercept	SE	Lag	SE	Sigma	SE	N	LL
CT	-0.016	0.036	0.000	0.002	0.022	0.095	-0.648	0.283	0.037	0.008	20.000	37.187
IA	-0.003	0.047	-0.001	0.002	0.077	0.132	-0.055	0.436	0.030	0.005	20.000	41.848
KY	0.029	0.060	-0.001	0.002	0.041	0.152	-0.469	0.249	0.041	0.010	19.000	33.824
MA	0.009	0.027	0.000	0.002	0.010	0.149	-0.387	0.379	0.037	0.006	20.000	37.619
MN	-0.002	0.015	-0.003	0.003	0.188	0.160	-0.393	0.515	0.029	0.005	17.000	36.076
TN	-0.007	0.073	0.000	0.003	-0.008	0.182	-0.139	0.206	0.048	0.011	20.000	32.198
NY	0.008	0.016	-0.001	0.002	0.037	0.107	-0.408	0.350	0.021	0.004	20.000	48.407
AK	0.001	0.010	-0.004	0.002	0.251	0.095	-0.653	0.184	0.032	0.006	20.000	40.367
AZ	-0.030	0.031	-0.003	0.003	0.194	0.209	-0.450	0.415	0.032	0.006	20.000	40.131
CO	-0.012	0.016	-0.004	0.001	0.233	0.079	-0.446	0.296	0.029	0.006	20.000	42.317
MI	-0.001	0.010	-0.002	0.001	0.124	0.080	-0.752	0.159	0.027	0.007	20.000	43.570
NC	-0.028	0.123	-0.006	0.003	0.390	0.274	-0.259	0.210	0.052	0.011	19.000	29.336
ND	0.008	0.018	-0.002	0.002	0.131	0.117	-0.287	0.396	0.035	0.006	20.000	38.681
OH	0.000	0.015	-0.004	0.002	0.216	0.090	-0.389	0.384	0.028	0.004	20.000	42.830
OK	-0.006	0.025	0.002	0.003	-0.070	0.168	-0.678	0.330	0.043	0.008	20.000	34.478
RI	0.027	0.019	-0.002	0.003	0.117	0.152	-0.182	0.403	0.037	0.008	20.000	37.759
TX	0.011	0.032	-0.005	0.004	0.276	0.195	-0.036	0.212	0.050	0.007	20.000	31.594
WI	0.022	0.021	-0.002	0.002	0.096	0.126	-0.357	0.285	0.030	0.008	20.000	41.472
WY	0.011	0.034	-0.002	0.003	0.124	0.189	-0.099	0.295	0.035	0.008	20.000	38.762

These models produce the estimates reported in Figure 4. Each model makes up a separate row of estimates.

Table A3: ARIMA Models Estimating Percent of Legislators Re-elected

State	Intervention	SE	Mood	SE	Intercept	SE	Lag	SE	Sigma	SE	N	LL
CT	0.015	0.093	-0.006	0.006	0.334	0.336	-0.106	0.208	0.074	0.010	20.000	23.643
IA	-0.005	0.185	0.000	0.003	-0.002	0.196	-0.576	0.215	0.056	0.009	20.000	29.079
KY	-0.069	0.098	0.001	0.002	0.040	0.139	-0.577	0.274	0.030	0.006	19.000	39.539
MA	-0.009	0.055	-0.002	0.002	0.152	0.111	-0.384	0.203	0.023	0.004	20.000	47.137
MN	-0.008	0.018	-0.002	0.003	0.124	0.159	-0.712	0.178	0.054	0.012	20.000	29.580
TN	-0.025	0.057	-0.001	0.002	0.058	0.134	-0.582	0.210	0.035	0.009	20.000	38.575
NY	-0.013	0.023	-0.001	0.003	0.071	0.168	-0.306	0.295	0.028	0.005	20.000	43.081
AK	-0.019	0.045	0.003	0.005	-0.156	0.268	-0.157	0.433	0.065	0.011	20.000	26.207
AZ	0.012	0.026	-0.004	0.003	0.202	0.172	-0.268	0.465	0.038	0.010	20.000	36.738
CO	0.012	0.033	-0.002	0.003	0.100	0.190	-0.525	0.350	0.054	0.011	20.000	29.984
MI	0.000	0.013	0.000	0.002	0.002	0.093	-0.315	0.261	0.029	0.007	20.000	42.500
NC	0.088	0.113	0.002	0.006	-0.191	0.431	-0.548	0.186	0.067	0.012	19.000	24.080
ND	0.020	0.027	-0.004	0.003	0.203	0.185	-0.611	0.205	0.076	0.024	20.000	22.837
OH	0.005	0.016	-0.001	0.002	0.075	0.108	-0.041	0.239	0.032	0.007	20.000	40.439
OK	-0.002	0.054	0.002	0.004	-0.095	0.254	0.037	0.300	0.057	0.010	20.000	28.900
RI	-0.007	0.021	0.000	0.002	0.027	0.132	-0.311	0.287	0.038	0.011	20.000	37.110
TX	0.002	0.021	-0.001	0.003	0.080	0.181	-0.608	0.144	0.047	0.009	20.000	32.625
WI	-0.008	0.039	0.001	0.002	-0.019	0.132	-0.777	0.249	0.036	0.006	20.000	37.432
WY	-0.009	0.027	-0.002	0.004	0.156	0.228	-0.614	0.316	0.042	0.006	20.000	34.952

These models produce the estimates reported in Figure 5. Each model makes up a separate row of estimates.

Table A4: ARIMA Models Estimating Incumbent Share of Legislative Vote

State	Intervention	SE	Mood	SE	Intercept	SE	Lag	SE	Sigma	SE	N	LL
CT	-0.003	0.017	-0.003	0.002	0.169	0.119	-0.374	0.317	0.030	0.005	20.000	41.657
IA	-0.022	0.027	-0.001	0.002	0.046	0.121	-0.686	0.171	0.041	0.011	20.000	35.127
KY	-0.020	0.114	0.003	0.003	-0.163	0.202	-0.225	0.386	0.039	0.008	19.000	34.805
MA	-0.098	0.037	-0.004	0.006	0.329	0.342	-0.185	0.380	0.064	0.011	20.000	26.673
MN	-0.012	0.007	0.001	0.001	-0.027	0.066	-0.533	0.202	0.019	0.005	20.000	50.408
TN	-0.008	0.160	-0.002	0.003	0.109	0.253	-0.125	0.209	0.037	0.009	20.000	37.299
NY	-0.016	0.020	-0.003	0.001	0.165	0.080	-0.523	0.175	0.025	0.005	20.000	45.549
AK	-0.008	0.044	-0.005	0.004	0.286	0.215	-0.224	0.443	0.066	0.013	20.000	26.014
AZ	-0.004	0.020	-0.002	0.002	0.118	0.120	-0.272	0.233	0.032	0.011	20.000	40.375
CO	0.013	0.037	-0.003	0.002	0.189	0.136	-0.246	0.284	0.032	0.007	20.000	40.170
MI	-0.010	0.021	0.000	0.001	0.007	0.061	-0.445	0.489	0.019	0.004	20.000	50.489
NC	0.221	0.839	0.001	0.003	-0.235	0.896	-0.070	0.270	0.045	0.016	19.000	31.930
ND	-0.008	0.012	-0.002	0.002	0.102	0.108	-0.154	0.345	0.024	0.005	20.000	46.572
OH	0.005	0.015	-0.001	0.002	0.039	0.111	-0.500	0.293	0.035	0.008	20.000	38.641
OK	-0.007	0.020	0.001	0.003	-0.066	0.195	-0.221	0.262	0.047	0.014	20.000	32.691
RI	-0.033	0.027	0.001	0.004	-0.060	0.251	-0.279	0.262	0.053	0.011	20.000	30.376
TX	-0.014	0.017	-0.002	0.002	0.132	0.097	-0.618	0.126	0.042	0.007	20.000	34.585
WI	0.005	0.028	0.000	0.002	-0.018	0.119	-0.408	0.276	0.034	0.010	20.000	38.908
WY	0.070	0.115	0.005	0.015	-0.343	0.912	0.170	0.534	0.080	0.013	20.000	22.070

These models produce the estimates reported in Figure 6. Each model makes up a separate row of estimates.

Appendix 2: Estimates from Robustness Tests Using Cross-sectional Time Series Models

Table A5: Pooled Cross-sectional Time Series Regression Models Estimating Effect of Corporate Spending Bans on Dependent Variables

Variable	Rep. Success Rate	Rep. Share of Vote	Inc. Success Rate	Inc. Share of Vote
Corporate Spending Ban	0.0029	-0.0034	0.0337	0.0011
	0.0103	0.0040	0.0055	0.0067
Union Spending Ban	0.0140	0.0167	-0.0049	0.0110
	0.0132	0.0072	0.0069	0.0109
Contribution limits for organizations	0.0246	0.0187	0.0287	0.0169
	0.0105	0.0069	0.0087	0.0088
Mood	-0.0043	-0.0023	-0.0008	-0.0013
	0.0005	0.0003	0.0004	0.0004
Lag	0.7777	0.7910	0.2898	0.7690
	0.0422	0.0335	0.0445	0.0389
Intercept	0.3222	0.2092	0.6691	0.2204
	0.0382	0.0240	0.0394	0.0334
N	941	924	927	927
R-squared	0.6160	0.6398	0.1652	0.6693

Table presents the ordinary least squares coefficients for cross sectional time series models employing state fixed effects. In three of the four models, the variable for corporate spending ban dummy variable fails to attain statistical significance. The one exception is Model 3 (incumbent re-election rates), where the coefficient is statistically distinct from zero. However, the effect is in the opposite direction than anticipated and the effect is not consistent with the ARIMA models presented in the paper.

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