

**Assessing the Potential Effects of *Citizens United*:
Policy and Corporate Governance in the States**

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Abstract

The U.S. Supreme Court's 2010 decision in *Citizens United v. FEC* was one of its most controversial in decades. Critics of the decision argued it would lead to a flood of corporate cash that would warp electoral and policy outcomes. We test for this possibility by exploiting variation in state-level campaign finance laws from 1977 through 2006. Through an analysis of three outcomes of interest to business over three decades—state elected officials' liberalism; relative minimum wage rates; and antitakeover laws—we find mixed evidence for critics' claims. Campaign finance regimes appear to have little impact on overall levels of policy liberalism and relative minimum wage rates, but they do appear to have a significant effect on corporate governance: when businesses are allowed to engage in unlimited independent expenditures, antitakeover laws shift in favor of management. These findings suggest that those critics of corporate political activity who believe that, above all else, it represents an agency problem for shareholders have the most to fear from the decision's potential effects.

The Supreme Court's 2010 ruling in *Citizens United v. Federal Election Commission* set off a firestorm of criticism, largely centered on the ruling's provisions that allowed corporations to make unlimited independent expenditures to influence electoral outcomes. Unlike money contributed directly to candidates or political parties, the Court's majority opinion reasoned that such independent expenditures did not lead to the appearance or presence of corruption, meaning the state had no constitutionally compelling justification for limiting this spending.¹ Subsequent decisions by lower federal courts and the Federal Election Commission have faced similar criticism for allowing corporate actors, as well as unions and individuals, to contribute unlimited sums of money to independent expenditure-only committees, a category that includes so-called Super Political Action Committees (Super PACs). Depending on their chosen route of engagement, independent spending by firms and others may go undisclosed to the public and shareholders or may be disclosed only long after they occur and then, only if they meet a certain financial threshold. The end result of these court rulings and administrative actions is that corporations, unions, organizations, and individuals have all been granted more latitude to engage in electoral politics financially than at any time since the 1940s (Briffault 2012).

In popular discourse and among its critics, *Citizens United* is linked to the notion that American politics is experiencing a "corporate takeover" that will result in public policy favoring the desires of business and its allies. More generally, these critics argue that the enlarged arena for corporate electoral involvement threatens the core democratic precept of equal opportunity to influence government (see, e.g., Bai 2012; Dworkin 2010). Proposals to address the fallout of the decision have ranged from requiring more comprehensive disclosure by government contractors and publicly held firms, to amending the Constitution to overturn the Court's equivalence of money with speech and of the speech rights of corporate persons and individuals.

¹ *Citizens United v. Federal Election Commission*, 130 S. Ct. 876, 899–913 (2010).

Missing from both the case law and the popular discourse is hard evidence about the political and policy impacts of campaign finance laws. Not only do reformers rest their claims of untoward business influence on conjecture, but so too did the Court in the case, when it asserted as a matter of established fact that independent expenditures do not lead to corruption or the appearance of it. In part, this result is a failure of social science, as little existing empirical work in political science, law, economics, or management addresses the question of how campaign finance regimes shape public policy outcomes of interest to business.

Given the newness of *Citizens United*, as well as the lack of required disclosure of many of the activities allowed by the case, we cannot conduct a rigorous examination of the decision's effects on national politics, particularly on public policymaking. However, federalism allows us to gain some traction on the potential policy impacts of *Citizens United*. Over the last century, the states have experimented with various forms of campaign finance reform, including bans on corporate independent expenditures. In this paper we exploit this variation in expenditure bans to gain analytical leverage on policy outcomes we might anticipate in the ruling's aftermath at the national level and in those states whose bans were invalidated. In particular, we are interested in three outcomes of interest to corporations – policy liberalism, in the form of left party power; relative minimum wage rates; and antitakeover laws – and whether or not these outcomes vary systematically at the state level with the manner in which states allow these actors to participate in electoral politics.

This study proceeds in four parts. First, we provide a brief background on the theoretical power of business in American politics, focusing on the effects of campaign contributions, and examine how historical variation in campaign finance regimes at the state level provides us with leverage to assess the potential policy effects of *Citizens United* at the national level. Second, we

introduce our policy outcomes of interest and our data sources. Third, we discuss our hypotheses, identification strategy, and modeling approach. And, fourth, we empirically test these hypotheses through analyses of state-level panel data covering the period from 1977–2006. We find that campaign finance laws have minimal effects on aggregate policy outcomes, with the important exception of antitakeover laws significantly shifting in management’s favor when businesses can engage in unlimited independent expenditures.

1. Business Power and Campaign Finance

Concerns over business’ influence on, and advantages in, the public policymaking process in the United States have been ever present. However, following the counter-mobilization of business in the wake of the public interest movements of the 1960s and 1970s (Vogel 1989) and the concurrent and continuing decline of labor unions (Francia 2006), many scholars, politicians, and popular writers have intensified their criticisms. In particular, these critics have focused on what they perceived as the outsized instrumental power of business, as exercised through campaign contributions, lobbying, and corporate philanthropy. That is, policymakers in the elected branches are so dependent on the resources that capital can provide them – in funding their campaigns, in providing information as part of the policy process, and in supporting the creation and maintenance of quasi-public goods, such as cultural institutions and universities – that they are reluctant to enact policies that are perceived as unfriendly to business for fear of electoral reprisal or a lack of electoral support (Lindblom 1977, Dryzek 1996).

The most extensive criticisms in this vein contend that business’ instrumental power, especially in comparison to labor, has become so great that the United States is devolving from a democracy into an oligarchy (Bartels 2008; Winters and Page 2009; Gilens 2012). For example,

Hacker and Pierson (2010) argue that since the early 1970s members of Congress have enacted regulatory and tax policies that overwhelmingly favor capital over labor due to campaign contributions from the former and the declining political power of the latter. Johnson and Kwak (2010) claim that the growing instrumental power of business, especially that of the financial sector, from the 1980s onward produced not only the regulatory policies that culminated in the recent financial crisis but also the allegedly toothless responses to that crisis, including the Dodd–Frank Act.

1.1 The Instrumental Power of Business: Lobbying and Campaign Donations

The empirical literature on campaign contributions and lobbying casts doubt on many of these claims. At the macro- and micro-levels, respectively, Smith (2000) and Hall and Wayman (1990) both find that when the interests of business clash with public opinion on salient policy issues, it is the public's preferences that tend to prevail. This dynamic is in part a result of business being a far more diverse and divided category of interests than many critics acknowledge (Werner and Wilson 2010). Differences exist not only between large and small firms but also across sectors, between labor and capital-intensive firms, between market incumbents and upstarts, and between export-oriented and non-export-oriented firms. Perhaps even more importantly, the strength of public preferences is also due to the relatively small role corporate and, more generally, organizational contributions have played in electoral politics in comparison to individual contributions (Ansolabehere, Snowberg, and Snyder 2005). Consequently, corporate PACs have lacked outsized power in the overall donor pool (Ansolabehere, de Figueiredo, and Snyder 2003).

Nevertheless, firms' PACs do contribute to political campaigns, and three perspectives are offered to explain this potentially puzzling behavior, given the lack of demonstrated effectiveness of the contributions in affecting electoral outcomes. First, some scholars view giving as a strategically motivated activity that benefits firms and industries (Snyder 1990; Grier, Munger, and Roberts 1994; Kroszner and Stratmann 1998; Stratmann 1995, 1998). These studies suggest that firms' patterns of giving reflect rational behavior to the point that the marginal cost of giving equals its marginal benefit. A second perspective views campaign contributions as more akin to a classic consumption good for executives than as an investment by the firm (Ansolabehere, de Figueiredo, and Snyder 2003). Firms' giving is thus neither particularly beneficial nor terribly costly in this view. A third school of thought claims that firms gain little from their PACs' contributions and argues that campaign contributions and other expenditures related to political activity are a manifestation of the principal-agent problem in which managers engage in politics at their shareholders' expense (Keim and Zardkoohi 1988; Wright 1990; Bebchuk and Jackson 2010; Coates 2012).

This last perspective is particularly important for understanding corporate behavior in the post-*Citizens United* environment. The independent expenditures that firms can engage in now can come directly from their corporate treasuries, unlike PAC monies, and may easily dwarf the amounts corporate PACs can give to individual candidates or overall, which can increase incentives for managers to increase their political involvement. Agency problems are further exacerbated to the degree that firms' managers use their instrumental political leverage to advocate for public policies that entrench management decision-making from shareholders' challenges. Winkler (2004) found that addressing such agency problems was a key motivation behind many of the Progressive Era campaign finance reforms at the federal level (i.e., the

Tillman Act of 1907), and Vogel (1978) argues that the policy preferences of American managers historically have reflected potential policies' impacts on managerial autonomy.

In contrast to the findings that campaign finance donations either produce insignificant or negative returns for firms, lobbying appears to pay greater dividends as a political instrument. Lobbying is not aimed at affecting the composition of policymaking bodies but rather the composition of the policy agenda and the substance of policy outcomes. Evidence suggests that the advantages that business has over labor in lobbying do have a payoff for corporate interests. Firms play a substantial role in shaping the policy agenda (Baumgartner et. al. 2009), and some outcomes of interest to business, such as effective tax rates (Richter, Samphantharak, and Timmons 2009) and contract awards (Goldman, Rocholl, and So 2009), vary significantly with lobbying expenditures and connections.

Building off of these literatures, we explore whether or not *Citizens United* and the legal and regulatory decisions that followed the case might alter our understanding of the instrumental power of business, given the newfound ability of corporations and allied interests both to spend directly without limits and to donate unlimited amounts to independent-expenditure committees.

Preliminary empirical explorations of *Citizens United*'s consequences for candidate and group behavior (Franz 2011), for electoral outcomes (Coleman 2010; La Raja and Shaffner 2012; cf. Klumpp, Mialon, and Williams 2012),² and in the financial markets (Werner 2011) have found little to no effects (or potential for effects) from the decision. Yet, since we can view *Citizens United* as altering the potential leverage of businesses in the pool of campaign financiers, the policy effects of the decision may be more implicit than explicit.

² The results of Coleman and La Raja and Shaffner parallel those of Gross and Goidel (2003), who examine campaign finance regulations and electoral competition generally in the states. Focusing on contribution limits (corporate, union, individual, PAC, family member, and candidate) and public financing (to parties and to candidates), Gross and Goidel find minimal impact of these regulations on partisan electoral outcomes.

1.2 Variation in State Campaign Finance Regimes

Campaign finance laws and regulations in the states changed dramatically across 1977–2006, the period that encompasses our three analyses. In the period following Watergate, states, like the federal government, added a range of new restrictions on candidates, parties, interest groups, and individual contributors (Parker and Coleman 2004).

Malbin and Gais' (1998) and Gross and Goidel's (2003) overviews of state campaign finance show how significantly these regimes have changed as well as some of the motivations and interests behind these changes. A common pattern was for a sharp increase in the number of states adopting a regulation in the 1970s and gradual growth in the number after that. According to Malbin and Gais (1998), in 1972 only 2 states limited individual contributions to state candidates. That number jumped to 21 states by 1980. And by 1996, 35 states did so. The number of states limiting PAC contributions doubled from jumped from 9 in 1976 to 32 in 1996, while limits on contributions from parties were in place in 3 states in 1976 and 19 states by 1996. Four states had public funding (to some degree) of elections in 1974; more than 5 times as many did in 1996.

Across the states, therefore, there was a general trend toward adding new restrictions, but the basket of restrictions in place across the states varied widely. Not only did states vary by the presence or absence of various regulations, but they also varied substantially in the nature of these regulations, such as the allowable dollar limits of individual contributions. One of the few areas that did not see substantial change in the number of states with regulations in place was campaign finance disclosure. By 1970, 45 states already had reporting and disclosure

requirements in place for state candidates (Gross and Goidel 2003). The exact requirements for disclosure, however, varied.

As Milyo and Primo (2006) suggest, at this point we may be at the “mature” stage of campaign finance regulation, with certain restrictions and requirements having become the norm around the country. Individual contribution limits and disclosure would be examples of these common restrictions and requirements, respectively. The recent actions of the Supreme Court and lower federal courts, however, have led to some disturbance in these mature regimes. In June 2012, the Court struck down restrictions on corporate independent expenditures in Montana, making it clear the majority on the Court believed the logic of *Citizens United* applied to state-level elections, and that the particular circumstances within states – Montana pointed to a history of corruption that preceded the ban on corporate spending – did not override the First Amendment considerations at the root of *Citizens United*. The conviction of the Court majority that aspects of the federal and state campaign finance regimes intrude on free speech and associational rights may disrupt other aspects of campaign finance regulation in future years.

2. Dependent Variables

We examine three state-level policy outcomes of interest to business over 30 years. We measured each policy outcome individually, using the state-year dyad as our unit of analysis. The three subsections that follow detail the specific indicators we employed to measure left party power, relative minimum wage rates, and antitakeover laws.

2.1 Left Party Power

Our first dependent variable is left party power, which we employ as a proxy for policy liberalism. As Kelly (2009) and Kelly and Witko (2012) note, the concept of left party power originates in the power resource theory literature in comparative public policy (see, e.g., Huber and Stephens 2001) and attempts to capture how low income groups will fare in terms of governmental policymaking. To measure left party power, we used the NOMINATE version of Berry et al.'s institutional ideology score by state-year (1998) from 1977–2006. This measure computes the relative liberalism and conservatism of the Democratic and Republican parties in each state based on their roll-call voting records, and then weights these scores by the degree of Democratic control of the state legislature and the governorship. Although American Democrats would not be considered a “left” party in a cross-national context, in every U.S. state the Democratic Party is to the left of its complementary Republican Party (see, Gelman et al. 2008). Thus, in a broad sense, this concept captures the general propensity for a state government to engage in redistributive policymaking that is unfavorable and costly to business. Because these common space ideological measures are also pegged to national politics, we need not adjust for the considerable ideological variation across state parties (i.e., the measure recognizes that, for example, Democrats in Alabama are ideologically of a different stripe than Democrats in Vermont). Further, we believe that this ideological indicator is a more appropriate than a simple measure of a state government's partisanship since it accounts for the liberal criticism of Democrats that they have become as friendly toward or beholden to business as Republicans.

2.2 Relative Minimum Wage Rates

Our second dependent variable analyzes state minimum wage rates relative to the federal rate. That is, for each state-year observation, we calculate its relative minimum wage using the following equation:

$$\frac{\text{State Minimum Wage}_{it} - \text{Federal Minimum Wage}_t}{\text{Federal Minimum Wage}_t}$$

The quotients resulting from these calculations then served as the dependent variable in our analysis. Over the time period we examine, the federal government enacted few significant increases in the minimum wage, leaving most policymaking area in this area to the states. As a result, considerable variation exists in state-level minimum wage rates (Ford, Minor, and Owens 2012): at the low end, several states have no statutory minimum wage, and at the high end, Washington, with a minimum wage of \$7.63 in 2006, surpassed the federal minimum at that time by \$2.48. Although these rates most substantially affect the labor costs of firms in the service industry, we view them as a salient manifestation of the specific policies that many firms may seek to affect via their political engagement.

2.3 State Antitakeover Laws

Our last dependent variable is an index of six binary indicators that measures whether or not various state-level statutes that entrench management at the expense of shareholders by making corporate takeovers more difficult were in force. These data come from Bebchuk and Cohen (2003) and capture the following items:

- control share acquisition statute,
- fair price statute,
- no freeze-outs in the business combinations statute for up to 3 years,

- no freeze-outs in the business combinations statute for more than 3 years,
- poison pill endorsement via statute,
- constitutencies statutes.

The resulting index ranges from 0 to 5, as states can have only one of the two freeze-out provisions. Higher values on the index represent legal environments that are friendlier to management at the expense of shareholders.

As Bebchuk and Cohen (2003) note, these statutes are important both for what they actually measure and what they signal (409), especially with regard to managerial autonomy.³

The data for the antitakeover index span a shorter time period than those for the previous two dependent variables, and thus, our analysis of this outcome is restricted to 1987–2001.⁴ Despite the shorter time span, we still capture the most recent active period in the U.S. market for corporate law and the potential for an interstate “race to the bottom” to protect managers at shareholders’ expense (Bebchuk 1992; Bebchuk and Ferrell 1999).

3. Identification Strategy

Prior to the decision in *Citizens United*, 27 states already had independent expenditure rules similar to those ushered in at the national level and in the other 33 states by the decision. We exploited changes in these regimes at the state level to identify the policy effects of campaign finance reform. That is, we examined whether significant changes occurred in our

³ In theory, strong anti-takeover laws combined with managers that keep shareholders’ interests primary could be seen as an ideal combination for shareholders in that managers operating in such an environment could extract greater value from those seeking to acquire their firms. In practice, however, these laws are seen as providing managers greater autonomy from shareholders, allowing them to extract value from the firms they run (see, e.g., Bertrand and Mullainathan 1999).

⁴ The results for the models with the other two dependent variables are robust to limiting them to this same time frame.

three dependent variables when there was a change in campaign finance laws governing firms' independent political participation.

3.1 Hypotheses

As discussed above, the extant literature provides unclear guidance as to the effect of campaign finance regulation on broader policy and political outcomes. Instrumentalists would suggest that these indicators will vary systematically by the presence or absence of a corporate expenditure ban, but skeptics of regulation would argue that because the perceived “return on investment” for most individual firms' political contributions is negative or, at most, zero, few corporations ought to take advantage of looser campaign finance regimes and participate more heavily in electoral politics. As a result, at the aggregate level, public policies may bear little relationship to the campaign finance regimes of the states that produce them.

We occupy the same middle ground as those scholars who view agency problems as the root of corporate engagement in campaign finance. This viewpoint leads us to be agnostic as to the effects of an independent expenditure ban on left party power and relative minimum wage rates since they are of greater salience to the public and do not directly affect managerial autonomy; although, we will adopt the naïve and popular hypotheses that the presence of an independent expenditure ban will lead to increases in left party power and relative minimum wage rates. In contrast to our weak prior beliefs for left party power and the minimum wage, we hypothesize that the presence of a corporate independent expenditure ban will decrease the friendliness of antitakeover laws toward management, as managers will have less ability to use the political process to produce legislation that entrenches themselves at the expense of their shareholders.

3.2 Identification & the Error Correction Model

We identify the effect of corporate independent expenditure bans based on changes to state laws. During the period of 1976–2006, 14 states opted to ban unlimited corporate independent expenditures. If we exclude the 9 left-censored states that had such bans in place before 1976 for our left party power and relative minimum wage analyses and the 12 censored (pre-1986) states in our antitakeover analysis, we have 41 and 38 states, respectively, to observe in a time-series cross-section approach.⁵ We coded a binary variable indicating whether or not in each state-year dyad a ban on corporate independent expenditures was in effect using data from the National Conference of State Legislatures (NCSL).

Actual levels of corporate independent expenditures or corporate donations to independent expenditure committees would be a more richly informed treatment variable, but we believe that using this binary variable is defensible for two reasons. First, it is methodologically realistic. Attempting to gather, and to code consistently, data on independent expenditures across 50 states over 30 years is an essentially impossible task that would raise serious questions regarding data quality and validity. Ideally, these data would be available to include in our analyses, but they are not. Second, focusing on the legal environment captures instrumental theorists' arguments that business power stems not just from its actual political spending but also its potential spending and the threat of electoral intervention.

⁵ Excluding the left-censored states allows us to identify most cleanly the effect of a ban's imposition without introducing sample selection bias, as the censoring is a function of an independent variable, and the error term of a binary regression predicting whether a state had a ban in place prior to the start of our time series would likely not be correlated with the error terms of our main regressions. For each of our outcomes of interest, however, we also present specifications that were estimated using data from all 50 states. Across all of our models, the two specifications' results are consistent for the variables of interest.

For each of our three dependent variables, we ran an error correction model (ECM) using ordinary least squares regression with panel corrected standard errors. Error correction modeling is a time-series approach that can easily be applied to a panel framework (Beck 2001). Chief among its benefits are its lack of imposed restrictions and its use of the first-differenced value of the dependent variable, which assures us that our panels are of stationary processes. To assess statistical significance in an ECM, we examine both the lagged and differenced values for each independent variable, and if either is statistically significant, we can state that there is a significant association between that variable and the dependent variable (De Boef and Keele 2008). In terms of their substantive interpretation, the differenced measure captures the variable's immediate impact, while the lagged term, in combination with the lagged value for the dependent variable, captures the error correction component of the variable or its long-term impact.

Following Wilson and Butler (2007), we conducted various diagnostic tests to arrive at our final models. Although the ECM captures the dynamics of our data, it does not take account whether or not unit effects, period effects, autocorrelation, or heteroskedasticity need addressing. First, through a series of F-tests, we concluded that it was necessary to include both unit effects for states and period effects for years. We did so by including dummy variables for all states and years but one. In our tables below we omit these coefficients from our results, but the unit effects have the benefit of capturing non-time varying or very slow-moving differences across the states (e.g., political culture), and the year effects account for across time macro trends that impact all of the states at once. Second, after differencing our dependent variable and including unit and period effects, tests for serial autocorrelation revealed it was not present in any of our models.

Finally, tests also revealed that our panel-corrected standard errors were heteroskedastic; we corrected for this violation by using Huber-White robust standard errors.

Our final model, which we estimate for each of the three outcomes, is presented in equation (1):

$$\Delta y_{it} = \lambda y_{it-1} + \beta \Delta \mathbf{X}_{it} + \psi \mathbf{X}_{it-1} + \alpha_i + \gamma_t + u_{it} \quad (1)$$

where, Δy_{it} equals the change in the dependent variable (one of the three policy outcomes) between year t and year $t-1$, y_{it-1} equals the dependent variable in year $t-1$, \mathbf{X} represents a vector of independent variables that includes our corporate independent expenditure ban and measures that vary across the three dependent variables (detailed below), α_i captures our unit (state) effects, γ_t captures our period (year) effects, and u_{it} our error term.⁶

Since corporate independent expenditure ban adoptions at the state-level have historically been justified as anticorruption measures (see, e.g., the arguments and *amicus curiae* briefs in the U.S. Supreme Court case challenging Montana’s ban mentioned earlier, *American Tradition Partnership, Inc. v. Bullock*) and were not adopted specifically in response to trends in any of our dependent variables, the threat of legislative endogeneity is low, and we can regard our variable for independent expenditure bans as largely exogenous to our three policy outcomes.⁷ As a result, we can formally identify the treatment effect (ψ or $\beta_{CorpIEBan}$) – that is, the average

⁶ Since our dependent variable is a differenced value that can take on a negative value, for the antitakeover index model, we treat this differenced value as a continuous variable and employ OLS regression rather than a panel negative binomial count model.

⁷ Further, in the model for which we have a significant result (antitakeover laws), both the dependent variable and the independent variable are the creation of the legislature, so it is unlikely that the relationship between the two is endogenous or that trends in antitakeover statutes, which are purposefully chosen by the legislature, place pressure on these very same actors to change campaign finance law rather than the absence of a ban on independent expenditures leading to more pro-management laws. We examine the dynamics of this relationship further by conducting a lead and lag analysis of trends in the dependent variable around the reform in section 4.4.

effect of a corporate independent expenditure ban has on each dependent variable – in equation (2) as:

$$\beta \text{ or } \psi_{CorpIEBan} = \mathbf{E}[\Delta y_{it} | y_{it-1}, \alpha_i, \gamma_t, \Delta \mathbf{X}_{it}, \mathbf{X}_{it-1}, CorpIEBan_{it \text{ or } it-1} = 1] - \mathbf{E}[\Delta y_{it} | y_{it-1}, \alpha_i, \gamma_t, \Delta \mathbf{X}_{it}, \mathbf{X}_{it-1}, CorpIEBan_{it \text{ or } it-1} = 0] \quad (2)$$

Testing whether or not these coefficients for the corporate independent expenditure ban equal 0 provides a direct test of whether or not the presence of the ban affects our dependent variable, and depending upon which coefficient (or both) is significant, we will also know if the effect is sudden or more gradual (or has elements of both).

3.3 Additional Independent Variables

In addition to our independent variable of interest, we included controls for political, demographic, and economic factors in all of our specifications. Across all three analyses, we used two additional variables to capture the remainder of the campaign finance regulatory environment. First, to examine the potential for countervailing power, we coded whether or not the state had a union independent expenditure ban using NCSL data. Second, to capture other, non-independent expenditure campaign finance effects we included an index of campaign finance laws. Primo and Milyo (2006) construct a 0 to 5 index that is a sum of the presence/absence of various campaign finance provisions by state-year and includes measures tapping donation limits, disclosure laws, and public funding. Using Primo and Milyo's underlying data, as well as data gathered by Werner and Mayer (2012), we expanded the 0 to 5

scale to a 0 to 10 scale. The 10 items in our campaign finance index are the absence (0) or presence (1) of:

- disclosure law,
- limits on individuals' donations to candidates,
- limits on organizations' donations to candidates,
- a ban on corporate contributions to candidates,
- a ban on union contributions to candidates,
- partial public funding for legislative candidates,
- full public funding for legislative candidates,
- public funding for gubernatorial candidates,
- expenditure limits for legislative candidates in the pre-*Buckley v. Valeo* era,
- expenditure limits for gubernatorial candidates in the pre-*Buckley v. Valeo* era.

In 1976, at the beginning of our analysis, states averaged 2.74 laws (standard deviation 1.27) and in 2006 states averaged 3.70 laws (standard deviation 1.46). The median number of reforms in place rose from 3 to 4 over the period, with the maximum score rising from 5 to 7 over that time. Generally, a score of 0 indicates a relatively unrestricted campaign finance environment for private money and a score of 10 indicates a wide range of restrictions on private money in campaigns.⁸

In addition to including the lagged and differenced versions of the three campaign finance variables, to predict the differenced value of left party power, we included the lags and differences for a series of variables tapping how left-leaning a state's government is likely to be.

⁸ Cronbach's alpha testing for the internal consistency of the scale ranges from .60 to .65, depending on the specifications of the analysis, which is a marginally acceptable coefficient. Because alpha is susceptible to higher values as the number of items in a scale increases, and our scale has 10 items, in future versions of the paper we will explore alternative specifications of the campaign finance index.

These variables included a revealed measure of the state electorate's preferences (the Democratic percentage of the two-party vote in the most recent presidential election), an expressed measure of the state's electorate's preferences (the citizen ideology measure from Berry et al. 1998), and various measures of the state's demographics. These demographic measures include the state's urban, non-white, and over 65 populations by percentage, as well as the percentages holding at least a college degree and employed in manufacturing.

Finally, because unions attempt to influence electoral and policy outcomes via non-campaign finance routes, we also included the percentage of each state-cycle's non farm-based workforce that is unionized (using data collected by Hirsch and Macpherson 2003 and updates on their website). Combined with the unit and period effects, we believe that these covariates effectively tap competing explanations for elites' liberalism.

To predict the relative minimum wage, in addition to its own lag and the lags and differences of the campaign finance and union density variables, we used the lagged and differenced values of control variables selected from a review of the existing literature on state minimum and living wage rates (Ford, Minor, and Owens 2012; Gallet 2004; Waltman and Pittman 2002), as well as the more general literature on social policy competition between the states (e.g., Berry, Fording, and Hanson 2003). First, to capture state-level economic conditions, we included the percentage of the adult population employed in the state, as well as the real (in thousands of 2006 Illinois dollars, adjusted following the approach developed by Berry, Fording, and Hanson 2000) disposable income per capita. Second, to account for additional political forces that might pressure states to adjust their relative minimum wage, we included citizen ideology, institutional ideology (the dependent variable in our left party power model), and the percentages of workers in farm-based employment, of the population between 18 and 24 years of

age, and of the population that is non-white. Third, to control for dynamics due to neighboring states' decisions, we included the population-weighted relative minimum wage in neighboring states.⁹ Finally, in a third specification, to account for national influences on state decisions, we counted the number of years since the last federal minimum wage increase and also accounted for the national inflation rate, as several state minimum wages are pegged to it. Including these national-level variables required us to drop our period effects from this specification.

We supplemented these policy-specific indicators by including a measure of business' lobbying power. To capture this last concept, we included the number of business associations existing in each state-year using data gathered by Spillman (2003) from the *Encyclopedia of Associations*, *National Trade and Professional Association Directory*, and *Associations Yellowbook*.¹⁰ This measure is a raw count of associations for each state by year, constructed based upon founding dates. Although it might seem appropriate to adjust this count for state population, doing so could be misleading. Nearly all states will have single associations representing their major industries and sectors, rather than some multiple of these associations as a state enlarges. There will be one state bar association representing attorneys, for example. Economically more diverse states will likely generate more associations, and we believe the raw count of associations best reflects their potential impact on policy outcomes.

In our antitakeover model, we included the three campaign finance variables, the non-farm percent unionized, the total number of business associations existing, left party power, the percent unemployed, and the percent change in gross state product. Lags and differences of these

⁹ We employed California as Hawaii's neighbor and Washington State as Alaska's neighbor when calculating this indicator.

¹⁰ Spillman collected these data from recent editions of these sources only, which suggests that there may be a mortality bias in our use of her data. That is, although we calculate our counts for each year based upon the year the various organizations listed as their founding, if an organization from the 1970s did not survive into the 2000s, then it would not be captured in the Spillman data, and thus, there may be a slight undercount of associations.

indicators were included, in addition to the unit and period effects, in order to capture potential political and economic determinates of corporate governance laws. In addition, and as in the minimum wage model, we included a third specification in our antitakeover model that required us to drop our period effects. Since antitakeover laws may result from interstate competition or competitive federalism (Cary 1974; Bebchuk and Ferrell 1999), we included a by-year, nationwide average index value to capture this dynamic.

4. Results and Extensions

The next sections discuss the results across our three models, as well as extensions and robustness checks of our findings. To preview our results, we find little evidence that campaign finance regimes affect policy liberalism and minimum wage rates, but strong evidence that these laws do influence policy in the less salient but still important area of managerial autonomy.

4.1 Left Party Power

We turn first to an analysis of general political outcomes in a state, specifically left party power. Results are presented in Table 1. Focusing first on the variables of chief theoretical and empirical interest, none of the campaign finance variables achieve statistical significance. There are no significant effects for the binary indicators capturing corporate and union spending bans, nor is there an effect for the campaign finance index. Bans on corporate and union independent expenditures and a higher overall level of campaign finance regulation in a state are not associated with the degree of liberalism among state elected officials. Similarly, unionization rates, a measure of the degree to which labor may mobilize its supporters in electoral politics, have no effect on left party power.

[Insert Table 1 about here.]

Of the control variables in our model, the most interesting significant result was the positive relationship between citizen ideology and left party power: as both the lagged and differenced value of this variable increased (as citizens became more liberal), so too did elected officials' liberalism, indicating that there is a strong substantive association between these two measures that operates in both the short and long term. This result provides support for the view that elections promote policy responsiveness. Similarly, the positive and significant result for the percentage of the Democratic share of the two-party presidential vote also reflects a positive correlation between citizens' revealed preferences and the policy decisions of their elected officials.

4.2 Relative Minimum Wage

The results for the relative minimum wage parallel those for left party power. Table 2 shows that neither the bans on corporate and union spending nor the campaign finance index have a significant effect on the relative minimum wage. The results diverge from those on left party power in that the union density measure has a significant relationship to the minimum wage, but it is signed opposite of expectation.

[Insert Table 2 about here.]

Of the variables tapping business influence, only the coefficient for the differenced version of the number of business associations is significant, and it is significant in only one of three specifications. In the third specification, as business associations increase in number, and likely spend more time lobbying relevant policymakers, the relative minimum wage decreases. To illustrate, when we set all variables in the first specification to their means (the mean

difference in the number of business associations is 0.902), the predicted relative minimum wage level is 1.42% above the federal minimum wage. After increasing the differenced number of business associations from its mean by one standard deviation (1.82), the predicted relative minimum wage level decreases substantially, to just 0.53% above than the federal minimum wage, a reduction of nearly two-thirds in the average state's relative minimum wage rate.

The significant results among our control variables in the third specification of the minimum wage model were signed as expected and in-line with previous research in this area. For economic factors, the national inflation rate was significant and positively signed, indicating that as inflation ate away at the minimum wage's effectiveness, a state was more likely to increase its relative minimum wage. Political factors also contributed to minimum-wage decision-making. When the federal government failed to act to adjust the wage and as the state government became more liberal, a state was more likely to increase its relative minimum wage.

4.3 State Antitakeover Laws

Our final test focuses on agency-related explanations for corporate campaign spending. In this analysis, we find significant and consistent results for campaign finance law on the friendliness toward management of state antitakeover laws. The presence of a corporate independent expenditure ban has an immediate and negative (for management) impact on the antitakeover index. Substantively, the average effect is comparable to the legislature repealing 0.5 to 1 pro-management statutes in a year.

[Insert Table 3 about here.]

This finding holds across two alternative specifications. First, even though it weakens our identification strategy, we include the 12 left-censored states in a second specification (thus,

allowing us to include all 50 states in our analysis), and although the statistical significance and substantive magnitude of the finding decline slightly, both remain above conventional thresholds. Second, in an additional robustness check of this finding, we drop our period effects and instead include a nationwide average of the antitakeover index that captures the potential for competitive federalism to affect states' corporate governance law. Unsurprisingly, this variable is significant in both the short and long run as states adjust to each other's behavior, but more importantly, our central finding for corporate independent expenditure bans retains its statistical and substantive significance.

In terms of the other variables that are statistically significant, there are interesting and contradictory associations between union power and antitakeover statutes. While a union expenditure ban is associated with higher levels of the antitakeover index (i.e., more pro-management laws), a result that squares with traditional antagonisms between labor and management, the percentage of the non-farm workforce that is unionized too is associated with higher levels of the index. Although we cannot make causal claims regarding the nature of these relationships, it does appear that centrally directed electoral activity (i.e., campaign expenditures) aids unions in their quest to shape public policy to a greater extent than the mere presence of their members. With the exception of the national average antitakeover index level, few of the non-political indicators appear to affect the antitakeover law index, especially when those states that adopted a corporate independent expenditure ban prior to 1986 are excluded.

To check our identification strategy, and as a falsification test of these panel results, we ran a cross-sectional fixed effects logistic regression model in which we reversed our causal effect, using the lagged value of the antitakeover index to predict whether a state had ban on corporate independent expenditures in the current period. That is, this test assessed whether a

state's imposition of an expenditure ban was exogenous to the level of its antitakeover index.

The result of this regression revealed that the lagged antitakeover index was a poor predictor of a state's adoption of an independent expenditure ban: although it was positively signed, it was not statistically significant ($p > 0.178$). This test lends credence to our argument that the adoption and implementation of an independent expenditure ban causes corporate governance lawmaking at the state legislative level to shift in a less pro-management direction, rather than this lawmaking influencing the presence of a corporate expenditure ban.

4.4 Lead and Lag Extension of State Antitakeover Laws

Although Table 4 reveals that there is a statistically significant and negative relationship between the presence of a corporate independent expenditure ban and a state's antitakeover index, a lead and lag analysis around the adoption of such bans can further reveal the dynamics of this relationship. To conduct this analysis, we reran the first specification in Table 4 and included 10 binary indicators to capture the 4 years leading up to reform, the year of reform, and the 5 years following reform in each state (we omitted an eleventh indicator [-5 years] to serve as a baseline).

[Insert Table 4 about here.]

Table 5 reveals the result of this trend analysis. Most of the change in antitakeover laws around the adoption of a corporate expenditure ban occurs prior to reform. That is, prior to corporate independent expenditures being banned in a state, legislatures appear to engage in policymaking that makes corporate governance laws more favorable to management. This result substantiates the earlier claims of Vogel (1978) and others who, based upon qualitative evidence,

argue that managers engage in corporate political activity with the goal of securing their autonomy from both their shareholders and the state.

[Insert Figure 1 about here.]

Figure 1 plots the results of Table 5 and illustrates the trend that appears around the adoption of corporate independent expenditure bans. In the 10 years surrounding a ban adoption, there is a downward movement in the trend, with a significant break coming during the year of reform. Although the only coefficients that achieve statistical significance (and thus, do not have confidence intervals that cross zero) are -3 and -1 years from ban adoption, the point estimates nevertheless reveal that once independent expenditure bans are enacted, legislatures appear to halt adopting antitakeover laws that favor management at shareholders' expense.

5. Conclusion

The U.S. Supreme Court's 2010 decision in *Citizens United v. FEC* was one of its most controversial in decades. Decried by critics as an assault on democracy and praised by supporters as a strong defense of the free speech rights, the decision led to both the fears and hopes that political and policy outcomes might change as a result of the case and subsequent decisions by lower federal courts and the Federal Election Commission.

With the decision so recent, however, there has been insufficient time to examine its potential impact on national political and policy outcomes. For analytical purposes, the system of federalism in the United States provides an opportunity to assess the possible effects of *Citizens United*. Prior to the Supreme Court's decision, about half the states allowed the kinds of corporate and union independent expenditures in state elections that were given the Court's approval in the *Citizens United* decision concerning federal elections.

Using a time-series, cross-sectional analysis covering all states from 1977 through 2006, we examined the effect of corporate independent expenditure bans and other campaign finance reforms on three key political and policy outcomes: the degree of left party power in a state; the relative minimum wage of a state; and the friendliness of state antitakeover laws toward management. We find no support for the popular and naïve hypotheses that the kind of corporate independent expenditures permitted by *Citizens United* significantly affect either left party power or relative minimum wage rates.

However, we do find a systematic relationship between the presence of independent expenditure bans and antitakeover laws. A lead and lag analysis of this relationship reveals that this effect is primarily driven by favorable policymaking when such bans are not in place. That is, when corporations are allowed to make unlimited independent expenditures, legislators appear to enact corporate governance regimes that are more favorable to management; in contrast, when legislators ban such expenditures, they do not repeal these pro-management laws, but they appear to halt efforts to make policy any friendlier to management. Overall, this set of findings suggests that the most general fears expressed by critics of the Court's decision have not been borne out in the experience of the states, but in the more specific area of shareholder protection, there is evidence that these protections were stronger in non-independent expenditure states than in states that allowed them.

At the national level, these lessons suggest that general political outcomes or policy outcomes are unlikely to change as a result of *Citizens United*, but in some more targeted areas effects might be found. In one sense, this means no more than that actors who are able to engage in political activity, whether lobbying, campaign contributions, or filing lawsuits, will achieve some narrow successes but will not systematically move policy or political outcomes broadly in

one direction or another. Independent expenditures are simply one more possible tool of political activity that may have effects similar to and as limited as these more common techniques.

What might explain the relative lack of connections between independent expenditure bans and broader political and policy outcomes? In addition to examining other policy areas to see how well the results in this paper travel to those domains, future research could explore several possibilities (Coleman 2010). Perhaps in states where businesses do not face spending restrictions, opponents are themselves more aggressive in this competitive environment. The splits among businesses mentioned earlier might blunt corporate influence. The difference between restrictive and non-restrictive states could have been blurred by election-motivated issue ads in the restrictive states. Lifting bans on corporate spending can benefit multiple corporate forms, including nonprofit corporations and groups (Citizens United itself was a nonprofit entity). Lastly, the analysis here does not consider the level of spending in states that allowed corporate and union spending. Although we believe that comparing the presence or absence of bans is the most direct test of the potential effects of *Citizens United*, we do not doubt that examining variations in spending within the “no-ban” groups across time would be of interest. As we mention earlier, however, a major, most likely insurmountable, obstacle would be the absence of reliable spending data at the state level across time.

In addition to considering the impact of various campaign finance regimes on political and policy outcomes, future research can also shed light on whether there is any link between these regulations, and particularly corporate and union spending bans, and the presence of corruption in a state (see, e.g., Cordis and Milyo 2013). The compelling governmental interest the Supreme Court has identified that allows limits on campaign contributions is not whether policy tilts to the left or right, whether one party wins “too many” or “too few” seats, or whether

managers are exploiting agency problems.¹¹ Rather, the interest that the Court has concluded provides sufficient justification to restrict contributions is the reality or appearance of the potential corrupting effects of campaign contributions received directly by candidates and parties. Independent expenditures were less problematic, the Court reasoned, because they are not under the control of a candidate or party and could not be coordinated with these actors. As we noted at the outset, neither the critics of the Court's decision nor the supporters have marshaled systematic evidence to support their view that independent expenditures have or do not have corrupting implications, making this a ripe area for future investigation.

¹¹ Of course an alternative, non-Court-based route to remedying the agency problems multiplied by *Citizens United* would be for the Securities and Exchange Commission or Congress to classify political engagement as non-ordinary business activity and to allow shareholders significant control over it.

Table 1: Error Correction Panel Model of State Left Party Power, 1977–2006

		States Without Pre-1976 Independent Expenditure Bans			
		Expenditure Bans		All States	
		Coeff.	Std. Error	Coeff.	Std. Error
Left Party Power	L	-0.253***	(0.021)	-0.253***	(0.019)
Corporate Spending Ban	L	-0.002	(0.025)	-0.008	(0.025)
	D	-0.055	(0.051)	-0.064	(0.052)
Union Spending Ban	L	-0.018	(0.028)	-0.011	(0.028)
	D	0.020	(0.060)	0.028	(0.060)
Campaign Finance Index	L	-0.006	(0.005)	-0.007	(0.004)
	D	-0.004	(0.010)	-0.001	(0.009)
% Democratic Two-Party Presidential Vote	L	0.156*	(0.088)	0.124	(0.084)
	D	-0.049	(0.125)	-0.005	(0.120)
Citizen Ideology	L	0.311***	(0.060)	0.319***	(0.056)
	D	0.954***	(0.057)	0.940***	(0.054)
% Union	L	-0.224	(0.206)	-0.152	(0.187)
	D	-0.005	(0.213)	0.095	(0.202)
% Urban	L	-0.249*	(0.142)	-0.051	(0.135)
	D	-0.132	(0.344)	-0.022	(0.327)
% Nonwhite	L	-0.007	(0.099)	-0.021	(0.100)
	D	0.303	(0.370)	0.271	(0.365)
% 65+	L	-0.060	(0.539)	-0.054	(0.524)
	D	2.757	(2.889)	2.433	(2.807)
% College +	L	-0.108	(0.345)	-0.024	(0.301)
	D	-0.228	(0.824)	-0.591	(0.734)
% Manufacturing	L	-0.080	(0.106)	-0.071	(0.101)
	D	0.458*	(0.277)	0.260	(0.262)
<i>n (observations)</i>			1271		1550
<i>n (states)</i>			41		50
<i>r</i> ²			0.352		0.348

L = Lag term; D = Difference term

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Error correction model with robust and panel corrected standard errors. Dependent variable is the differenced left party power by state-year; state and year effects are suppressed for space.

Table 2: Error Correction Panel Model of State Relative Minimum Wage Rates, 1977–2006

		States Without Pre-1976 Independent Expenditure Bans		All States		States Without Pre-1976 Independent Expenditure Bans	
		Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Relative Minimum Wage	L	-0.177***	(0.024)	-0.184***	(0.023)	-0.191***	(0.024)
Corporate Spending Ban	L	-0.011	(0.021)	-0.007	(0.020)	-0.004	(0.021)
	D	-0.041	(0.052)	-0.043	(0.052)	0.072	(0.054)
Union Spending Ban	L	-0.007	(0.022)	-0.008	(0.022)	-0.020	(0.023)
	D	-0.035	(0.053)	-0.035	(0.053)	-0.075	(0.055)
Campaign Finance Index	L	0.003	(0.004)	0.003	(0.004)	0.003	(0.004)
	D	0.008	(0.008)	0.008	(0.007)	0.011	(0.008)
% Union	L	-0.004**	(0.002)	-0.002	(0.001)	-0.002	(0.002)
	D	-0.003	(0.002)	-0.001	(0.002)	0.001	(0.002)
Total Business Associations Existing	L	-0.001	(0.001)	-0.001	(0.001)	-0.001	(0.001)
	D	-0.005	(0.003)	-0.004	(0.002)	-0.005**	(0.003)
Citizen Ideology	L	-0.001	(0.001)	-0.001	(0.001)	0.001	(0.001)
	D	-0.001	(0.001)	-0.001	(0.001)	0.001	(0.001)
Left Party Power	L	0.001	(0.001)	0.001*	(0.001)	0.001*	(0.001)
	D	-0.001	(0.001)	-0.001	(0.001)	-0.001	(0.001)
Weighted Relative Minimum Wage in Neighboring States	L	0.023	(0.028)	0.027	(0.025)	0.025	(0.028)
	D	0.074	(0.045)	0.079	(0.042)	0.348	(0.039)
% Employed	L	0.002	(0.002)	0.001	(0.001)	0.001	(0.001)
	D	0.004	(0.004)	0.006*	(0.004)	0.003	(0.004)
% Farm Employment	L	-0.002	(0.004)	-0.003	(0.003)	-0.001	(0.004)
	D	0.001	(0.002)	-0.015	(0.014)	-0.002	(0.014)
% 18-24	L	-0.001	(0.003)	-0.001	(0.003)	-0.003	(0.003)
	D	0.001	(0.005)	0.001	(0.004)	0.004	(0.005)
% Nonwhite	L	0.002**	(0.001)	0.002***	(0.001)	0.002	(0.001)
	D	0.002	(0.003)	0.002	(0.002)	-0.003	(0.003)
Real Disposable Income per capita (\$1000)	L	0.002	(0.002)	0.003	(0.002)	0.001	(0.002)
	D	-0.004	(0.005)	0.992	(0.005)	-0.003	(0.004)
Years Since Federal Increase	L					0.001	(0.0001)
	D					0.008***	(0.002)
National Inflation Rate	L					0.004**	(0.002)
	D					-0.002	(0.002)
<i>n</i> (observations)			1271		1550		1271
<i>n</i> (states)			41		50		41
R^2			0.318		0.353		0.241

L = Lag term; D = Difference term

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Error correction model with robust and panel corrected standard errors. Dependent variable is the differenced relative minimum wage rate by state-year; state and year effects are included in the first two specifications but are suppressed for space. State effects alone are included in the final specification.

Table 3: Error Correction Panel Model of State Antitakeover Laws, 1987–2001

		States Without Pre-1986 Independent Expenditure Bans		All States		States Without Pre-1986 Independent Expenditure Bans	
		Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Antitakeover Laws	L	-0.398***	(0.043)	-0.433***	(0.036)	-0.398***	(0.044)
Corporate Spending Ban	L	-0.648**	(0.282)	-0.549**	(0.275)	-0.627**	(0.275)
	D	-0.361	(0.335)	-0.363	(0.328)	-0.348	(0.326)
Union Spending Ban	L	0.608**	(0.308)	0.560*	(0.303)	0.588*	(0.301)
	D	0.447	(0.409)	0.456	(0.405)	0.400	(0.404)
Campaign Finance Index	L	-0.012	(0.047)	-0.004	(0.046)	0.001	(0.047)
	D	-0.040	(0.068)	-0.027	(0.063)	-0.024	(0.068)
% Union	L	4.416**	(2.063)	2.653	(1.789)	3.288*	(1.719)
	D	3.403*	(2.046)	1.138	(1.804)	2.782	(1.973)
Total Business Associations Existing	L	0.002	(0.004)	0.006	(0.004)	0.002	(0.004)
	D	0.016	(0.017)	0.025	(0.017)	0.015	(0.017)
Left Party Power	L	-0.151	(0.124)	-0.108	(0.098)	-0.167	(0.127)
	D	-0.174	(0.171)	-0.377***	(0.146)	-0.194	(0.163)
% Unemployed	L	2.156	(1.985)	1.600	(1.700)	-1.037	(1.797)
	D	5.076*	(3.025)	5.276**	(2.669)	2.209	(2.535)
% Change in Gross State Product	L	-0.155	(0.421)	-0.235	(0.403)	-0.012	(0.375)
	D	-0.234	(1.626)	-0.889	(1.575)	0.034	(1.325)
Nationwide Antitakeover Index Average	L					0.338***	(0.081)
	D					0.546**	(0.235)
<i>n</i> (observations)		570		750		570	
<i>n</i> (states)		38		50		38	
<i>r</i> ²		0.405		0.439		0.380	

L = Lag term; D = Difference term

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Error correction model with robust and panel corrected standard errors. Dependent variable is the differenced state antitakeover law index by state-year; state and year effects are included in the first two specifications but are suppressed for space. State effects alone are included in the final specification.

Table 4: Effect of Corporate Independent Expenditure Bans on State Antitakeover Laws, by Year from Ban

	Difference in Antitakeover Laws
-4 years ban	0.043 (0.224)
-3 years ban	0.479** (0.225)
-2 years ban	0.265 (0.226)
-1 year ban	0.487** (0.203)
Year of reform	-0.021 (0.261)
+1 year ban	-0.063 (0.173)
+2 years ban	-0.202 (0.200)
+3 years ban	-0.040 (0.200)
+4 years ban	-0.153 (0.172)
+5 years ban	-0.154 (0.170)
r^2	0.413

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Error correction model with robust and panel corrected standard errors. Dependent variable is the differenced antitakeover index by state-year; independent variables from the first specification in Table 4 and state and year effects are suppressed for space. States that adopted a corporate independent expenditure ban prior to 1986 are excluded.

$n = 570$ (obs), 38 (states)

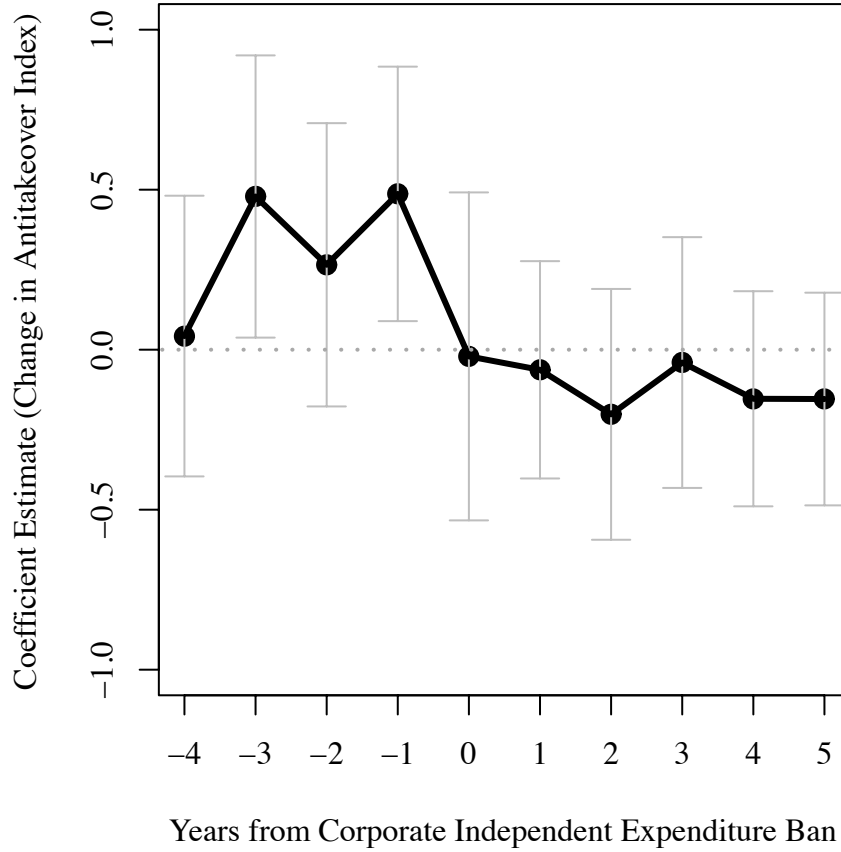


Figure 1: Coefficient estimates for Leads and Lags from Table 5, with 95% confidence intervals. Sample excludes states that enacted a corporate independent expenditure ban prior to 1986.

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