

The (Non-)Effects of Campaign Finance Spending Bans on Macro Political Outcomes: Evidence From the States

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This paper seeks to understand the effect of campaign finance laws on electoral and policy outcomes. Spurred by the recent Supreme Court decision, *Citizens United v. FEC (2010)*, which eliminated bans on corporate and union political spending, the study focuses on whether such bans generate consequences notably different from an electoral system that lacks such bans. We observe three key outcomes: partisan control of government, incumbent reelection rates and corporate tax burdens. Using historical data on regulations in 49 American states between 1935 and 2009 we test alternative models for evaluating the impact of corporate and union spending bans put in place during this period. The results indicate that spending bans appear to have limited, if any, effect on these 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 candidates, but no studies to our knowledge have observed how *spending* limits might affect macro political outcomes.

In this paper, we examine whether and to what extent laws aimed at restricting corporate or labor union spending affect macro political outcomes. To accomplish this, our analysis

¹ See *Citizens United v. Federal Elections Commission* 558 U.S. ____ (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.

exploits the fact that prior to the 2010 Court decision, states were given the authority to regulate whether corporations and unions could engage in unlimited campaign spending to influence campaigns for state offices. Immediately prior to *Citizens United*, 28 states permitted corporations to spend money independently in state elections, while 35 allowed labor unions to do the same. 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 and policy data. Thus, state-level variation makes it possible to compare political outcomes in states with and without prohibitions on corporate and union spending, as well as to compare outcomes before and after such prohibitions were enacted. Accordingly, we construct a panel time series dataset on campaign finance laws and state legislative outcomes as far back as 1935. During this period, 20 states implemented a ban on corporate spending, providing especially strong leverage in assessing the impact of a regulatory intervention. We test whether regulations that banned spending by either corporations or unions predict incumbent reelection rates, partisan control of government and important policy outcomes.

Our approach allows us to contribute to the discipline's broader understanding of the efficacy of campaign finance restrictions on partisan control of legislatures, incumbent reelection rates, and the share of taxes paid by corporations. We find that spending bans have little or no impact on these outcomes, confirming previous findings about the marginal impact of campaign finance rules on various political outcomes (see, for example, Ansolabehere, deFigueiredo, and Snyder 2003).

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 501c4 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 they 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 Campaign Finance Restrictions on Political Outcomes

The contentious regulatory history over keeping corporate and labor union money out of politics makes this study especially relevant. Thus, we examine whether the presence or absence of prohibitions on political spending influences electoral and policy outcomes. The scholarly consensus is that campaign finance restrictions tend to have minimal or no impact on political outcomes, although robust debates continue to roil journals in the profession (Mann 2003). 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 political and policy outcomes.

Despite the lack of empirical work on the effect of laws that restrain spending, the campaign finance literature provides solid theoretical grounding to predict the behavior of interest groups. First, we know that corporations -- and unions to lesser extent -- tend to support *incumbents* with their campaign contributions (Lowery and Brasher 2004). At first glance, this finding suggests that restrictions on the political activity of corporations or unions should benefit challengers who suffer financial disadvantages (Abramowitz 1991). Several studies of state legislative elections indicate that contribution limits augment the number of contested races (Hamm and Hogan 2008; Stratmann and Aparicio-Castillo 2006) and decrease incumbent vote shares (Stratmann 2010; Stratmann and Aparicio-Castillo 2006) although one study finds the exact opposite for US Senate races (Lott 2006).² Overall, the research suggests that putting contribution restrictions on corporations and unions in legislative elections should help challengers.

Research on political spending should lead to similar conclusions. While there is some dispute over whether campaign spending has a greater marginal impact on vote shares for challengers (Jacobson 1978; Jacobson 1980), or the same as incumbents (Gerber 1998; Krasno and Green 1988), the logical conclusion is consistent with respect to campaign spending by corporations and unions. Restrictions on these groups should benefit challengers, since they tend to support incumbents.

² One study of gubernatorial elections indicates that increases in competition result from limits on *individual* contributors rather than limits on groups such as corporations, unions or political action committees (Milyo, Primo, and Groseclose 2006).

A second relevant finding from the campaign finance literature deals with the *partisan behavior* of interest groups. Corporations tend to hedge their contribution strategies by giving to members of both parties (Herrnson 2008). This behavior supports an “insider” strategy of gaining access to policy makers, even though corporate interests tend to align with and support the pro-business Republican party (Brunell 2005). Unions, in contrast, tend to pursue an electoral strategy of supporting Democrats -- with an emphasis on incumbents -- who are more supportive of labor union interests (Francia 2006). Additional research shows that labor unions are typically more active than corporations in spending funds directly on campaigns through issue advertising and voter mobilization (Magleby, Monson, and Patterson 2007). These findings suggest that a corporate ban on spending should have minimal effect on partisan outcomes in elections. On the other hand, a ban on political spending by labor unions potentially helps Republicans because unions are active in campaigns and strongly favor Democratic candidates.

A third relevant finding in the literature addresses the link between campaign finance and *public policy*. The vast majority of this research focuses on the relationship between political contributions by PACs and member votes. One meta-analysis of this literature indicates that PAC contributions show relatively few effects on voting behavior (Ansolabehere, deFigueiredo, and Snyder 2003). Some studies have looked more broadly beyond roll call votes to demonstrate that members can act in ways to help contributors through their power of agenda setting and controlling the flow of legislation (Hall and Wayman 1990). One shortcoming of these studies is that they are confined to looking at contributions from PACs; yet members presumably benefit from soft money spending by outside organizations. Previous work on PAC contributions would lead one to conclude that corporate and union spending should have no effect on policy outcomes. On the other hand, outside spending is typically confined to a small number of

wealthy organizations with sophisticated political operations (Apollonio and LaRaja 2004). Most of these organizations tend to be advocacy groups and ideologically-oriented partisan organizations, including labor unions (Magleby, Monson, and Patterson 2007). Given that so few groups compete by spending money on campaigns it is plausible that such groups have outsized influence on policy outcomes, particularly if politicians rely heavily on them or fear them. Labor unions, which typically engage in outside campaigns more than corporations, are especially vulnerable to rules that constrain their electoral activity. We expect laws that restrict labor union spending to improve the policy climate for corporations, which could mean an environment with lower wages and lower corporate taxes. While corporations do not appear to engage in outside spending as much as other groups, we expect laws that restrict them to have the opposite effect.

At this point, we can summarize the likely outcomes of our analysis, keeping in mind that the scholarly consensus is that the effect of campaign finance restrictions on political outcomes is marginal. Overall we expect that a ban on *corporate spending* should hurt incumbents, cause more Republicans to lose office, and reduce tax advantages for corporations. In contrast, a ban on *labor union spending* should also hurt incumbents, but help elect Republicans to office, and improve tax policies that are favorable to corporations.

Methodology

To examine whether corporate spending bans influence political and policy 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 1935 (1948 for Hawaii and Alaska) 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.³ We then researched these laws to determine the date when the law was passed. When a law was passed during an election year, we coded its impact as beginning in the following election year. Twenty-two states had a corporate spending ban in place for at least part of the period, and all but two of those states enacted that ban during this period. These twenty interventions—where states enacted a corporate spending ban when they had not previously had one in place—provide particularly strong leverage for determining the effects of these types of bans.

We focus on three dependent variables that we expect to be influenced by the existence of a corporate spending ban—the partisan balance in the legislature’s lower chamber, the incumbent re-election rate for the legislature, and the percentage of tax revenue that comes from corporations. The data on partisanship in state legislatures, 1935-2009, is from a dataset constructed by Klarner (2003), using *The Book of States*, the *Statistical Abstract of the United States* and other state-specific sources.⁴ We operationalize the partisan balance in the lower chamber as the percentage of seats held by Republicans following the election. Excluded from this calculation are seats held by minor or independent party politicians. We focus on the balance of seats in the lower chamber since, in most states, the lower house has more seats and elections happen more frequently than those for the upper chamber. We expect that Republicans will not perform as well in winning legislative seats after a state enacts a corporate spending ban.

³ Available at <http://www.ncsl.org/legislatures-elections/elections-campaigns/citizens-united-and-the-states.aspx> (Accessed January 20, 2011).

⁴ The Klarner data are updated through 2010 at <http://www.indstate.edu/polisci/klarnerpoltics.htm>. Accessed September 9, 2011.

Data on incumbent re-election rates come from state legislative election returns, 1967-2009, provided by Klarner et al. (2011). The incumbency re-election rate is calculated simply as the percent of all incumbents who ran for either chamber of the state legislature in that year who won re-election. We expect that in states that enact spending bans, incumbents will be less successful in winning elections compared to states where such bans do not exist.

The third variable we incorporate examines whether there are policy consequences for having a corporate spending ban. Specifically, if corporations are able to use independent spending to influence elected officials, then one policy outcome they may wish to effect is the amount of taxes they pay. While we do not have data on corporate tax rates in each state during the period of our study, we do have budget data that indicate how much revenue a state earned from corporate taxes relative to how much tax revenue a state attracted overall. The data are from the US Census Bureau's report series "State Government Finance" (2011) available for 1955-2009. Thus, this third variable is the percentage of all tax revenue collected by a state in a particular year that came from corporate taxes. We expect that this value will be larger in states that enacted a corporate spending ban.

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 undermine the effect of enacting such a ban. In particular, we take account of three other types of policies: bans on contributions from corporations, bans on contributions from unions, and bans on campaign spending by unions. Appendix 1 provides a detailed account of the years during which each state had each type of ban in place. Union contribution and spending bans were less common than similar bans on corporations. Just fifteen states had a union contribution ban in place at some point during the period of our study and none of those states

had the ban in place for the entire period. Union spending bans were also implemented by just 15 states, none of which had the ban for the entire period for which we have data.

Table 1 indicates the frequency with which bans co-occur for observations in our dataset. Several patterns are worth noting. First, the existence of one type of ban in a state is typically a strong predictor of other types of bans also being in place. This is particularly true for the less common types of bans. Bans on union contributions and spending were least common, as we only observed such bans for about one of every ten state-election year dyads. However, states that had enacted these types of bans had almost always enacted bans on corporations as well. Bans on corporate contributions were in place for over half of our observations and there were very few instances where other types of regulations were in place in the absence of such a ban.

Second, while many states had a corporate contribution ban enacted when they did not have a ban on corporate spending, the opposite was never true. In all 316 cases where a state had enacted a corporate spending ban, a ban on corporate contributions was also in place. Thus, in analyzing our data, it is impossible to determine the effects of a corporate spending ban absent a corporate contribution ban.

Fortunately, there are many instances where a corporate spending or contribution ban was in place while similar regulations were 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. Another fourteen states introduced bans on both corporate and union spending at the same time. The plots in Figure 1 show these twenty states where a corporate spending ban was enacted during the period of our study. For each state, the figure indicates when the ban was enacted and it also plots the values of the three dependent variables we examine. Figure 1 reveals no obvious

differences between states with a ban on both corporations and unions versus those with only a ban on corporations. Moreover, at first glance the plots do not appear to indicate that the implementation of a corporate spending ban affects any of the three dependent variables.

[FIGURE 1 ABOUT HERE]

Estimation Approach

We take two different approaches to examining whether the introduction of a corporate spending ban influenced political and policy outcomes in the states. First, we include all 49 states (excluding Nebraska) in a cross-sectional time series regression analysis. The 49 states are panels in these models with the biennial election years serving as the time points. In each model, we include four dummy variables for each type of ban listed in Table 1.

Many scholars have warned about the difficulties entailed in constructing cross sectional time series models (e.g. Stimson 1985; Beck and Katz 1995; Achen 2001; Wilson and Butler 2007) and it is important to note that there is some debate in the literature about the appropriate approach to modeling cross sectional time series data like what we are modeling here. Two important issues are how to appropriately model the temporal dynamics of the data and whether to use a fixed or random effects approach. After considering and testing several different approaches to modeling temporal dynamics, we ultimately settled on the use of a one time period lagged value of the dependent variable in each of the models. These lags allow us to account for the fact that much of the level of these dependent variables is the result of inertia from previous periods. For example, due to high incumbency re-election rates, the Republican share of the lower chamber at time t will generally be a function of the Republican share of seats in that chamber at $t-1$. Similarly, since state revenue streams are likely to change only gradually, in the

absence of some policy intervention, the share of tax revenue received from corporations at time t is likely to be strongly related to the value of that variable at $t-1$. Additionally, in the case of the models predicting Republican success in winning seats in the lower chamber, the lag also helps to account for the likelihood that Democratic-controlled legislatures would be more likely to adopt corporate spending bans than Republican-controlled legislatures.

Of additional importance is the issue of unit heterogeneity and the extent to which fixed or random effects should be incorporated. A random effects estimation approach allows us to estimate the effects of the coefficients both across and within units (states). This may be desirable since it takes advantage of both cross-state and within-state differences in the types of campaign finance laws that are in place. However, in doing so, we assume that the units are independent of each other and that no local factors exist. Even with a lagged value of the dependent variable included in the model, this assumption is not likely to hold across states. Nevertheless, we are also sensitive to concerns that fixed effects models are much more likely to generate estimates finding null compared to random effects estimations. Thus, we present the results from both fixed and random effects models.

Given the “exceedingly frail” (Wilson and Butler 101, p. 121) nature of time-series cross-section analysis, we augment this analysis by conducting 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 ban during the period for which we have data. 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.⁵ 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. Two of the twenty states that enacted a ban between 1937 and 2009 must be excluded from this analysis—Pennsylvania because its ban was enacted too close to the beginning of the series (1937) to allow for a meaningful pre-/post-comparison and South Dakota, which enacted its ban too late in the series (2007).

For each state, we estimate a separate ARIMA model for each of the three dependent variables, amounting to 54 models in all. We specify an AR(1) process for each of these 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 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.⁶ Appendix 2 includes the full results from each of these models. For ease of presentation, here 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

⁵ A particularly strong case for our analysis is Colorado, where the introduction of a spending ban in 2002 resulted from an initiated constitutional amendment passed overwhelmingly by Colorado voters in the general election that year. Since this was one instance where a spending ban was enacted relatively exogenously, rather than by the politicians themselves, it provides additional causal leverage for our analysis. Thus, it is worth noting that the non-effects we observe in Colorado are similar to what we find more generally.

⁶ 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.

present is generated by giving more weight to coefficients that are estimated with more precision (i.e. those with smaller standard errors).

Cross-sectional Time Series Analysis

Table 2 presents results from six models—a fixed and a random effects model for each of the three dependent variables. The sample size for each pair of models differs because some of our dependent variables were not available for the entire time period. Specifically, data on incumbent re-election rates were only available beginning in 1960 and tax revenue data was only available from 1942 onward. In each model, the lagged value of the dependent variable was statistically significant and relatively strong. The inclusion of this lag in the model also contributed to relatively high R-squared values for most of the models.

[TABLE 2 ABOUT HERE]

Overall, the coefficients for the different types of campaign finance laws are relatively small and in most cases lack statistical significance. To demonstrate this point, Figure 2 plots the predicted values of each dependent variable for three different conditions—the absence of either corporate or union spending bans, the presence of corporate spending bans, and the presence of corporate and union spending bans.⁷ For each prediction, we set the conditions to a condition where the state already had both corporate and union contribution bans in place. The lag was set to 50% for the party balance model, 90% for the incumbency model, and 10% for the corporate tax model. The predictions were generated from the fixed effects models.

[FIGURE 2 ABOUT HERE]

⁷ Recall that there is only one state where there was a union spending ban but not a corporate spending ban. Given the limited sample size, we do not generate predictions for this condition.

With regard to party balance, the model predicts that when a state had union and corporate contribution bans but no spending bans on either type of entity, that Republicans would control 46.5% of the seats in the lower chamber. Adding a corporate spending ban generated an almost identical prediction (45.9%), and having both types of spending bans in place also generated a very similar prediction (48.8%). In each condition, the confidence intervals largely overlap, indicating that we cannot be confident that there would be any difference in the partisan balance in the legislature under a corporate (or union) spending ban.

Similarly small and statistically indistinguishable predictions were generated from the other models as well. Predictions from the fixed effects incumbency model indicated that incumbents were about 3 percentage points more successful in winning re-election when a corporate spending ban was enacted, but no more or less successful at doing so when both types of spending bans were in place. The predictions from the corporate spending ban model were also very close regardless of the types of spending bans that were in place.

Thus, overall, the results from the cross sectional time series analyses suggest that corporate spending bans have little, if any effect on political and policy outcomes. The one factor that corporate spending bans did appear to influence was incumbency re-election rates. Incumbents were about 3 percentage points more successful in running for re-election when a state had a corporate spending ban, but not a spending ban on unions. Aside from this modest effect, the coefficients for the key variables were small and lacked statistical significance. Of course, as we note above, our approach to modeling the cross sectional time series data may be at least partially responsible for the small coefficients detected. Specifically, the use of a lagged value of the dependent variable in conjunction with fixed effects may have served to dampen the effect of the dummy variables indicating the types of laws in place in each state (Achen 2001).

Thus, in the following section, we conduct a state-by-state analysis to conduct a more fine-grained test of the effects of a corporate spending ban intervention.

State-by-State Intervention Analysis

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 presented in this figure. The first plot includes the six states that implemented a corporate spending ban 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 -1.57 for the model estimated with data from Connecticut. However, the standard error for the confidence interval for this estimate is quite large, which means that Connecticut is only given .39% of the total weight in calculating the summary figure. In contrast, the estimate for Massachusetts was much more precise, which meant that a great deal of weight was placed on that estimate when calculating the summary score at the bottom of the figure. While the weighted average is a useful summary statistic, we caution against assigning too much importance to it. Thus, in our discussion below, we are careful to also discuss the effect of the intervention state-by-state.

[FIGURE 3 ABOUT HERE]

Of the six coefficients plotted in the first panel of Figure 3, three are in the (expected) negative direction, while three are in the positive direction. However, the coefficients were quite small in all six states, and in only one case—Massachusetts, where the coefficient was actually positive—could we be even 90% confident that the coefficient was different from zero. The weighted average of these coefficients is also small and statistically indistinguishable from zero. Thus, we cannot be confident that corporate spending bans reduced Republican success in winning seats to the state legislature. In fact, it is worth noting that the 95% confidence interval for this average has a lower bound of $-.37$, indicating that even if the ban hurts Republicans, the effect is so small as to be negligible.

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—five coefficients are positive while seven are negative. There was a statistically significant negative coefficient for the implementation of both bans in Arizona, indicating that Republicans fared worse after the bans were implemented in that state. However, in every other state, the size of the coefficient was small and not statistically significant. Accordingly, the weighted average for the twelve states is just $-.28$ and the small 95% confidence interval around this estimate indicates that we can be confident that implementing a union and corporate spending ban concurrently was unlikely to produce any substantial changes in the partisan balance of a state's legislature.

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 Wisconsin (3rd most professional legislature), Massachusetts (fifth), Michigan (sixth), and Ohio (seventh). In none of

these states did the coefficient for the intervention differ significantly from zero, a pattern that is repeated in the other analyses as well.

Figure 4 presents the same type of plots for the dependent variable measuring the percentage of incumbents who won re-election at each time point. For five of the six states, the models estimated a negative coefficient for the intervention variable. The largest (and only statistically significant) effect was for Kentucky, where the model predicts that the implementation of the corporate spending ban led to a 7.9 percentage point decrease in the percentage of Republican seats in the lower chamber. However, in other states the coefficients were smaller and the weighted average effect produced by the meta-analysis was -2.91. Thus, based on the models estimated for these six states, we can be confident that corporate spending bans reduced the incumbent re-election rate by nearly 3 percentage points. This finding conforms to theoretical expectations that restrictions on corporations, which typically support incumbents, should help challengers fare better in elections. However, the second plot in Figure 4 suggests that corporate spending bans are only effective in reducing re-election rates if they are instituted without union spending bans. The coefficients for these twelve states were typically small and statistically indistinguishable from zero and the weighted average effect was estimated to be almost zero (.12).

[FIGURE 4 ABOUT HERE]

Finally, Figure 5 produces plots for coefficients from the models examining whether corporate spending bans lead to increases in corporate tax revenue. In the first set of states where only corporate spending bans were introduced, we find small regression coefficients that are generally statistically indistinguishable from zero. Only the model estimate for Iowa produced a statistically significant coefficient, with the results indicating that the corporate spending ban led

to an increase in corporate tax revenue of approximately 1 percentage point. The weighted estimate for all six states was just .24, indicating that implementing corporate spending bans had marginal, if any, effects on the share of a state's tax revenue that came from corporations. The estimates in the second plot are similarly small and, in all but one case, lack statistical significance. Like Iowa, the model for Wisconsin estimates that the implementation of both a corporate and a union spending ban concurrently led to about a 1 percentage point increase in corporate tax revenue. However, the weighted average for these twelve states is again small and not statistically distinguishable from zero.

[FIGURE 5 ABOUT HERE]

Overall, the results from this section are consistent with those from the cross sectional time series analysis. Corporate spending bans appeared to have little if any effect on the political and policy outcomes we examined. 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. Incumbent re-election rates did decrease when a corporate spending ban was enacted, but only when such a ban existed absent a ban on union spending. When both bans were in place, there was little difference in the success of incumbents seeking re-election. Finally, the implementation of a corporate spending ban did little or nothing to alter the tax burden of corporations in a state.

Conclusion

Drawing on decades of data on campaign finance laws in the American states, our largely null findings cast doubt on several dire forecasts about the consequences of *Citizens United*. Spending bans did not appear to be effective in altering the partisan balance in legislatures nor

did it have any measurable effect on the extent to which states drew revenue from corporations. Though we detected some tendency for spending bans to influence the incumbent re-election rate, the effects were not large. 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 three outcomes – partisan control of government, incumbency reelection rates and corporate taxation. While these measures allow us to gain a sense of how spending bans affect the partisan balance in government, the extent to which spending bans may influence turnover in government, and one important policy outcome of interest to corporations, 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 other policy outcomes such as labor laws. 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 or policymaking in this study, even in the states with larger economies and more professional legislatures.

Despite these limitations, our findings are important for understanding the role of campaign finance restrictions on macro political and policy outcomes. Indeed, previous studies have looked at the relationship between political contributions and votes, but have not examined directly how variations in laws influence aggregate political or policy outcomes. Even before the *Citizens United* decision, interest groups engaged directly in electoral politics (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). Thus, previous studies, which focus only on political contributions to candidates, may miss broader dynamics of influence through other campaign activities. Notably, however, our findings are consistent with the literature on candidate financing and the broader scholarly consensus that campaign finance laws tend to have a marginal impact on outcomes.

We offer two explanations for why the laws we examined likely have minimal impact. First, interest groups are highly adaptable. Research shows 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; 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. 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 second 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.

In the aftermath of the *Citizens United* decision we expect business and labor interests to exploit opportunities to influence elections by raising large sums of campaign money from corporations, unions and wealthy individuals. Candidates and party organizations will likely seek ways to compensate for the relative disadvantage of having to raise funds with limits on both the source and size of the contribution. “Super PACs” that are loosely affiliated with candidates will emerge as key players in campaigns. Since our findings suggest that bans on spending do not affect key electoral outcomes, future reforms might instead focus on improving transparency and accountability. In addition to requiring greater disclosure from outside groups, new laws might be crafted to help channel additional funds through more accountable political committees affiliated clearly with the political parties and candidates.

Table 1: Co-occurrences of Bans in Dataset

Situation	Total Cases	Cases with corporate spending ban in place	Cases with corporate contribution ban in place	Cases with union spending ban in place	Cases with union contribution ban in place
Corporate contribution ban	955 (55.1%)	316 (33.1%)		184 (19.3%)	144 (15.1%)
Corporate spending ban	316 (18.2%)		316 (100%)	173 (54.8%)	133 (42.1%)
Union contribution ban	148 (8.5%)	144 (89.9%)	133 (97.3%)	148 (100%)	
Union spending ban	188 (10.8%)	173 (92.0%)	184 (97.9%)		148 (78.3%)

Table 2: Results from Cross-Sectional Time Series GLS Regression Models

Variable	% Republican (Random Effects)	% Republican (Fixed Effects)	% Incumbents Re-elected (Random Effects)	% Incumbents re-elected (Fixed Effects)	% Revenue from Corp. Taxes (Random effects)	% Revenue from Corp. Taxes (Fixed effects)
Lag	0.87* (0.02)	0.72* (0.05)	0.49* (0.04)	0.25* (0.05)	0.89* (0.02)	0.79* (0.04)
Corporate spending ban	-1.17 (0.89)	-0.57 (1.55)	1.43 (0.91)	3.31* (0.65)	-0.18 (0.21)	-0.81 (0.41)
Union spending ban	3.22* (0.83)	2.85 (2.55)	-0.65 (1.56)	-2.21* (0.95)	0.53 (0.40)	1.43* (0.71)
Corporate contrib. ban	0.07 (0.63)	-1.85 (1.44)	-1.42* (0.49)	-1.83 (1.16)	0.02 (0.11)	-0.06 (0.28)
Union contrib. ban	-1.64* (0.53)	-1.02 (1.80)	0.99 (1.32)	2.10* (0.84)	-0.25 (0.42)	-0.54 (0.57)
Intercept	5.75* (1.06)	13.33* (1.98)	48.34* (3.77)	70.43* (3.05)	0.72* (0.17)	1.39 (0.25)
R-squared (overall)	0.80	0.79	0.28	0.24	0.81	0.81
N	1685	1685	909	909	1518	1518

Note: Robust standard errors in parentheses.

Figure 1: Plots Showing Proportion of Republican Seats in Lower Chamber for States that Changed Their Laws Regarding Corporate Spending

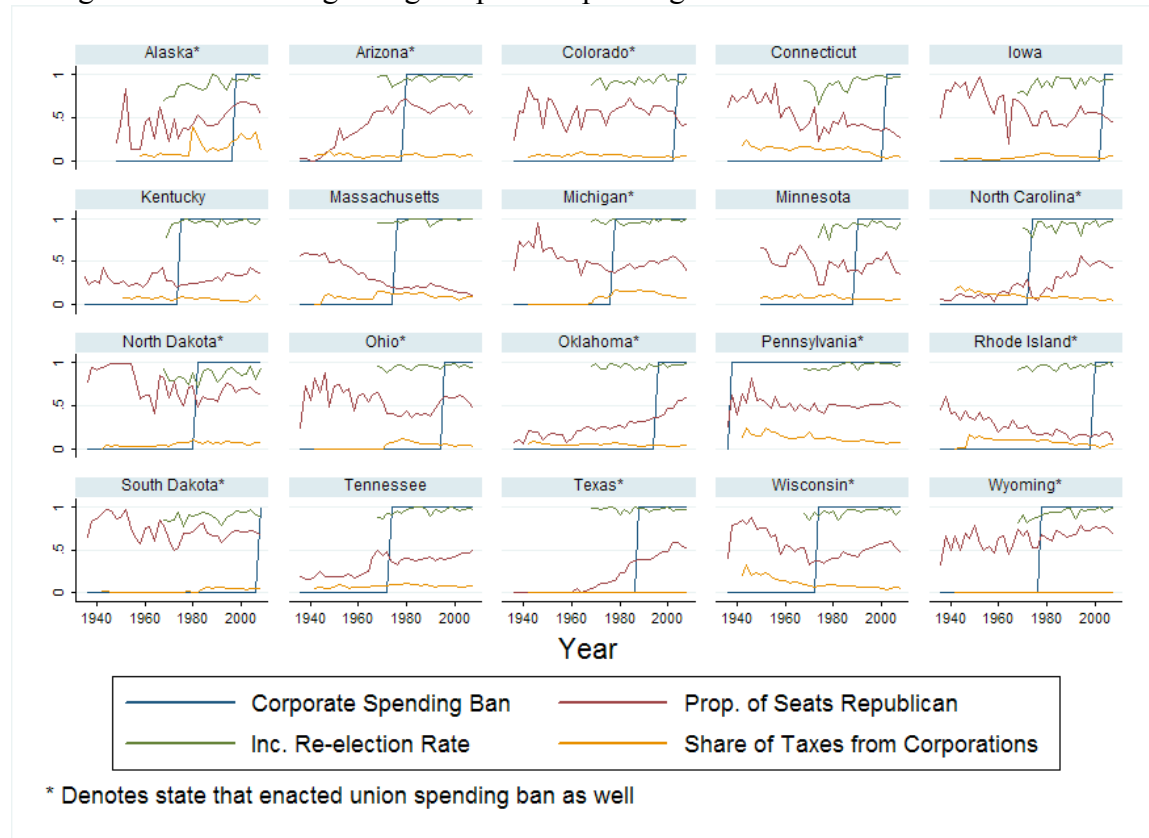


Figure 2: Predict Effects from Pooled Cross Sectional Time Series Models

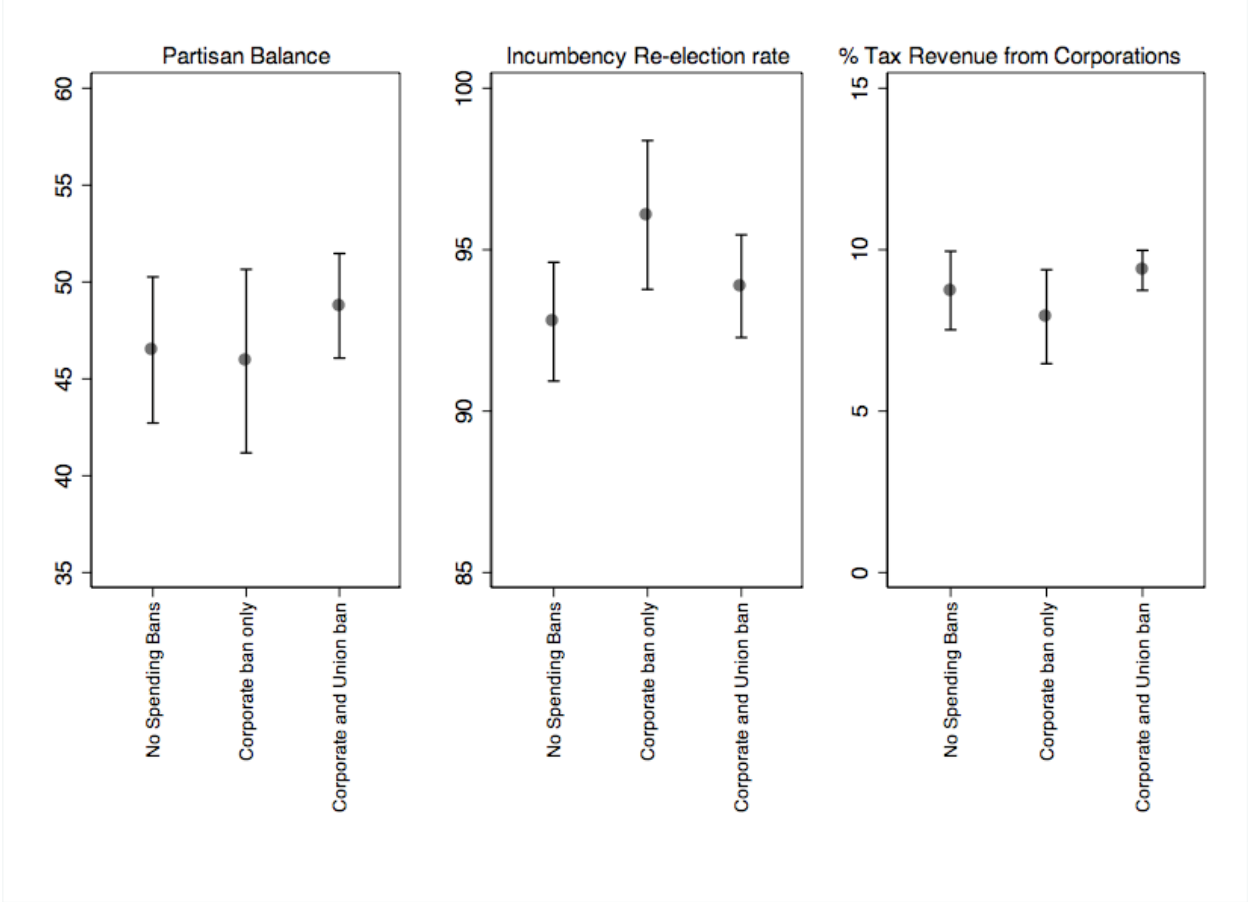


Figure 3: Summary of Effects of Corporate Spending Ban on Republican Share in Lower Chamber From State-by-State ARIMA Models

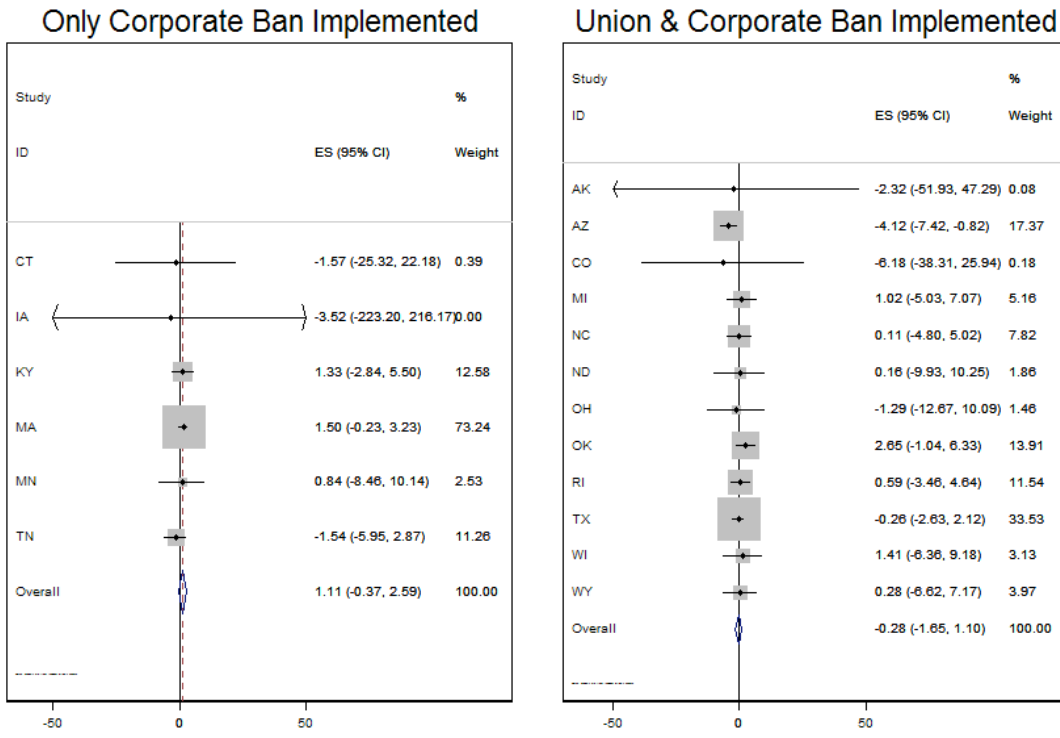


Figure 4: Summary of Effects of Corporate Spending Ban on Incumbent Re-election Rate From State-by-State ARIMA Models

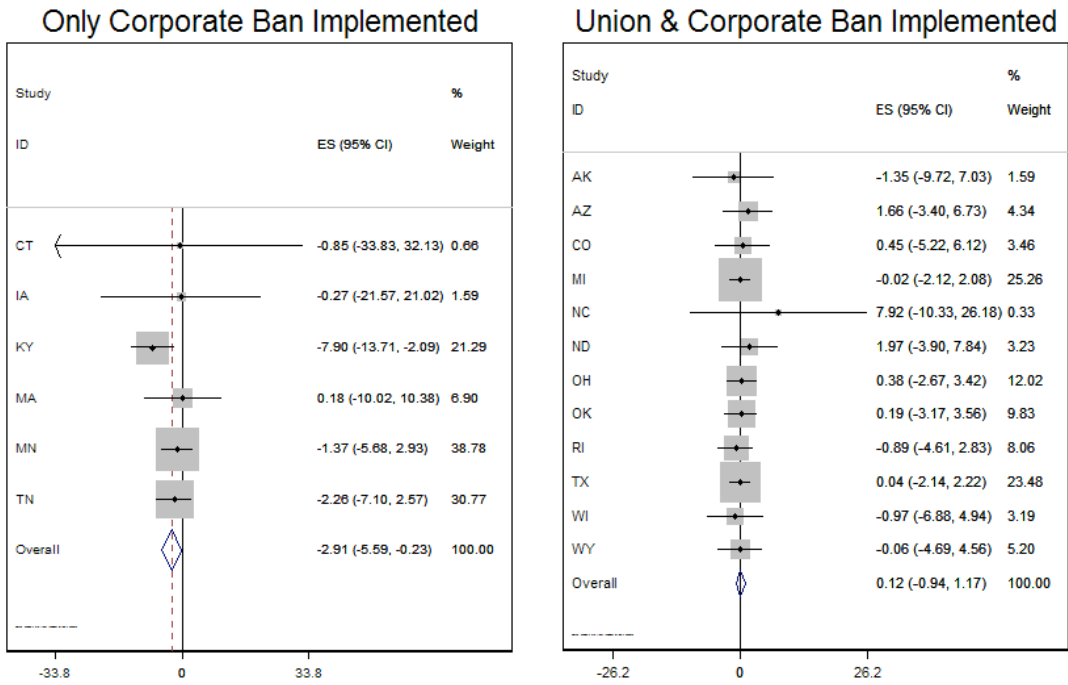
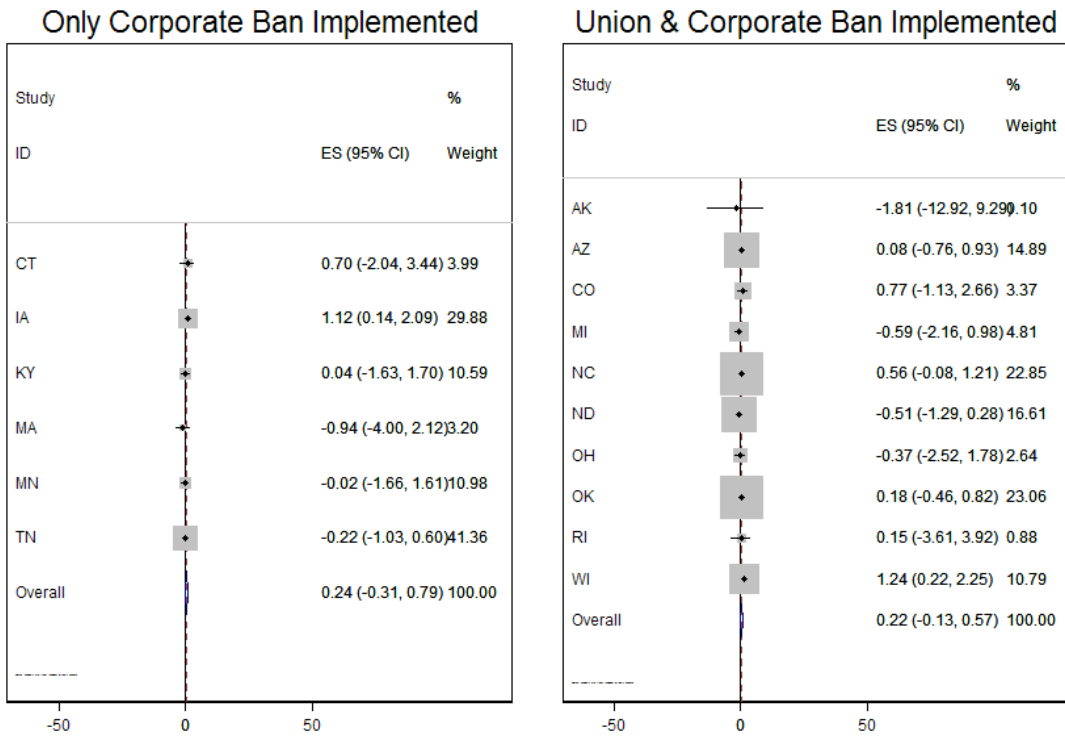


Figure 5: Summary of Effects of Corporate Spending Ban on Percentage of Revenue from Corporate Taxes From State-by-State ARIMA Models



Appendix 1

Table A1: Details on When States Had Restrictions in Place

State	Corp. Spending Ban	Union Spending Ban	Corp. Contribution Ban	Union Contribution Ban
Alabama			1915-1981	
Alaska	1996-2010	1996-2010	1996-	1996-
Arizona	1978-2010	1978-2010	1913-	1979-
Arkansas				
California				
Colorado	2002-2010	2002-2010	1909-1963, 2002-	2003-
Connecticut	2000-2010		1907-	
Delaware				
Florida			1897-1967	
Georgia			1908-1968	
Hawaii			1913-1973	
Idaho				
Illinois				
Indiana			1911-1976	
Iowa	2003-2010		1907-	
Kansas				
Kentucky	1974-		1891-	
Louisiana			1915-1975	
Maine				
Maryland			1908-1968	
Massachusetts	1975-2010		1907-	
Michigan	1976-	1976-	1913-	1976-
Minnesota	1988-2010		1912-	
Mississippi			1908-1978	
Missouri			1897-1978	
Montana	1912-		1912-	
Nebraska			1897-1976	
Nevada				
New Hampshire		1979-	1911-2000	1979-
New Jersey				
New Mexico				
New York			1906-1974	
North Carolina	1973-2010	1973-2010	1934-	1973-
North Dakota	1981-	1981-	1907-	1995-
Ohio	1995-	1995-	1908-	2005-
Oklahoma	1994-	1994-	1907-	2007-
Oregon			1909-1983	
Pennsylvania	1937-	1937-	1906-	1980-
Rhode Island	1998-	1998-	1992-	1988-
South Carolina				
South Dakota	2007-2010	2007-2010	1907-	2007-
Tennessee	1972-2010		1897-	
Texas	1987-	1987-	1907-	1987-
Utah			1917-1971	
Vermont				
Virginia				
Washington				
West Virginia	1908-2010		1908-	
Wisconsin	1973-	1973-	1905-	1973-
Wyoming	1977-	1977-	1911-	1977-

Appendix 2

Table A2: ARIMA Models Estimating Republican Share of Lower Chamber Seats

Variable	CT	IA	KY	MA	MN	TN	AK	AZ	CO	MI	NC	ND	OH	OK	RI	TX	WI	WY
Intervention	-1.57 (12.12)	-3.52 (112.09)	1.33 (2.13)	1.5 (0.88)	0.84 (4.74)	-1.54 (2.25)	-2.32 (25.31)	-4.12 (1.68)	-6.18 (16.39)	1.02 (3.09)	0.11 (2.51)	0.16 (5.15)	-1.29 (5.81)	2.65 (1.88)	0.59 (2.06)	-0.26 (1.21)	1.41 (3.96)	0.28 (3.52)
Intercept	-1.04 (1.35)	-0.13 (2.37)	-0.48 (0.9)	-2.03 (0.45)	-1.37 (2.39)	1.67 (1.46)	1.52 (2.77)	3.27 (0.98)	0.71 (1.8)	-0.75 (1.26)	0.99 (2.1)	-0.58 (2.28)	0.38 (1.33)	0.95 (0.54)	-1.21 (0.72)	1.47 (0.92)	-0.73 (1.53)	0.59 (1.37)
ARMA																		
AR(1)	-0.59 (0.15)	-0.37 (0.13)	-0.33 (0.16)	-0.37 (0.19)	0.02 (0.36)	-0.02 (0.2)	-0.36 (0.12)	-0.38 (0.15)	-0.38 (0.17)	-0.62 (0.09)	-0.28 (0.16)	-0.30 (0.19)	-0.72 (0.09)	-0.4 (0.21)	-0.58 (0.13)	0.01 (0.26)	-0.3 (0.14)	-0.55 (0.13)
MA																		
Sigma	11.35 (1.55)	15.66 (1.49)	5.91 (0.61)	2.79 (0.39)	9.85 (1.36)	4.84 (0.51)	19.31 (1.96)	5.53 (0.55)	13.67 (1.59)	9.32 (0.98)	7.11 (0.64)	12.87 (1.33)	11.75 (1.41)	4.21 (0.42)	5.82 (0.85)	3.34 (0.39)	9.1 (0.68)	10.61 (1.18)
N	35	36	36	36	29	36	30	36	36	36	36	36	36	36	36	36	36	36

Table A3: ARIMA Models Estimating Percentage of Tax Revenue Coming from Corporations

Variable	CT	IA	KY	MA	MN	TN	AK	AZ	CO	MI	NC	ND	OH	OK	RI	WI
Intervention	0.7 (1.4)	1.12 (0.5)	0.04 (0.85)	-0.94 (1.56)	-0.02 (0.84)	-0.22 (0.42)	-1.81 (5.67)	0.08 (0.43)	0.77 (0.97)	-0.59 (0.8)	0.56 (0.33)	-0.51 (0.4)	-0.37 (1.1)	0.18 (0.33)	0.15 (1.92)	1.24 (0.52)
Intercept	-0.55 (0.36)	0 (0.2)	-0.05 (0.47)	0.77 (0.84)	-0.09 (0.29)	0.21 (0.28)	0.84 (2.94)	-0.03 (0.26)	-0.01 (0.21)	0.5 (0.92)	-0.72 (0.27)	0.39 (0.35)	0.15 (0.52)	-0.11 (0.16)	0.14 (1.19)	-1.32 (0.3)
ARMA																
AR(1)	-0.35 (0.15)	0.11 (0.22)	-0.38 (0.24)	0 (0.35)	-0.29 (0.17)	-0.02 (0.18)	-0.21 (0.31)	-0.54 (0.14)	-0.12 (0.18)	0.04 (0.28)	-0.83 (0.07)	-0.44 (0.19)	0.21 (0.24)	-0.28 (0.21)	-0.29 (0.6)	-0.95 (0.03)
MA																
Sigma	2.41 (0.37)	0.86 (0.1)	1.68 (0.27)	2.82 (0.35)	1.83 (0.27)	1.15 (0.18)	9.05 (1.14)	1.68 (0.21)	1.24 (0.18)	1.96 (0.2)	1.35 (0.21)	1.51 (0.25)	1.39 (0.12)	0.92 (0.15)	2.93 (0.34)	2.02 (0.25)
N	32	33	28	33	29	33	25	33	33	33	33	33	33	33	33	33

NOTE: Texas and Wyoming are excluded from this analysis because they reported no corporate tax revenue during the entire series.

Table A4: ARIMA Models Estimating Percent of Legislators Re-elected

Variable	CT	IA	KY	MA	MN	TN	AK	AZ	CO	MI	NC	ND	OH	OK	RI	TX	WI	WY
Intervention	-0.85 (16.83)	-0.27 (10.86)	-7.9 (2.96)	0.18 (5.21)	-1.37 (2.2)	-2.26 (2.47)	-1.35 (4.27)	1.66 (2.59)	0.45 (2.89)	-0.02 (1.07)	7.92 (9.31)	1.97 (3)	0.38 (1.55)	0.19 (1.72)	-0.89 (1.9)	0.04 (1.11)	-0.97 (3.01)	-0.06 (2.36)
Intercept	0.39 (1.9)	0.79 (0.87)	7.94 (2.93)	0.07 (5.11)	1.25 (1.44)	2.64 (2.43)	1.66 (1.98)	-1.28 (2.27)	0.22 (1.16)	0.14 (0.93)	-7.14 (9.12)	-1.31 (2.49)	-0.21 (0.89)	0 (0.59)	0.4 (0.8)	-0.07 (0.68)	1.2 (3.23)	0.76 (1.93)
ARMA																		
AR(1)	-0.05 (0.18)	-0.57 (0.2)	-0.71 (0.26)	-0.31 (0.23)	-0.75 (0.17)	-0.46 (0.29)	-0.12 (0.34)	-0.3 (0.36)	-0.51 (0.35)	-0.32 (0.25)	-0.53 (0.19)	-0.56 (0.2)	-0.02 (0.24)	-0.46 (0.29)	-0.32 (0.29)	-0.58 (0.22)	-0.77 (0.25)	-0.58 (0.28)
MA																		
Sigma	7.64 (0.96)	5.7 (0.91)	3.36 (0.83)	2.39 (0.31)	5.82 (1.23)	3.29 (0.58)	6.27 (1.25)	4.1 (1.13)	5.15 (0.84)	2.84 (0.68)	6.9 (1.28)	7.99 (2.26)	3.25 (0.76)	3.28 (0.56)	3.8 (1.1)	2.85 (0.43)	3.63 (0.48)	4.38 (0.8)
N	35	36	36	36	29	36	30	36	36	36	36	36	36	36	36	36	36	36

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